Essays on Insurance Policy and Provider Choice

Essays on Insurance Policy and Provider Choices

A dissertation presented

by

Ruohua Annetta Zhou

to

The Harvard Committee on Higher Degrees in Health Policy

in partial fulfillment of the requirements

for the degree of

Doctor of Philosophy

in the subject of

Health Policy (Economics)

Harvard University

Cambridge, Massachusetts

May 2018

Dissertation Advisors:
David M. Cutler (Chair)

Author:
Ruohua Annetta Zhou

Essays on Insurance Policy and Provider Choices

Abstract

This dissertation includes one essay on the role of reputation on patients' choice of providers, one on the effect of Medicaid expansion on hospital finances and strategy, and one observational study on the heterogeneity of provider site choices by insurance status.

Chapter One studies the information content of third-party awarded "top doctor" awards and examines the causal effect of these awards on patients' choice of surgeons and surgeons' practice patterns. I find that cardiac surgeons who have been winners of "top doctor" awards have risk-adjusted mortality rates for corollary bypass surgery procedures 0.15 standard deviations below the average cardiac surgeon. This holds true even conditional on other provider characteristics that may be publicly available, including experience, previous volume, the prestige of the hospital, and training background. I find no evidence that award-winning increased Medicare FFS inpatient or outpatient volume. There is small evidence that award-winning increased the share of a surgeon's patients who traveled from far-away. Award-winning did not increase the share of a surgeon's patients who were medically complex. I also find that winning a "top doctor" award more than doubled the likelihood that a cardiac surgeon or an orthopedic surgeon practiced at a specialty hospital. Overall, the results suggest that demand responses to "top doctor" awards are modest in the Medicare FFS market but likely exists in other markets.

Chapter Two studies the effect of the Medicaid expansion as part of the Affordable Care Act on hospital financial status and strategy. I found that Medicaid expansion as part of the Affordable Care Act increased Medicaid revenue and decreased the cost of uncompensated care. However, the increase in Medicaid revenue did not translate into increases in total revenue for private hospitals, suggesting that the financial gain from Medicaid expansion is offset in other channels.

This offset does not exist in public hospitals, which experienced an increase in their total revenue as a result of the Medicaid expansion. I argue that these results are explained by a model where hospitals are risk-averse firms that incur nonmonetary "strategic search cost" when searching for ways to improve their financial status. In this model, a Medicaid revenue gain decreases the marginal return of additional effort invested in generating revenue elsewhere and reduces the total amount of such effort, leading to an offset in total revenue. Consistent with the finding that Medicaid expansion only leads to an improvement in hospital's overall financial health for public hospitals, I found that public hospitals in Medicaid expansion states increased their salary expenditure and capital investment post-expansion. I found no evidence of an effect of Medicaid expansion on hospital expenditure, capital investment, or 30-day readmission or mortality rates of the Medicare population for private hospitals.

Chapter Three (with Katherine Baicker, Sarah Taubman, and Amy N. Finkelstein) addresses a common misperception that the uninsured use the ED more than the insured. We show that uninsured and insured adults use the ED at the same rate - and in very similar circumstances. The disconnect between this fact and widespread perceptions may be because uninsured adults use other forms of care, such as doctor visits and hospitalizations, substantially less. In other words, the uninsured are not more likely than the insured to use the ED, but the uninsured do get a disproportionate share of their care in the ED. These patterns should help inform ongoing discussions about health insurance reform.

Contents

List of Tables

List of Figures

Acknowledgments

First and foremost, I want to thank my dissertation committee - David, Amy, and Bapu - for their time and feedback. I couldn't have asked for a better group of mentors and role models.

To Joe Newhouse, Tom McGuire, Kathy Swartz, and Kate Baicker, for their support and advice through my Ph.D journey.

To Jean Roth and Mohan Ramanujan for their support with data.

To Debbie Whitney, Colleen Yout, Jessica Livingston, Ayers Heller, and Clare Dingwell. This journey is much calmer with all of your help.

To my partners in crime in the economics track, Katherine Donato, Cassie Peitzman, Hannah Neprash, Daria Pelech, and Jacob Wallace.

To Mark, for your unconditional, incentive-incompatible love. To Lillian, for always putting a smile on my face and for showing me that there are more difficult things than finishing a Ph.D.

To my parents Jianxin Zhou and Fangming Zhang, for always being there for me.

Chapter 1

Superstar Surgeons: the Effect of Reputation on Surgeon Behavior and Patient Demand

1.1 Introduction

Reputation – beliefs or opinions of desirable qualities – is an important factor in health care markets. In a survey study of Medicare beneficiaries, over 80% of respondents reported that the reputation of a surgeon or a hospital is "very important" or "extremely important" in their decision of where to go for major surgery(Schwartz et al., 2005). Although reputation is frequently mentioned as a factor that affects both patient and provider behavior, its empirical effects on market outcomes are still not well understood. Part of the challenge is that reputation may be generated through a complex process, making it especially difficult to measure: it may come from word of mouth among colleagues or patients, online ratings, public recognition of awards and titles, or marketing and publicity.

Understanding the role of reputation in health care market is important for several reasons. First, there is growing interest in leveraging market forces to improve the quality and efficiency of health care. Whether these policies would be successful depends on whether patients and their referring doctors are actively responding to information that is available to them. On the one hand, a large literature has found that travel distance plays an overwhelming role in provider choice[1]. This literature would suggest that convenience is the main determinant of provider choice. On the other hand, studies on the effect of quality measures on demand have found only modest responses (Chandra et al., 2015, Cutler et al., 2004, Johnson, 2011, Kolstad, 2013). One interpretation of such findings is that the role of consumer-driven health care is limited. An alternative explanation is that quality reporting does not capture factors that patients are using to choose providers. In addition, reputation may also have important implication for provider behavior, either because it brings additional patient demand (extrinsic motivation), or because it directly brings satisfaction (intrinsic motivation) (Kolstad, 2013). Understanding the role of reputation in motivating provider behavior is important for designing optimal incentive structure and payment policy.

In this study, I empirically study the effect of surgeon reputation on patient demand and provider behavior using data on third-party awarded "top doctor" awards. I first present the characteristics of "top doctor" winners to illustrate the type of information that these awards may capture. I show that "top doctor" winners have higher procedure volume, are more experienced, are more likely to be graduates of high-ranked medical schools, and are more likely to practice at a hospital ranked high in the U.S. World and New Report. Winners are also more likely to work on more intensive cases, as indicated by higher Medicare facility payment per claim. Among cardiac surgeons in New York, California, and Pennsylvania, winners of "top doctor" awards have risk-adjusted mortality rates for corollary bypass surgery procedures 0.15 standard deviations below the average cardiac surgeon. This holds true even conditional on other provider characteristics that may be publicly available, including experience, previous volume, the prestige of the hospital, and training background. This difference suggests that a physician's social network captures valuable quality information that's not available from observable physician characteristics. Releasing such information to the public may have potential to direct more choice towards the higher-quality providers.

---

[1] For example, (Ho, 2006) finds that If a hospital moves an additional mile away from a patient's home, this reduces the probability that the patient will choose it by 21%.

I then examine three types of outcomes to access the responses of patient demand and surgeon behavior to "top doctor" awards: patient volume, patient composition, and practice settings. I use an event-study framework with a set of propensity-score matched control group. The event of interest is the first time when a surgeon wins a "top doctor" award. I find that "top doctor" winners did not consistently see an increase in either Medicare FFS inpatient or outpatient volume after winning an award for the first time, relative to a group of non-winners with similar characteristics.

In some cases, such as when supply is inelastic, demand responses may not be reflected in quantity changes. Inelastic supply and excess demand are likely to be true in the market for experienced surgeons. For surgeons who are likely to become "top doctor" winners, I find their Medicare FFS inpatient volume decreased with experience, but their outpatient volume and share of patients who travel a long distance to seek care increased with experience, even after controlling for the underlying time trend. These different patterns suggest that patients value surgeon experience, but supply-side decisions on how a surgeon wants to allocate her time may dictate procedure volume at the surgeon level more than demand.

However, even in markets with inelastic supply and excess demand, demand responses to reputation may still lead to other observable changes in market equilibrium through either monetary or non-monetary prices. Monetary price is administratively set in Medicare FFS, but the reallocation of non-monetary prices, such as travel distance and wait time, may lead to changes in patient composition. To test this hypothesis, I examine the effect of award-winning on two dimensions of patient composition: travel distance and case complexity. I find that award-winning led to a small change in the share of a surgeon's Medicare FFS patients who come from far-away but did not change the share of patients with complex cases, as measured by Exlihauser comorbidity score or facility claims.

Last, I look at whether award-winning changed the practice setting of surgeons. Additional reputation may increase a surgeon's bargaining power to negotiate more favorable work situations. For example, with additional reputation, a surgeon may rely less on the resources of a general hospital for referrals and move toward specialty hospitals. In these specialty hospitals, they may have greater financial gain through partial ownership and more scheduling flexibility. Two of the most common types of specialty hospitals are cardiac specialty hospitals and orthopedic specialty hospitals. I find that award-winning led to a large increase in the likelihood of a surgeon practicing at a specialty hospital for both cardiac and orthopedic surgeons. Overall, the results suggest that demand responses to "top doctor" awards may be modest in the Medicare FFS market but likely exists elsewhere.

1.2 Existing Literature

There is a large and growing literature on the effect of "information on quality" on market outcomes in health care and other markets. The exact information format and media studied vary greatly. Much of this literature does not explicitly differentiate "quality" and "reputation". Part of the reason is that reputation can be a function of true quality. However, reputation is also affected by many factors beyond quality. In health care markets, the link between reputation and quality may be especially spurious because quality in this market can be difficult to define, measure and interpret.

I organize these various information formats studied in the literature into a spectrum represented in Figure 1.1. On the left side of the spectrum is pure information provision of quality measures. Information formats on this end of the spectrum are very data-oriented: inputs are precisely defined, the calculation of output measures are highly quantitative, and the output is usually a statistical measure. On the right side of the spectrum is "believes and opinions." "Believes and opinions" may or may not bare correlation to quantitative measures of quality, but overall, information formats on this end of the spectrum are often based on a qualitative input, such as recommendations and subjective ratings, and the output is a highly summarized one, such as a title or an award. Formats on both ends of the spectrum have their advantages and disadvantages. Formats towards the left end of the spectrum tend to be more objective and comes with clear definitions. Some may argue that these types of information formats provide more accurate measures of quality. However, such fine-grained quality information may often be difficult to interpret. Furthermore, these types of information tend to be one-dimensional and may inefficiently favor measurable outcomes over "valued" outcomes. Formats toward the right end of the spectrum tend to be more accessible and salient to consumers and consumer choice may be more responsive to these types of information. Studies have found that anecdotes often have greater influence in individuals' decision-making than statistics, because unlike statistical reports, awards and titles that bear suggestions of "recommendation from trusted sources" may be more representative to decision-makers (Kahneman et al., 1974). These awards and recognition may also represent a broader range of metrics than single-dimensional report cards. However, the methods behind these information platforms are often opaque and may magnify any biases in individuals' heuristic decision-making process.

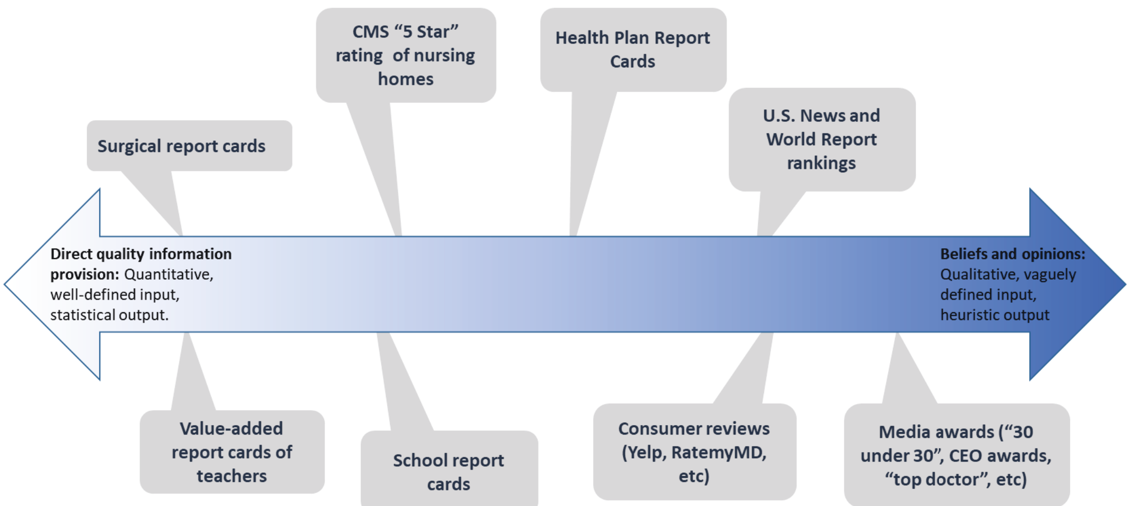


Figure 1.1: *Measures of quality and reputation*

There has been a number of studies on surgical report cards, an information provision platform towards the left end of the spectrum. The majority of these studies use cardiac report card data from New York and Pennsylvania, two of the earliest states to publicly release this information. Overall, these studies suggest that the demand side response to these types of report cards are small. Jha and Epstein (2006) find no change in the market share of cardiac patients in response to the New York Cardiac Surgery Report Cards. Cutler et al. (2004) find a decrease in patient volume for the small percentage of hospitals identified as performing significantly below the state average, but they find no evidence that high-performing hospitals attracted a greater number of patients. On the other hand, a number of other studies have found a significant supply-side response to the release of these report cards. Dranove et al. (2003) and (Zhang, 2011) find evidence of patient sorting after the release of report cards, and Kolstad (2013) finds evidence of quality improvement that is driven by the intrinsic motivation of providers.

Another literature looks at the effect of health plan ratings on health plan choices. Compared to surgical report cards, the information provision style of these plan ratings is more towards the right end of the spectrum. For example, in an example Medicare Report Card appearing in Medicare & You 2001, health plans are ranked by "percentage of patients who rated their own care as the best possible" (Dafny and Dranove 2005). In contrast to the studies on surgical report cards, the majority of the work on the effect of these health plan ratings find a significant consumer response to health-plan ratings choice (Wedig and Tai-Seale, 2002; Beaulieu, 2002; Scanlon et al., 2002; Chernew et al., 2004; Jin and Sorensen, 2005; Dafny and Dranove, 2005). There are also many studies on the effect of ratings and rankings in markets outside of health care, such as school ratings and restaurants reviews (Figlio and Lucas, 2004, Jin and Leslie, 2003, Jin and Whalley, 2007, Sorensen, 2007).

There are many fewer studies that look at the effect of information platforms towards the right end of the spectrum on provider choices in the health care market. Studies on this type of information formats often try to isolate the effect of reputation publicity, and decision heuristic from the effect of the underlying quality measures. Pope (2009) studied U.S. News and World Report rankings of hospitals and found that controlling for the underlying scores used to generate in the rankings, an improvement in rank by one spot is associated with an increase in both non-emergency patient volume and revenue of approximately 1%. Luca (2011)looked at the effect of online doctor ratings on the number of appointments booked with primary care physicians. They find that half a star improvement in ratings, on a scale of 1 to 5 stars, leads to a 10% increase in the likelihood, at the mean, that a doctor will fill an appointment. Neither of these studies looks at the effect of rankings or ratings on provider behavior. One study that does look at the effect of reputation on the behavior of those receiving the reputation is work by Malmendier and Tate (2009), which look at the effect of shifts in CEO status due to CEO awards conferred by major national media organizations. They found that firms with award-winning CEOs subsequently underperform, CEO compensations increase and CEOs spend more time on activities outside the company likes writing books and sitting on outside boards.

1.3 Conceptual framework

In this section, I consider how the market equilibrium may respond to additional reputation. Studies on the effect of quality information have used patient or procedure volume as the main outcome to evaluate demand responses (Ho, 2006, Johnson, 2011, Kolstad, 2013). This approach also implicitly assumes supply as perfectly elastic and patients can always receive care from their preferred provider(Figure 1.2, Panel (a)). This assumption is considered reasonable in studies on the Medicare Fee-for-service population because prices are administered and are either found or assumed to be high enough to make the marginal patient profitable (Kolstad, 2013, Pope, 2009).

In reality, supply may not be perfectly elastic, even if prices to providers are set by regulators and are above marginal cost. One common factor that affects the supply elasticity is capacity constraint. Fixed cost may constraint the number of operating rooms available in a hospital. Organizational dynamics may constrain the amount of operating time and staff support for individual surgeons. When such capacity constraint becomes binding, the supply curve may be perfectly inelastic (Figure 1.2, Panel (b)), demand shift may not result in any quantity change.

Alternatively, supply may be upward sloping (Figure 1.2, Panel (c)). This can happen even in markets with administered prices if a provider derives positive non-monetary benefit from serving an additional patient but faces upward-sloping marginal cost. This scenario is also more representative of markets with negotiated prices. In this scenario, demand shift is reflected in quantity change, but the quantity change understates the magnitude of the demand shift.

Finally, supply may even be downward sloping (Figure 1.2, Panel (d)). While the empirical evidence of a downward sloping supply curve is not widely established, this is a reasonable approximation if there are different segments of the markets and prices only adjust in some markets. For example, if additional reputation increases the demand for both privately insured patients, but prices only adjust in the private market, the supply for Medicare FFS patients would be downward sloping (with respect to private market payment rates). In this scenario, an outward shift in demand would actually lead to a decrease in quantity, at least for a subset of the market. There is another scenario where an outward shift in demand may lead to a decrease in quantity–if the supply curve (of any shape) shifts inward as a result of an increase in reputation.

The supply curve may shift inward because additional reputation increases the attractiveness of the provider's outside-option other than clinical care: a physician with additional reputation may take on more educational or administrative roles.

The shape of the supply curve will likely vary by specialties, procedures, types of providers, and time horizon. The assumption of a perfectly elastic supply curve is probably more likely to be true for quick and simple procedures than for long and complex procedures. It may also be more reasonable of an assumption than for hospitals than for individual physicians, who have a natural limit against increasing their capacity indefinitely. In summary, Figure 1.2 shows us the following predictions:

1. Additional reputation, even if it leads to a demand increase, may not lead to an increase in quantity. The quantity response depends on the elasticity of supply.

2. If price is allowed to be flexible and supply is not perfectly elastic, demand response to additional reputation would lead to higher prices.

3. If price is fixed, demand response to additional reputation may still lead to higher non-monetary prices.

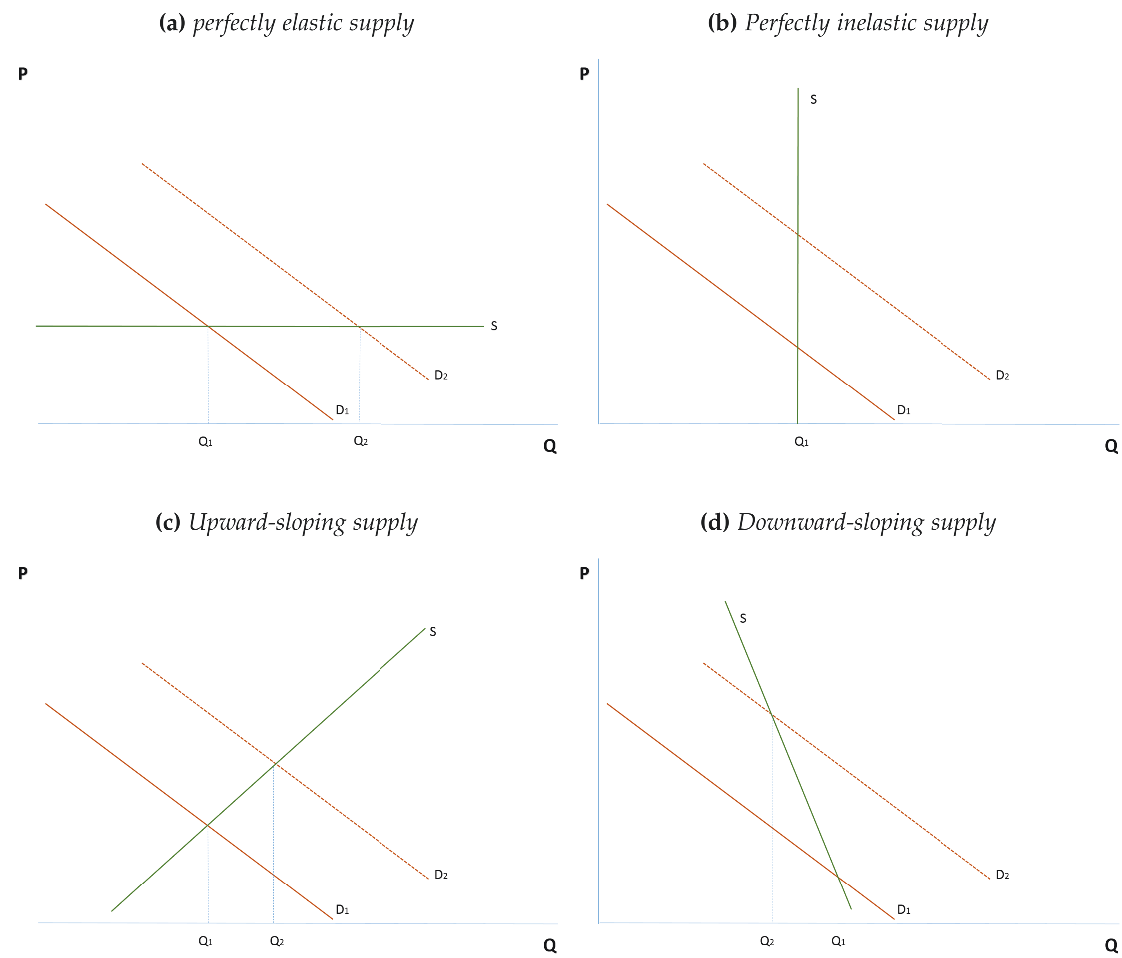
In this paper, I will first look at the effect of additional reputation on inpatient and outpatient procedure volume for Medicare FFS patients. Because this population experiences administered prices, I will not show results on monetary prices but will consider it in future work, but I show some results on changes in surgeons practice setting, which may be a reflection of price changes. Finally, the third prediction suggests that when supply curve is not perfectly elastic, studies on the demand responses of reputation, or of other provider characteristics, should look at the composition of patients rather than focusing on quantity. Even in the Medicare FFS market, where monetary prices are set administratively, non-monetary prices may play a significant role in adjusted market equilibrium when demand shifts. I will look at the effect of award-winning on two dimensions of patient composition: travel distance and case complexity.

When prices are administered, supply-side equilibrium and demand side equilibrium are determined separately, which may lead to excess demand for highly desirable providers. In such cases, demand increase as a result of additional reputation may lead to changes in a broad range of patient characteristics. Such changes can be driven by two forces. The first is differential demand responses across different groups of patients. For example, the demand of medically complex patients may be more likely to respond to additional reputation, either because these patients are more likely to read the magazines and advertisement that publishes "top doctors", or that they are more active choosers of providers. A second force that may drive changes in patients characteristics is different priorities to see a preferred provider in the face of excess demand, even when demand of different types of patients respond uniformly to additional reputation of a provider. For example, additional reputation may make lead to an equal increase in demand for patients from high-income areas and low-income areas, but in the face of capacity constraint on the supply side, patients from high-income areas are able to get higher priority to see their preferred provider. This higher priority can be patient directed: they may take more initiative to schedule an appointment in a timely fashion. It can also be provider driven: surgeons may give preference to patients from higher income areas because they find these patients easier to manage. In reality, these different type of forces can coexist and move in different direction. So an observed change in patient composition is a sufficient but not necessary condition for any demand response to additional reputation.

Figure 1.2: *Market response to a demand shift with different supply curves*

(a) *perfectly elastic supply* **(b)** *Perfectly inelastic supply*

(c) *Upward-sloping supply* **(d)** *Downward-sloping supply*



1.4 Description of the awards

"Top doctor" awards became popular in the 2000s. Although "top doctor" listing and advertisements can be found on many media, most of the listings cite awards conferred by a handful of firms. The largest national awards include *Castle Connolly Top doctors* (awarded by Castle Connolly Medical Ldt.) , *Super Doctors* (created by MSP Communications,formally Key Professional Media), *Best Doctors* (created by Best Doctor), and *Top Doctors by Consumer Checkbook*. Some large local newspapers or magazine also give their own award, mostly in areas where the national players do not have a large presence.

The methods by which the firms give awards are similar. The main component of candidacy is peer recommendation. Awarding firms send inquiries to hospitals and physician offices, asking physicians, nurses, and hospital administrators to recommend who they think are the top doctors in their area. In some cases, a panel of "experts" review the recommended individuals for their training, experience, and general reputation. Clinical outcome measures, such as risk-adjusted mortality rates, do not directly enter the award decision. Usually, about 1% to 5% in a market receives an award.

There is much controversy over what these "top doctor" awards actually measures. The response rates are low. Some have accused these awards of being a "popularity contest", and physicians and hospital administrators may be motivated to recommend doctors in their own institution. However, it is clear that the award brings visible gains in publicity to physicians. The names of "top doctors" are available on the website of the awarding firms. Awarding firms also partners with local newspapers and magazines to publish the list, either online or in print. For the partnering newspaper or magazine, the issue featuring "top doctors" is often the best-selling issue of the year. In addition, winning doctors and their organizations may choose to market themselves as a "top doctor" in other media, such as on their own practice websites, in airline magazines, or in additional ads in newspapers and magazines.

1.5 Data

1.5.1 Data on "top doctors"

I have data on the years an individual was named a "top doctor" from two of the largest national players: *Castle Connolly* and *Super Doctors*. The data set contains a list of all physicians who are listed on the firms websites as of January 2017. For each physician, I have data on her name, specialty, years she was named a "SuperDoctor" or a "Castle Connolly Top Doctor," and region where she was named. I link these physicians by name and specialty to the National Provider Number (NPI) registry [2] to obtain their NPIs.

[2] https://npiregistry.cms.hhs.gov/

In my data, the "year of winning" for SuperDoctors is the year when the title is awarded. The "year of winning" provided by Castle Connolly is the year when the winner's information is published on a Castle Connolly-partnered magazine, newspaper, or website. A physician may actually receive the Castle Connolly title and be able to purchase paid advertising bearing the Castle Connolly name a year or two before this publication. I include winners of either awards in my analyses on the information content of "top doctor" awards. For analyses on the impact of winning a "top doctor" awards, I limit my sample to winners of "SuperDoctor" only so that I can identify a precise event-time. Castle Connolly has being naming "top doctors" since the early 2000s, and SuperDoctors entered the "top doctor" award market in 2004.

There are a number of other major "top doctor" rating firms. Analyses on the effect of "top doctor" award using data from one awarding agency captures the marginal effect of one additional award. Nevertheless, the effect of a "top doctor" award may be the most interesting on the extensive margin (going from no award to one award, rather than going from having some award to having one more award). I use self-reported data from Doximity (see description later) to exclude physicians who have been named a "top doctor" by another agency, such as Consumer Checkbook. The remaining sample I focus on is a list of physicians for whom winning a "SuperDoctor" title is likely their first time receiving a similar type of award. I present frequency of the SuperDoctor awards by specialty in Appendix A.3.

A concern about my data is that whether physicians who have won an award many years ago and have since left clinical practice remains on the list of SuperDoctor winners as of January 2017. I do not have confirmation on whether the firm that awards the SuperDoctor award removes nonactive winners from their website, but removing non-active winners from the website is a confirmed practice at Castle Connolly. For this reason, I further restrict my analyses to a subset of SuperDoctor winners who have Medicare claims data both in the beginning (2000) and the end (2014) of my sample period. This restriction allows me to abstract from physicians' decisions to leave Medicare FFS (or clinical practice altogether). I separately show the likelihood of exiting Medicare FFS between winners and non-winners in Appendix A.3.1.

1.5.2 Other data

I also have an indicator on whether a physician has ever been named a "top doctor" by Consumer Checkbook as of 2014 from Doximity (though the exact year of winning is unknown). I use this indicator to exclude doctors who may have won Consumer Checkbook "top doctor" award from the control group of the event study specification. The Doximity data also includes a rich set of physician characteristics such as age, sex, year of completion and name of the medical school, residency, and board certification. Doximity is an online professional network for physicians. It has assembled data on all US physicians (both those who are registered members of the service as well as those who are not) from multiple sources and data partnerships, including the national plan and provider enumeration system national provider identifier registry, state medical boards, specialty societies such as the American Board of Medical Specialties, and collaborating hospitals and medical schools. Previous studies have validated data for a random sample of physicians in the Doximity database by using manual audits. The data are current as of 2014 (Jena et al., 2015, 2016).

Procedure volume data primarily comes from Medicare Fee-for-service inpatient claims from 2000 to 2014. Among the data elements are diagnosis and procedure codes, patient age, patient zip, institutional provider id, and operating physician NPI/UPIN. I exclude claims from patients younger than 65. There are several advantages of focusing on Medicare inpatient admissions. First, it allows me to identify the distance a patient traveled to receive care because the hospital's location can be identified. Second, the Medicare FFS population faces administered prices, so we can reasonably assume that price is not an important factor in patient's choice of providers. This population also does not face network restrictions, so their choices can freely respond when there is a change in the desirability of one provider versus another. Focusing on the Medicare FFS population also has a few limitations. First, in spheres where the physician can negotiate prices, the price effect of award-winning is actually important and interesting. I will not be able to capture this effect. Second, award-winning physicians may shift between Medicare patients and privately insured patients.

I also use cardiac report card data from California, New York, and Pennsylvania. I use these data to measure the clinical quality of cardiac surgeons. NY report cards on surgeons are available to the public as early as 2000. PA surgeon report cards are available to the public as of 1998 [3], and CA report cards on surgeons are available only since 2007.

I link the Medicare data to the Medicare Provider of Service file to obtain information on hospital characteristics, such as location and teaching status. I also link hospital ranking data from the U.S. News and World Report.

1.5.3 Sample selection

I limit my sample to physicians who are identified in the Doximity data as having one of the seven specialties: General Surgery, Neurosurgery, Obstetrics and Gynecology, Orthopaedic Surgery, (Cardio)Thoracic Surgery, Urology, and Vascular Surgery. These specialties are a subset of the 14 surgical specialties identified by the American College of Surgeons. The seven excluded specialties are Colon and Rectal Surgery, Gynecological Oncology, Oral and Maxillofacial Surgery, Ophthalmology, Otolaryngology, Plastic Surgery, and Pediatric Surgery. Colon and Rectal Surgery is excluded because the specialty is very small and the winning rate of-of "top doctor" awards is extremely high, making the generation of a comparison group difficult. Plastic Surgery is excluded because very few awards are given to this specialty. Gynecological Oncology and Oral and Maxillofacial Surgery are excluded because they are not listed as specialties in Doximity data. Ophthalmology and Otolaryngology are excluded because these specialties a large share of these specialties work is non-surgical, and many of the surgical procedures in these specialties are performed in physician offices and not in hospitals or other institutions. Pediatric Surgery is excluded because most of these surgeon's patients are not covered by Medicare.

In addition, I limit to the sample of surgeons who are actively practicing in both 2000 and 2014. Actively practicing is defined as having at least one inpatient or outpatient claim. This is because surgeons who have retired or moved from clinical practice could have been removed from the winner database I have. I also exclude winners who first received an award after 2014, because I do not observe any post-winning data on volume or patient characteristics.

[3] But 1998 report cards are only presented in a graphic format and lack precision. For my analysis, I only include report cards information from PA starting in 2002, when the first subsequent report cards became available

I include winners of either award (regardless of whether they also won other awards) in my analyses on the information content of "top doctor" awards. For analyses on the impact of winning a "top doctor" awards, I limit my sample to winners of "SuperDoctor" only so that I can identify a precise event-time.

1.5.4 Information content of "top doctor" awards

Tables 1.1 show the characteristics of winners of either "Super Doctor" or "Castle Connolly Top Doctor" versus surgeons who never won a major "top doctor" award. Column 1 of Table 1.1 shows the characteristics of all winners who won an award between 2000 and 2014 (including those who also won another award at some point) during the year when they first won a title. Column 2 of Table 1.1 shows the average characteristics of all physician-year observations that correspond to physicians who never won a major "top doctor" award. All means are standardized to the 2010 level.

Table 1.1 shows that compared to non-winners, "top doctor" winners have drastically different characteristics. Winners have higher inpatient and outpatient volume, are more experienced, and are more likely to have gone to a top medical school or to be working in a top-ranked hospital. Winners also perform different types of procedures than non-winners: the average institutional payment for procedures (inpatient or outpatient) performed by winners is over $\$8,000$, compared to under $\$6,000$ for the non-winners. This suggests that winners are performing more complex and more intensive procedures that may require longer hospital stays or more costly technology. Interestingly, the patients of winners and those of none winners are fairly similar in complexity, as measured by the number of Elixhauser comorbidity scores. Winners are much more likely to have patients from a high-income zip codes. This difference is mostly a reflection of the geographic location of the winners compared to the non-winners.

An interesting question is whether "top doctor" awards can direct patients to surgeons with higher clinical quality. To answer this question, we focus on a set of surgeons for whom we have clinical quality information: cardiac surgeons in New York, Pennsylvania, and California. For these surgeons, we have data on their risk-adjusted mortality rates (RAMRs) for Cardiac Bypass Surgery (CABG). To make these rates comparable between years and states, I standardize the RAMR measure to a z-score within the year-state distribution. The released RAMR usually averages three years of clinical data. I measure "current RAMR" for year *t* as the most recent report card that contains year *t*'s data. This report card is usually released 2 to 3 years after year *t* and is not directly observable for patients making choices at year *t*. Alternatively, quality data that are available for patients making decisions in year *t* (Latest available RAMR) usually use data that are 2-3 years old. Table 1.2 regresses this measure of "current clinical quality" over a number of surgeon characteristics. Observations are at the physician-year level. Column (1) shows that if a patient goes to a cardiac surgeon who has already won a "top doctor" award, on average, she is going to a surgeon whose CABG RAMR is 0.15 standard deviations below the mean. Column (2) shows that this result is robust regardless if we focus on winners in the earlier years or winners in the later years. Columns (3) shows that the result is robust after controlling for a number of surgeon characteristics that may be observable to patients or their referring physicians, such as experience, previous volume (Medicare FFS only), whether the surgeon went to a top medical school, or whether the surgeon works at a top hospital. Columns (4) and (5) show that the results are robust even after controlling for the latest available RAMR in the public cardiac report cards. In other words, "top doctor" awards may have more up-to-date information about a surgeon's clinical quality than the report card information based on data that are 2-3 years old. Overall, results from Table 1.2 suggest that "top doctor" awards do provide valuable information that may help patients choose surgeons with higher clinical quality. To the extent that we think clinical quality can change over time, "top doctor" awards may provide even more up-to-date information than publicly available report cards. Remember that the main input into "top doctor" awards is peer recommendation. This result also suggests that other doctors in the community have valuable information on who may be a high-quality surgeon, and "top doctor" awards may have the potential to spread such information more broadly.

A potential concern is that the difference in RAMRs between "top doctor" winners and non-winners are not driven by true quality differences but by unobserved patient risk factors that are not properly controlled for in the calculation of RAMRs in the report cards. We have seen in Table 1.1 that the patients of "top doctor" winners and those of non-winners have very similar co-morbidity scores. However, winners are more likely to see patients from a high-income zipcode.

Table 1.1: *Characteristics of winners and non-winners, all surgeons*

	Winner-years	Non-winners	p
N. discharges	36.47	25.54	0.00
N. Op procedures	56.62	42.73	0.00
Years since medical school	24.00	22.99	0.00
went to top 20 Med. school	0.15	0.02	0.00
main hospital is teaching	0.54	0.37	0.00
main hospital is top 50 USNews	0.21	0.05	0.00
Shr Medicare patients from 15m+	0.42	0.39	0.00
Mean facility payment of procedures	8376.84	5659.46	0.00
N. hospitals operating in	2.08	1.83	0.00
Patient comorbility	2.04	2.02	0.02
Standardized patient median zip income	0.74	0.19	0.00
N	8409	1009579	

Note: Column 1 shows the characteristics of all "Super Doctor" or "Castle Connolly Top Doctor" winners who won the award between 2004 and 2014 (including those who also won another award at some point) during the year when they first won the title. Column 2 shows the average characteristics of all physician-year observations that corresponds to physicians who never won a major "top doctor" award. Patient comorbidity is measured in number of exlihauser comorbidity conditions. Patient median zip income is measured in average zip median income z score relative to national distribution. All means are standardized to the 2010 level.

It has been documented that patient socioeconomic status is an important predictor of mortality (Bennett et al., 2010), and these factors are not adjusted for in the calculation of RAMRs. To check whether the clinical quality difference between winners and non-winners is driven by the income level of their patients, Table 1.3 repeats the analyses of Table 1.2 while controlling for the share of Medicare FFS patients from a high-income zipcode. We do see that surgeons with a higher share of patients in high-income areas have lower RAMR on average. This correlation can be consistent with a story where higher-income is inherently associated with lower mortality rates, but it is also possible that high-income patients are better at seeking out high-quality surgeons. Regardless, after controlling for patient income, "top doctor" winners still have CABG RAMR levels 0.12-0.18 standard deviations below the average cardiac surgeon.

Table 1.2: *Predictor of current-year RAMR*

	(1)	(2)	(3)	(4)	(5)
	b/se	b/se	b/se	b/se	b/se
Has already won	-0.15**		-0.20**	-0.17**	
a top doctor award	0.06		0.06	0.06	
Has already won		-0.14*			-0.16*
a top doctor award, pre 2005		0.06			0.07
Has already won		-0.19			-0.21
a top doctor award, post 2005		0.11			0.11
Last years total			-0.03	-0.00	-0.00
patient volume is more than 100			0.04	0.04	0.04
Operating at a			0.01	0.00	0.00
US.News top 20 heart hospital			0.08	0.08	0.08
experience			0.00	0.00	0.00
			0.00	0.00	0.00
experience2			0.00	0.00	0.00
			0.00	0.00	0.00
Graduates from			0.15*	0.13	0.13
top 20 Med Sch			0.08	0.07	0.07
Latest available				0.19***	0.18***
CABG RAMR (z-score)				0.02	0.02
Constant	-0.11	-0.11	-0.13	-0.05	-0.05
	0.46	0.46	0.44	0.43	0.43
Time fixed effect	Yes	Yes	Yes	Yes	Yes
Hrr fixed effect	Yes	Yes	Yes	Yes	Yes
N	2574.00	2574.00	2374.00	2374.00	2374.00

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Dependent variable: Standardized (z-score based on state-year distribution) CABG risk-adjusted mortality rate from the first Cardiac Report Card that includes data from year t.

1.6 Event Study Methods

The previous section suggests that "top doctor" awards capture valuable information on who may be a high-quality surgeon from surgeons' peer networks, and "top doctor" awards may have the potential to spread such information more broadly. To test whether the publicity of such information affects how patients choose their providers, I use an event-study framework to study the effect of winning a "top doctor" award on various outcomes. Because a surgeon may receive the award for multiple years, and surgeons who receive the award in one year almost always receive it in all subsequent years, the event I am interested in is the first time the surgeon

Table 1.3: *Predictor of current-year RAMR*

	(1)	(2)	(3)	(4)	(5)	(6)
	b/se	b/se	b/se	b/se	b/se	b/se
share of	-0.22*	-0.18	-0.24*	-0.20*	-0.19	-0.15
patients from high-income zipcodes	0.10	0.10	0.10	0.10	0.10	0.10
Has already won		-0.12*		-0.18**		-0.16*
a top doctor award		0.06		0.06		0.06
Last years total			-0.02	-0.02	0.00	0.00
patient volume is more than 100			0.05	0.04	0.04	0.04
Operating at a			-0.03	0.02	-0.04	0.01
US.News top 20 heart hospital			0.08	0.08	0.08	0.08
experience			0.00	0.00	0.00	0.00
			0.00	0.00	0.00	0.00
experience2			0.00	0.00	0.00	0.00
			0.00	0.00	0.00	0.00
Graduates from			0.11	0.15*	0.09	0.13
top 20 Med Sch			0.07	0.08	0.07	0.07
Latest available					0.19***	0.18***
CABG RAMR (z-score)					0.02	0.02
Constant	-0.01	-0.07	0.02	-0.03	0.07	0.02
	0.44	0.44	0.44	0.44	0.43	0.43
Time fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Hrr fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
N	2393.00	2393.00	2374.00	2374.00	2374.00	2374.00

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Dependent variable: Standardized (z-score based on state-year distribution) CABG risk-adjusted mortality rate from the first Cardiac Report Card that includes data from year t.

wins an award (SuperDoctor). In my first specification, I perform the event-study only on the set of surgeons who ever won a "top doctor" award using the equation:

$$Y_{it} = \gamma_i + \tau_t + \sum_{\tau=-7}^{-1} \delta_{-\tau} D_{i,t-\tau} + \sum_{\tau=1}^{7} \delta_{+\tau} D_{i,t+\tau} + \epsilon_{it} \tag{1.1}$$

- Y_{it} is outcome for physician i at time t
- γ_i is physician fixed effect, τ_t is calendar time fixed effect.
- $D_{i,t-\tau}$, $D_{i,t+\tau}$ are indicators if the physician was treated τ years before or after calendar year t. For control physicians, the Ds are calculated relative to their hypothetical "treatment" year. ($\tau = 0$ is omitted, and $\tau < -7$ and $\tau > 7$ are collapsed into single categories.)
- $\beta_{-\tau}$ are the time-varying treatment effects of interest.

This estimation model is an extension of the difference-in-differences model that allows for time-varying effects. In general, inference on the effect of the event rests on the testable assumption that $\beta_{+\tau} = 0$. In other words, there is no pre-trend or anticipation effect of the outcome prior to the event.

In reality, there is likely an underlying non-linear trend of many outcomes of interest, particularly patient volume, over a physician's career. Also, if we have reasons to believe that there is anticipation or preparation effect of winning a top-doctor award, the pre-trend of outcomes may also be affected. To account for this difference, we would ideally want to find a control group of surgeons who are identical to the winning surgeons in every way (on average) except that they did not win a "top doctor" award. As Table 1.1 illustrates, winners and non-winners are different in many dimensions, and it is challenging to find a subgroup of non-winners who look identical to the winners. As an alternative, I construct control groups that matches on the pre-winning trend of outcomes of interest and run the following specification (physicians who have ever won other "top doctor" awards, such as Castle Connolly Top Doctor or Consumer Checkbooks "top doctor", are excluded from both the control group and the treatment group).

$$Y_{it} = \gamma_i + \tau_t + \sum_{\tau=-7}^{-1} \delta_{-\tau} D_{i,t-\tau} + \sum_{\tau=1}^{7} \delta_{+\tau} D_{i,t+\tau} + \sum_{\tau=-7}^{-1} \beta_{-\tau} D_{i,t-\tau} T_i + \sum_{\tau=1}^{7} \beta_{+\tau} D_{i,t+\tau} T_i + \epsilon_{it} \quad (1.2)$$

- Y_{it} is outcome for physician i at time t
- γ_i is physician fixed effect, τ_t is calendar time fixed effect.
- $D_{i,t-\tau}$, $D_{i,t+\tau}$ are indicators if the physician was treated τ years before or after calendar year t. For control physicians, the Ds are calculated relative to their hypothetical "treatment" year. ($\tau = 0$ is omitted, and $\tau < -7$ and $\tau > 7$ are collapsed into single categories.)
- T_i is an indicator that physician i is ever treated.
- No pre-trend (relative to control group) means $\beta_{+\tau}$=0
- $\beta_{-\tau}$ are the time-varying treatment effects.

1.6.1 Construction of propensity score matched controls

To facilitate the comparison between winners and non-winners, who are different in many dimensions, I construct a control group of non-winners who have similar levels of and trends in the key pre-winning outcomes. Specifically, I implement a matching strategy using propensity scores, performing one-to-two matching without replacement (using stata's psmatch2 command) and calipers (defining the maximum difference in the propensity score that was allowable for a match of 0.01 of the propensity score. Matching was stratified by year and specialty: for winners in specialty X who won Super Doctors for the first time in year t, the propensity score are calculated among all active physicians in specialty X who are active in year t, and the two non-winners with the closest score to each winner is selected. The year t in which a non-winner is selected as a control is the non-winner's "hypothetical winning year", around which we will plot outcomes of interest. The match variables are inpatient Medicare FFS volume in year $t-4$ through $t-2$, outpatient Medicare FFS volume in year $t-4$ through $t-2$, the share of patients who travel more than 15 miles to see the provider, zipcode median income of the average patient, and the Elixhauser comorbidity score of the average patient. I use this match throughout my analyses instead of redoing the match for each individual outcome of interest in order to preserve sample consistency.

Table 1.1 shows that a number of other characteristics also strongly predicts winning. These characteristics include whether the physician went to a top medical school and whether the physician practices at a US News and World report top 50 hospitals. Since these outcomes are very rare, and that we expect them to influence the level more than the trend of outcomes of interest, these additional variables are not included to construct the matched sample.

Table 1.4 shows the result of the propensity score matched sample construction. Column 1 of Table 1.4 shows the characteristics of surgeons who ever won a "SuperDoctor" award. Column 2 shows the characteristics of a subset of "SuperDoctor" winners included in the analysis sample: the set of winners who never won another major top doctor award (Castle Connolly Top Doctor or Consumer Checkbook Top Doctor) and have Medicare FFS claims in both 2000 and 2014 ("active treatments"). For this subset, we can accurately identify the year a physician first won any major "top doctor" award. We can also abstract from the possible complication that those who move out of the clinical practice may be removed from the list of "SuperDoctor" winners.

Column 3 of Table 1.4 shows the characteristics of a subset of physician-year observations that correspond to physicians who never won a major "top doctor" award but have Medicare FFS claims in both 2000 and 2014 ("active controls"). The comparison of Columns 3 and 2 tells a similar story as Table 1.1, but the differences are less pronounced in some characteristics, such as procedure volume, years of experience, and the likelihood of having gone to a top medical school. Column 4 displays the t-test values between Columns 3 and 2.

Column 5 of Table 1.4 shows the characteristics of physician-years that are matched to those in Column 2 using propensity scores. Propensity scores are calculated using inpatient Medicare FFS volume in year $t-4$ through $t-2$, outpatient Medicare FFS volume in year $t-4$ through $t-2$, share of patients who travel more than 15 miles to see the provider, zipcode median income of the average patient, and the Elixhauser comorbidity score of the average patient. Column 6 displays the t-test values between Columns 2 and 5. Balance is achieved between Column 2 and 5 on all variables used in matching and one variable not used in matching–years since medical school graduation. This is assuring because experience can be a major determinate of a physician's workload and performance. There is still a significant difference in other characteristics between Column 5 and 2. However, differences are significantly decreased due to matching. For example, the average institutional payment for procedures in the matched sample is much closer to those of "active treatments" in Column 2 than to those of "active controls" in Column 3. The same holds true for the share of a physician's patients from high-income zipcodes. Winners are still much more likely to have gone to a top medical school or to be working in a top hospital, but these are rare characteristics in both groups.

Table 1.4: *Characteristics of winners and non-winners, active surgeons between 2000 and 2014*

	All SuperD	Active SuperD	Active Ctrl.	p	Matched Ctrl.	p
N. discharges	41.04	53.83	49.22	0.00	52.33	0.39
N. Op procedures	58.66	59.09	76.54	0.00	59.39	0.93
Years since medical school	26.69	28.37	27.19	0.00	27.92	0.11
went to top 20 Med. school	0.15	0.05	0.01	0.00	0.02	0.00
main hospital is teaching	0.52	0.47	0.33	0.00	0.32	0.00
main hospital is top 50 USNews	0.24	0.17	0.03	0.00	0.05	0.00
Shr Medicare patients from 15m+	0.40	0.37	0.39	0.15	0.38	0.87
Mean facility payment of procedures	9519.31	9303.24	6288.11	0.00	8017.42	0.00
N. hospitals operating in	2.22	2.69	2.42	0.00	2.49	0.00
Patient comorbility	2.05	2.18	2.17	0.86	2.18	0.88
Standardized patient median zip income	0.76	0.65	0.10	0.00	0.62	0.24
N	4732	1282	226292		2425	

Note: Column 1 shows the characteristics of a subset of "SuperDoctor" winners included in the analysis sample: the set of winners who never won another major top doctor award. Column 2 shows the characteristics of a subset of physician-year observations that corresponds to physicians who never won a major "top doctor" award but have Medicare FFS claims in both 2000 and 2014 ("active controls"). Column 3 displays the t-test values between Columns 1 and 2. Column 4 shows the characteristics of physician-years that are matched to those in Column 1 using propensity scores. Propensity scores are calculated using inpatient Medicare FFS volume in year $t-4$ through $t-2$, outpatient Medicare FFS volume in year $t-4$ through $t-2$, share of patients who travel more than 15 miles to see the provider, zipcode median income of the average patient, and the elixhauser comorbidity score of the average patient. Column 5 displays the t-test values between Columns 1 and 4. Patient comorbidity is measured in number of exlihauser comorbidity conditions. Patient median zip income is measured in average zip median income z score relative to national distribution. All means are standardized to the 2010 level.

1.7 Results

1.7.1 Effect of award-winning on patient volume

Figures 1.3 and 1.5 show the estimate of equation 1.1 for the log of inpatient and outpatient FFS Medicare volume. Figures 1.4 and 1.6 show the estimate of equation 1.2 for the log of inpatient and outpatient FFS Medicare volume for a set of propensity score matched controls.

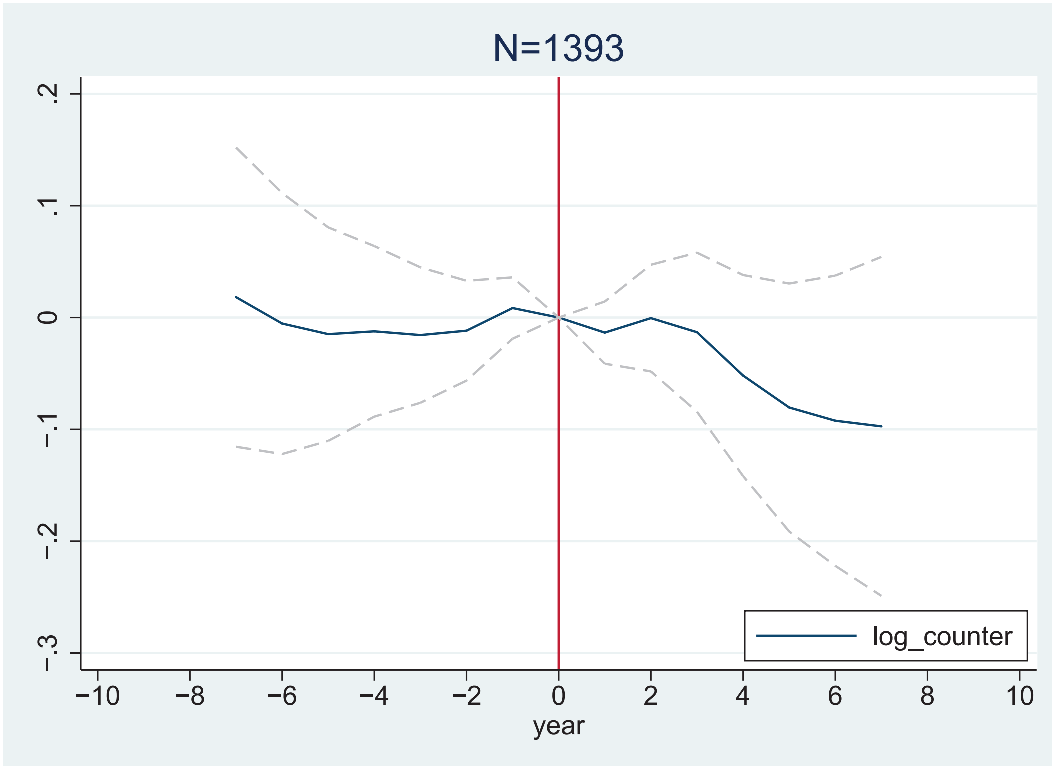
Note first that there's an overall decline in inpatient volume for a surgeon over their career span around the time they are 28 years out of medical school, even after controlling for the calendar year fixed effect. This decline may be driven by a negative relationship between demand and physician experience or a negative relationship between supply and experience. There is very little direct empirical evidence on patient demand as a function of physician experience. However, (Tsugawa et al., 2017) has found that patients of physicians older than 40 have higher mortality rates than patients of younger physicians. So it would not be surprising that some patients and their referring physicians are actively steering away from older surgeons. However, this explanation is not consistent with the fact that outpatient volume steadily increases over a physician's career (Figure 1.5), and so does the a physician's share of patients who travel more than 15 miles for care(Figure 1.10). A more likely explanation is on the supply side. As a physician age, they may shift their labor from inpatient operations elsewhere, which can be outpatient services, administrative or training roles, or leisure. These supply-side decisions suggest that we cannot assume supply to be perfectly elastic on the physician level, and analysis on patient volume will be an underestimate of demand responses to "top doctor" awards.

Second, Figures 1.4 and 1.6 show that relative to the propensity score matched control group, Super Doctor winners do not experience any increase in either inpatient or outpatient procedure volume after winning.

Figures A.6 and A.7 in the appendix show a version of Figures 1.4 and 1.6 separately for the 7 specialties. The finding that award-winning does not affect inpatient or outpatient volume is consistent across all specialties.

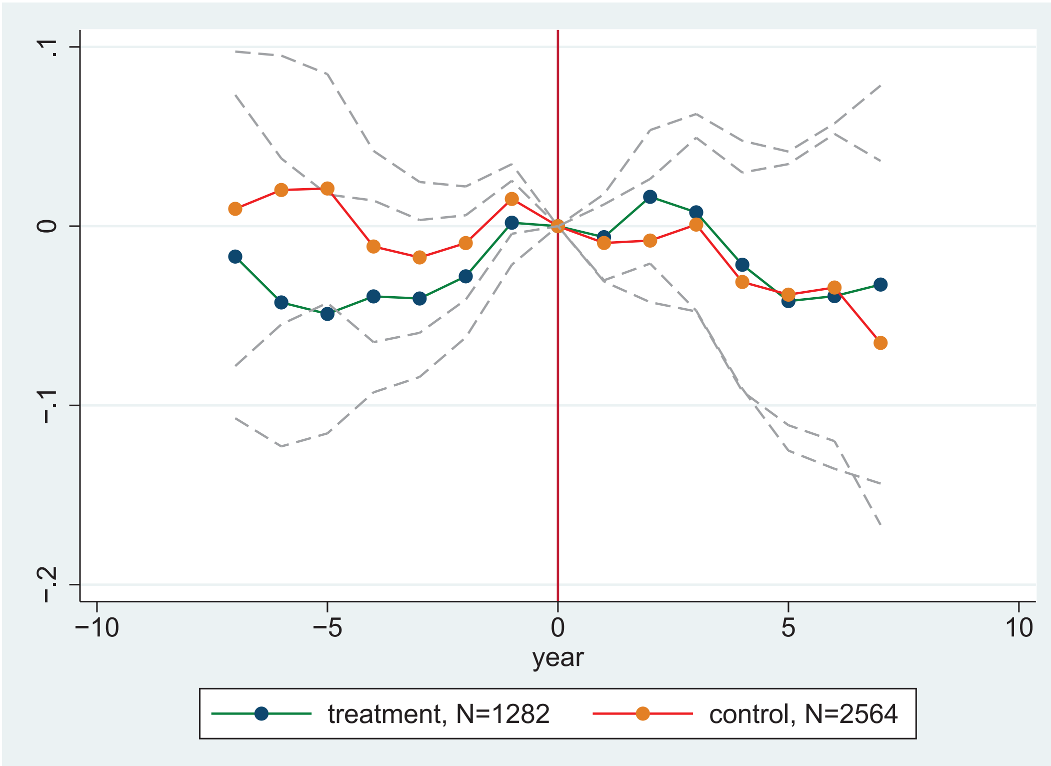
This result suggests that either patient demand does not respond to the receipt of "SuperDoctor" awards for the subset of winners in my sample for most specialties, or that that any increase in demand could not be accommodated by restrictions on the supply side, where the physician's optimization problem determines the volume of care delivered.

Figure 1.3: *Event study of Medicare FFS inpatient volume, active surgeons in both 2000 and 2014, winners of Super Doctors only*



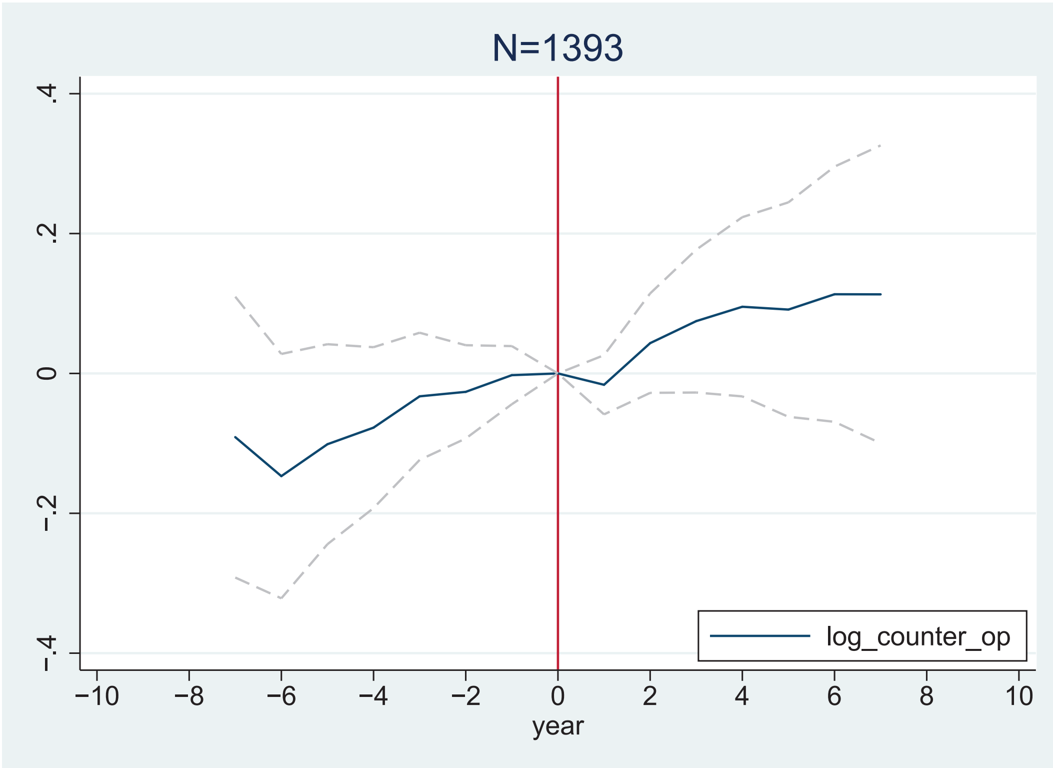
Event study of log Medicare FFS inpatient volume. Sample includes SuperDoctor winners who first won the award between 2004 and 2014, is active in Medicare claims data in both 2000 and 2014, and has never won another major "top doctor" award.

Figure 1.4: *Event study of Medicare FFS inpatient volume, active surgeons in both 2000 and 2014, winners of Super Doctors only and propensity score matched control group*



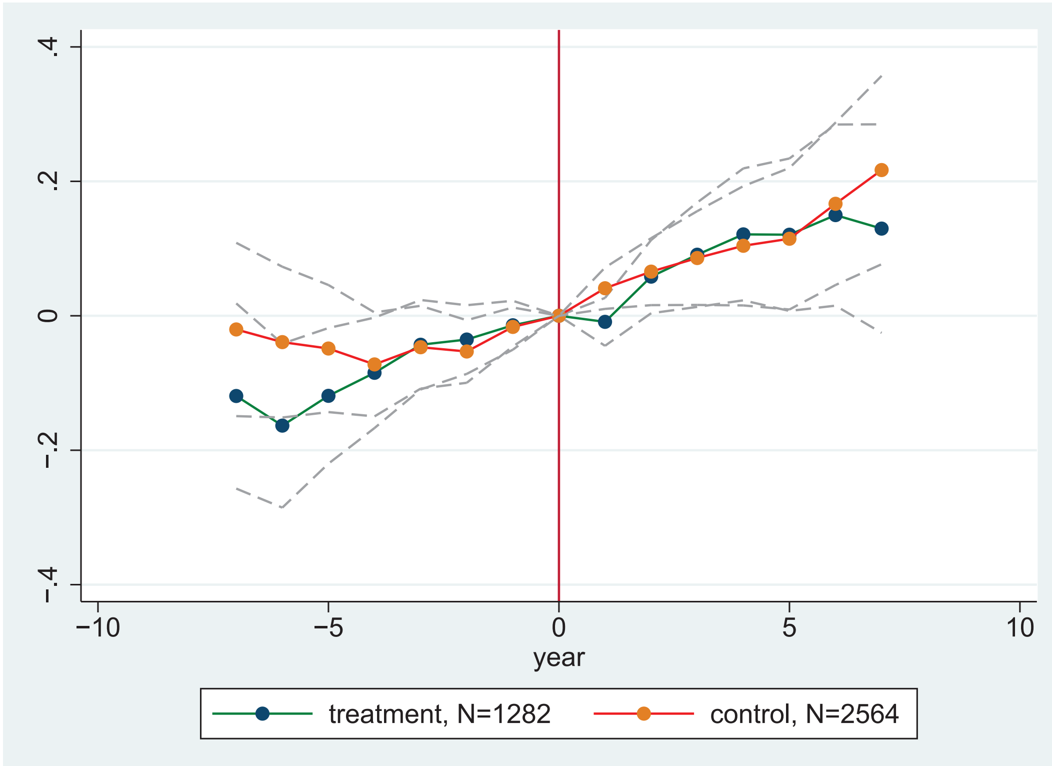
Note: Event study of log Medicare FFS inpatient volume. Treatment sample includes SuperDoctor winners who first won the award between 2004 and 2014, is active in Medicare claims data in both 2000 and 2014, and has never won another major "top doctor" award. Propensity scores are calculated using inpatient Medicare FFS volume in year $t-4$ through $t-2$, outpatient Medicare FFS volume in year $t-4$ through $t-2$, share of patients who travel more than 15 miles to see the provider, zipcode median income of the average patient, and the elixhauser comorbidity score of the average patient.

Figure 1.5: *Event study of Medicare FFS outpatient volume, active surgeons in both 2000 and 2014, winners of Super Doctors only*



Event study of log Medicare FFS outpatient volume. Sample includes SuperDoctor winners who first won the award between 2004 and 2014, is active in Medicare claims data in both 2000 and 2014, and has never won another major "top doctor" award.

Figure 1.6: *Event study of Medicare FFS outpatient volume, active surgeons in both 2000 and 2014, winners of Super Doctors only and propensity score matched control group*



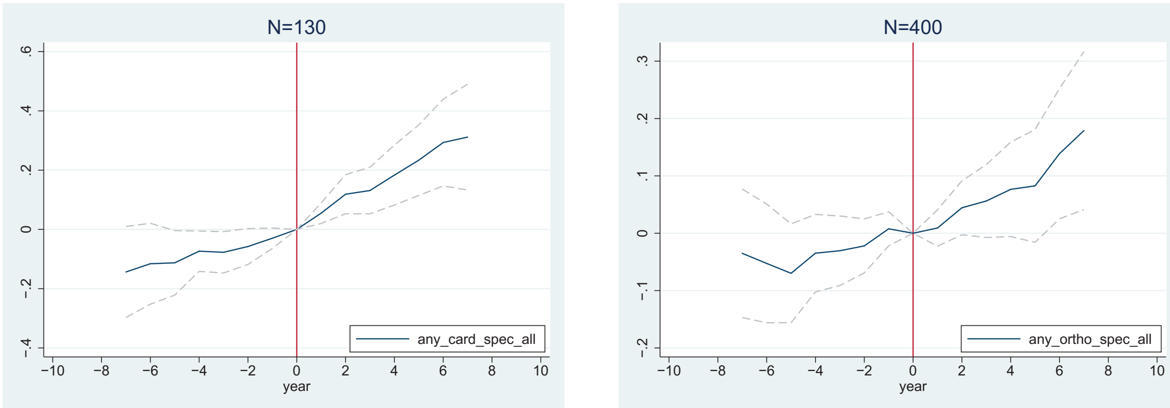
Note: Event study of log Medicare FFS outpatient volume. Treatment sample includes SuperDoctor winners who first won the award between 2004 and 2014, is active in Medicare claims data in both 2000 and 2014, and has never won another major "top doctor" award. Propensity scores are calculated using inpatient Medicare FFS volume in year $t-4$ through $t-2$, outpatient Medicare FFS volume in year $t-4$ through $t-2$, share of patients who travel more than 15 miles to see the provider, zipcode median income of the average patient, and the elixhauser comorbidity score of the average patient.

1.7.2 Changes in practice setting

The effect of additional reputation may be reflected in monetary prices in markets where prices adjust flexibly to demand. This is not the case for the Medicare FFS market. Nevertheless, there may be indirect ways to detect changes in a physician's bargaining power. One potential channel is to see if award-winning leads surgeons to move towards different types of hospitals. Additional reputation may make it easier to negotiate operating room at a prestige hospital, which attracts more patients. It may also make surgeons more likely to operate in a specialty hospital, where they have more flexibility and have enough reputation to attract patients without the reputation of a general hospital. Figures 1.7 and 1.8 show the event-study results on whether the surgeon's performed any procedure in a specialty hospital. A cardiac specialty hospital is defined as one where 60% or more of its Medicare FFS discharges has a cardiac procedure (see Appendix A.1 for definition) during any year between 2000 and 2014. An orthopedic specialty hospital is defined as one where 60% or more of its Medicare FFS discharges has an orthopedic procedure (see Appendix A.1 for definition) during any year between 2000 and 2014.

For both cardiac surgeons and orthopedic surgeons, the likelihood of practicing at a specialty hospital is very similar between the winners and the matched controls before award-winning. This is assuring especially given that no variable of practice setting is used in the propensity matching process. Cardiac surgeons who are named "SuperDoctor" shows a clear increase in their likelihood of operating at a cardiac specialty hospital relative to the matched controls. This difference is confirmed in Table 1.5, presents difference-in-differences estimate of Equation 1.2, where the relative year indicators are aggregated over multiple years to capture an aggregate effect. Four to seven years after winning the award, the likelihood of practicing at a cardiac specialty hospital is 15 percentage points higher. This is a very large increase, given that only six percent of cardiac surgeons in our sample performed procedures at a cardiac specialty hospital in 2005. The results for orthopedic surgeons are similar. Four to seven years after winning the award, the likelihood of practicing at an orthopedic hospital is about eight percentage point higher for orthopedic surgeons. Both effects are statistically significant at the 5 percent level.

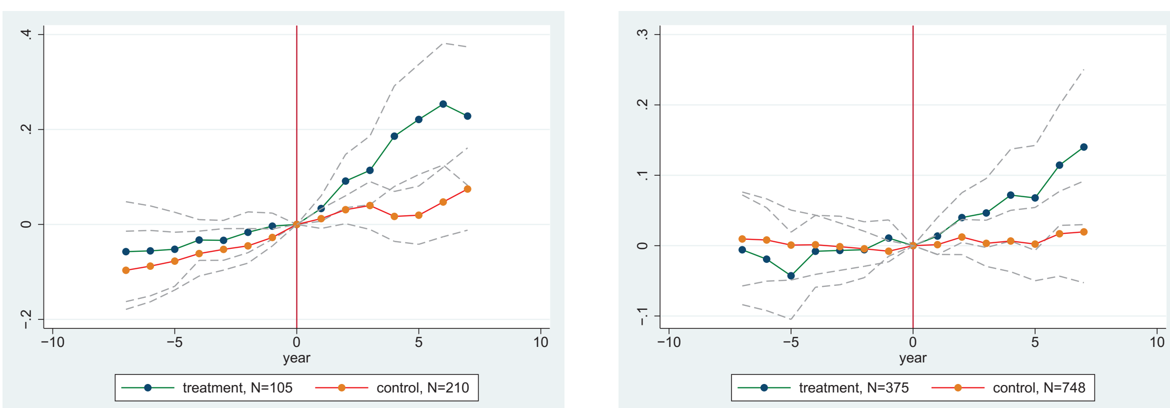
Figure 1.7: *Specialty hospitals*



Performing at any cardiac specialty hospital. *Performing at any orthopedic specialty hospital.*

Note: Event study of physician hospital affiliation,cardiac surgeons, propensity score matched controls. A cardiac specialty hospital is defined as one where 60% or more of its Medicare FFS discharges has a cardiac procedure (see Appendix A.1 for definition) during any year between 2000 and 2012. A orthopedic specialty hospital is defined as one where 60% or more of its Medicare FFS discharges has an orthorpedic procedure (see Appendix A.1 for definition) during any year between 2000 and 2012.

Figure 1.8: *Specialty hospitals, with control group*



Performing at any cardiac specialty hospital. *Performing at any orthopedic specialty hospital.*

Note: Event study of physician hospital affiliation,cardiac surgeons, propensity score matched controls. A cardiac specialty hospital is defined as one where 60% or more of its Medicare FFS discharges has a cardiac procedure (see Appendix A.1 for definition) during any year between 2000 and 2012. A orthopedic specialty hospital is defined as one where 60% or more of its Medicare FFS discharges has an orthorpedic procedure (see Appendix A.1 for definition) during any year between 2000 and 2012.

Table 1.5: *Diff-in-diffs regression of practicing in any cardiac or orthopedic specialty hospital*

	(1) Cardiac Spec Hosp b/se	(2) Cardiac Spec Hosp b/se	(3) Ortho Spec Hosp b/se	(4) Ortho Spec Hosp b/se
post	-0.01		-0.02*	
	0.02		0.01	
post*win	0.05		0.04*	
	0.03		0.02	
post0_3		0.00		-0.01
		0.01		0.01
post0_3*win		-0.01		0.02
		0.02		0.01
post4_7		-0.03		-0.02
		0.02		0.01
post4_7*win		0.15**		0.08**
		0.05		0.03
phys. FE	Yes	Yes	Yes	Yes
year FE	Yes	Yes	Yes	Yes
year*spec FE	Yes	Yes	Yes	Yes
N	4410	4410	16588	16588

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Covariates that are included but not displayed including indicator variables on being 8 or more years prior to the true or hypothetical first year of award-winning, 8 ore more year after the true or hypothetical first year of award-winning, and the interaction of these indicators with a "true winner" treatment indicator. An indicator for being 1 to 7 years prior to first year of award-winning, which captures the "pre" period, is omitted. Treatment sample includes SuperDoctor winners who first won the award between 2004 and 2014, is active in Medicare claims data in both 2000 and 2014, and has never won another major "top doctor" award. Propensity scores are calculated using inpatient Medicare FFS volume in year $t-4$ through $t-2$, outpatient Medicare FFS volume in year $t-4$ through $t-2$, share of patients who travel more than 15 miles to see the provider, zipcode median income of the average patient, and the elixhauser comorbidity score of the average patient. A "hypothetical award-winning" year for the control group is the year t on which that physician is selected as a matched control.

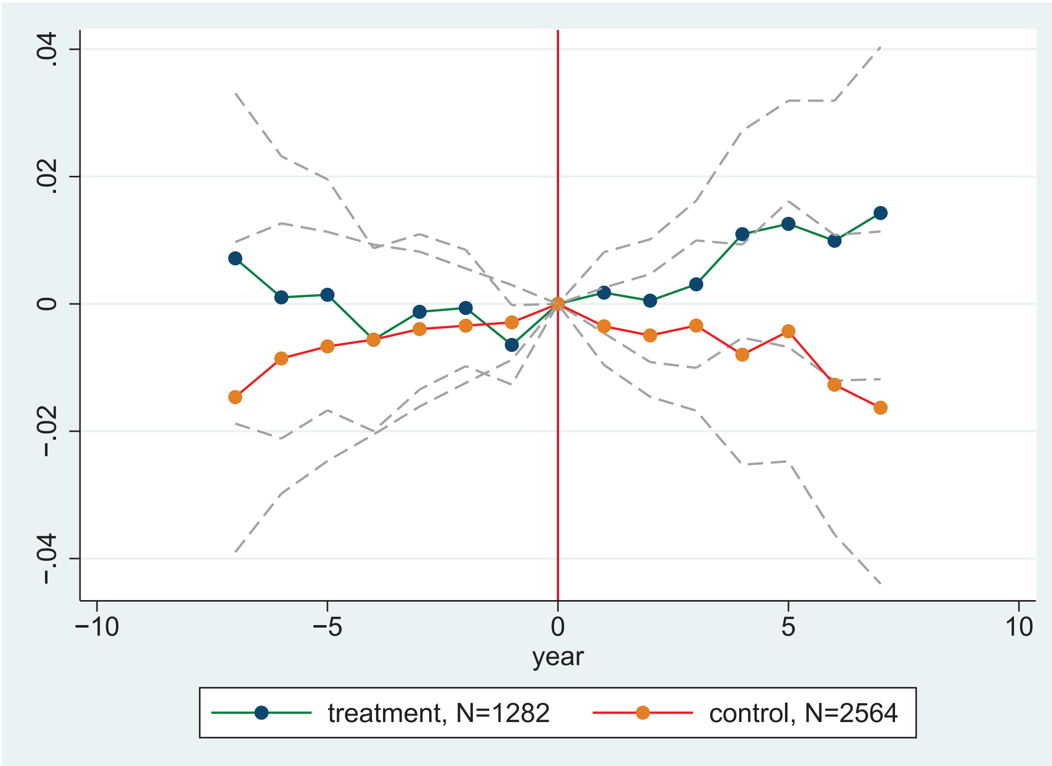
1.7.3 Results on patient composition

This section explores a number of dimensions of patient composition as evidence of demand response to award-winning. As suggested in Section 1.3, a change in patient composition is a sufficient, but not necessary condition for demand responses as a result of additional reputation. When demand increases but supply is inelastic, we may see changes in patient composition even if we do not see any response in patient volume.

Patient travel distance

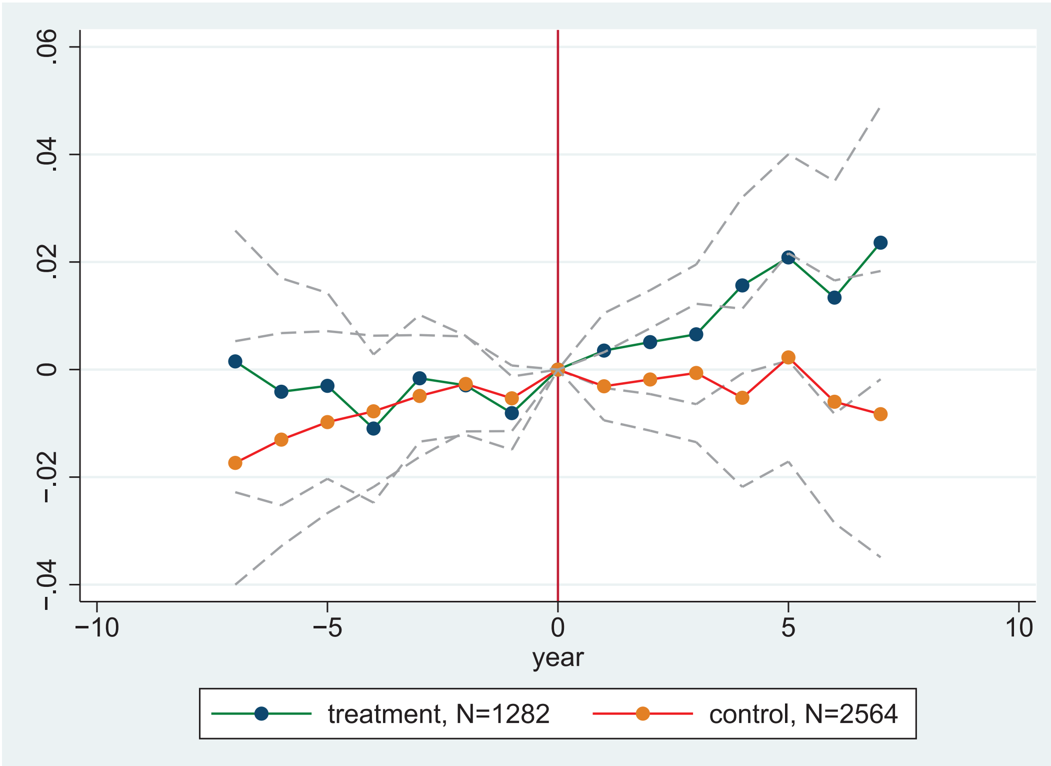
Figures 1.9 to 1.11 shows the share of a surgeon's patients who travel more than 30 miles, 15 miles or 5 miles for either inpatient or outpatient care. The share of patients who travel far away stayed pretty similar between the winners and propensity score matched controls in the years right before winning but they start to diverge after winning. Winners seem to experience a slight increase in the share of their patients who travel from far away. Table 1.6 shows the difference-in-differences regression results corresponding to figures 1.9 to 1.11, with the post-winning years divided into two segments. The share of patients who travel more than 15 miles away to see a "top doctor" winners increased by two percentage point in the four to seven years after winning. This increase is small in magnitude considering that the average surgeon have 40% of her patients travel for more than 15 miles.

Figure 1.9: *Event study of share of patients who travel from more than 30 miles away, winners of Super Doctors only and propensity score matched control group.*



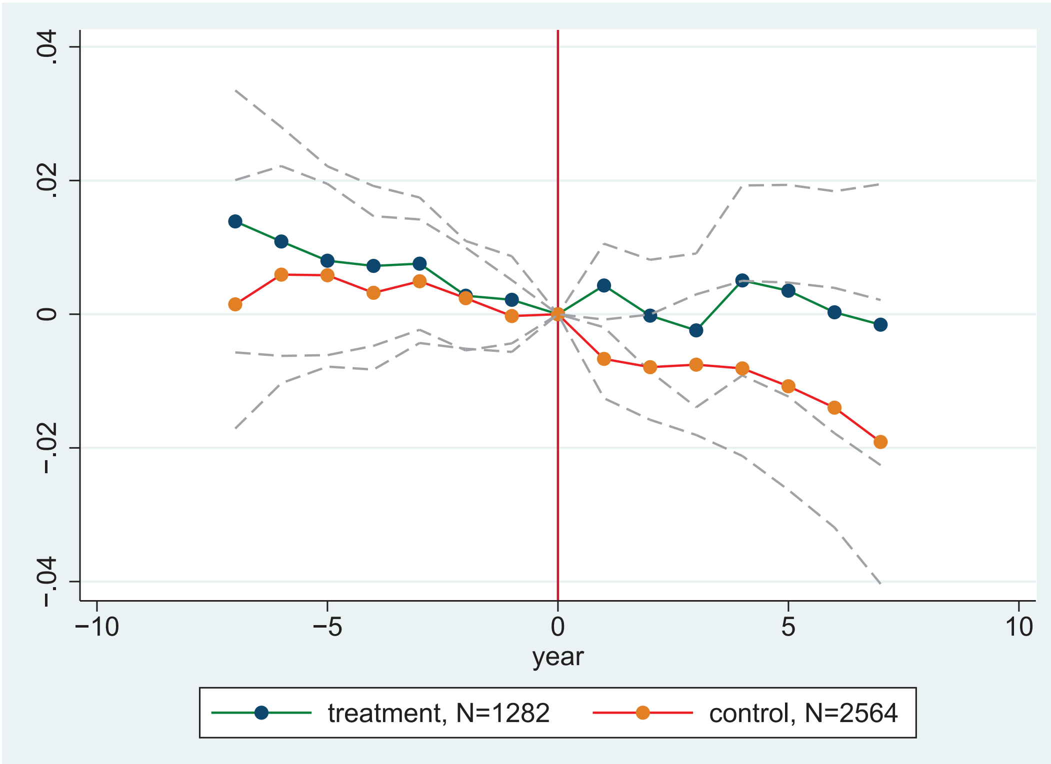
Note: Event study of share of Medicare FFS patients traveling more than 30 miles away. Travel distance is calculated as distance between the zipcode centroid of the patient's zipcode of residence and the zipcode of the provider facility on the claim. Treatment sample includes SuperDoctor winners who first won the award between 2004 and 2014, is active in Medicare claims data in both 2000 and 2014, and has never won another major "top doctor" award. Propensity scores are calculated using inpatient Medicare FFS volume in year $t-4$ through $t-2$, outpatient Medicare FFS volume in year $t-4$ through $t-2$, share of patients who travel more than 15 miles to see the provider, zipcode median income of the average patient, and the elixhauser comorbidity score of the average patient.

Figure 1.10: *Event study of share of patients who travel from more than 15 miles away, winners of Super Doctors only and propensity score matched control group.*



Note: Event study of share of Medicare FFS patients traveling more than 15 miles away. Travel distance is calculated as distance between the zipcode centroid of the patient's zipcode of residence and the zipcode of the provider facility on the claim. Treatment sample includes SuperDoctor winners who first won the award between 2004 and 2014, is active in Medicare claims data in both 2000 and 2014, and has never won another major "top doctor" award. Propensity scores are calculated using inpatient Medicare FFS volume in year $t-4$ through $t-2$, outpatient Medicare FFS volume in year $t-4$ through $t-2$, share of patients who travel more than 15 miles to see the provider, zipcode median income of the average patient, and the elixhauser comorbidity score of the average patient.

Figure 1.11: *Event study of share of patients who travel from more than 5 miles away, winners of Super Doctors only and propensity score matched control group.*



Note: Event study of share of Medicare FFS patients traveling more than 5 miles away. Travel distance is calculated as distance between the zipcode centroid of the patient's zipcode of residence and the zipcode of the provider facility on the claim. Treatment sample includes SuperDoctor winners who first won the award between 2004 and 2014, is active in Medicare claims data in both 2000 and 2014, and has never won another major "top doctor" award. Propensity scores are calculated using inpatient Medicare FFS volume in year $t-4$ through $t-2$, outpatient Medicare FFS volume in year $t-4$ through $t-2$, share of patients who travel more than 15 miles to see the provider, zipcode median income of the average patient, and the elixhauser comorbidity score of the average patient.

Case complexity

Figures 1.12 and 1.13 shows the coefficients from equation 1.2 for two measures of case complexity for all Medicare FFS inpatient and outpatient procedures performed by SuperDoctor winners and a group of propensity score matched controls. Figure 1.12 measures case complexity using facility payment. Procedures with higher facility payment typically either have a longer length of stay or use more intensive technology. After winning, the intensity of the winners' cases decline

Table 1.6: *Diff-in-diffs regression of share of patients who travel from far away*

	(1) shr Mdcr 5m+	(2) shr Mdcr 15m+	(3) shr Mdcr 30m+
post0_3	-0.00	0.00	0.00
	0.00	0.00	0.00
post0_3*win	0.00	0.00	-0.00
	0.00	0.00	0.00
post4_7	-0.00	-0.00	-0.01
	0.00	0.01	0.01
post4_7*win	0.01*	0.02*	0.02*
	0.01	0.01	0.01
phys. FE	Yes	Yes	Yes
year FE	Yes	Yes	Yes
year*spec FE	Yes	Yes	Yes
N	55604	55604	55604

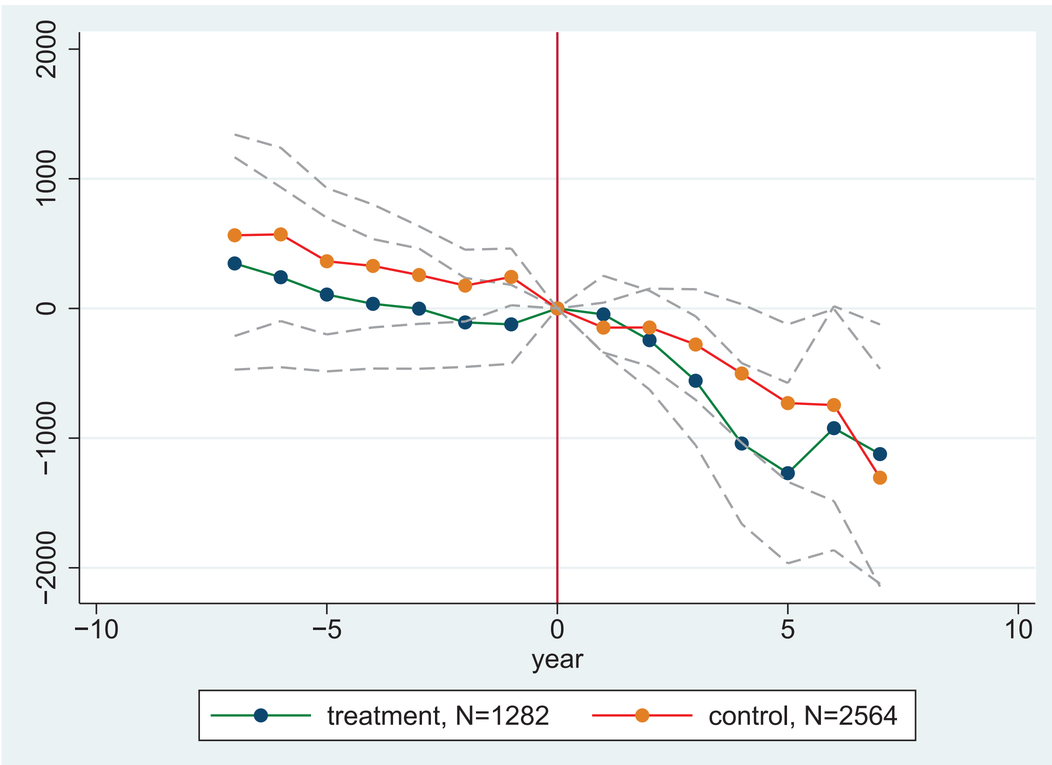
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Covariates that are included but not displayed including indicator variables on being 8 or more years prior to the true or hypothetical first year of award-winning, 8 ore more year after the true or hypothetical first year of award-winning, and the interaction of these indicators with a "true winner" treatment indicator. An indicator for being 1 to 7 years prior to first year of award-winning, which captures the "pre" period, is omitted. Treatment sample includes SuperDoctor winners who first won the award between 2004 and 2014, is active in Medicare claims data in both 2000 and 2014, and has never won another major "top doctor" award. Propensity scores are calculated using inpatient Medicare FFS volume in year $t-4$ through $t-2$, outpatient Medicare FFS volume in year $t-4$ through $t-2$, share of patients who travel more than 15 miles to see the provider, zipcode median income of the average patient, and the elixhauser comorbidity score of the average patient. A "hypothetical award-winning" year for the control group is the year t on which that physician is selected as a matched control.

at a slightly sharper rate, and the gap between the winners and the control group widens, suggesting a possible shift towards less intensive cases among the winners. The difference between the control and treatment group is significant at the 5% level at five years post award-winning.

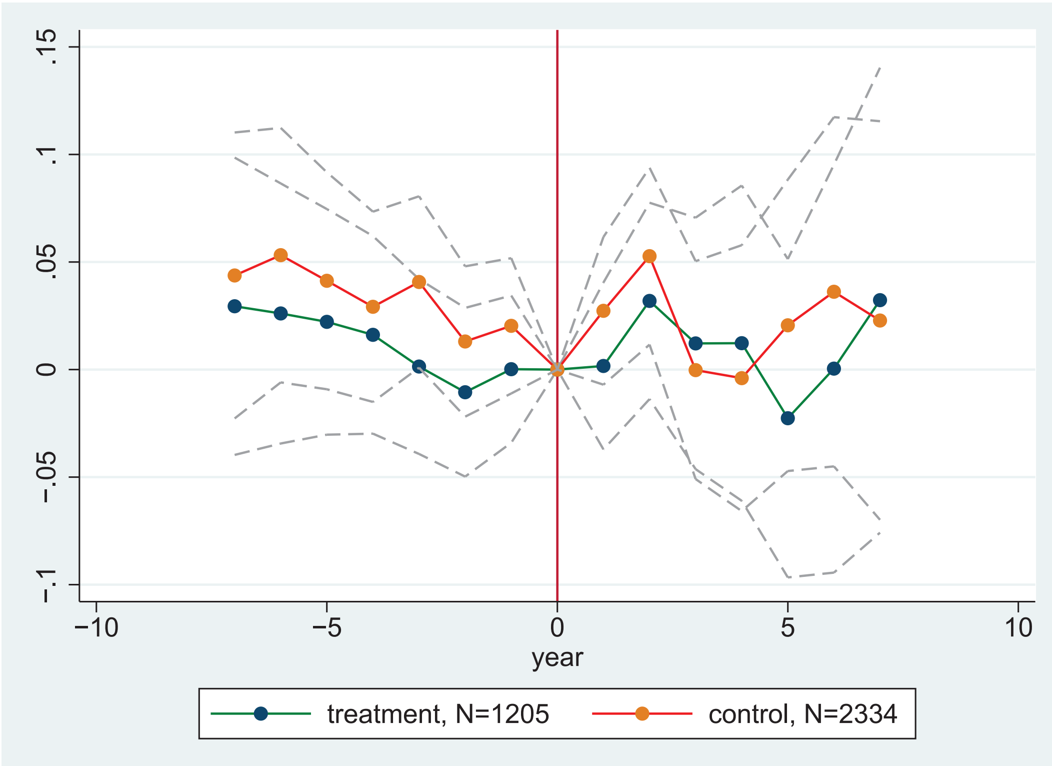
Figure 1.13 measures case complexity using the number of Elixhauser comorbidity scores. There is no significant difference in the mean Elixhauser score between "SuperDoctor" winners and the matched control group either before or after award-winning.

Figure 1.12: *Event study of average facility payment amount for Medicare inpatient or outpatient procedures, winners of Super Doctors only and propensity score matched control group.*



Event study of average facility payment amount for Medicare inpatient and outpatient procedures.Treatment sample includes SuperDoctor winners who first won the award between 2004 and 2014, is active in Medicare claims data in both 2000 and 2014, and has never won another major "top doctor" award. Propensity scores are calculated using inpatient Medicare FFS volume in year $t-4$ through $t-2$, outpatient Medicare FFS volume in year $t-4$ through $t-2$, share of patients who travel more than 15 miles to see the provider, zipcode median income of the average patient, and the elixhauser comorbidity score of the average patient.

Figure 1.13: *Event study of average elixhauser comorbidity scores for Medicare inpatient or outpatient procedures, winners of Super Doctors only and propensity score matched control group.*



Event study of average elixhauser comorbidity scores for Medicare inpatient and outpatient procedures. Treatment sample includes SuperDoctor winners who first won the award between 2004 and 2014, is active in Medicare claims data in both 2000 and 2014, and has never won another major "top doctor" award. Propensity scores are calculated using inpatient Medicare FFS volume in year $t-4$ through $t-2$, outpatient Medicare FFS volume in year $t-4$ through $t-2$, share of patients who travel more than 15 miles to see the provider, zipcode median income of the average patient, and the elixhauser comorbidity score of the average patient.

Taking together, there is no strong evidence that the receipt of a "SuperDoctor" award changes the share of patients who travel from far-away. There is some suggestive evidence that award winners perform procedures with lower intensity after winning, but this change may be driven either by changes in patient composition or changes in practice setting or style.

1.7.4 non-clinical activities

Another potential effect of additional reputation is that it may increase the time a physician spends on non-clinical activities, such as consulting services, speaking engagement, or administrative roles. One dataset that may shed light on this issue is the Medicare Open Payment data on industry payments to physicians. Unfortunately, the data has only been consistently reported for three years from 2014 to 2016, making it challenging to perform panel analyses. Here I provide descriptive evidence on the participation in non-clinical activities. Figure 1.14 shows mean total industry payment amount for surgeons who won either a Castle Connolly Top Doctor or a SuperDoctor award (not necessarily the first time) in 2015 and surgeons who have never won an award as of 2017. The Figure shows that award winners receive much higher industry payments than non-winners, but the panel is not long enough to assess whether award-winning led to additional increases in payments among the winners. To the extent that we think award-winning physicians have a higher "stock" of reputation even without the marginal gain of additional reputation through receiving the award, industry activity and payment do appear to be positively correlated with reputation. Future work will use the more years of data to assess changes in these payments after a physician receives an award.

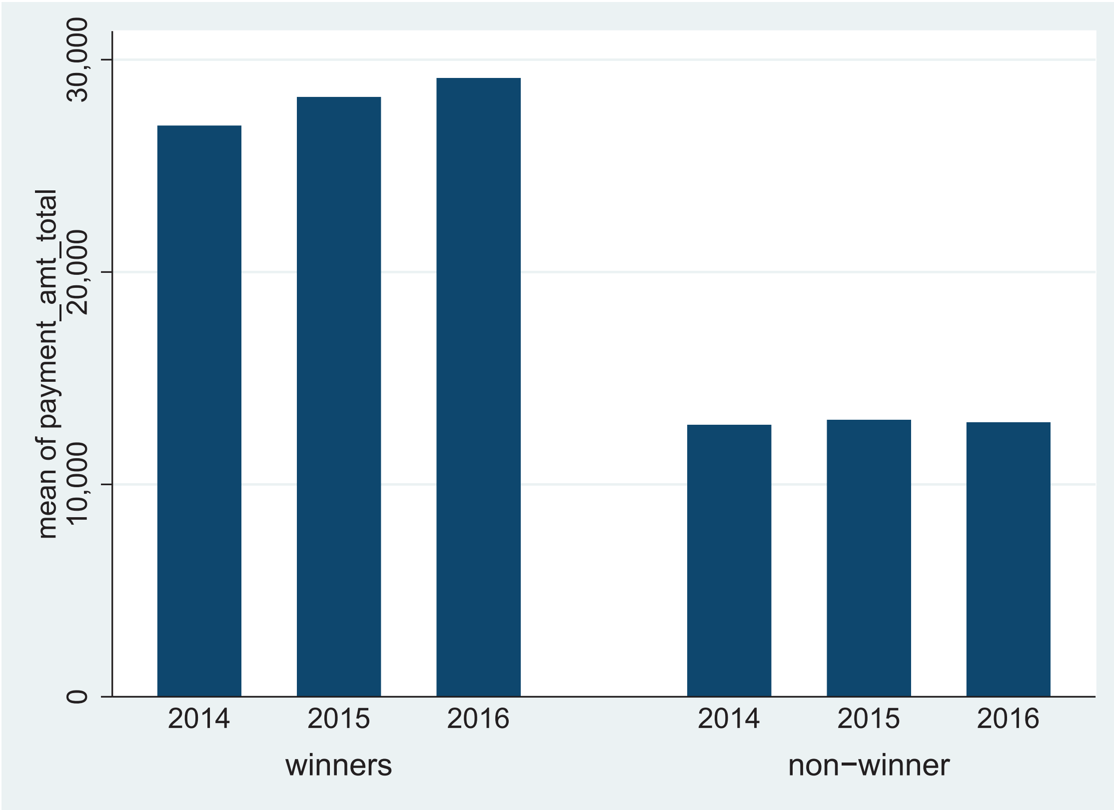


Figure 1.14: *Total industry payment amount for surgeons who won either a Castle Connolly Top Doctor or a SuperDoctor award (not necessarily the first time) in 2015 and surgeons who have never won "top doctor" award as of 2017, Medicare Open Payments data.*

1.8 Discussion

In this study, I first document the characteristics of "top doctor" winners. "Top doctor" winners are more likely to have gone to a top medical school, more likely to work at a highly ranked hospital, more likely to work at a teaching hospital, and have higher procedure volume prior to receiving an award. For cardiac surgeons in states where CABG report cards are available, winners of "top doctor" awards have lower risk-adjusted mortality rates than non-winners, suggesting that "top doctor" awards may capture valuable quality information from a surgeon's peer network.

I found little effect of receiving a "top doctor" award for the first time on a surgeon's inpatient or outpatient volume. Overall, I show evidence that the supply of procedures at a physician is unlikely perfectly elastic. As a physician who has been about 25-30 years out of Medical school gains more years of experience, she decreases the number of inpatient procedures performed and increases the number of outpatient procedures performed. This is true even controlling for the overall trend of declining patient procedures and increase in inpatient procedures. Furthermore, the share of patients who travel from far away increases as a physician gains more experience. Taken together, this set of facts suggests that demand for inpatient service for a given physician may increase with experience, but supply-side decisions make physicians reduce their inpatient procedure volume over time. This nature of the supply cuve means that we cannot rely on volume response to assess demand responses at the physician level. It is possible that award-winning has increased the demand for Medicare FFS patients, but the surgeon chose to hold fixed or even decrease the volume of procedures supplied because additional reputation increased their payoff elsewhere: they may be able to charge higher prices in the privately insured market or be invited to participate in more non-clinical activities such as teaching and consulting.

When quantity is constrained on the supply side, change in demand may not lead to change in total quantity, but can still lead to changes in patient composition. I find a small effect of receiving a "top doctor" award on the share of patients who travel from far away, and no effect on the complexity of patients as measured by Elixhauser comorbidity scores.

Lastly, I find that winning the award increased the share of cardiac and orthopedic surgeons who perform at specialty hospitals. This effect is economically large. Working in a specialty hospital is usually associated with higher financial gain (many specialty hospitals are physician-owned) and more flexibility in scheduling staff and facility time. This result suggests that award-winning may have increased the surgeon's bargaining power to make more favorable work arrangement. This increase in bargaining power likely comes from an increase in demand somewhere, or at least believes in increased demand for these surgeons by their bargaining counter-parties (partners of specialty hospitals or insurance companies).

These findings imply that although "top doctor" award may signal high-quality physicians, the publicity of these awards does not seem to be an effective mechanism to direct Medicare FFS patients to higher-quality surgeons. However, there might be gains from these "top doctor" titles accrued to physicians. An important next step is to study how the payer-mix of a surgeon's cases and the surgeon's negotiated prices respond the receipt of "top doctor" awards. Another interesting next step is to understand the heterogeneity of top-award across providers. Since providers can purchase additional advertisement citing their award-winning status beyond the publicity automatically provided by the awarding company, it may be interesting to consider whether the impact of these awards differs between providers who actively advertise and those who do not.

Chapter 2

The Effect of Medicaid Expansion on Hospital Financial Performance and Strategy

2.1 Introduction

Expanding Medicaid eligibility to cover all adults with income up to 138 percent of Federal Poverty Line (FPL) is a key component of the Affordable Care Act (ACA). Originally a mandate for all states, this expansion established a new coverage pathway for millions of uninsured adults who were previously excluded from Medicaid, beginning January 1, 2014. The law also provided for 100% federal funding of the expansion through 2016, declining gradually to 90% in 2020 and future years. However, the Supreme Court ruling on the ACA in June 2012 effectively made the Medicaid expansion optional for states.

As of March 2018, 33 states (including Washington D.C.) have chosen to expand Medicaid. Of these 33 states, coverage under the Medicaid expansion became effective on January 1st, 2014 in all except for the following: Michigan (4/1/2014), New Hampshire (8/15/2014), Pennsylvania (1/1/2015 Indiana (2/1/2015), Alaska (9/1/2015), Montana (1/1/2016), Louisiana (7/1/2016), and Maine (to be determined). 18 states have no intention of adopting the Medicaid expansion at this time.[1]

State-level variation in the choice to adopt the Medicaid expansion provides an opportunity to study the impact of a large-scale public insurance program. Timely evidence of such impact may influence policy decisions of the 18 states who have not yet made the decision to expand Medicaid as well as providing valuable information for the states that have already done so. An important policy question is how such expansion may affect hospitals. Many view the Medicaid expansion as financially beneficial to hospitals because it decreases the cost of uncompensated care and increases revenue when formerly uninsured patients obtain health coverage. On the other hand, expansion may also cost hospitals if Medicaid crowds out existing private insurance coverage from employers or from the insurance marketplace, which usually provides higher payment to hospitals than Medicaid. Another reason that the Medicaid may impact hospitals financially is that Medicaid expansion may increase the utilization of those who were previously uninsured (Finkelstein et al., 2012, Taubman et al., 2014). If Medicaid payment is above the cost of care for such additional utilization, the expansion will result in additional revenue to hospitals. On the other hand, if Medicaid payment is below the cost of care for such additional utilization, the expansion will bring additional cost rather than gain.

A 2013 report from the Urban Institute projected that the Medicaid expansion would bring a net gain to hospitals. Altogether, for each dollar in private revenue that a Medicaid expansion eliminates, hospitals' Medicaid revenue rises by $2.59(Dorn et al., 2013). This estimate does not take into account any potential change in utilization as a result of the expansion, With data on hospital revenue and cost available three years after the Medicaid expansion became effective in 33 states, it is now interesting and possible to re-examine the impact of this expansion using data on realized, rather than projected, outcomes.

It is worth noting that the ACA includes a number of other provisions that may affect hospitals' financial situation. The ACA will cut funding to "Disproportionate Share Hospitals" (DSH) by $56 billion during 2013-2022 (though this provision is delayed until 2018). In addition, the legislation will also reduce Medicare fee-for-service hospital payments by $260 billion. However, both of

[1] Kaiser Family Foundation (http://kff.org/health-reform/state-indicator/state-activity-around-expanding-medicaid-under-the-affordable-care-act/)

these provisions are at the federal level, without discretion of the states. This study focuses on the marginal impact of the Medicaid expansion to hospitals, in the existence of these other provisions. Interpretation of cross-state comparisons rests on the assumption that Medicare fee-for-services cuts do not affect states differentially in a way that is correlated to their decisions and magnitudes of Medicaid expansion. Under such assumption, if the Urban Institutes' projection holds true, hospitals in states that choose to expand will still see greater revenue gain (less revenue loss) relative to those in states without expansion. To verify this, I compare the hospital-level cost and revenue in states that expanded Medicaid as of January 1st, 2014 to those in states that have not done so as of July 2016.

The discussion on the potential financial impact of the Medicaid expansion on hospitals so far has assumed hospitals to be static, passive agents. In reality, hospitals may respond strategically to policy changes. Thus the impact of a policy on a hospital's financial situation may not be fully reflected in net changes in total cost and total revenue. Furthermore, from a policymaker's perspective, we care about the financial impact of Medicaid expansion on hospitals because such changes may affect hospitals' ability to deliver care. If Medicaid expansion changes hospitals' financial performance by increasing revenue and decreasing debt, it is interesting to ask what hospitals do with the additional income. Do they hold the additional money in reserve? Do they invest in new technology and resources? Do they expand in scale and scope? Previous literature has found that policies that led to increased revenue for hospitals may not necessarily lead to improvement in patient outcomes because hospitals did not allocate the additional revenue to care delivery. Work by Duggan (2000) evaluating California'ss DSH program found that infant mortality rates were unaffected because subsidies of over $1 billion per year did not translate into increased spending on patients. Results from Baicker and Staiger (2005) suggest that the impact of the DSH program varies by state. In states where the government could engage in strategic behavior to capture most of the DSH money, there was little net impact on hospitals resources or patient care. On the other hand, in states that were less able to divert the targeted funds, additional DSH money was associated with improved patient outcomes.

Hospitals' responses to their financial situation may also have allocative implications. One way in which hospitals may respond to a Medicaid expansion is that they may change the price they charge to private payers, a phenomenon often labeled "cost-shifting" in the literature. The rationale is that hospitals may have been charging private payers additional amounts in order to subsidize the provision of uncompensated care. As more uninsured are covered by Medicaid, this cross-subsidization is no longer necessary, and hospitals may decrease the price for private payers in order to gain additional market share. Another way in which hospitals may change their strategy is altering the service mix. For example, hospitals may have been allocating their resources across different service lines in a way to discourage utilization from less profitable patients. Medicaid expansion may change such incentives and consequently the service line mix of hospitals.

The hospital market is served by hospitals with many different organizational structures. These organizational structures may lead hospitals to respond to changes in their financial situation differently. To test this, I estimate the strategic response of not-for-profit, for-profit, and public hospitals separately. I also separately look at hospitals that served a high share of uninsured patients prior to the ACA expansion. These hospitals may be disproportionally affected by the Medicaid expansion.

This paper contributes to a range of literature in health economics and health policy. A timely analysis of a key component of the ACA, it contributes to a large literature on the impact of public insurance. The bulk of this literature focuses on the impact on enrollees, but a there is a growing interest in looking at the impact of public insurance on providers (Buchmueller et al., 2014, Garthwaite, 2012). Even less evidence is available on how public insurance may affect patient outcomes *through* providers. By looking at the effect of Medicaid expansion on private prices, this paper contribute to an older literature on cost-shifting, a phenomenon where hospitals shift costs to private patients because of inadequate revenue from Medicare and Medicaid (Cutler, 1998, Frakt, 2011). Lastly, taking advantage of a large-scale policy change that directly affects hospitals' bottom line, this paper shed light on the decision-making process of hospitals. The impact of Medicaid expansion on hospital strategy and investment decision can improve our systematic understanding of hospitals' objective function, especially given that the majority of hospitals are not-for-profit. Findings from this paper will highlight the role that hospital strategic decisions plays in determining whether public policy changes translates into their intended goals.

Consistent with other papers on this literature (Dranove et al., 2016, Nikpay et al., 2015), I found that Medicaid expansion as part of the Affordable Care Act increased Medicaid revenue and decreased the cost of uncompensated care. However, the increase in Medicaid revenue did not translate into increases in total revenue, suggesting that the financial gain from Medicaid expansion was offset in other channels. I explore different potential explanations for this offset. I found that the offset is too large to be explained by crowd-out of private insurance alone or by changes in state-local indigent funds and other government appropriations. I find no evidence of price-shifting from privately insured patients for the average hospital. However, for hospitals that served a high share of uninsured patients before the expansion, I find evidence consistent with a negative effect of Medicaid expansion on private insurance prices. I show two other pieces of evidence that partially explain this gap. First, there was a stagnation of the sum of private revenue and non-inpatient revenue from Medicare for hospitals in hospitals in Medicaid expansion states around the time of expansion. At this same time, there was an increase in this revenue for hospitals in non-expansion states around the same time. Second, hospitals in the expanding states saw greater decreases in non-operating revenue. I argue that these results are explained by a model where hospitals are risk-averse firms that may incur nonmonetary "strategic search cost" when searching for ways to improve its financial status. In this model, a Medicaid revenue gain decreases the marginal return of additional effort invested to generate revenue elsewhere and reduced the total amount of such effort, leading to an offset of total revenue.

Public hospitals are an exception to the findings above. Public hospitals in Medicaid expansion states experienced the largest increase in Medicare revenue, and this increase translated to an increase in total revenue almost dollar for dollar, if not more. One potential explanation is that public hospitals have fewer strategic levers to use to boost their revenue had the expansion had not taken place. Consistent with experiencing a gain in total revenue and operating margin, public hospitals in Medicaid expansion states had greater salary expenditure and greater capital investments compared to public hospitals in non-expansion states after the expansion.

2.2 Data

The main data source for this analysis is the Medicare Cost Reports. These reports reflect information as reported to the Healthcare Cost Report Information System (HCRIS) by Medicare administrative contractors. A fiscal year as defined in these data covers October from the previous calendar year to September of the current calendar year. As of March 2018, data for the entire fiscal year of 2016 (October 2015-September 2016) are available. I consider data from the 2014 fiscal year to fall under the post-ACA Medicaid expansion period. This seems reasonable because 2/3 of this fiscal year falls after January 1, 2014, the date when the expansion became effective in the majority of the expanding states. In addition, average monthly Medicaid enrollment in the whole country during the first nine months of 2014 period is about 113% of the pre-ACA average, suggesting that the Medicaid expansion had a large, immediate effect on coverage. I use all data available in the latest release of the Medicare cost reports, covering information from fiscal year 2011 to fiscal year 2016.

The Medicare cost reports cover a rich set of information, including total patient revenue and expenses, Medicare/Medicaid revenues and expenses, as well as types of expenses/cost, admissions, number of beds, and number of residents. However, these reports are not without limitations. First, past studies have shown that there appeared to be extremely large outliers for many variables that appear to be due to misreporting rather than true differences (Cutler, 1998, Dranove and Lindrooth, 2003). Following conventional practice in the literature, these outliers will be dropped[2]. Second, information included in the cost reports is self-reported by hospitals for the purpose of receiving payment from CMS. Although CMS has made a reasonable effort to ensure that the provided data/records/reports are up-to-date, accurate, complete, and comprehensive at the time of disclosure, it does not ensure that the information has not been misrepresented or misinterpreted by the hospital. It is not surprising that hospitals may have an incentive to "optimizing" reporting, making these reports have limited reliability for understanding the true resource cost to hospitals (Kane and Magnus, 2001, Magnus and Smith, 2000).

[2]Specifically, I drop observations that are greater than five times the 99th percentile of the variable distribution within a year. This cutoff is more generous than the conventional practice of dropping outliers greater than two times the 99th percentile of the distribution to account for the inherent skewness of some financial outcomes. The results are robust to using alternative cutoffs of two or three times the 99th percentile.

However, these reports can still provide valuable and reasonably reliable information for this study for a couple of reasons. First, the main focus of this paper is the financial need of the hospital rather than true resource cost. Second, to the extent that hospital employs discretion in "optimizing" reporting practices, one would expect such practices to be fairly consistent within the same hospital over time. Recent studies have suggested that the quality of cost-report data has improved dramatically since the adoption of the new form format in 2010 (Dranove et al., 2017).

I am interested in the effect of Medicaid expansion on a number of hospital-level outcomes. First, to confirm that the Medicaid expansion did lead to an increase in Medicaid revenue through more Medicaid-covered admissions, I look at the effect of the expansion on Medicaid revenue, cost of uncompensated care, and number of admissions paid by Medicaid. Second, to measure hospital's overall financial performance, I look hospitals total revenue, total expenses, total net income, total margin, operating revenue, operating expenses, operating margin. Ideally, I would be able to divide total operating revenue by payer. In practice, this is not quite possible in the Cost Report data. For Medicaid patients, I observe total Medicaid revenue (including DSH, which may count as non-operating revenue). For Medicare patients, I distinctively observe FFS revenue. For uninsured patients, I observe payment from State and County Indigent programs, which may count as operating or non-operating revenue depending on how it is implemented. To understand how the composition of revenue changes in response to Medicaid expansion, I look at each of the aforementioned component separately as well as a "residual" component, which equals to total revenue minus Medicaid revenue, Medicare FFS revenue, and payment from State and County Indigent programs. This residual revenue is a combination private revenue, Medicare payment for outpatient services,and other Medicare payments not tied to inpatient visits(e.g. payment to affiliated psychiatric facilities, hospice facilities, inpatient rehabilitation facilities, home health care services and outpatient services. Payment for Medical Education is included.). Finally, I also examine non-operating revenue (which includes investment income, parking fees, income from cafeteria and gift shops, private donations, etc) as a separate category. Third, to measure hospital behavior and strategy, I look at total salary expenses, capital investment in building and building improvement, capital investment in land and land improvement, capital investment in fixed and movable equipment, and total capital investment. I also measure estimated private prices, where private prices are calculated from the Medicare cost reports using the formula $\left(\frac{\text{private revenue}}{\text{no. private patients}}\right) \times (\text{case mix adjuster})$(Cutler, 1998). [3].

If the Medicaid expansion has led to changes in hospital strategy, we are particularly interested in if such change affected hospital quality and patient outcomes. I measure patient outcome using 30-day readmission rates and 30-day mortality rates from the Hospital Compare datasets produced by Center for Medicare and Medicaid Services (CMS). These rates are calculated for three conditions (heart failure, heart attack, and pneumonia) for the Medicare population. While this is not a population that's directly affected by the Medicaid population, we still consider these measures useful measures of hospital qualities for a couple of reasons. First, previous research has shown that the quality of hospital care exhibits characteristics of "public good," in the sense that improvement in quality for patients of one payer group also means improvement in quality for patients of other payer groups (Chen et al., 2010b). Second, precisely because these quality measures are not calculated from the Medicaid population, any changes in the composition of patient risk profile as a result of the Medicaid expansion is unlikely to affect this quality measure directly.

I also look at the heterogeneity of the effect of Medicaid expansion based on a number of hospital characteristics. First, I divide hospitals by ownership status. Second, I divide based on the share of discharges that were uninsured in 2009. Counts of uninsured admissions are not available in the Cost Report. For this second analysis, I include data from The State Inpatient Databases(SID) from the Healthcare Cost and Utilization Project(HCUP) from 12 states: Arkansas, Colorado, Iowa, Kentucky, Maryland, New Jersey, New York, Oregon, Vermont, Florida, Utah, and Wisconsin.

[3] see Appendix B.3.1 for details

2.3 Empirical Strategy

2.3.1 Sample Selection

Theoretically speaking, a hospital is considered "treated" in a fiscal year if Medicaid expansion happened at the beginning of that fiscal year. In practice, fiscal years vary from one hospital to another. The Medicare cost reports standardize all fiscal years to be from 09/30 of the previous year to 10/01 of the current year. The majority of states that have chosen to expand Medicaid as of October 2015 did so on 01/01/2014. We considered all of these states that expanded on 01/01/2014 "expansion states". In addition, Michigan expanded Medicaid on 04/01/2014. We also considered Michigan an expansion state, because half of the 2014 fiscal year is post-expansion. Eight other states adopted the Medicaid expansion between 4/1/2014 and 1/1/2016, including Indiana (2/1/2015), New Hampshire (8/15/2014), Pennsylvania(1/1/2015), Rhode Island (1/1/2015), Vermont(1/1/2015), Washington(1/1/2015), Alaska (9/1/2015), Montana (1/1/2016). I dropped these states from my sample. In addition, I dropped Arizona, Connecticut, California, Massachusetts, Minnesota, and District of Columbia. We discarded Arizona and Massachusetts because those states had increases in Medicaid eligibility for childless adults that were not tied to changes in income limits for eligibility. We discarded Connecticut, District of Columbia, California and Minnesota because these states expanded Medicaid eligibility for childless adults during the 2011âĂŞ13 period. Louisiana expanded Medicaid on 7/1/2016. I considered Louisiana a non-expansion state for my analysis because 2016 was the last year of data in my sample. Maine adopted the Medicaid expansion through a ballot initiative in November 2017. I considered Maine a non-expansion state in my analysis.

I limited my sample to short-term acute hospitals with six years of complete financial data. This restriction abstracted from mergers, acquisitions, and hospital closures. I also focused on hospital with more than 50 beds. In addition, I excluded hospitals with missing data on Medicaid revenue, cost of uncompensated care, total revenue, Medicaid discharges, or total discharges during any year between 2011 and 2016. The final sample included 720 hospitals in expansion states and 800 hospitals in non-expansion states.

2.3.2 Difference-in-differences analysis

Similar to (Dranove et al., 2016), I use a difference-in-differences model to compare states that have adopted the expansion and those that did not.

$$Y_{ist} = \theta_t + s_i + \beta_1(PostACA_t \times Expand_s) + X_{ist}^{'}\delta + \epsilon_{ist} \tag{2.1}$$

where Y_{ist} an outcome of interest, θ_t captures year fixed effects, and s_i state fixed effects. X_{ist} is a vector of time-invariant hospital characteristics. $PostACA_t$ is a variable that is 1 for fiscal year 2014 and later, $Expand_s$ is an indicator variable for whether state s expanded Medicaid in 2014. The coefficient of interest is β_1, which captures the effect of Medicaid expansion. I cluster standard errors at the state level.

A key assumption is that the pre-expansion trend in Y_{ist} is similar between the expanding and non-expanding states. I estimate an alternative model that both tests for this assumption and controls for pre-trend differences if such differences exist. This alternative model takes the form

$$Y_{ist} = \theta_t + s_i + \sum_{t>2010} \beta_t(d_t \times Expand_s) + X_{ist}^{'}\delta + \epsilon_{ist} \tag{2.2}$$

where d_t is an indicator variable for year t for all years in my data.

In some cases, the parallel trend assumption may be violated. In these cases, I perform an alternative specification that accounts for pre-trend following Wolfers(2006):

$$Y_{ist} = \theta_t + s_i + \sum_{s} \alpha_s State_s \times Year + \sum_{t>2014} \beta_t(d_t \times Expand_s) + X_{ist}^{'}\delta + \epsilon_{ist} \tag{2.3}$$

where α_s captures state-level pre-trend prior to the expansion.

2.3.3 Differences between hospitals

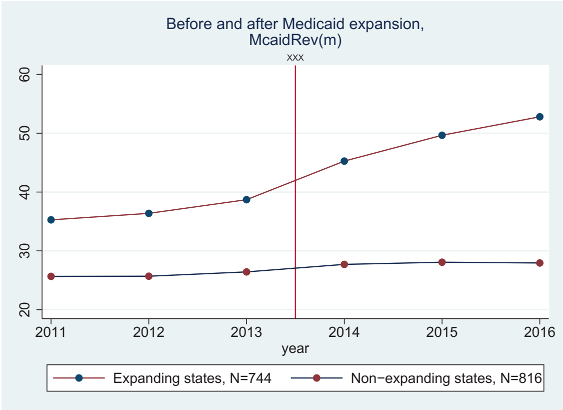
For all the effects examined above, it is interesting to look at how such effect may differ across different types of hospitals. In this section, I estimate the heterogeneous impact of Medicaid expansion on hospital financial status based on different dimensions of heterogeneity in hospital characteristics. First, I estimate the varying effect by hospital ownership (public, for-profit, not-for-profit). Second, I estimate the varying effect by whether the hospital has a high share of uninsured discharges in 2009 in states with HCUP-SID data. For this heterogeneity analysis, estimate Equations 2.1 to 2.3 separately for each subgroup of interest.

2.4 The effect of Medicaid expansion on Medicaid revenue and cost of uncompensated care

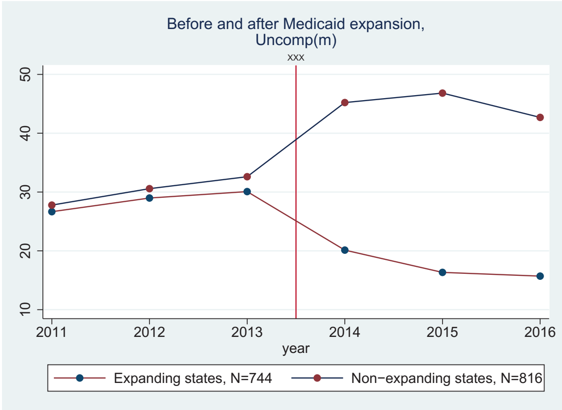
Graph 2.1 shows that Medicaid expansion led to an increase in Medicaid revenue and a decrease in the cost of uncompensated care among hospitals in Medicaid expanding hospitals. Column (1) and (2) of Table 2.1 shows the difference-in-differences of the effect of Medicaid expansion on Medicaid revenue and cost of uncompensated care per Equation 2.1. These columns show that the Medicaid expansion led to a roughly 11.5 million increase in Medicaid revenue and a 25 million decrease in cost of uncompensated care per hospital. Table 2.2 shows that, consistent with Graph 2.1, the similar pre-trend assumption for Medicaid revenue and cost of uncompensated care between expansion and non-expansion states holds, and that the effect of the expansion on Medicaid revenue is increasing over time since the implementation of the expansion. The increase in Medicaid revenue among hospitals in expansion states following expansion is roughly 5% of a hospital's total revenue. Note that the decrease in the cost of uncompensated care among hospitals in expansion states relative to that among hospitals in non-expansion states post-2014 is partially driven by a discontinuous increase among hospitals in non-expansion states. This increase is robust to the exclusion of outliers (at 2*99th, 3*99th, or 5*99th percentile of the hospital-year distribution) and to subgroup analysis that focuses on urban hospitals or hospitals in eight representative metropolitan areas (Detroit, Chicago, Cleveland, Newark, Atlanta, Dallas, Miami, and Birmingham, see Appendix B.1). It is likely the increase in the cost of uncompensated care among hospitals in non-expansion states is a result of reporting changes. If we think such reporting change did not happen in non-expansion states, the magnitude of the effect of Medicaid expansion on the cost of uncompensated care is halved.

Graph 2.2 looks at the average Medicaid discharge and total discharges for hospitals in expansion states and non-expansion states over time. It shows that the increase in Medicaid revenue among hospitals in expansion states post-expansion corresponds to an increase in Medicaid discharges between 2013 and 2014. This increase does not appear to continue in 2015 and 2016. It is possible that the increase in Medicaid revenue among hospitals in expansion states is driven more by changes in case-mix rather than changes in the total number of utilization. Further more, we see a decrease in the number of Medicaid discharges among hospitals in non-expansion states between 2013 and 2014. This is despite the fact that Medicaid enrollment has been increasing slightly in these states (Courtemanche et al., 2017). One possible explanation is that the trend in Medicaid discharges signals an effort of hospitals in non-expansion states to shy away from Medicaid patients in order to protect their bottom line. This explanation is consistent with the fact that total discharges in these hospitals increased post-2014 even though Medicaid discharges continue to decrease. Columns (3) and (4) of Table 2.2 shows that hospitals in expansion and non-expansion states show differential trends in both Medicaid discharges and total discharges prior to the ACA expansion. Once state-specific trends are controlled for, as in Table 2.3, hospitals in expansion states saw an increase in Medicaid discharges and an increase in total discharges (though not statistically significant) relative to hospitals in non-expansion states after expansion.

Figure 2.1: *Medicaid Revenue and Cost of Uncompensated Care*



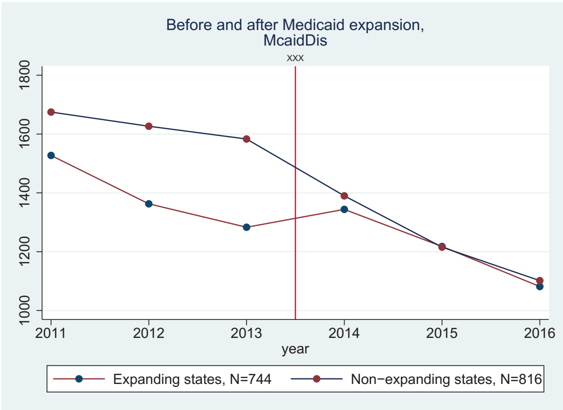
Medicaid revenue (millions)



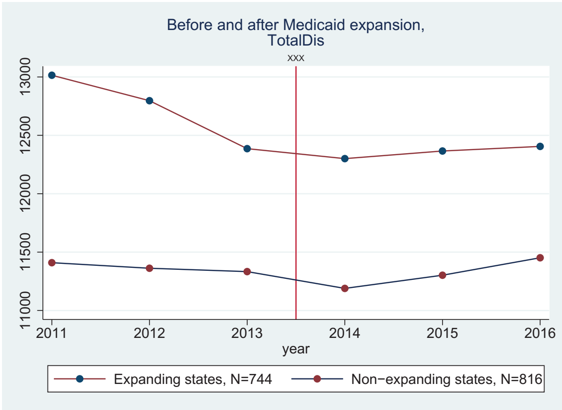
Cost of uncompensated care (millions)

Note: Mean Medicaid revenue (DSH payment included) and cost of uncompensated care for hospitals in Medicaid expansion states and non-expansion states. Only short-term acute hospitals with complete financial data for all years from 2011 to 2016 are included. Expansion states include AR, CO, DE, HI, IA, IL, KY, MD, MI, ND, NJ, NM, NV, NY, OH, OR, PA, RI, VT, WA, WV, non-expansion states include AL, FL, GA, ID, KS, LA, ME, MO, MS, NC, NE, OK, SC, SD, TN, TX, UT, VA, WI, WY. Cost of uncompensated care is calculated from Form S10 of the Medicare Cost Report and equals to the sum of charity care charges (minus payment for charity care) and bad debt.

Figure 2.2: *Medicaid Discharges and Total Discharges*



Medicaid discharges



Total discharges

Note: Mean Medicaid inpatient discharges and total inpatient discharges for hospitals in Medicaid expansion states and non-expansion states. Only short-term acute hospitals with complete financial data for all years from 2011 to 2016 are included. Expansion states include AR, CO, DE, HI, IA, IL, KY, MD, MI, ND, NJ, NM, NV, NY, OH, OR, PA, RI, VT, WA, WV, non-expansion states include AL, FL, GA, ID, KS, LA, ME, MO, MS, NC, NE, OK, SC, SD, TN, TX, UT, VA, WI, WY.

Table 2.1: *Medicaid revenue and cost of uncompensated care, Diff-in-diff*

	(1)	(2)	(3)	(4)
	McaidRev(m)	Uncomp(m)	McaidDis	TotalDis
	b/se	b/se	b/se	b/se
expand*post	11.52***	-25.01***	245.88	-114.44
	1.44	4.49	153.18	158.98
Urban hospital	15.77***	14.75***	541.18***	5319.13***
(CMS)	2.46	2.19	69.58	285.11
teaching	40.26***	31.28***	1243.07***	8850.30***
	5.24	4.88	156.99	727.05
Public Hospital	33.63***	19.11***	726.16***	3350.03**
	7.66	4.06	187.35	1073.19
Private Non	5.76	12.17**	286.75	3625.06***
Profit	3.20	3.44	173.79	877.04
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	9618	9618	9618	9618

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Tables shows estimates from Equation 2.1. Standard errors are clustered at the state level. Column 1 shows the effect of Medicaid expansion on Medicaid revenue (including DSH payment) in millions. Column 2 shows the effect on the cost of uncompensated care. Column 3 shows the effect of Medicaid discharges, Column 4 shows the effect on total discharges. Expansion states include AR, CO, DE, HI, IA, IL, KY, MD, MI, ND, NJ, NM, NV, NY, OH, OR, PA, RI, VT, WA, WV, non-expansion states include AL, FL, GA, ID, KS, LA, ME, MO, MS, NC, NE, OK, SC, SD, TN, TX, UT, VA, WI, WY. Cost of uncompensated care is calculated from Form S10 of the Medicare Cost Report and equals to the sum of charity care charges (minus payment for charity care) and bad debt.

2.5 The effect of Medicaid expansion on overall hospital financial health

Figure 2.3 shows mean total revenue and operating revenue for hospitals in expansion and non-expansion states over time. Hospitals in expansion states have slightly faster growth in total and operating revenue before 2014, but there is no discernible additional gain in total or operating revenue for hospitals in expansions states after expansion relative to hospitals in non-expansion states. Table 2.5 shows that once controlling for state-specific pre-trend, there's no increase in total revenue in hospitals in Medicaid expansion states relative to hospitals in non-expansion states after expansion. There's a 0-6 million increase in operating revenue, but this increase is not statistically significant and is much smaller than the 5-10 million increase in Medicaid revenue shown in Table 2.3. At the same time, the magnitude of increase in total expenses and operating

Table 2.2: *Medicaid revenue and cost of uncompensated care, Diff-in-diff*

	(1) McaidRev(m) b/se	(2) Uncomp(m) b/se	(3) McaidDis b/se	(4) TotalDis b/se
expand*2011	-2.57**	1.49	155.27	573.49*
	0.90	1.44	89.07	219.95
expand*2012	-1.67*	1.00	36.08	416.00*
	0.72	0.93	55.26	200.34
expand*2014	5.72***	-22.18***	267.10**	163.43
	0.86	4.19	82.88	211.54
expand*2015	10.61***	-27.01***	339.84	274.34
	1.45	5.29	185.87	267.64
expand*2016	14.00***	-23.35***	322.05	208.42
	2.05	4.61	214.03	271.93
Urban hospital	15.77***	14.75***	541.24***	5319.68***
(CMS)	2.46	2.19	69.62	285.18
teaching	40.28***	31.27***	1243.21***	8850.21***
	5.24	4.88	156.99	727.29
Public Hospital	33.63***	19.12***	726.28***	3350.47**
	7.66	4.06	187.40	1073.24
Private Non	5.76	12.18**	286.64	3625.30***
Profit	3.20	3.45	173.69	876.97
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	9618	9618	9618	9618

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Tables shows estimates from Equation 2.2. Standard errors are clustered at the state level. Column 1 shows the effect of Medicaid expansion on Medicaid revenue (including DSH payment) in millions. Column 2 shows the effect on the cost of uncompensated care. Column 3 shows the effect of Medicaid discharges, Column 4 shows the effect on total discharges.Expansion states include AR, CO, DE, HI, IA, IL, KY, MD, MI, ND, NJ, NM, NV, NY, OH, OR, PA, RI, VT, WA, WV, non-expansion states include AL, FL, GA, ID, KS, LA, ME, MO, MS, NC, NE, OK, SC, SD, TN, TX, UT, VA, WI, WY. Cost of uncompensated care is calculated from Form S10 of the Medicare Cost Report and equals to the sum of charity care charges (minus payment for charity care) and bad debt.

Table 2.3: *Medicaid revenue and cost of uncompensated care, Diff-in-diff, controlling for pre-trend*

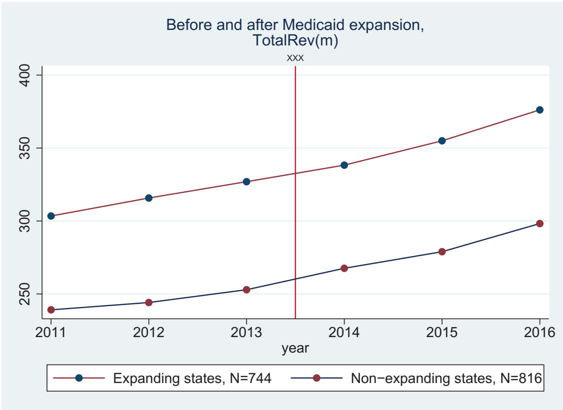
	(1) McaidRev(m) b/se	(2) Uncomp(m) b/se	(3) McaidDis b/se	(4) TotalDis b/se
expand*2014	4.57***	-21.53***	358.63***	407.23
	1.00	4.38	96.31	274.02
expand*2015	8.17***	-25.61***	509.04*	804.96
	1.51	5.78	210.44	416.02
expand*2016	10.29***	-21.20***	568.93*	1025.90
	2.10	5.81	262.36	533.44
Urban hospital	15.80***	14.68***	543.39***	5325.25***
(CMS)	2.46	2.20	69.05	285.63
teaching	40.31***	31.20***	1246.90***	8855.70***
	5.25	4.86	157.72	729.09
Public Hospital	33.65***	19.12***	726.75***	3357.95**
	7.68	4.06	187.03	1076.57
Private Non	5.77	12.18**	290.08	3633.33***
Profit	3.22	3.45	174.29	879.54
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Pretrend	Yes	Yes	Yes	Yes
N	9618	9618	9618	9618

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

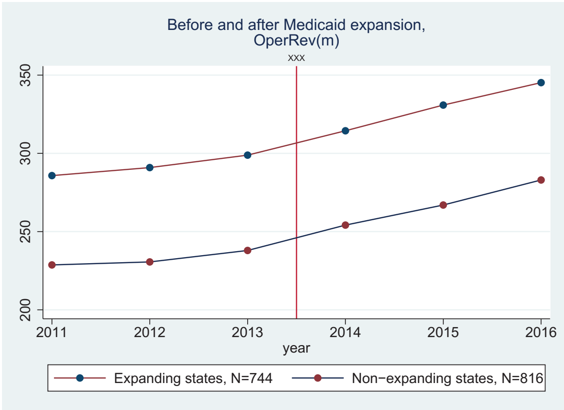
Note: Tables shows estimates from Equation 2.3. Standard errors are clustered at the state level. Column 1 shows the effect of Medicaid expansion on Medicaid revenue (including DSH payment) in millions. Column 2 shows the effect on the cost of uncompensated care. Column 3 shows the effect of Medicaid discharges, Column 4 shows the effect on total discharges. Expansion states include AR, CO, DE, HI, IA, IL, KY, MD, MI, ND, NJ, NM, NV, NY, OH, OR, PA, RI, VT, WA, WV, non-expansion states include AL, FL, GA, ID, KS, LA, ME, MO, MS, NC, NE, OK, SC, SD, TN, TX, UT, VA, WI, WY. Cost of uncompensated care is calculated from Form S10 of the Medicare Cost Report and equals to the sum of charity care charges (minus payment for charity care) and bad debt.

expenses are even greater. Taking all together, Medicaid expansion lead to almost no change in hospitals' total margin and operating margin among large post-acute hospitals. This is consistent with findings by Young (2017) using tax filing data.

Figure 2.3: *Total and operating revenue*



Total revenue (millions)



Operating revenue (millions)

Note: Operating Revenue includes revenue from patient care. Total revenue is operating revenue plus non-operating revenue, which includes revenue from investment, private donations, parking, gift shops, cafeteria, and other auxiliary services. Expansion states include AR, CO, DE, HI, IA, IL, KY, MD, MI, ND, NJ, NM, NV, NY, OH, OR, PA, RI, VT, WA, WV, non-expansion states include AL, FL, GA, ID, KS, LA, ME, MO, MS, NC, NE, OK, SC, SD, TN, TX, UT, VA, WI, WY.

Table 2.4: *Overall Financial Health, Diff-in-Diffs, Dynamic Effect*

	(1) TotalRev(m) b/se	(2) TotalExp(m) b/se	(3) NetInc(m) b/se	(4) TotalMargin b/se	(5) OperRev(m) b/se	(6) OperExp(m) b/se	(7) OperMargin b/se
expand*2011	-9.09	-4.15	-4.93	-0.01	-3.38	-3.35	0.01
	5.59	4.41	3.53	0.01	4.92	4.43	0.01
expand*2012	-1.86	-1.44	-0.43	-0.00	-0.22	-1.33	0.00
	2.95	2.69	1.63	0.00	2.64	2.74	0.00
expand*2014	-0.16	0.77	-0.93	0.00	2.27	0.91	0.01*
	2.73	2.42	1.35	0.01	2.42	2.37	0.00
expand*2015	10.04	12.36*	-2.33	-0.00	10.44	12.51*	0.00
	5.89	5.86	2.86	0.01	5.17	5.80	0.01
expand*2016	13.42	21.09*	-7.67	-0.01	9.83	20.35*	-0.00
	8.08	7.95	3.85	0.01	7.25	7.95	0.01
Urban hospital	125.74***	117.27***	8.47***	0.01	118.54***	116.88***	0.02*
(CMS)	8.93	8.30	1.91	0.01	8.19	8.26	0.01
teaching	274.15***	258.34***	15.81***	0.01	251.98***	256.88***	-0.01
	20.73	20.45	3.18	0.00	18.72	20.56	0.01
Public Hospital	156.49***	161.33***	-4.84	-0.04***	136.14***	160.77***	-0.10***
	33.15	31.28	4.49	0.01	31.63	30.77	0.02
Private Non	110.32***	109.38***	0.94	-0.02	98.83***	108.08***	-0.04***
Profit	21.29	20.02	4.35	0.01	20.34	19.98	0.01
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	9618	9618	9618	9618	9582	9582	9582

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Tables shows estimates from Equation 2.2. Standard errors are clustered at the state level. Operating Revenue includes revenue from patient care. Total revenue is operating revenue plus non-operating revenue, which includes revenue from investment, private donations, parking, gift shops, cafeteria, and other auxiliary services. Operating expenses includes expenses related to patient care. Total expenses equals operating expenses plus non-operating expenses. Net Income is the difference between total revenue and total expenses. Total margin equals to $\frac{(\text{total revenue} - \text{total expenses})}{\text{total revenue}}$. Operating margin equals to $\frac{(\text{operating revenue} - \text{operating expenses})}{\text{operating revenue}}$. Expansion states include AR, CO, DE, HI, IA, IL, KY, MD, MI, ND, NJ, NM, NV, NY, OH, OR, PA, RI, VT, WA, WV, non-expansion states include AL, FL, GA, ID, KS, LA, ME, MO, MS, NC, NE, OK, SC, SD, TN, TX, UT, VA, WI, WY.

2.6 Explaining the lack of total revenue gain

The finding that Medicaid expansion led to large gains in Medicaid revenue and a large decrease in the cost of uncompensated care, but little improvement in hospitals' overall financial health suggests that the financial gain from Medicaid expansion is offset elsewhere. This section explores where such offset may have come from.

One possible explanation is the crowd-out of private insurance. While moving the uninsured to Medicaid may improve hospitals' revenue stream, Medicaid expansion may also lead some previously privately-insured individual to move to Medicaid. Since Medicaid reimbursement is usually much lower than private insurance reimbursement, such crowd-out may decrease hospitals' revenue stream. I cannot estimate the extent of crowd-out directly from Cost Report data because the data do not report privately-insured utilization and uninsured utilization separately. The Urban Institute projects the magnitude of crowd-out to be $3 for every $10 (Dorn et al., 2013). For crowd-out to explain the difference between gains in Medicaid revenue and changes in operating revenue, crowd-out need to be $4 to $5 on every $10. Estimates of the magnitude of crowd-out using post-expansion data find crowd-out effect much smaller than that predicted by the Urban Institute, with a number of studies finding no evidence of crowd-out and a few studying finding a very small effect (Courtemanche et al., 2017, McMorrow et al., 2015, Miller and Wherry, 2017). Existing evidence suggests that crowd-out unlikely provides a full explanation of the offset. While it's not possible to look at crowd-out directly in the Medicare Cost Report data, I can provide a bound for the effect. Table 2.3 show that Medicaid expansion increased Medicaid revenue by 4.57 million in 2014, and increased Medicaid discharges by 358.63 visits. At the same time, we see in Table 2.5 show that the Medicaid expansion led to an 0.09 million increase in operating revenue. In combination, these two tables suggest that there's an offset of 4.48 million outside of Medicaid. If we think all of these 4.48 million offset can be explained by crowd-out of private insurance by Medicaid, let's assume the maximum magnitude of crowd-out–100%–and see how big the payment shortfall would have to be for each visit. 100% crowd-out means that all 368.63 additional Medicaid visits added by the Medicaid expansion would have been paid by private insurance without the expansion. We do not know what share of this 4.48 million came from inpatient services, but we do have expenses for inpatient and outpatient services, and

Table 2.5: *Overall Financial Health, Diff-in-Diffs, Controlling for Pre-trend*

	(1) TotalRev(m) b/se	(2) TotalExp(m) b/se	(3) NetInc(m) b/se	(4) TotalMargin b/se	(5) OperRev(m) b/se	(6) OperExp(m) b/se	(7) OperMargin b/se
expand*2014	-5.60	-1.52	-4.07	-0.00	0.09	-0.88	0.01*
	4.60	3.29	3.03	0.01	3.93	3.18	0.01
expand*2015	0.06	8.00	-7.94	-0.01	6.57	9.05	0.01
	8.20	7.66	6.13	0.01	7.32	7.55	0.01
expand*2016	-1.10	14.65	-15.75	-0.02	4.27	15.21	0.01
	10.27	8.94	8.18	0.02	9.36	8.84	0.01
Urban hospital	125.83***	117.32***	8.51***	0.01	118.64***	116.91***	0.02*
(CMS)	8.87	8.25	1.90	0.01	8.14	8.21	0.01
teaching	274.35***	258.53***	15.82***	0.01	252.18***	257.07***	-0.01
	20.82	20.51	3.19	0.00	18.80	20.62	0.01
Public Hospital	156.42***	161.23***	-4.81	-0.04***	136.15***	160.67***	-0.10***
	33.26	31.37	4.49	0.01	31.75	30.87	0.02
Private Non	110.28***	109.34***	0.94	-0.02	98.83***	108.02***	-0.04***
Profit	21.35	20.08	4.35	0.01	20.40	20.03	0.01
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pretrend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	9618	9618	9618	9618	9582	9582	9582

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Tables shows estimates from Equation 2.3. Standard errors are clustered at the state level. Operating Revenue includes revenue from patient care. Total revenue is operating revenue plus non-operating revenue, which includes revenue from investment, private donations, parking, gift shops, cafeteria, and other auxiliary services. Operating expenses includes expenses related to patient care. Total expenses equals operating expenses plus non-operating expenses. Net Income is the difference between total revenue and total expenses. Total margin equals to $\frac{(\text{total revenue}-\text{total expenses})}{\text{total revenue}}$. Operating margin equals to $\frac{(\text{operating revenue}-\text{operating expenses})}{\text{operating revenue}}$. Expansion states include AR, CO, DE, HI, IA, IL, KY, MD, MI, ND, NJ, NM, NV, NY, OH, OR, PA, RI, VT, WA, WV, non-expansion states include AL, FL, GA, ID, KS, LA, ME, MO, MS, NC, NE, OK, SC, SD, TN, TX, UT, VA, WI, WY.

the ratio of expenses for inpatient services and expense for outpatient services is roughly 3 : 1, this means about 3/4 of the additional 4.48 million additional Medicaid revenue, or 3.36 million, came from payment shortfall on these 368.63 additional Medicaid visits. This would suggest a *shortfall* of $9114.83 per discharge. This is a very large number, considering that the Healthcare Cost and Utilization Project estimate the average cost of a hospital discharge in 2013 to be about 11000.[4]. If we assume a lower crowd-out rate, the implied payment shortfall (difference between private payment and Medicaid payment) per discharge would have to be even greater. Therefore, it is unlikely that crowd-out can fully explain the magnitude of offset we see.

To further explore where revenue offset may come from, I look at different components of revenue in the Medicaid Cost Report. One hypothesis is that the revenue offset comes from state and local government rolling back programs that support uncompensated care after the Medicaid expansion. In panel (2) of Table 2.6 shows how state and local revenue for indigent care changed in hospitals in Medicaid expansion states relative to those in non-expansions states after 2014, controlling for state-specific trends. The differential change is almost zero and far from being able to offset the gain in Medicaid revenue. Another channel of payment to hospitals from governments is through government appropriations, which is part of non-operating revenue. An example of government appropriation is a bond issued to fund a hospital expansion project. Column (7) of Table 2.6 shows changes in non-operating government appropriation. Although there appears to be a decrease in non-operating governmental appropriation to hospitals in expansion states relative to those in non-expansion states in 2015 and 2016, the magnitude of such decrease is very small and not enough to offset the gains in Medicaid revenue. Note another element that is widely discussed in the ACA Medicaid expansion is the provision to roll back DSH payments. However, the onset of DSH payment cuts was delayed until 2018 and did not affect my sample period. Furthermore, the measure of Medicaid revenue in this paper is net of DSH payments.

A natural next step is to see if the offset comes from decreases in Medicare revenue or patient revenue from private insurance. Neither of these revenue figures can be clearly extracted from the Cost Report Data. Instead, Column (3) shows the effect of Medicaid expansion on Medicare FFS payments for inpatient services only. There is a -2.7 million decrease in Medicare FFS payments

[4]https://www.hcup-us.ahrq.gov/reports/statbriefs/sb176-Hospital-Cost-Projections-by-Age-2013.pdf

for hospitals in expansion states relative to those in non-expansion states after expansion, but the decrease is not statistically significant. Column (4) shows the effect of Medicaid expansion on "residual revenue", which equals to total revenue minus Medicaid revenue, Medicare FFS revenue, and payment from State and County Indigent programs. This residual revenue is a combination of private revenue, Medicare payment for outpatient services, and other Medicare payments not tied to inpatient visits. Column (4) shows that decreases in this "residual revenue" explain almost all of the offset of Medicaid revenue. While this "residual revenue" comes from many parts, it can be driven by three main factors: prices of privately-insured patients, volume of Medicare and privately-insured patients, and case mix of Medicare and privately-insured patients. Change in any of these component likely signals a change in hospital strategy.

Another strategy that have gained attention in the policy debate is cost-shifting. The idea is that is that hospitals may have been charging private payers additional amounts in order to subsidize the provision of uncompensated care. As more uninsured are covered by Medicaid, this cross-subsidization is no longer necessary, and hospitals may decrease the price for private payers in order to gain additional market share (Frakt, 2014). Follow the convention in the literature, I construct a proxy measure for private inpatient prices using a formula from Lewis and Pflum (2017).

$$\text{Discharge Price } = \frac{[\text{Gross Inpatient Revenue} \times (1 - \text{discount})] - \text{Medicare Payments}}{\text{Non-Medicare Discharges}} \tag{2.4}$$

However, as shown in Appendix B.3.1, this measure is affected not only by private inpatient prices, but also the distribution of discharges that are privately-insured, uninsured, or on Medicaid. While it is not possible to test whether any cost-shifting exists, testing whether this proxy measure is smaller than or equal to zero provides a one-sided test on whether the extent of cost-shifting completely offset the gain in Medicaid revenue from the ACA expansion, assuming zero crowd-out of private insurance. Column 8 shows the estimates on this proxy measure after controlling for state-specific trends. The coefficients are all positive, and the standard error is roughly equal to the point estimate for 2014, 2015, and 2016. In a one-sided test, we can reject the null hypothesis that this proxy measure is smaller than or equal to 0, in other words, the null hypothesis that price-shifting completely offsets gains from Medicaid revenue, with round 85% certainly, slightly lower than the 95% certainly used in conventional statistical inference.

All of the previous components focus on revenue directly related to patient care. Column (5) looks at changes in non-operating revenue, which includes investment income, parking fees, income from cafeteria and gift shops, private donations, and government appropriation. There was a four to five million decrease in total non-operating revenue among hospitals in Medicaid expansion states relative to hospitals in non-expansion states after the expansion, and the difference was statistically significant in 2014. Column (6) and (7) show that such decrease was not driven by roll-back of private donations or government appropriation, suggesting that most of the effect would come from investment income and income from ancillary businesses including cafeteria, gift shops, and parking.

2.6.1 Theoretical model of revenue offset

The previous section has shown that the gain in Medicaid revenue in Medicaid expansion states is mostly offset through slower revenue growth from privately insured patients, Medicare revenue from non-inpatient services, and non-operating revenue from hospitals' auxiliary services. This is consistent with a model where hospitals are risk-averse firms that may incur nonmonetary "strategic search cost" when searching for ways to improve its financial status.

Specifically, let hospital's utility function be:

$$U = U(P(S), C, S) \tag{2.5}$$

where P is net profit, C is a measure of "community benefit", which measures a hospital's desire to provide services that may not be profitable but serves the community. The last term S measures the non-monetary (the monetary cost is incorporated into P) "strategic cost" that hospitals may invest in to boost its revenue, such as convening board meetings, seeking new management, streamlining operations, and revamping billing practices. Assuming that $U'(P) > 0$, $U''(P) < 0$, $U'(S) > 0$, $U''(S) < 0$, and $U'(S) < 0$, $U''(S) > 0$. In equilibrium, we have:

Table 2.6: *Revenue Details, Diff-in-Diffs, Controlling for Pre-trend*

	(1) McaidRev(m) b/se	(2) StateCounty(m) b/se	(3) MDC Rev(m) b/se	(4) PrivRev(m) b/se	(5) NonOpRev(m) b/se	(6) Donation(m) b/se	(7) Non-OpGovAppr(m) b/se	(8) EstPrivPrice b/se
expand*2014	4.57***	-0.02	0.59	-10.81**	-5.58**	0.09	0.18	995.47
	1.00	0.10	0.92	3.94	1.85	0.08	0.14	1327.13
expand*2015	8.17***	0.08	1.62	-10.30	-6.37*	0.10	-0.13	1555.49
	1.51	0.16	1.42	6.78	3.01	0.12	0.18	1836.45
expand*2016	10.29***	0.17	2.77	-14.76	-5.49	0.13	-0.01	2077.74
	2.10	0.22	1.86	8.26	3.36	0.15	0.28	2430.99
Urban hospital	15.80***	0.10*	23.55***	86.18***	7.10***	0.10*	0.39*	3008.66***
(CMS)	2.46	0.05	1.77	6.39	1.14	0.04	0.17	273.48
teaching	40.31***	0.30***	54.26***	179.66***	23.06***	0.33***	1.39***	2669.73***
	5.25	0.06	4.34	12.84	2.77	0.06	0.36	306.88
Public Hospital	33.65***	0.48*	24.10***	97.85***	23.84***	0.51***	3.33**	-395.84
	7.68	0.20	6.13	24.19	3.32	0.10	0.94	551.15
Private Non	5.77	0.21**	22.25***	82.21***	11.70***	0.33***	0.26	-606.96*
Profit	3.22	0.06	3.91	15.05	1.41	0.06	0.22	267.73
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pretrend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	9618	9540	9618	9576	9612	9138	9516	9498

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Tables shows estimates from Equation 2.3. Standard errors are clustered at the state level. Column (1) shows the result for Medicaid revenue (including DSH). Column (2) shows revenue from state and county indigent programs. Column (3) shows Medicare FFS revenue. Column (4) shows a measure of "private residual revenue", which equals to total revenue minus Medicaid revenue (including DSH), Medicare FFS revenue, and payment from State and County Indigent programs. This residual revenue is a combination of private revenue (including Medicare Advantage but does not include Medicaid managed care, Medicare payment for outpatient services,and other Medicare payments not tied to inpatient visits. Column (5) shows non-operating revenue. Column (6) shows revenue from private donations. Column (7) shows governmental appropriations. Column (6) and (7) are both components of (5). Column (8) show a measure of estimated private price, as defined in Appendix B.3.1.

$$\frac{\partial U*}{\partial P*} = \frac{\partial U*}{\partial C*} = -\frac{\partial U*}{\partial S*} \tag{2.6}$$

When the Medicaid expansion brings in extra revenue and increases P, thus decreases $\frac{\partial U}{\partial P}$. In order to reach new equilibrium, the hospital may choose to decrease S, which brings up $\frac{\partial U*}{\partial S*}$ and brings down $\frac{\partial U}{\partial P}$ to achieve a new equilibrium. In other words, Medicaid revenue gain decreases the marginal return of additional effort invested to generate revenue elsewhere and reduces the total amount of such effort, leading to an offset of total revenue.

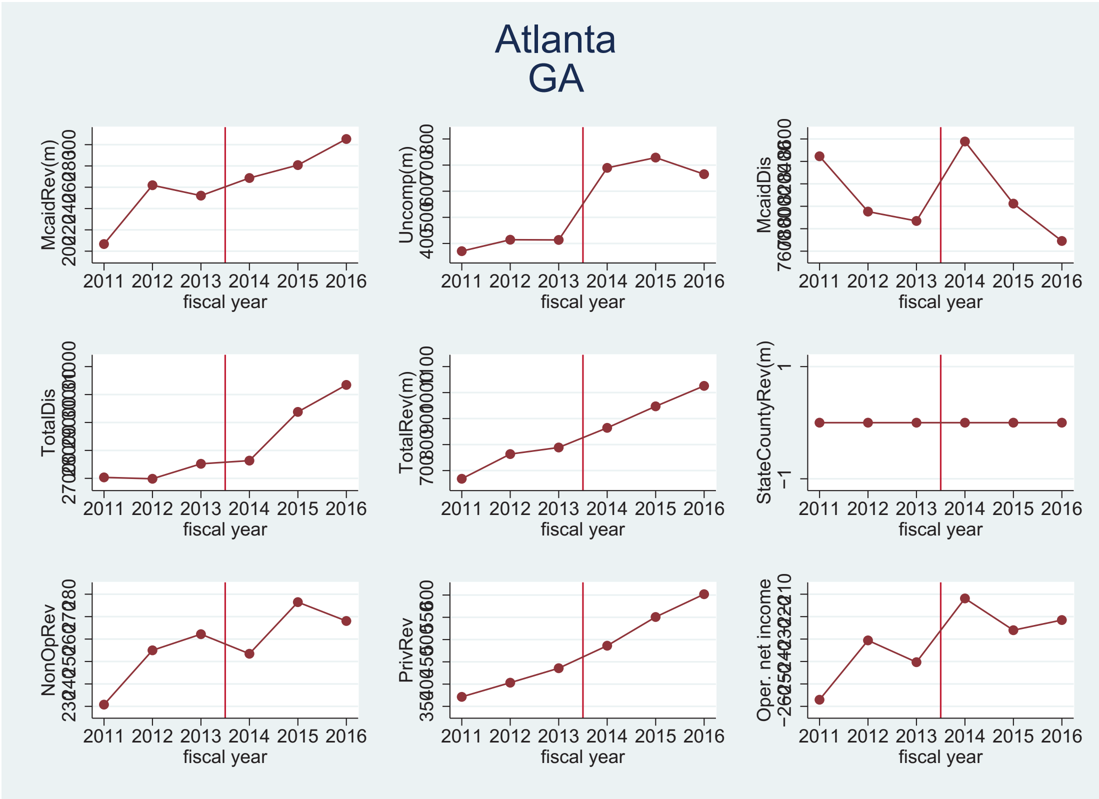
Note that another result from this model is that increases in P through a revenue transfer may lead hospitals to increase C. In other words, if we think of hospitals are non-profit entities that care about both profits and missions and experience diminishing marginal return on both dimensions, additional revenue transfer may enable the hospital to spend more on mission-driven activities, such as expanding service lines with high needs but low profitability (behavior health) or increase the generosity of its charity care policy. Future work can look at these outcomes more directly.

2.6.2 Case Study: hospital strategy in non-expansion states

The finding that hospitals in Medicaid expansion states experienced sharp increases in Medicaid revenue relative to hospitals in non-expansion states after expansion but not did see clear gains in total revenue or overall financial status is consistent with anecdotal reports that large safety net hospitals in non-expansion states are not doing visibly worse financially relative to safety net hospitals in expansion states. Kaiser Health News looked at the financial report of a dozen safety net hospitals in Florida Texas, Georgia, Tennessee, South Carolina, Virginia, and Kansas, and found them to perform better financially in 2014 than in 2013 (Galewitz, 2015). The reporters attributed these hospitals financial health to the rising economy and the growth of individual health insurance exchanges. However, safety-net hospitals in non-expansion states are also doing a lot more to stay financially healthy.

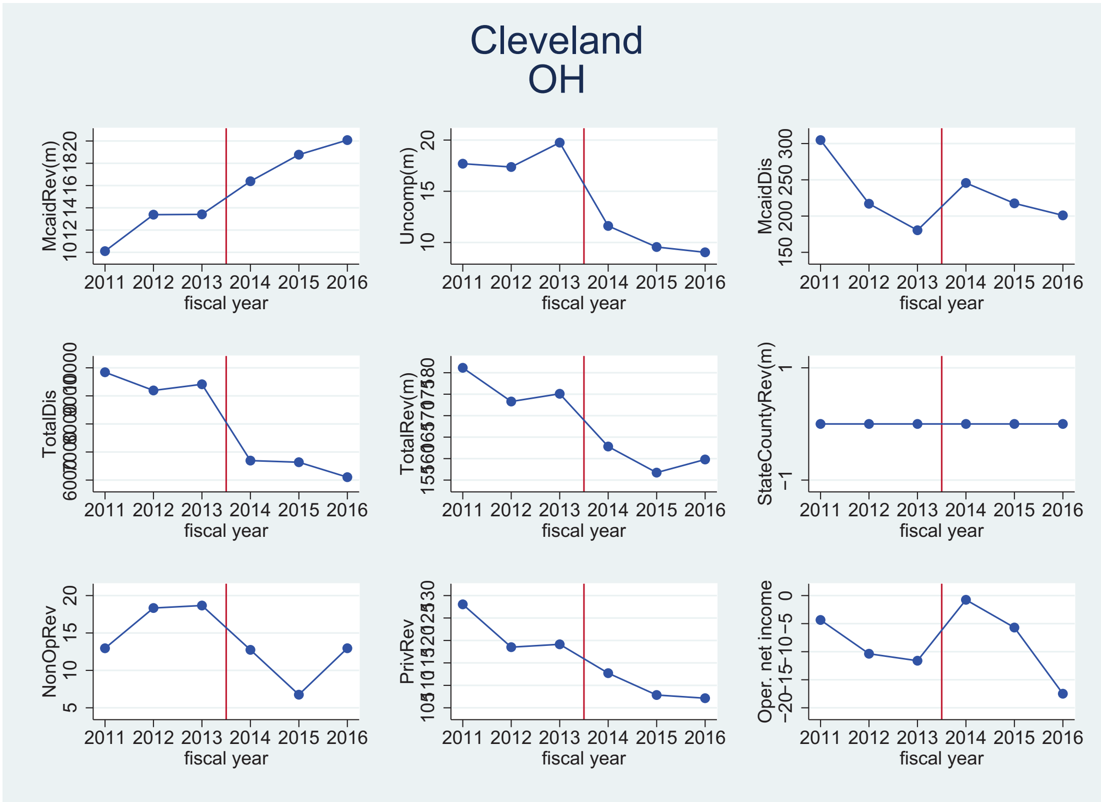
Grady Memorial Hospital Grady Memorial Hospital's story of reemerging from the deep financial trouble is widely publicized. The safety net hospital in Atlanta was on the verge of bankruptcy in the late 2000s. Since then it has been a success story of how a dying hospital may turn itself around. Grady Memorial Hospital changed management in 2011 and started turning a profit in 2012, and its profit margin has been steadily growing since. Although private donations are injected into Grady Memorial Hospital at the beginning of the turnaround, its latest CEO cites revamping the revenue cycle, which includes medical-coding, billing and collections, and investment in a new electronic medical system to assist these practices the major reason that the hospital continued to grow financially (Miller, 2017). Grady brought on a new CEO in 2011 and a new CFO in 2012 (Miller, 2017). The effect of these strategic decisions are visible in their visible steady increase in all channels of revenue. Taking on these strategies is operationally and politically complex and risky. The new dramatic strategy change appears to be necessitated by Grady's near-closure financial status. On the other side, many hospitals in expansion state who face moderate financial challenges did not institute similar drastic strategic changes. Some even suffer from poor management. Figure 2.5 shows the financial data for Marymount Hospital of Cleveland Clinic. Despite gains in Medicaid revenue and Medicaid discharges, the hospital experienced a continued decline in total revenue and total discharges. In fact, in 2015, a report revealed that the lab at Marymount Hospital was engaging in sloppy practices and was violating many government regulations between 2013 and 2015. Poor oversight has been cited as a reason for such violations.(Ross, 2015)

Figure 2.4: *Grady Memorial Hospital, Atlanta, GA*



Note: Financial data for Grady Memorial Hospital in Atlanta, GA. Georgia did not expand Medicaid.

Figure 2.5: *Marymount Hospital of Cleveland Clinic, Cleveland, OH*



Note: Financial data for Marymount Hospital of Cleveland Clinic, Cleveland, OH. Ohio is a Medicaid expansion state.

2.7 Hospital strategy and patient outcomes

Table 2.7 shows the effect of the Medicaid expansion on various measures of hospital strategy on how it spends its money. I focus on labor expenditure and capital investment. Hospitals in Medicaid expansion states spend a fairly similar amount on salary expenses and capital investment relative to hospitals in non-expansion states after the expansion. This is not surprising, given that previous sections have shown that hospitals in expansion states do not actually have higher total revenue after the expansion relative to those in non-expansion states.

Table 2.8 shows the effect of the Medicaid expansion on a set of measures of patient care quality. These measures are constructed using the Medicare population and are not directly affected by characteristics of Medicaid patients or changes in patient payer mix. According to this table, hospitals in expansion states experienced a small increase in the 30-day mortality rates for heart failure patients and pneumonia patients. The magnitude of the increase is about 1% of the mean mortality rates for these conditions across all hospitals across all sample years. There's a small decrease in 30-day readmissions rates for Pneumonia patients, but this reduction could be explained by the increase in mortality rates because patients who pass away cannot be readmitted. Overall, Table 2.8 suggests that there's a very small decrease in patient care quality as measured in these six domains in hospitals in expansion states relative to hospitals in non-expansion states after expansion. One possible explanation is hospitals in expansion states experienced a disruption in their operations without receiving many additional resources to deal with such interruptions. Another likely explanation is that hospitals in non-expansion states, in their quest to boost revenue through other channels, may resort to up-coding patients' complexity, making their risk-adjusted mortality rates appear lower.

Table 2.7: *Hospital Strategy, Diff-in-diffs, Controlling for Pre-trend*

	(1) SalaryExp(m) b/se	(2) NetBuiltInv(m) b/se	(3) NetEquipInv(m) b/se	(4) NetLandInv(m) b/se	(5) NetCapInv(m) b/se
expand*2014	0.29	0.13	1.07	0.14	1.22
	1.27	1.72	1.42	0.19	2.88
expand*2015	2.19	0.93	1.39	0.15	1.74
	2.36	2.54	1.95	0.25	3.80
expand*2016	4.60	-0.39	2.28	0.38	1.56
	3.23	3.75	2.59	0.39	5.59
Urban hospital	41.88***	2.83***	1.78***	1.35***	6.03***
(CMS)	3.40	0.55	0.39	0.21	0.82
teaching	94.71***	5.29***	4.46***	2.53***	12.89***
	10.43	0.79	0.57	0.38	1.29
Public Hospital	64.62***	6.67***	3.32***	2.39***	12.26***
	12.73	1.39	0.80	0.58	2.18
Private Non	44.28***	3.74**	2.22**	1.96***	8.21***
Profit	8.28	1.09	0.81	0.52	2.11
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Pretrend	Yes	Yes	Yes	Yes	Yes
N	9618	9498	9492	9438	9552

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Tables shows estimates from Equation 2.3. Standard errors are clustered at the state level. Column (1) shows estimate on Salary expenses (part of operating expenses). Column (2) shows net investment in buildings and building improvement. Column (3) shows net investment in fixed and movable equipment. Column (4) shows net investment in land and land improvement. Column (5) is the sum of Columns (2) to (4).

2.8 Heterogeneity across hospitals

2.8.1 By ownership

Table 2.9 shows the effect of the Medicaid expansion on Medicaid revenue and cost of uncompensated care for public hospitals, private for-profit hospitals, and private not-for-profit hospitals. By far, public hospitals saw the greatest gain in Medicaid revenue as a result of Medicaid expansion, followed by private not-for-profit hospitals. The gain in Medicaid revenue for public hospitals is as much as $41 million in 2016. Parallel to these differences, Table 2.10 shows that public hospitals in expansion states experienced quite large gains in total revenue, operating revenue, total margin, and operating margin, relative to hospitals in non-expansion states. Table 2.11 shows suggestive evidence the additional revenue gain from Medicaid expansion enabled public hospitals in expansion states to spend relatively more in salary and make a greater investment in building and equipment than public hospitals in non-expansion states, although the estimates are imprecise. Figure 2.6 confirms that this difference in spending and investment is driven by changes in public hospitals in expansion states after 2014. Private for-profit hospitals in Medicaid expansion states appeared to be shrinking after expansion relative to private for-profit hospitals in non-expansion states. Those in expansion states had lower total revenue, lower total expenses, lower salary expenses, and lower investment in buildings and equipment. A possible explanation is that the Medicaid expansion reduced the comparative advantage of private for-profit hospitals, which derived the least direct gain from the expansion. Another thing to notice is that public hospitals in expansion states did not experience the type of offset in their Medicaid revenue gain after the expansion as private hospitals in these states did. One possible explanation is that public hospitals have fewer strategic levers to move to improve their financial situation. In other words, the "non-monetary strategic cost" S plays a relatively weak role in contributing to a public hospital's profit. For example, public hospitals may not change management or form a joint venture as easily as private hospitals.

Table 2.13 shows the effect of the Medicaid expansion on patient outcomes of Medicare patients for public, private for-profit, and private non-profit hospitals. These estimates are fairly noisy, but we can still see that the positive effect of the expansion on 30-day mortality rates is mostly

Table 2.8: *Patient Outcomes, Diff-in-diffs, Controlling for Pre-trend*

	(1) 30dReadm(HA) b/se	(2) 30dReadm(HF) b/se	(3) 30dReadm(PN) b/se	(4) 30dMortal(HA) b/se	(5) 30dMortal(HF) b/se	(6) 30dMortal(PN) b/se
expand*2014	0.04	-0.06	-0.14*	0.07	0.12*	0.11
	0.05	0.06	0.07	0.05	0.05	0.07
expand*2015	0.06	-0.08	-0.03	-0.04	0.18*	0.11
	0.12	0.10	0.18	0.10	0.09	0.20
expand*2016	0.07	-0.21	-0.09	-0.11	0.20	0.14
	0.18	0.14	0.21	0.12	0.12	0.23
Urban hospital	0.06	-0.13	0.04	-0.23**	-0.36**	-0.45**
(CMS)	0.09	0.13	0.11	0.08	0.11	0.13
teaching	0.13*	-0.00	0.20*	-0.08	-0.16*	-0.22*
	0.06	0.10	0.08	0.06	0.07	0.10
Public Hospital	-0.15	-0.35**	-0.15	-0.10	0.04	-0.03
	0.11	0.11	0.10	0.10	0.13	0.15
Private Non	-0.22**	-0.52***	-0.35***	-0.21***	0.13	-0.24*
Profit	0.07	0.11	0.09	0.05	0.08	0.11
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Pretrend	Yes	Yes	Yes	Yes	Yes	Yes
N	7536	9342	9354	8184	9342	9360

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Tables shows estimates from Equation 2.3. Unit is percentage points. Columns (1) to (3) show 30-day readmission rates for Medicare patients with Heart Attack, Heart Failure, and Pneumonia. Columns (4) to (6) show 30-day mortality rates for Medicare patients with Heart Attack, Heart Failure, and Pneumonia. Data is missing for hospitals with insufficient cases.

driven by private non-profit hospitals. This suggests that either private non-profit hospitals are more likely to engage in up-coding in the absence of a Medicaid expansion, or that the additional labor expenditure and capital investment by public hospitals translated into an improvement in care quality.

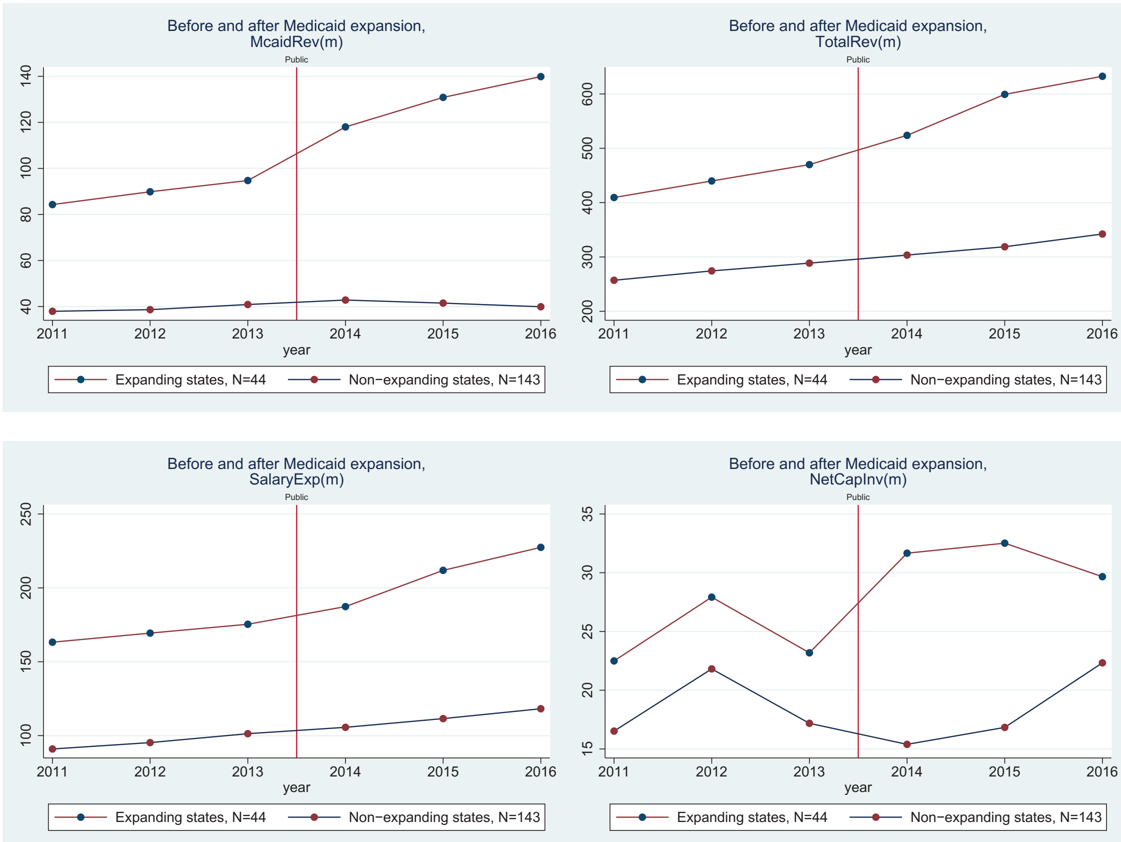
Table 2.9: *Medicaid Revenue and Cost of Uncompensated care, Diff-in-Diffs, Controlling for Pre-trend*

	Public Hospitals			
	(1)	(2)	(3)	(4)
	McaidRev(m)	Uncomp(m)	McaidDis	TotalDis
expand*2014	21.88**	-4.36	479.15*	1878.84
	6.33	5.98	213.41	1155.21
expand*2015	32.26***	1.56	879.59*	3491.12
	8.61	7.26	373.11	1818.63
expand*2016	45.03***	9.57	1113.57	5011.98*
	9.45	10.36	563.88	2355.99
Observations	1157	1157	1157	1157
	Private For-Profit Hospitals			
	(1)	(2)	(3)	(4)
	McaidRev(m)	Uncomp(m)	McaidDis	TotalDis
expand*2014	2.37	-39.61***	477.94***	-367.23
	1.78	7.50	108.51	299.97
expand*2015	3.63	-48.60***	489.37*	16.16
	2.43	10.01	189.69	411.67
expand*2016	7.68*	-40.11***	565.60*	-189.70
	3.44	9.87	269.26	535.07
Observations	2237	2237	2237	2237
	Private Non-Profit Hospitals			
	(1)	(2)	(3)	(4)
	McaidRev(m)	Uncomp(m)	McaidDis	TotalDis
expand*2014	4.13**	-10.54*	267.51**	144.81
	1.32	4.50	95.87	217.26
expand*2015	7.24***	-12.52	404.06	385.95
	1.72	6.21	234.77	381.63
expand*2016	7.88**	-13.24	401.85	337.90
	2.37	6.84	274.61	527.17
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	6224	6224	6224	6224

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Tables shows estimates from Equation 2.3. Standard errors are clustered at the state level. Column 1 shows the effect of Medicaid expansion on Medicaid revenue (including DSH payment) in millions. Column 2 shows the effect on the cost of uncompensated care. Column 3 shows the effect of Medicaid discharges, Column 4 shows the effect on total discharges. Expansion states include AR, CO, DE, HI, IA, IL, KY, MD, MI, ND, NJ, NM, NV, NY, OH, OR, PA, RI, VT, WA, WV, non-expansion states include AL, FL, GA, ID, KS, LA, ME, MO, MS, NC, NE, OK, SC, SD, TN, TX, UT, VA, WI, WY. Cost of uncompensated care is calculated from Form S10 of the Medicare Cost Report and equals to the sum of charity care charges (minus payment for charity care) and bad debt.

Figure 2.6: *Revenue, Salary expenditure, and Capital Investment of Public Hospitals*



Note: Mean Medicaid revenue, total revenue, salary expenditure, and net capital investment for hospitals in expansion and non-expansion states. Expansion states include AR, CO, DE, HI, IA, IL, KY, MD, MI, ND, NJ, NM, NV, NY, OH, OR, PA, RI, VT, WA, WV, non-expansion states include AL, FL, GA, ID, KS, LA, ME, MO, MS, NC, NE, OK, SC, SD, TN, TX, UT, VA, WI, WY.

2.8.2 By share of uninsured discharges

Tables 2.14 to 2.12 show heterogeneity on the effect of Medicaid expansion across hospital with high vs. low share of uninsured discharges in 2009. High share of uninsured discharges is defined as having a share of uninsured discharges at the top 30th percentile, which roughly equals to 7%. This analysis limits to AR, CO, IA, KY, MD, NJ, NY, OR, VT, FL, UT, WI. because of the availability of HCUP data. Not surprisingly, hospitals with high share of uninsured discharges saw greater gain in Medicaid revenue from the expansion, however, these hospitals did not see higher gain in total revenue. Both groups of hospitals experience off-set of the revenue gain from

Table 2.10: *Overall Financial Health, Diff-in-Diffs, Controlling for Pre-trend*

	Public Hospitals						
	(1) TotalRev(m)	(2) TotalExp(m)	(3) NetInc(m)	(4) TotalMargin	(5) OperRev(m)	(6) OperExp(m)	(7) OperMargin
expand*2014	27.82	18.93	8.88	0.04***	35.52*	19.67	0.07**
	19.93	17.74	8.08	0.01	16.66	17.71	0.02
expand*2015	92.08	83.16	8.93	0.04*	89.16	81.80	0.04
	60.59	57.60	13.80	0.02	59.74	57.99	0.03
expand*2016	116.86	135.56	-18.70	-0.03	117.48	130.98	0.01
	85.17	71.51	17.22	0.04	77.04	71.76	0.05
Observations	1157	1157	1157	1157	1140	1140	1140
	Private For-Profit Hospitals						
	(1) TotalRev(m)	(2) TotalExp(m)	(3) NetInc(m)	(4) TotalMargin	(5) OperRev(m)	(6) OperExp(m)	(7) OperMargin
expand*2014	-28.21**	-21.69**	-6.52	0.01	-23.43**	-22.17**	0.02
	9.61	7.31	5.58	0.02	8.48	7.22	0.01
expand*2015	-31.09	-23.72*	-7.37	0.01	-24.36	-24.47*	0.02
	15.34	10.15	8.53	0.02	13.90	9.98	0.02
expand*2016	-47.73*	-31.90*	-15.83	0.01	-39.78*	-32.99*	0.03
	22.17	15.48	11.70	0.03	19.00	15.41	0.03
Observations	2237	2237	2237	2237	2237	2237	2237
	Private Non-Profit Hospitals						
	(1) TotalRev(m)	(2) TotalExp(m)	(3) NetInc(m)	(4) TotalMargin	(5) OperRev(m)	(6) OperExp(m)	(7) OperMargin
expand*2014	-4.80	-0.64	-4.15	-0.00	0.08	0.82	0.01
	5.72	4.37	4.40	0.01	4.86	4.04	0.01
expand*2015	-0.07	7.90	-7.96	-0.00	5.79	11.08	0.01
	9.11	10.97	10.76	0.02	7.43	10.55	0.02
expand*2016	-5.55	8.79	-14.33	-0.01	-1.14	12.19	0.01
	13.25	13.47	15.20	0.02	11.49	13.05	0.02
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	6224	6224	6224	6224	6205	6205	6205

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Tables shows estimates from Equation 2.2. Standard errors are clustered at the state level. Operating Revenue includes revenue from patient care. Total revenue is operating revenue plus non-operating revenue, which includes revenue from investment, private donations, parking, gift shops, cafeteria, and other auxiliary services. Operating expenses includes expenses related to patient care. Total expenses equals operating expenses plus non-operating expenses. Net Income is the difference between total revenue and total expenses. Total margin equals to $\frac{(\text{total revenue} - \text{total expenses})}{\text{total revenue}}$. Operating margin equals to $\frac{(\text{operating revenue} - \text{operating expenses})}{\text{operating revenue}}$. Expansion states include AR, CO, DE, HI, IA, IL, KY, MD, MI, ND, NJ, NM, NV, NY, OH, OR, PA, RI, VT, WA, WV, non-expansion states include AL, FL, GA, ID, KS, LA, ME, MO, MS, NC, NE, OK, SC, SD, TN, TX, UT, VA, WI, WY.

Table 2.11: *Revenue Details, Diff-in-Diffs, Controlling for Pre-trend*

				Public Hospitals				
	(1) McaidRev(m)	(2) StateCounty(m)	(3) MDC Rev(m)	(4) PrivRev(m)	(5) NonOpRev(m)	(6) Donation(m)	(7) Non-OpGovAppr(m)	(8) EstPrivPrice
expand*2014	21.88**	-0.47	2.98	4.11	-9.46	0.12	0.98	875.99
	6.33	0.73	4.44	14.59	6.79	0.40	1.16	458.94
expand*2015	32.26***	-0.30	10.05	50.20	8.07	-0.60*	-0.55	907.54
	8.61	0.97	10.96	43.19	6.81	0.29	1.55	1325.31
expand*2016	45.03***	-0.11	17.89	53.95	2.44	-0.15	-1.58	228.52
	9.45	1.28	12.92	66.82	12.77	0.91	1.40	1803.79
Observations	1157	1133	1157	1145	1157	1086	1151	1140
				Private For-Profit Hospitals				
	(1) McaidRev	(2) StateCounty)	(3) MDC Rev	(4) PrivRev	(5) NonOpRev	(6) Donation	(7) Non-OpGovAppr	(8) SimPrivPrice
expand*2014	2.37	-0.04	-1.18	-29.35**	-4.78	0.04	-0.05	-1196.84
	1.78	0.08	1.50	8.98	3.99	0.10	0.17	636.83
expand*2015	3.63	0.09	0.10	-34.91*	-6.73	0.10	-0.08	-1890.35*
	2.43	0.14	2.40	13.90	5.83	0.12	0.26	925.01
expand*2016	7.68*	0.16	-1.11	-54.45**	-7.95	0.16	-0.17	-2318.75
	3.44	0.17	3.42	19.42	7.46	0.21	0.40	1280.91
Observations	2237	2237	2237	2237	2237	2213	2225	2209
				Private Non-Profit Hospitals				
	(1) McaidRev	(2) StateCounty)	(3) MDC Rev	(4) PrivRev	(5) NonOpRev	(6) Donation	(7) Non-OpGovAppr	(8) SimPrivPrice
expand*2014	4.13**	-0.04	0.15	-8.98	-4.26	0.17	0.26	2691.42
	1.32	0.13	1.18	5.00	2.21	0.12	0.14	2613.94
expand*2015	7.24***	0.01	0.70	-8.42	-5.21	0.30	0.06	3879.49
	1.72	0.24	1.99	7.77	3.43	0.19	0.27	3590.25
expand*2016	7.88**	0.11	1.59	-15.30	-3.85	0.30	0.32	5215.66
	2.37	0.31	2.80	11.11	4.19	0.23	0.39	4606.67
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	6224	6170	6224	6194	6218	5839	6140	6149

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Tables shows estimates from Equation 2.3. Standard errors are clustered at the state level. Column (1) shows the result for Medicaid revenue (including DSH). Column (2) shows revenue from state and county indigent programs. Column (3) shows Medicare FFS revenue. Column (4) shows a measure of "private residual revenue", which equals to total revenue minus Medicaid revenue (including DSH), Medicare FFS revenue, and payment from State and County Indigent programs. This residual revenue is a combination of private revenue (including Medicare Advantage but does not include Medicaid managed care, Medicare payment for outpatient services,and other Medicare payments not tied to inpatient visits. Column (5) shows non-operating revenue. Column (6) shows revenue from private donations. Column (7) shows governmental appropriations. Column (6) and (7) are both components of (5). Column (8) show a measure of estimated private price, as defined in Appendix B.3.1.

Table 2.12: *Hospital Strategy, Diff-in-Diffs, Controlling for Pre-trend*

	Public Hospitals				
	(1) SalaryExp(m)	(2) NetBuiltInv(m)	(3) NetEquipInv(m)	(4) NetLandInv(m)	(5) NetCapInv(m)
expand*2014	8.91	6.93	7.66	0.79	9.86
	6.26	3.84	4.92	1.32	8.92
expand*2015	35.35	10.90	1.49	1.87	8.27
	21.35	7.16	5.97	1.96	11.66
expand*2016	53.36	8.41	-1.58	2.59	1.78
	26.74	9.26	6.98	2.45	14.17
Observations	1157	1091	1085	1085	1091
	Private For-Profit Hospitals				
	(1) SalaryExp(m)	(2) NetBuiltInv(m)	(3) NetEquipInv(m)	(4) NetLandInv(m)	(5) NetCapInv(m)
expand*2014	-5.33*	-2.91	-1.99	-0.20	-5.11
	2.13	6.64	3.45	0.18	8.67
expand*2015	-4.47	-6.89	-2.36	-0.15	-9.47
	3.71	12.59	4.64	0.40	15.50
expand*2016	-6.61	-1.82	-1.67	-0.05	-3.59
	5.44	12.54	5.72	0.48	15.68
Observations	2237	2237	2231	2163	2237
	Private Non-Profit Hospitals				
	(1) SalaryExp(m)	(2) NetBuiltInv(m)	(3) NetEquipInv(m)	(4) NetLandInv(m)	(5) NetCapInv(m)
expand*2014	0.41	-2.16	-0.64	0.32	-2.08
	1.79	2.64	1.96	0.27	4.03
expand*2015	0.87	-1.72	-0.77	0.25	-2.39
	3.12	3.56	2.90	0.33	5.67
expand*2016	2.18	-3.61	1.04	0.54	-1.92
	4.30	5.34	3.51	0.62	8.13
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	6224	6170	6176	6190	6224

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Tables shows estimates from Equation 2.3. Standard errors are clustered at the state level. Column (1) shows estimate on Salary expenses (part of operating expenses). Column (2) shows net investment in buildings and building improvement. Column (3) shows net investment in fixed and movable equipment. Column (4) shows net investment in land and land improvement. Column (5) is the sum of Columns (2) to (4).

Table 2.13: *Patient Outcomes, Diff-in-Diffs, Controlling for Pre-trend*

	Public For-Profit Hospitals					
	(1) 30dReadm(HA)	(2) 30dReadm(HF)	(3) 30dReadm(PN)	(4) 30dMortal(HA)	(5) 30dMortal(HF)	(6) 30dMortal(PN)
expand*2014	0.30	0.10	-0.08	0.12	-0.01	-0.25
	0.20	0.25	0.21	0.16	0.23	0.27
expand*2015	0.49	-0.15	-0.37	0.12	-0.35	-0.77
	0.41	0.39	0.38	0.33	0.39	0.64
expand*2016	0.96	-0.15	-0.20	0.35	-0.54	-0.93
	0.60	0.49	0.59	0.53	0.50	0.91
Observations	807	1130	1142	942	1130	1142
	Private For-Profit Hospitals					
	(1) 30dReadm(HA)	(2) 30dReadm(HF)	(3) 30dReadm(PN)	(4) 30dMortal(HA)	(5) 30dMortal(HF)	(6) 30dMortal(PN)
expand*2014	-0.08	-0.29*	-0.04	0.03	0.16	0.18
	0.13	0.14	0.13	0.17	0.19	0.18
expand*2015	-0.11	-0.24	0.14	-0.06	0.26	0.17
	0.25	0.28	0.24	0.29	0.35	0.39
expand*2016	-0.28	-0.24	0.20	-0.19	0.36	0.14
	0.40	0.36	0.37	0.43	0.50	0.59
Observations	1680	2184	2172	1828	2184	2172
	Private Non-Profit Hospitals					
	(1) 30dReadm(HA)	(2) 30dReadm(HF)	(3) 30dReadm(PN)	(4) 30dMortal(HA)	(5) 30dMortal(HF)	(6) 30dMortal(PN)
expand*2014	0.01	-0.01	-0.12	0.15*	0.16	0.14
	0.07	0.07	0.08	0.06	0.08	0.09
expand*2015	-0.06	0.03	0.13	0.12	0.29*	0.21
	0.13	0.12	0.20	0.13	0.13	0.21
expand*2016	-0.13	-0.09	0.05	0.08	0.32	0.27
	0.18	0.18	0.22	0.20	0.17	0.26
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	5049	6028	6040	5414	6028	6046

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Tables shows estimates from Equation 2.3. Unit is percentage points. Columns (1) to (3) show 30-day readmission rates for Medicare patients with Heart Attack, Heart Failure, and Pneumonia. Columns (4) to (6) show 30-day mortality rates for Medicare patients with Heart Attack, Heart Failure, and Pneumonia. Data is missing for hospitals with insufficient cases.

Medicaid. For hospitals with high share of uninsured patients in 2009, we can actually reject with 95% confidence that the offset is *not* explained by a lower private insurance prices/or less severe patient mix.

Table 2.14: *Medicaid Revenue and Cost of Uncompensated care, Diff-in-Diffs, Controlling for Pre-trend*

High share of uninsured discharges in 2009				
	(1)	(2)	(3)	(4)
	McaidRev(m)	Uncomp(m)	McaidDis	TotalDis
expand*2014	9.52*	-49.93*	1207.16***	-57.69
	3.21	16.04	57.00	133.89
expand*2015	9.66*	-67.29*	2031.50***	262.95
	3.10	23.67	123.67	277.49
expand*2016	12.88*	-53.81	2480.83***	759.24
	4.57	25.98	150.97	467.52
Observations	972	972	972	972
Low share of uninsured discharges in 2009				
	(1)	(2)	(3)	(4)
	McaidRev(m)	Uncomp(m)	McaidDis	TotalDis
expand*2014	2.28	-13.78	639.68***	175.40
	1.42	8.43	91.53	128.85
expand*2015	7.07**	-15.15	1277.80***	624.13**
	2.00	8.10	226.98	189.21
expand*2016	10.05*	-11.02	1391.70***	1026.64*
	4.31	7.26	313.35	371.83
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	2298	2298	2298	2298

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Tables shows estimates from Equation 2.3. Standard errors are clustered at the state level. Column 1 shows the effect of Medicaid expansion on Medicaid revenue (including DSH payment) in millions. Column 2 shows the effect on the cost of uncompensated care. Column 3 shows the effect of Medicaid discharges, Column 4 shows the effect on total discharges. Expansion states include AR, CO, IA, KY, MD, NJ, NY, OR, VT, non-expansion states include FL, UT, WI. Cost of uncompensated care is calculated from Form S10 of the Medicare Cost Report and equals to the sum of charity care charges (minus payment for charity care) and bad debt. High share of uninsured discharges is defined as having a share of uninsured discharges at the top 30th percentile, which roughly equals to 7%.

2.9 Conclusion

In this paper, I study the effect of the Medicaid expansion on hospitals financial status and strategy. I found that Medicaid expansion as part of the ACA significantly increased Medicaid revenue and decreased the cost of uncompensated care in expansion states relative to non-expansion states. However, the increase in Medicaid revenue did not translate to an increase in total revenue or profit margin for the average hospital. A deep dive into different revenue components suggests that the increase in Medicaid revenue in hospitals in expansion states is offset by slower growth or decreases in private and Medicare outpatient revenue and in non-operating revenue. This offset pattern is consistent with a model where hospitals are risk-averse firms that may incur nonmonetary "strategic search cost" when searching for ways to improve its financial status. In this model, a Medicaid revenue gain decreases the marginal return of additional effort invested to generate revenue elsewhere and reduces the total amount of such effort, leading to an offset of total revenue.

One exception to the findings above is public hospitals. Among all hospitals in Medicaid expansion states, public hospitals saw the greatest increase in Medicaid revenue after expansion relative their peers in non-expansion states. For public hospitals, this revenue increase directly translates to increases in total revenue and total margin. In other words, the Medicaid expansion increased the financial situation of public hospitals in expansion states. These public hospitals did not experience revenue offsets as private hospitals did. Consistent with the "strategic search cost" model, this difference may be because public hospitals have few strategic levers to employ to boost its financial status in the absence of the expansion.

I also examined the effect of the Medicaid expansion on hospital strategy regarding labor expenditure and capital investment. Consistent with the findings on total revenue, I only find evidence of increased labor expenditure and capital investment after expansion for public hospitals in expansion states. This suggests that when hospitals' financial situation improve, part of the financial gain is spent on patient services. Future work will examine such investment in greater detail using alternative data sources such as the American Hospital Association Survey or state-level certificate of need filings.

Table 2.15: *Overall Financial Health, Diff-in-Diffs, Controlling for Pre-trend*

	High share of uninsured discharges in 2009						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	TotalRev(m)	TotalExp(m)	NetInc(m)	TotalMargin	OperRev(m)	OperExp(m)	OperMargin
expand*2014	-13.72*	-12.92*	-0.81	-0.00	-9.36	-12.22*	0.02
	4.94	4.29	4.41	0.01	5.02	4.43	0.01
expand*2015	-27.41*	-22.62*	-4.79	0.01	-23.75*	-21.72*	0.02
	11.04	9.07	6.49	0.02	8.60	9.36	0.01
expand*2016	-41.13*	-3.88	-37.25***	-0.05**	-34.78*	-4.68	-0.03
	15.71	15.66	3.91	0.01	14.82	15.50	0.02
Observations	972	972	972	972	966	966	966
	Low share of uninsured discharges in 2009						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	TotalRev(m)	TotalExp(m)	NetInc(m)	TotalMargin	OperRev(m)	OperExp(m)	OperMargin
expand*2014	-8.28	1.07	-9.35	-0.01	-5.75	0.53	0.00
	6.73	5.16	9.59	0.02	5.28	4.83	0.01
expand*2015	1.95	6.42	-4.47	0.02	3.82	5.14	0.03
	9.57	7.19	13.31	0.03	6.99	6.92	0.02
expand*2016	7.13	15.19	-8.06	0.01	9.41	14.45	0.03
	10.58	11.77	17.09	0.03	9.25	11.38	0.03
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	2298	2298	2298	2298	2298	2298	2298

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Tables shows estimates from Equation 2.2. Standard errors are clustered at the state level. Operating Revenue includes revenue from patient care. Total revenue is operating revenue plus non-operating revenue, which includes revenue from investment, private donations, parking, gift shops, cafeteria, and other auxiliary services. Operating expenses includes expenses related to patient care. Total expenses equals operating expenses plus non-operating expenses. Net Income is the difference between total revenue and total expenses. Total margin equals to $\frac{(\text{total revenue}-\text{total expenses})}{\text{total revenue}}$. Operating margin equals to $\frac{(\text{operating revenue}-\text{operating expenses})}{\text{operating revenue}}$. Expansion states include AR, CO, IA, KY, MD, NJ, NY, OR, VT, non-expansion states include FL, UT, WI. High share of uninsured discharges is defined as having a share of uninsured discharges at the top 30th percentile, which roughly equals to 7%

Table 2.16: *Revenue Details, Diff-in-Diffs, Controlling for Pre-trend*

	High share of uninsured discharges in 2009							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	McaidRev(m)	StateCounty(m)	MDC Rev(m)	PrivRev(m)	NonOpRev(m)	Donation(m)	Non-OpGovAppr(m)	EstPrivPrice
expand*2014	9.52*	1.10	1.64	-25.94***	-3.33	-0.17	1.08	-1456.77***
	3.21	0.54	2.12	3.29	2.96	0.11	1.36	185.75
expand*2015	9.66*	1.47	0.13	-39.01**	-3.33	-0.13	-0.44	-2036.36***
	3.10	0.90	2.97	11.52	5.96	0.17	0.88	252.90
expand*2016	12.88*	1.93	1.79	-58.18***	-5.25	-0.26	-0.86	-2946.75**
	4.57	1.28	4.13	10.41	4.56	0.15	1.49	653.19
Observations	972	960	972	960	972	930	960	948
	Low share of uninsured discharges in 2009							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	McaidRev	StateCounty)	MDC Rev	PrivRev	NonOpRev	Donation	Non-OpGovAppr	SimPrivPrice
expand*2014	2.28	-0.19	0.95	-11.16	-2.53	0.14	0.05	-881.67
	1.42	0.16	0.92	6.95	2.27	0.11	0.27	602.37
expand*2015	7.07**	-0.15	3.21*	-8.61	-1.87	0.05	-0.40	-983.03
	2.00	0.20	1.40	9.92	3.52	0.16	0.21	624.52
expand*2016	10.05*	-0.25	4.03	-6.66	-2.27	0.13	-0.24*	-941.06
	4.31	0.26	2.00	10.57	2.96	0.18	0.08	962.59
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	2298	2256	2298	2280	2298	2160	2274	2274

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Tables shows estimates from Equation 2.3. Standard errors are clustered at the state level. Column (1) shows the result for Medicaid revenue (including DSH). Column (2) shows revenue from state and county indigent programs. Column (3) shows Medicare FFS revenue. Column (4) shows a measure of "private residual revenue", which equals to total revenue minus Medicaid revenue (including DSH), Medicare FFS revenue, and payment from State and County Indigent programs. This residual revenue is a combination of private revenue (including Medicare Advantage but does not include Medicaid managed care, Medicare payment for outpatient services,and other Medicare payments not tied to inpatient visits. Column (5) shows non-operating revenue. Column (6) shows revenue from private donations. Column (7) shows governmental appropriations. Column (6) and (7) are both components of (5). Column (8) show a measure of estimated private price, as defined in Appendix B.3.1. High share of uninsured discharges is defined as having a share of uninsured discharges at the top 30th percentile, which roughly equals to 7%.

Although a number of studies have shown that Medicaid expansion has led to increases in Medicaid revenue and decreases in cost of uncompensated care, this study is one of the first to highlight that for the average hospital, the gain in Medicaid revenue did not translate to improvement in the hospital's overall financial situation. Findings in this paper inform our understanding of the incidence of Medicaid expansion: when hospitals have other strategic levers to improve its financial status, Medicaid expansion may benefit hospitals by reducing the need to perform costly strategic searches to find the revenue-maximizing path, resulting in an offset of the additional Medicaid revenue. When such offset happens, the effect on patient care is ambiguous. The offset means that the Medicaid expansion brings little additional monetary resources that could be spent on patient care. At the same time, if we think organizations can only focus on a small number of priorities at a time, less focus on profit-maximization may mean that hospital management can give greater focus to patient services. When strategic levers to improve a hospital's financial status are limited, as in the case of public hospitals, additional revenue from the expansion may translate directly to gains in overall financial performance and additional spending and investment in patient services.

Future work will bring in discharge data to assess the degree of crowd-out directly and to refine the role "strategic search cost" may play in offsetting Medicaid revenue gain. Furthermore, an implication of the model of hospitals as a risk-averse organization who care about profit, community value, and monetary and non-monetary cost is that additional gain in Medicaid revenue may shift hospital's priority more from profit-seeking to providing a social good. One way this priority shift may manifest is through the provision of unprofitable services that are highly needed by the community, such as behavioral health. Future work using more detailed data on what technology and service lines hospitals are focusing on after the expansion will shed light on this question.

Chapter 3

The Uninsured Do Not Use the ED More - They Use Other Care Less

3.1 Introduction

With the future of Medicaid and other publicly subsidized insurance uncertain, there is active discussion about the role of insurance in steering patients towards more effective health care. There is a pervasive view that, relative to the insured, the uninsured both use the emergency department (ED) more and use it in less appropriate circumstances. (Abelson, 2008, Agency for Healthcare Quality and Research, 2013, Leonard, 2016, Robert Wood Johnson Foundation, 2013) Similar assumptions are often made in the academic literature as well.(Chen et al., 2010a, Komaroff, 2014, Newton et al., 2008) These views persist despite existing evidence that the uninsured do not use the ED dramatically more (or in very different circumstances) than the insured, and in fact use the ED much less than the publicly insured.(Finkelstein et al., 2016, Hope, 2013, Hunt, 2011, Nather, 2013, Newton et al., 2008, Politico Staff, 2009, Snyder, 2013, Taubman et al., 2014)

Indeed, one of the common arguments in favor of expanding health insurance coverage is that it can relieve ED crowding and reduce medical costs by shifting care to more efficient primary care settings.(Hope, 2013, Hunt, 2011, Nather, 2013, Politico Staff, 2009, Snyder, 2013) This belief that health insurance coverage will reduce ED use has mixed support in the quasi-experimental literature,(Chen et al., 2010a, Garthwaite et al., 2017, Miller, 2012, Nikpay et al., 2017, Sommers et al., 2016, Sommers and Simon, 2017) and is not supported by the results of the Oregon Health Insurance Experiment. This randomized controlled evaluation of the impact of expanding Medicaid to cover uninsured working-age adults found that Medicaid coverage increased ED use across a broad range of visit types, conditions and sub-populations, and that this increase persisted over the two years of the study.(Finkelstein et al., 2016, Taubman et al., 2014)

An increase in ED use due to Medicaid coverage of the uninsured is consistent with basic economic theory: insurance lowers the cost to the patient of using the ED and therefore increases demand. But the Oregon Experiment's finding was nonetheless greeted with considerable attention and surprise.(Beck, 2014, Heintzman et al., 2014, Komaroff, 2014, Tavernise, 2014) This surprise suggests the importance of re-examining common assumptions about patterns of ED use among the uninsured and the publicly and privately insured. We first update prior studies of ED use by insurance status,(McLaughlin and Mortensen, 2003, Mortensen and Song, 2008, Newton et al., 2008, Tang et al., 2010, Zuckerman and Shen, 2004, **?**, **?**, **?**)drawing on more recent data from 2013 (right before the ACA). We show the prior findings persist: among working-age adults, the uninsured use the ED at very similar rates and in very similar circumstances as the insured overall âĂŞ and use the ED substantially less than those on Medicaid.

We also show that while the uninsured do not use the ED substantially more than the insured use the ED, the uninsured do use other types of care much less than the insured. This may help create and perpetuate the misperception that the uninsured use the ED more than the insured.

Indeed, one of the common arguments in favor of expanding health insurance coverage is that it can relieve ED crowding and reduce medical costs by shifting care to more efficient primary care settings. (Beck, 2014, Billings and Mijanovich, 2000, Heintzman et al., 2014, Tavernise, 2014, **?**) This belief that health insurance coverage will reduce ED use has mixed support in the quasi-experimental literature,(Agency for Healthcare Quality and Research, 2013, Chen et al., 2010a, **?**, **?**) and is not supported by the results of the Oregon Health Insurance Experiment. This randomized controlled evaluation of the impact of expanding Medicaid to cover uninsured working-age adults found that Medicaid coverage increased ED use across a broad range of visit types, conditions and sub-populations, and that this increase persisted over the two years of the study.(Finkelstein et al., 2016, Taubman et al., 2014)

An increase in ED use due to Medicaid coverage of the uninsured is consistent with basic economic theory: insurance lowers the cost to the patient of using the ED and therefore increases demand. But the Oregon Experiment's finding was nonetheless greeted with considerable attention and surprise.(Beck, 2014, Heintzman et al., 2014, Komaroff, 2014, Tavernise, 2014) This surprise suggests the importance of re-examining common assumptions about patterns of ED use among the uninsured and the publicly and privately insured.

We first update prior studies of ED use by insurance status, (McLaughlin and Mortensen, 2003, Mortensen and Song, 2008, Newton et al., 2008, Tang et al., 2010, Zuckerman and Shen, 2004, **?**, **?**, **?**)drawing on more recent data from 2013 (right before the ACA). We show the prior findings persist: among working-age adults, the uninsured use the ED at very similar rates and in very similar circumstances as the insured overall - and use the ED substantially less than those on Medicaid.

We also show that while the uninsured do not use the ED substantially more than the insured use the ED, the uninsured do use other types of care much less than the insured. This may help create and perpetuate the misperception that the uninsured use the ED more than the insured.

3.2 Methods and Data

We draw on two national data sources to examine care utilization among insured and uninsured adults aged 19-64. We use the 2013 Medical Expenditure Panel Survey (MEPS) to analyze rates of use of different types of care by insurance status. The MEPS is a population-based survey designed to be nationally representative of the civilian, non-institutionalized population. We define âĂIJinsuredâĂİ as having had insurance of any kind at any point during 2013; among insured individuals we further distinguish between "privately insured" (those who have any private insurance coverage during the year) and "publicly insured" (insured adults who only had public insurance during the year). Within the publicly insured, we further break out those who were ever on Medicaid during the year, which is almost 90 percent of publicly insured adults in this age range.

We use the 2013 National Hospital Ambulatory Medical Care Survey (NHAMCS) to analyze different types of ED visits by insurance status. The NHAMCS data come from a nationally-representative sample of electronic patient records from ED visits. We classify a visit as "insured" if its payer type is private insurance, Medicare, Medicaid or "other" insurance; we classify as "uninsured" those visits with payer type of self-pay, non-charge, or charity. Visits with a payer type listed as "unknown", Workers Compensation, or missing are excluded from the analysis. Among insured visits, we again further distinguish between those that are "privately insured", and those that are "publicly insured" (i.e. on Medicare or Medicaid), and within the publicly insured visits we further break out those visits covered by Medicaid, which represent about two-thirds of publicly insured ED visits in our age range.

We analyze health care utilization by insurance status. We look at utilization rates for different types of care: ED visits, outpatient visits, and hospital admissions.

Within the ED setting, we also examine use of ED visits based on the circumstances of the visit. There is little consensus on how to identify ED visits for conditions that might be treated in less costly settings.(**?**) We therefore analyze three different, common metrics of the circumstances of the visit: triage status upon arrival, eventual diagnosis, and the likelihood that the visit might have been avoided had better primary care been available.(Finkelstein et al., 2016, Kellermann and Weinick, 2012, Taubman et al., 2014)

Triage status is recorded on the patient's medical record in the NHAMCS. Triage reflects a preliminary assessment of urgency that is assigned to patients upon arrival at the ED; it may not reflect the patient's actual condition following more definitive evaluation and treatment.

The eventual (ex-post) diagnosis and treatment of the patient can also be used to classify visits based on the Billings et al. algorithm.(Billings and Mijanovich, 2000) This algorithm - which was developed to assess community access to primary care - distinguishes between emergency visits (i.e. requiring care within 12 hours) and non-emergency visits. Within emergency visits, it further distinguishes between those that would likely be treatable in a primary care setting ("emergent, primary care treatable"), those that require ED care but might have been preventable by timely ambulatory care ("emergent, preventable"), and those that require ED care and were not preventable ("emergent, non preventable").

Our final metric identifies visits for "Ambulatory Care Sensitive Conditions" using the AHRQ Prevention Quality Indicators algorithm.(Agency for Healthcare Quality and Research, 2013) These are visits for conditions for which good outpatient care could potentially have prevented the need for the visit, or for which early intervention could have prevented complications or worsening of the condition.

We note several limitations of our analyses. First, there is no clear consensus on how to assess the circumstances of an ED visit for "appropriateness" or "urgency"; each of the measures we present has its limitations,**?** and our results do not speak to the health value of any particular ED visit.

Second, our analysis of utilization patterns by insurance type is descriptive only and does not reflect the causal effects of insurance per se; its purpose is to help understand and interpret the results of prior causal estimates of the impact of health insurance on ED use. Relatedly, the uninsured have different socioeconomic, health, and demographic characteristics from other groups, and it is unlikely that we can control for all of these differences.

Third, the self-reports of ED visits in the MEPS may undercount actual visits, with potentially differential undercounting by insurance status. We present supplementary analysis to gauge the scope of this undercount, suggesting that adjustment for differential undercounting can attenuate the magnitude of some of the key differences discussed, but that our qualitative conclusions are nonetheless robust.

This coarse comparison between insured and uninsured masks important differences among the insured. In particular, while the uninsured appear to use the ED slightly more than the privately insured, they use it substantially less than the publicly insured, including the subset of the publicly insured on Medicaid. For example, the average number of ED visits for the uninsured (0.18) is only slight higher than for the privately insured (0.15) but dramatically lower than for adults on Medicaid (0.52). It is the high ED utilization of Medicaid enrollees that stands out here.

Second, in contrast to the similarity of ED use by the insured and uninsured, use of outpatient care and inpatient hospital care is much lower for the uninsured than for the insured. Only about two-fifths of uninsured adults had an outpatient visit, compared to about three-quarters of insured adults or three-quarters of adults on Medicaid. Uninsured adults averaged about 2 outpatient visits per person per year, compared to about 6 for the insured overall and almost 9 for those on Medicaid. Similarly, uninsured adults are less than half as likely to have an inpatient hospital admission (3.0%) as the insured (7.6%) overall, and less than a fifth as likely as those on Medicaid (16.9%). Thus, while the uninsured are not over-represented in the ED relative to the insured, they do use the ED for a substantially larger share of their medical care than the insured do.

The differences in Table 3.1 may reflect differences in the underlying characteristics of the populations with different insurance coverage. Indeed, as emphasized by the Institute of Medicine's Committee on the Consequences of Uninsurance, (Institute of Medicine, 2002) the uninsured tend to be sicker and to delay care relative to the insured. As noted, the goal here is to be descriptive, rather than attempt to provide causal estimates of the impact of insurance coverage on ED use. Still, it is helpful to investigate the extent to which the results in Table 3.1 reflect differences in underlying demographics across the populations. Table 3.2 therefore repeats the analysis in Table 3.1, after adjusting for differences in demographics and self-reported health. This adjustment if anything makes the prior findings starker. Other studies have used more more extensive adjustors and reached similar conclusions.(14) Appendix C provides more detail on the methods we used, the challenges with more extensive adjustments, and robustness to alternative approaches including more extensive adjustment.

Another concern with Table 3.1 is that utilization is self-reported, and it has been noted in the literature that the MEPS may undercount ED visits - and may do so differentially by insurance type.(Bernard et al., 2012, Zuvekas and Olin, 2009) In Appendix C we discuss this issue in more depth and report the results from two important sensitivity analyses. First, we follow Tang et al. (2010) and approximate rates of ED visits by insurance status by combining NHAMCS data on counts of ED visits by insurance status with Census data on population counts by insurance status.(Tang et al., 2010) While a somewhat cruder metric, it confirms the prior literature that ED visits may be undercounted in the MEPS, and suggests that they may be disproportionately undercounted for the uninsured. Using the adjusted estimates, however, the main results continue

Table 3.1: *Utilization by Insurance Status (MEPS 2013)*

	Uninsured Adults	Insured Adults	Privately Insured Adults	Publicly Insured Adults	Adults on Medicaid
ED visits					
any (%)	12.2	13.7	11.1	28.9	29.3
number	0.177	0.202	0.149	0.521	0.523
	(0.649)	(0.604)	(0.443)	(1.352)	(1.355)
Outpatient visits					
any (%)	41.8	76.6	76.7	75.7	74.5
number	2.144	6.215	5.731	9.111	8.729
	(7.658)	(10.676)	(8.720)	(21.144)	(21.325)
Hospital admissions					
any (%)	3.0	7.6	6.2	16.0	16.9
number	0.036	0.095	0.075	0.219	0.230
	(0.252)	(0.356)	(0.275)	(0.767)	(0.800)
N (individuals)	5,853	15,930	12,115	3,815	3,410

[a] SOURCE Authors' analysis of data from the Medical Expenditure Panel Survey. NOTES Table reports results for adults (ages 19-64) in the 2013 MEPS, by insurance status. All results are weighted using final person weights which are designed to be nationally representative of the civilian, non-institutionalized US population. Standard deviations (for continuous outcomes) are shown in parentheses.

to hold: the uninsured still use the ED much less than Medicaid patients (although, as shown in Appendix Table C.1, the estimated utilization by the uninsured is now about half as much as those in Medicaid, instead of the one-third rate reported in Table 3.1). Second, we use additional data from a low-income population in Oregon where we can directly measure both the population denominator and ED use in administrative data; this supplementary analysis (Appendix Table C.2) confirms the finding in the MEPS that the uninsured use the ED much less than those with Medicaid.

Table 3.2: *Utilization by Insurance Status (MEPS 2013), Adjusted for Demographics and Self-Reported Health*

	Uninsured Adults	Insured Adults	Privately Insured Adults	Publicly Insured Adults	Adults on Medicaid
ED visits					
any (%)	9.6	14.3	13.0	22.0	22.4
number	0.126	0.214	0.187	0.383	0.383
	(0.011)	(0.007)	(0.007)	(0.028)	(0.029)
Outpatient visits					
any (%)	47.6	75.2	76.0	81.3	81.4
number	2.649	6.097	5.802	8.757	8.634
	(0.231)	(0.118)	(0.139)	(0.330)	(0.377)
Hospital admissions					
any (%)	2.0	7.9	7.2	13.3	14.3
number	0.021	0.099	0.088	0.178	0.194
	(0.005)	(0.004)	(0.004)	(0.016)	(0.018)
N (individuals)	5,853	15,930	12,115	3,815	3,410

SOURCE Authors' analysis of data from the Medical Expenditure Panel Survey. NOTES Table reports results for adults (ages 19-64) in the 2013 MEPS, by insurance status, and adjusted based on covariates to be representative of the adult population aged 19-64. Covariates used are: gender, age (in 10 year bins), indicator for married, indicators for race/ethnicity (non-Hispanic white, non-Hispanic black, non-Hispanic other / multi race, Hispanic), indicator for 12 or more years of education, poverty status indicators (less than 100% federal poverty line (FPL), 100-125% FPL, 125-200% FPL, 200-400% FPL, >400% FPL) and indicators for self reported health (fair/poor; good, or excellent/very good) . All results are weighted using final person weights which are designed to be nationally representative of the civilian, non-institutionalized US population. Standard errors (for continuous outcomes) are shown in parentheses.

3.3 Types of Care Use

In addition to rates of care use, we also examine types of care use. Table 3.3 reports the results. Like the frequency of ED visits from Table 3.1, the conditions and circumstances for ED visits in Table 3.3 are also quite similar for the insured and the uninsured. Moreover, here the circumstances for ED use are also quite similar between the uninsured and the publicly insured or Medicaid-covered adults. These findings hold across all three measures of the circumstances of the visit: triage status upon arrival, coding based on eventual diagnosis, and the likelihood that the ED visit might have been prevented through better primary care.

For example, about 38 percent of the uninsured's visits are classified as immediate/emergent or urgent, which is slightly lower than for insured adults (about 44 percent) but very similar to Medicaid-covered adults (about 40 percent). A similar share of visits is classified as "emergent, non preventable": 21.3 percent for the uninsured compared to 23.2 percent for the insured and 19.5 percent for Medicaid patients. The share of visits that are potentially avoidable with better primary care (visits for "ambulatory-care sensitive conditions") is in fact slightly lower for uninsured than insured adults or Medicaid-covered adults.

Once again, we consider how these results are affected by adjusting for differences across the populations with different insurance. Table 3.4 shows that they are not. Appendix Table C.8 again provides more detail.

3.4 Discussion

There is a widely held perception that Medicaid coverage for the previously uninsured will lower ED use. This perception seems to be grounded, at least in part, in the widespread belief that the uninsured have higher rates of ED use overall, and use the ED for less "appropriate" circumstances than the insured. In this paper, we offer insights into the potential sources of the disconnect between the evidence and the conventional wisdom.

First, we confirm prior findings that, among working age adults, the uninsured may use the ED somewhat more than the privately insured, but they use it substantially less than those on

Table 3.3: *Type of ED Visit (NHAMCS 2013)*

	Uninsured Adults	Insured Adults	Privately Insured Adults	Publicly Insured Adults	Adults on Medicaid
Weighted visits (000s)	17,191	54,443	26,040	24,698	16,483
N (visits)	2,973	10,412	4,748	4,815	3,353
By triage status					
Immediate/ Emergent (%)	6.2	7.7	7.9	7.4	6.4
Urgent (%)	31.7	36.1	35.9	35.4	33.8
Semiurgent/Nonurgent (%)	37.2	29.9	27.6	31.6	34.6
No triage/Unknown (%)	25.0	26.4	28.5	25.6	25.2
By eventual diagnosis					
Nonemergent (%)	23.0	22.5	21.2	23.9	24.9
Emergent					
ED care not needed (primary care treatable) (%)	33.2	32.8	33.9	31.8	32.6
ED care needed, preventable (%)	5.1	5.7	4.9	6.6	6.6
ED care needed, not preventable (%)	21.3	23.2	25.6	20.7	19.5
Unclassified (%)	17.3	15.7	14.4	16.9	16.5
By condition					
Chronic (%)	15.0	15.6	13.6	17.8	17.2
Ambulatory-care sensitive condition (%)	4.3	5.7	4.6	7.3	7.0

SOURCE Authors' analysis of data from the National Hospital Ambulatory Medical Care Survey. NOTES Table reports results for ED visits by adults aged 19-64 from the 2013 NHAMCS, by insurance status. We omit visits for childbirth.

Table 3.4: *Type of ED Visit (NHAMCS 2013), Adjusted for Demographics*

	Uninsured Adults	Insured Adults	Privately Insured Adults	Publicly Insured Adults	Adults on Medicaid
Weighted visits (000s)	17,191	54,443	26,040	24,698	16,483
N (visits)	2,973	10,412	4,748	4,815	3,353
By triage status					
Immediate/ Emergent (%)	6.6	7.6	7.6	7.4	7.1
Urgent (%)	32.4	35.8	35.6	35.1	33.7
Semiurgent/Nonurgent (%)	35.9	30.3	28.3	32.0	33.4
No triage/Unknown (%)	25.1	26.3	28.5	25.5	25.8
By eventual diagnosis					
Nonemergent (%)	22.6	22.6	21.8	23.5	23.7
Emergent					
ED care not needed					
(primary care treatable) (%)	32.9	32.9	34.1	31.9	32.1
ED care needed, preventable (%)	5.3	5.7	4.8	6.6	6.7
ED care needed, not preventable (%)	21.8	23.1	25.0	21.0	20.5
Unclassified (%)	17.5	15.7	14.2	17.0	17.0
By condition					
Chronic (%)	15.5	15.5	13.2	17.9	18.3
Ambulatory-care sensitive condition (%)	4.6	5.6	4.5	7.1	7.0

SOURCE Authors' analysis of data from the National Hospital Ambulatory Medical Care Survey. NOTES Table reports results for ED visits by adults aged 19-64 from the 2013 NHAMCS, by insurance status and adjusted based on covariates to be representative of non childbirth ED visits by the adult population aged 19-64. Covariates used are: gender, age (in 10 year bins), and indicators for race / ethnicity (non-Hispanic white, non-Hispanic black, non-Hispanic other, Hispanic). We omit visits for childbirth.

Medicaid. The uninsured also use the ED in similar circumstances and for similar conditions as their insured counterparts. Indeed, a striking finding is the similarly high rate of visits for both the insured and uninsured for conditions that are likely non-emergent (roughly one-fifth) or potentially amenable to being treated in other settings (another third).

Our descriptive findings that - contrary to the conventional wisdom - the uninsured use the ED substantially less than those on Medicaid is consistent with empirical evidence of the impact of insurance on ED use. Results from a randomized evaluation of Medicaid coverage in Oregon found that Medicaid coverage of the previously uninsured increased ED use (Finkelstein et al., 2016, Taubman et al., 2014). Likewise, the RAND Health Insurance Experiment from the 1970s, which randomized the amount of consumer cost-sharing among insured individuals, found that more comprehensive coverage increased ED use.(Newhouse, 1993)

Results from quasi-experimental studies are more mixed. For example, some analyses of the 2014 Medicaid expansions found that Medicaid increases use and access to the ED,(Garthwaite et al., 2017, Nikpay et al., 2017) while others found it decreased in ED use.(Newhouse, 1993) Analysis of the 2006 health insurance expansion in Massachusetts has found either no effect on emergency department use,(Chen et al., 2010a) or reduced emergency department use.(Miller, 2012) A recent review of the evidence of the impact of health insurance on ED use suggests that differences across studies may point to a "complex" relationship between insurance and ED use, which may vary based on the characteristics of the population covered, the nature of the insurance for the newly covered, and the nature of care availability for the uninsured, among other factors;(Sommers and Simon, 2017) others have likewise emphasized potential heterogeneity in the impact of insurance on ED use.(Kowalski, 2016) Methodological differences may also be a factor.

Second, we show that the uninsured use other types of care such as outpatient visits or hospitalizations much less than the insured. As a result, uninsured patients are most likely encountered in the ED. This may contribute to the enduring misperception that the uninsured use the ED more than the insured - it is not that they use the ED more than other populations, but rather that they use other types of care less than other populations. Our finding is also consistent with a robust finding of both the experimental and quasi-experimental literature: insurance increases access to and use of health care.(Card et al., 2008, Finkelstein et al., 2012, Newhouse, 1993) Indeed, the Institute of Medicine's major reviews of the consequences of uninsurance highlighted barriers to access among most types of care.(Institute of Medicine, 2001) There is therefore consensus in the literature that insurance expansions increase health care use.(Sommers and Simon, 2017)

The key distinction in care use therefore, is the relative paucity of use of non-ED care - such as clinics or hospitalizations âĂŞ by the uninsured compared to the insured. This reflects both financial and non-financial access barriers by the uninsured. EMTALA provides the uninsured with a legal right to care through the ED - although it does not protect them against the financial consequences of expensive ED visits. The uninsured, however, may be legally denied care in non ED settings. Other non-financial barriers to health care access for poor populations - both insured and uninsured âĂŞ including factors such as stigma, difficulty finding and building relationships with providers, or confusion about insurance benefits or the cost of care.(Allen et al., 2014a,b)

As policy-makers contemplate large-scale changes in health insurance programs and subsidies, a realistic view of the existing patterns of health care utilization is valuable. Our results contribute to the body of evidence that the uninsured use the ED at similar rates and in similar circumstances to the insured overall - but much less than those covered by Medicaid. At the same time, we find that the uninsured use other services substantially less than their insured counterparts - and dramatically less than those on Medicaid, a result that is consistent with a body of evidence on the barriers to non-ED care faced by the uninsured. This suggests that a focus on ED utilization alone may be misguided.

References

Abelson, R. (2008). Uninsured Put a Strain on Hospitals.

Agency for Healthcare Quality and Research (2013). Prevention Quality Indicators.

Allen, H., Wright, B. J., and Baicker, K. (2014a). New medicaid enrollees in oregon report health care successes and challenges. *Health Affairs*, 33(2):292–299.

Allen, H., Wright, B. J., Harding, K., and Broffman, L. (2014b). The role of stigma in access to health care for the poor. *Milbank Quarterly*, 92(2):289–318.

Baicker, K. and Staiger, D. (2005). Fiscal Shenanigans, Targeted Federal Health Care Funds, and Patient Mortality. *The quarterly journal of economics*, 120:345–386.

Beck, M. (2014). Medicaid Expansion Drives Up visits to ER.

Bennett, K. M., Scarborough, J. E., Pappas, T. N., and Kepler, T. B. (2010). Patient socioeconomic status is an independent predictor of operative mortality. *Annals of Surgery*, 252(3):552–557.

Bernard, D., Cowan, C., Selden, T., Cai, L., Catlin, A., and Heffler, S. (2012). Reconciling Medical Expenditure Estimates from the MEPS and NHEA, 2007. *Medicare & Medicaid Research Review*, 2(4):E1–E20.

Billings, J. and Mijanovich, T. (2000). Emergency Room Use: the New York Story. *Coommonwealth Fund*.

Buchmueller, T. C., Miller, S., and Vujicic, M. (2014). HOW DO PROVIDERS RESPOND TO PUBLIC HEALTH INSURANCE EXPANSIONS ?

Card, D., Dobkin, C., and Maestas, N. (2008). The impact of nearly universal insurance coverage on health care utilization: Evidence from medicare. *American Economic Review*, 98(5):2242–2258.

Chandra, A., Finkelstein, A., Sacarny, A., and Syverson, C. (2015). Healthcare Exceptionalism? Performance and Allocation in the U.S. Healthcare Sector.

Chen, C., Scheffler, G., and Chandra, A. (2010a). Massachusetts' Health Care Reform and Emergency Department Utilization. *New England Journal of Medicine*, 363(1):1–3.

Chen, H. F., Bazzoli, G. J., Harless, D. W., and Clement, J. P. (2010b). Is quality of cardiac hospital care a public or private good? *Medical Care*, 48(11):999–1006.

Courtemanche, C., Marton, J., Ukert, B., Yelowitz, A., and Zapata, D. (2017). Early Impacts of the Affordable Care Act on Health Insurance Coverage in Medicaid Expansion and Non-Expansion States. *Journal of Policy Analysis and Management*, 36(1):178–210.

Cutler, D. M. (1998). Cost shifting or cost cutting? The incidence of reductions in Medicare payments. *Tax Policy and the Economy, Volume 12*, 12(January):1–28.

Cutler, D. M., Huckman, R. S., and Landrum, M. B. (2004). The Role of Information in Medical Markets: An Analysis of Publicly Reported Outcomes in Cardiac Surgery. *AEA Papers and Proceedings*, May:342–346.

Dorn, S., Buettgens, M., Holahan, J., and Carroll, C. (2013). The Financial Benefit to Hospitals from State Expansion of Medicaid. Technical Report March, Urban Institute.

Dranove, D., Garthwaite, C., and Ody, C. (2016). Uncompensated Care Decreased At Hospitals In Medicaid Expansion States But Not At Hospitals In Nonexpansion States. *Health Affairs*, 35(8):1471–1479.

Dranove, D., Garthwaite, C., and Ody, C. (2017). How do nonprofits respond to negative wealth shocks? The impact of the 2008 stock market collapse on hospitals. *RAND Journal of Economics*, 48(2):485–525.

Dranove, D., Kessler, D., McClellan, M., and Satterthwaite, M. (2003). Is More Information Better? The Effects of "Report Cards" on Health Care Providers.

Dranove, D. and Lindrooth, R. (2003). Hospital consolidation and costs: Another look at the evidence. *Journal of Health Economics*, 22:983–997.

Duggan, M. G. (2000). Hospital Ownership and Public Medical Spending. *Quarterly Journal of Economics*, 115:1343–1373.

Figlio, D. N. and Lucas, M. E. (2004). What's in a grade? School report cards and the housing market. *American Economic Review*, 94(3):591–604.

Finkelstein, A., Taubman, S. L., Wright, B. J., Bernstein, M., Gruber, J. H., Newhouse, J. P., Allen, H. L., and Bai (2012). THE OREGON HEALTH INSURANCE EXPERIMENT: EVIDENCE FROM THE FIRST YEAR*. *The Quarterly Journal of Economics*, 127:1057–1106.

Finkelstein, A. N., Allen, H. L., Wright, B. J., Taubman, S. L., and Baicker, K. (2016). Effect of Medicaid Coverage on ED Use – Further Evidence from Oregon's Experiment. *New England Journal of Medicine*, 375(16):1505–7.

Frakt, A. B. (2011). How much do hospitals cost shift? a review of the evidence. *Milbank Quarterly*, 89(1):90–130.

Frakt, A. B. (2014). The end of hospital cost shifting and the quest for hospital productivity.

Galewitz, P. (2015). Economy boosts safetynet hospitals in states not expanding Medicaid.

Garthwaite, C., Gross, T., Notowidigdo, M. J., and Graves, J. A. (2017). Insurance expansion and hospital emergency department access: evidence from the Affordable Care Act. *Annals of internal medicine*, 166(3):172–9.

Garthwaite, C. L. (2012). The Doctor Might See You Now: The Supply Side Effects of Public Health Insurance Expansions.

Heintzman, J., Gold, R., Bailey, S., and DeVoe, J. (2014). The Oregon experiment re-examined: the need to bolster primary care. *BMJ*.

Ho, K. (2006). The welfare effects of restricted hospital choice in the US medical care market. *Journal of Applied Econometrics*, 21(7):1039–1079.

Hope, C. (2013). Florida's Failure to Accept Federal Funding to Expand Medicaid Would Create Coverage Gap. Technical report, Center for Children and Families, Georgetown University Health Policy Institute.

Hunt, K. (2011). Romney's health-care moment. *Politico*.

Institute of Medicine (2001). *Coverage matters: insurance and health care*. National Academies Press, Washington (DC), first edit edition.

Institute of Medicine (2002). *Care without coverage: too little, too late*. National Academies Press, Washington (DC), first edit edition.

Jena, A. B., Khullar, D., Ho, O., Olenski, A. R., and Blumenthal, D. M. (2015). Sex Differences in Academic Rank in US Medical Schools in 2014. *JAMA*, 314(11):1149.

Jena, A. B., Olenski, A. R., and Blumenthal, D. M. (2016). Sex Differences in Physician Salary in US Public Medical Schools. *JAMA Internal Medicine*, 176(9):1294.

Jin, G. Z. and Leslie, P. (2003). The Effect Of Information On Product Quality: Evidence From Restaurant Hygiene Grade Cards. *The Quarterly Journal of Economics*, 118:409–451.

Jin, G. Z. and Whalley, A. (2007). The power of information: How do U.S. news rankings affect the financial resources of public colleges? Technical report, National Bureau of Economic Research.

Johnson, E. M. (2011). Ability , Learning and the Career Path of Cardiac Specialists.

Kahneman, D., Slovic, P., and Tversky, A. (1974). Judgment under uncertainty: heuristics and biases. *Science*, 185(4157):1124–1131.

Kane, N. M. and Magnus, S. A. (2001). The Medicare Cost Report and the limits of hospital accountability: improving financial accounting data. *Journal of health politics, policy and law*, 26:81–105.

Kellermann, A. and Weinick, R. (2012). Emergency departments, Medicaid costs, and access to primary care - understanding the link. *New England Journal of Medicine*, 366(23):2141–3.

Kolstad, J. T. (2013). Information and Quality When Motivation Is Intrinsic :. *The American Economic Review*, 103(7):2875–2910.

Komaroff, A. (2014). Medicaid Expansion in Oregon Led to More Short-Term ED Use. *NEJM Journal Watch*.

Kowalski, A. E. (2016). Dong more when you're durnning LATE: aplying marginal treatment effect methods to examine treatment effect heterogeneity in experiments. *NBER Working Paper*, 22363.

Leonard, K. (2016). Obamacare Has Barely Made a Dent in ER Visits. *US News & World Report*.

Lewis, M. S. and Pflum, K. E. (2017). Hospital systems and bargaining power: evidence from out-of-market acquisitions. *RAND Journal of Economics*, 48(3):579–610.

Luca, M. (2011). Reviews ,Reputation ,and Revenue: The Case of Yelp.com Reviews. *Harvard Business School NOM Unit Working Paper*.

Magnus, S. A. and Smith, D. G. (2000). Better Medicare Cost Report data are needed to help hospitals benchmark costs and performance. *Health care management review*, 25:65–76.

Malmendier, U. and Tate, G. (2009). SUPERSTAR CEOs. *Quarterly Journal of Economics*, 124(4):1593–1638.

McLaughlin, C. G. and Mortensen, K. (2003). Who walks through the door? The effect of the uninsured on hospital use. *Health Affairs*, 22(6):143–155.

McMorrow, S., Kenney, G. M., Long, S. K., and Anderson, N. (2015). Uninsurance among young adults continues to decline, particularly in medicaid expansion states. *Health Affairs*, 34(4):616–620.

Miller, A. (2017). In a notoriously tough job, Grady chief survives and thrives.

Miller, S. (2012). The effect of insurance on emergency room visits: An analysis of the 2006 Massachusetts health reform. *Journal of Public Economics*, 96:893–908.

Miller, S. and Wherry, L. R. (2017). Health and Access to Care during the First 2 Years of the ACA Medicaid Expansions. *New England Journal of Medicine*, 376(10):947–956.

Mortensen, K. and Song, P. H. (2008). Minding the gap: A decomposition of emergency department use by medicaid enrollees and the uninsured. *Medical Care*, 46(10):1099–1107.

Nather, D. K. (2013). ACA flip burns concservatives. *Politico*.

Newhouse, J. P. (1993). *Free for all? Lessons from the RAND Health Insurance Experiment*. Harvard University Press, Cambridge (MA).

Newton, M. F., Keirns, C. C., Cunningham, R., Hayward, R. A., and Stanley, R. (2008). Uninsured Adults Presenting to US Emergency Departments: Assumptions vs Data. *Jama*, 300(16).

Nikpay, S., Buchmueller, T., and Levy, H. (2015). Early medicaid expansion in connecticut stemmed the growth in hospital uncompensated care. *Health Affairs*, 34(7):1170–1179.

Nikpay, S., Freedman, S., Levy, H., and Buchmueller, T. (2017). Effect of the Affordable Care Act Medicaid expansion on emergency department visits: evidence from state-level emergency department databases. *Annals of Emergency Medicine*, 70(2):215–25.

Politico Staff (2009). Arena Digest: What obstacles await Obama on health care? *Politico*.

Pope, D. G. (2009). Reacting to rankings: Evidence from "America's Best Hospitals". *Journal of Health Economics*, 28(6):1154–1165.

Robert Wood Johnson Foundation (2013). Low-Income Patients Say ER is Better Than Primary Care. Technical report, Robert Wood Johnson Foundation, Princeton, New Jersey.

Ross, C. (2015). Sloppiness, lax oversight revealed at Cleveland Clinic's Marymount lab; overhaul includes firings.

Schwartz, L. M., Woloshin, S., and Birkmeyer, J. D. (2005). How do elderly patients decide where to go for major surgery? Telephone interview survey. *BMJ (Clinical research ed.)*, 331(7520):821.

Snyder, O. o. G. R. (2013). Snyder calls for Medicaid expansion to improve health, save money; Greater access to care, lower business costs among benefits [press release]. Technical report, Office of Governor Rick Snyder, Michigan, USA.

Sommers, B. D., Blendon, R. J., Orav, E. J., and Epstein, A. M. (2016). Changes in utilization and health among low-income adults after Medicaid expansion or expanded private insurance. *JAMA Internal Medicine*, 176(10):1501–9.

Sommers, B. D. and Simon, K. I. (2017). Health Insurace and emergency department use - a complext relationship. *New England Journal of Medicine*, 376(18):1708–11.

Sorensen, A. T. (2007). Bestseller lists and product variety. *Journal of Industrial Economics*, 55(4):715–738.

Tang, N., Stein, J., Hsia, R. Y., Maselli, J. H., and Gonzales, R. (2010). Trends and characteristics of US emergency department visits, 1997-2007. *JAMA - Journal of the American Medical Association*, 304(6):664–670.

Taubman, S. L., Allen, H. L., Wright, B. J., Baicker, K., and Finkelstein, A. N. (2014). Medicaid increases emergency-department use: evidence from Oregon's Health Insurance Experiment. *Science*, 343:263–8.

Tavernise, S. (2014). Emergency Visits Seen Increasing With Health Law. *The New York Times*.

Tsugawa, Y., Newhouse, J. P., Zaslavsky, A. M., Blumenthal, D. M., and Jena, A. B. (2017). Physician age and outcomes in elderly patients in hospital in the US: observational study. *BMJ*, page j1797.

Young, G. (2017). State Medicaid Expansion and Hospital Financial Status. In *AcademyHealth*, New Orleans.

Zhang, Y. (2011). Are Two Report Cards Better than One? The Case of CABG Surgery and Patient Sorting.

Zuckerman, S. and Shen, Y. C. (2004). Characteristics of occasional and frequent emergency department users do insurance coverage and access to care matter? *Medical Care*, 42(2):176–182.

Zuvekas, S. H. and Olin, G. L. (2009). Validating household reports of health care use in the medical expenditure panel survey. *Health Services Research*, 44(5 PART 1):1679–1700.

Appendix A

A.1 Definition of procedures

Procedures are identified using the ICD-9 procedure code in Medicare inpatient data. Higher-level procedures are identified using using single level CCS groupings.

- Heart procedures (used to identify specialty hospitals): CCS group 43-64
- CABG: CCG group 44
- Orthopedic surgery (used to identify specialty hospitals): CCS group 142-164, excluding ICD-9 codes 76.XX (surgery on the facial system)

A.2 Identification of "top doctor" winners

I limit my sample to physicians who are identified in the Doximity data as having one of the seven specialties: General Surgery, Neurosurgery, Obstetrics and Gynecology, Orthopaedic Surgery, (Cardio)Thoracic Surgery, Urology, and Vascular Surgery. I then use Castle Connolly and SuperDoctor data to identify award-winning status within this sample. Castle Connolly data comes with NPIs, so I can identify if a surgeon in the sample has won an award in any specialty. SuperDoctor's NPI needs to be obtained using probabilistic matching and some manual lookup. To reduce the burden of manual look up, I focus on SuperDoctor winners with one of the three listed specialties in the following areas: Cardiac Surgery, Colon and Rectal Surgery, Gynecologic Oncology, Neurological Surgery, Obstetrics and Gynecology, Orthopedic Surgery, Pediatric

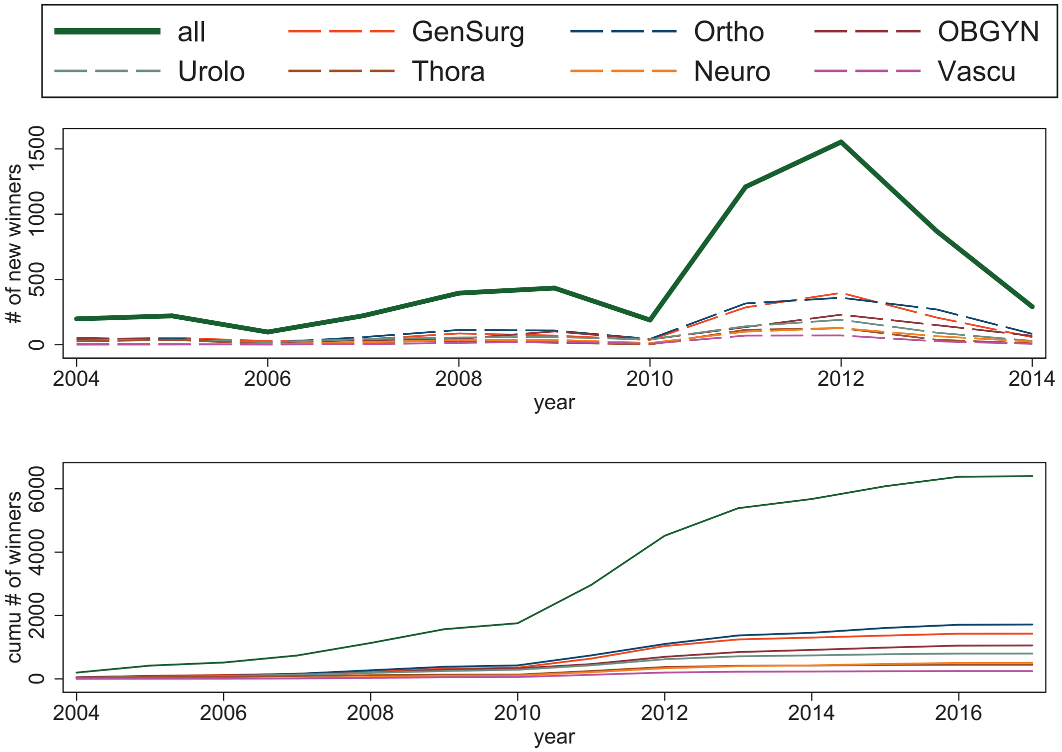
Surgery, Plastic Surgery, Sports Medicine, General Surgery, (Cardio)Thoracic Surgery, Transplant Surgery, Urology, Vascular Surgery. I consider these the set of specialties under which my sample of surgeons can win an award. It's unlikely, for example, a surgeon would win "SuperDoctor" award for "Allergy and Immunology". Even if they did, it would not be very relevant for patient demand in inpatient services. To reduce the chance that I under-identified winners in my sample, I exclude any individual who is not identified as a winner of SuperDoctor in my match but is listed as a SuperDoctor as of 2014 by Doximity.

For my analyses on winners of SuperDoctor, I drop any physician who shows having won a SuperDoctor award fewer than seven years after Medical school. There are six such observations and are deemed to be data errors (either from NPI look up or from wrong medical school graduation date in Doximity data).

A.3 Distribution of awards

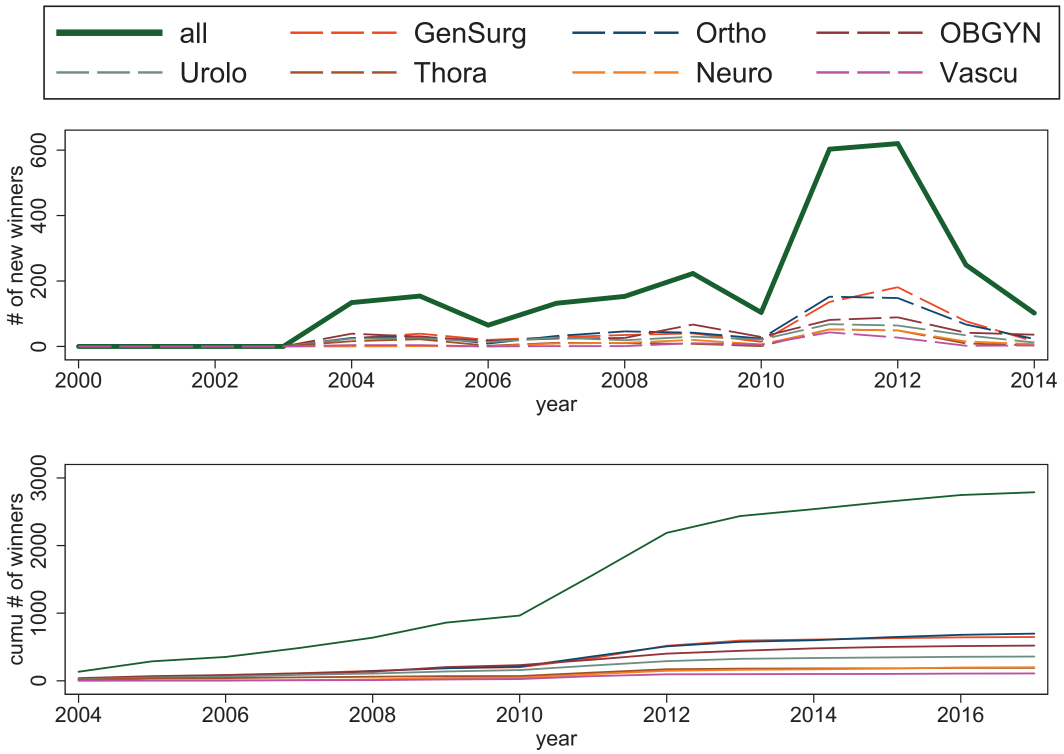
This section shows the distribution of award by specialty and over time. Figures A.1 and A.2 show the number of winners and first-time winners for the SuperDoctor award across all 7 sample specialties over time. Figure A.1 includes all SuperDoctor winners, while A.2 excludes who also won another major "top doctor" award such as Castle Connolly Top Doctors or Consumer Checkbook Top Doctors. The top panel shows the number of first-time winners, and the bottom panel shows the number of winners who have ever received the award by a certain year. With very rare exceptions, physicians who are named a "top doctor" in one year are also named in subsequent years.

Figure A.1: *Count of SuperDoctor winners by specialty*



Note: Graphs shows the number of "top doctor" winners by specialty over time. The top panel shows the number of first-time winners, and the bottom panel shows the number of ever-winners

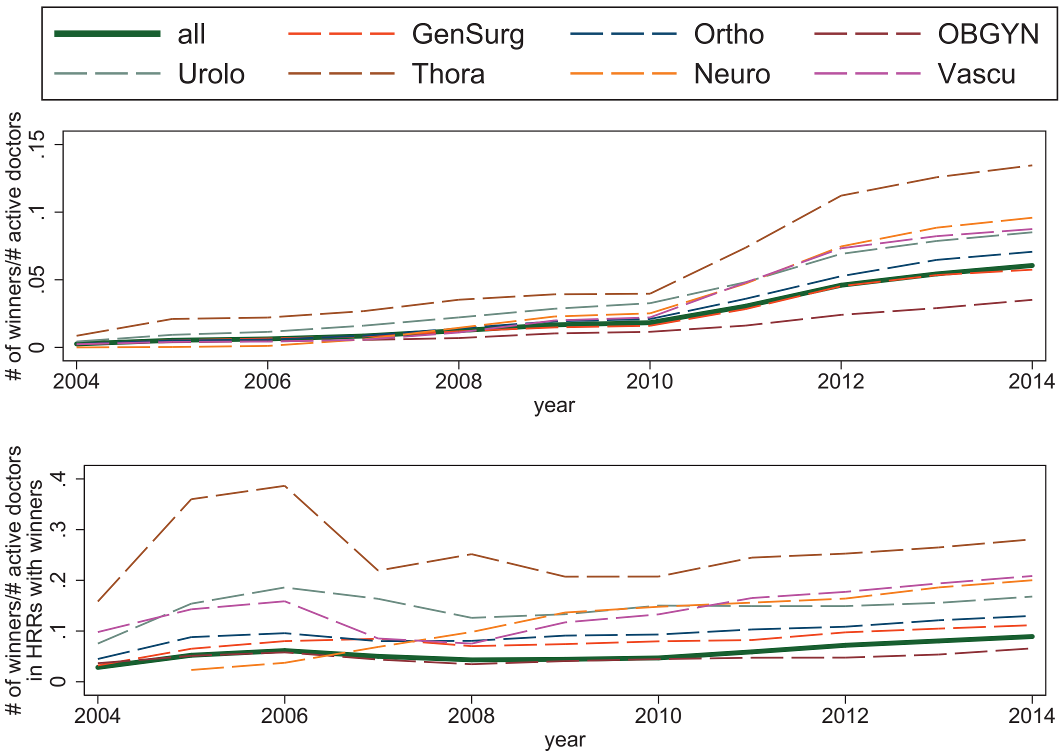
Figure A.2: *Count of SuperDoctor winners by specialty, excluding winners of other awards*



Note: Graphs shows the number of "top doctor" winners by specialty over time. The top panel shows the number of first-time winners, and the bottom panel shows the number of ever-winners

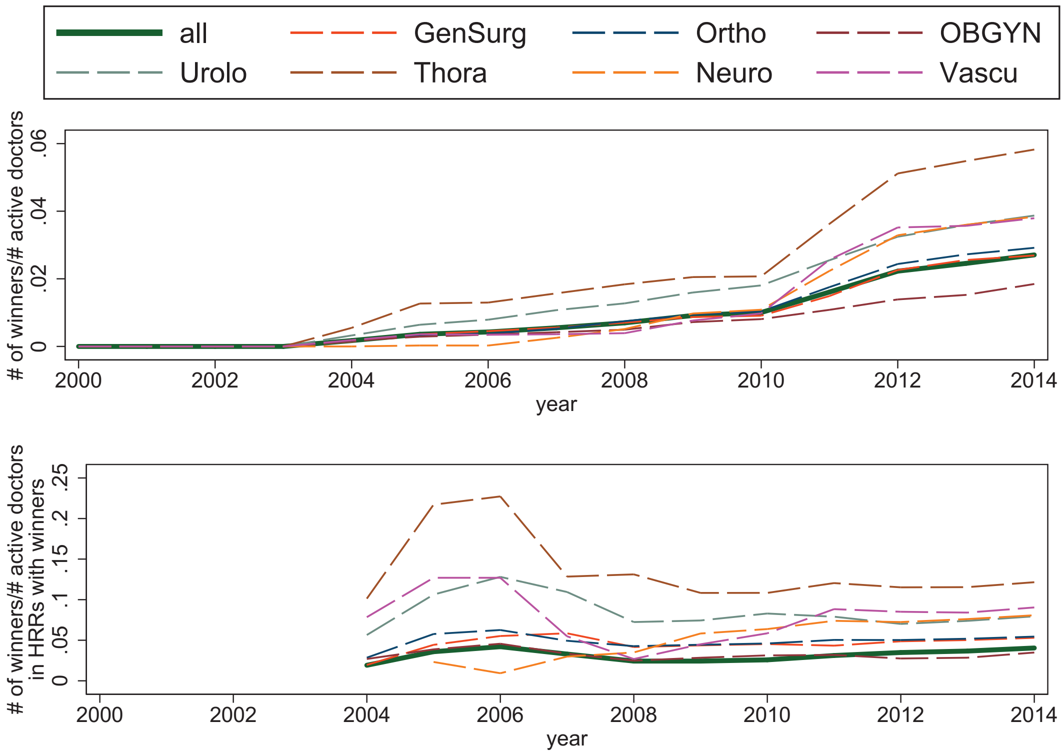
In addition to knowing the total number of winners, one may be interested in knowing the "rate of winning" in order to understand how rare these awards are. Calculating this "rate of winning" precisely is challenging because the pool of "at risk" winners is unknown to the researcher. "Top doctor" winners are often selected within geographic markets, and the location and size of these markets depend on the publishing partnership the awarding firms have. For example, the earliest Super-Doctor awards are all in Texas, named "SuperDoctors of Texas", In later years, SuperDoctors expanded to "SuperDoctors of Southern California", "SuperDoctors of Washington D.C", but I cannot identify the geographic boundary of "Southern California" and "Washington D.C." by the awarding firm's definition. Here I present two estimates of "winner rates," one is the share of winners out of all doctors who are active in Medicare FFS in that specialty in the state, one is the share of winners out of all doctors active in a zipcode that has had a winner by that year. The former is a potential underestimate, and the later is a potential overestimate, so we can see these two numbers as provider a bound for the true "winning rate".

Figure A.3: *Share of SuperDoctor winners by specialty*



Note: Graphs shows the share of active doctors who have ever won a "top doctor" award. The demoninator in the top panel includes all active doctors in the specialty in a given year. The denominator in the bottom panel includes all active doctors in the specialty praciting in HRRs that ever had a winner in that specialty on or prior to the calendar year. Data ends in 2014 because denominator data is not available afterwards.

Figure A.4: *Share of winners by specialty, exclude winners of other awards*



Note: Graphs shows the share of active doctors who have ever won a "top doctor" award. The demoninator in the top panel includes all active doctors in the specialty in a given year. The denominator in the bottom panel includes all active doctors in the specialty praciting in HRRs that ever had a winner in that specialty on or prior to the calendar year. Data ends in 2014 because denominator data is not available afterwards.

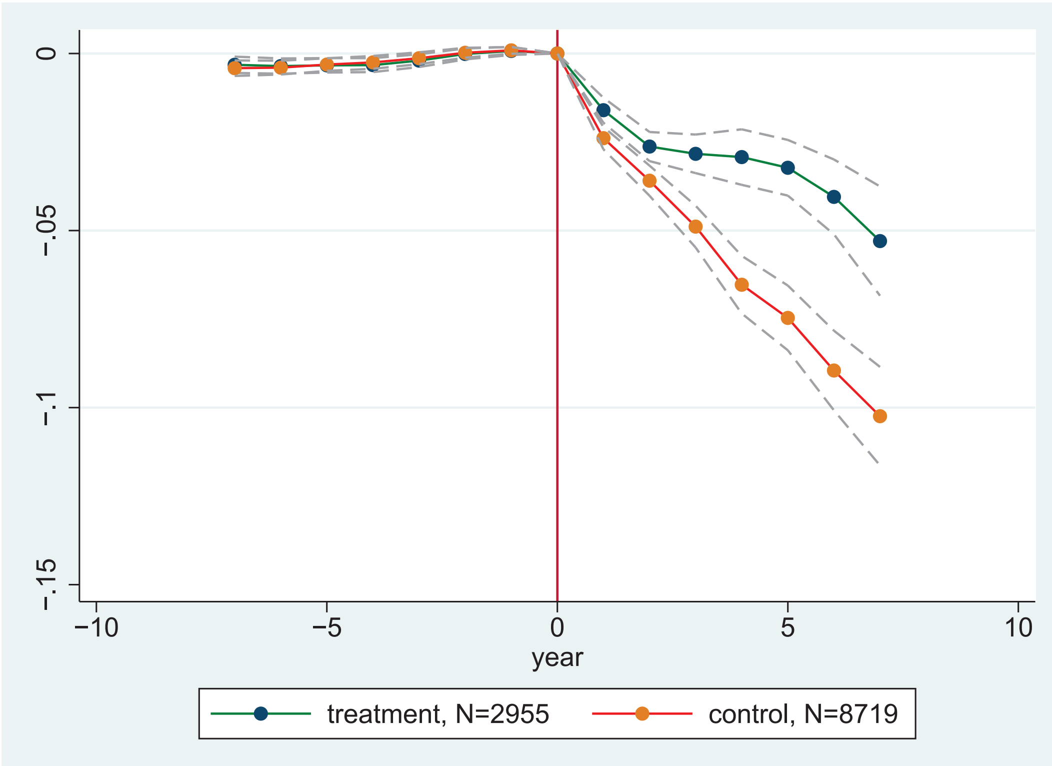
Figures A.3 and A.4 show these two calculations of "winning rates". Figure A.3 includes all SuperDoctor winners, while A.4 excludes who also won another major "top doctor" award. These figures show that the "top doctor" awards are uncommon but not exceptionally rare, and there is substantial heterogeneity by specialty. For larger specialties such as orthopedic surgery, general surgery, and Ob/gyn, the share of surgeons who have ever won SuperDoctor by 2014 may be under 10%. For smaller specialties such as cardiac surgery,15% to 30% of surgeons in 2012 have ever been a winners.

A.3.1 Retirement from Medicare FFS claim

In my main analyses, I limit the sample of SuperDoctor winners who have Medicare claims data both in the beginning (2000) and the end (2014) of my sample period. This choice derives from concerns regarding whether physicians who have won an award many years ago and have since left clinical practice remain on the list of SuperDoctor winners as of January 2017. This section plots the event-study estimates of Equation 1.2 on a broader sample of SuperDoctor winners who first won the award between 2004 and 2014, has not won another major "top doctor" award, and is active *at any point during my sample period*, with an indicator variable of whether the physician has any Medicare FFS claim. The control group is matched to winners on years of experience using the following procedure: for each winner who first won the award in year t, find three non-winners within the same specialty who are also active in year t and have the closest value of years post medical school graduation as the winner.

Figure A.5 shows that the current Super-doctor list retains some physicians who left clinical practice, but we are not confidence that they retained all (the fact that they retained some may just be a lag in data purging, or a difference between retiring and retiring from Medicare FFS. If we assume that Super doctor list retained all those who left clinical practice, this is showing that winners are less likely to retire or move out of Medicare FFS after winning.

Figure A.5: *Event study of being active in Medicare FFS, winners of Super Doctors only and experience matched control group (change to propensity score matched.*

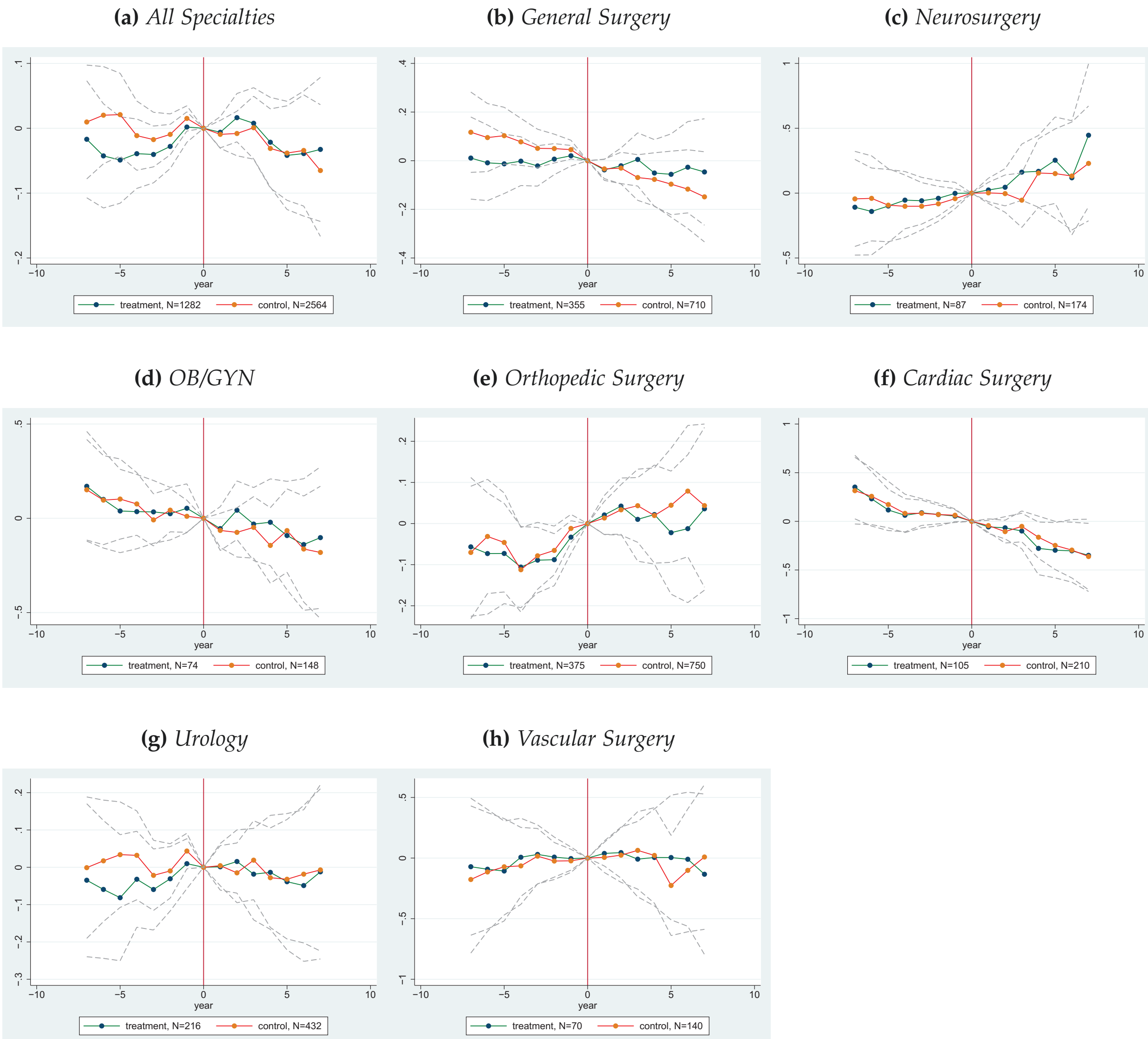


Event study of being active in Medicare FFS. The treatment sample include SuperDoctor winners who first won the award between 2004 and 2014, has not won another major "top doctor" award, and is active *at any point during my sample period*, with an indicator variable of whether the physician has any Medicare FFS claim. The control group is matched to winners on years of experience using the following procedure: for each winner who first won the award in year t, find 3 non-winners within the same specialty who are also active in year t and have the closest value of years post medical school graduation as the winner.

A.4 Heterogeneity by specialty

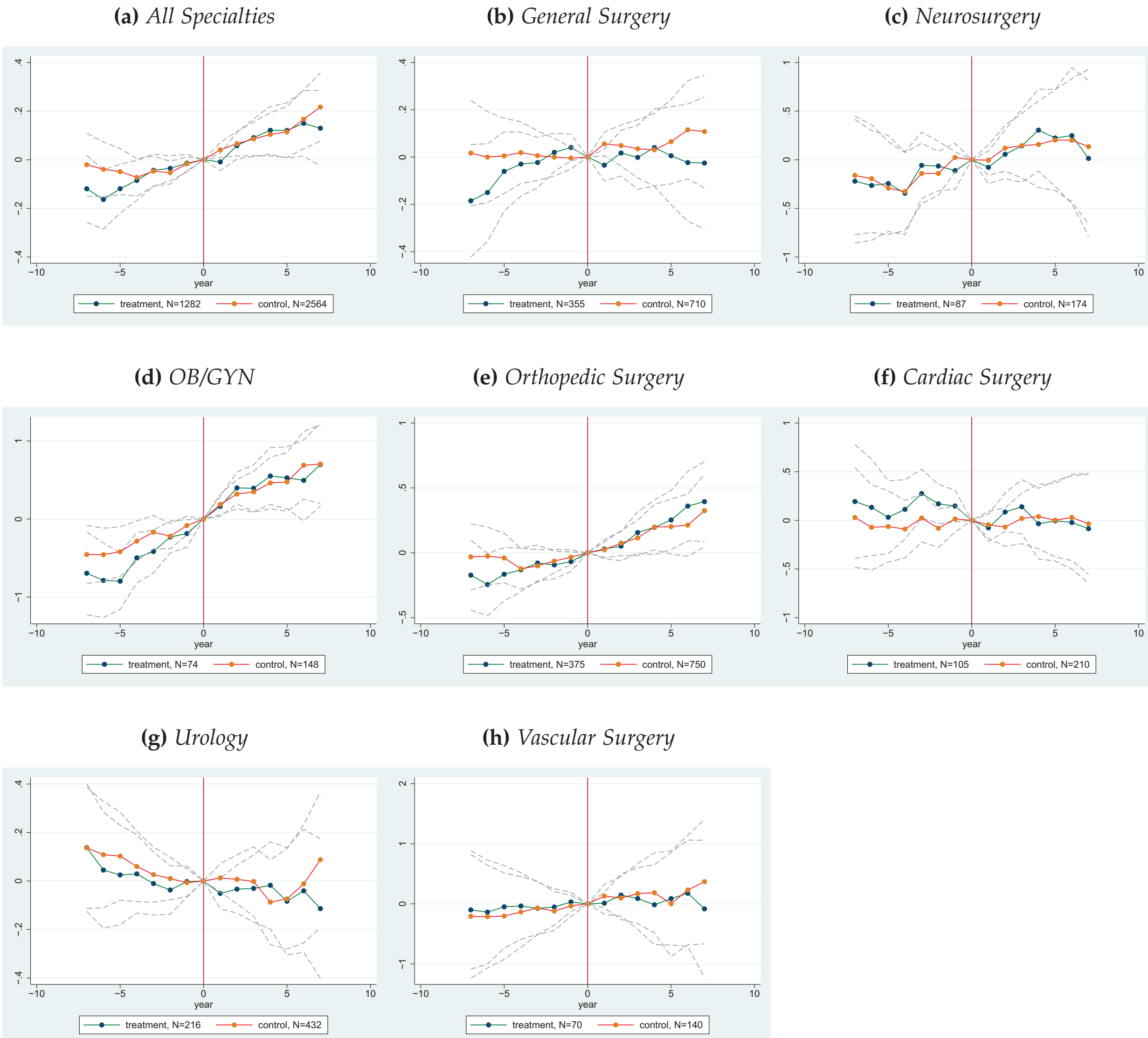
See Figures A.6 to A.7

Figure A.6: *Event study of log of inpatient procedure by specialty*



Note: Event study of log Medicare FFS inpatient volume by specialty. Treatment sample includes SuperDoctor winners who first won the award between 2004 and 2014, is active in Medicare claims data in both 2000 and 2014, and has never won another major "top doctor" award. Propensity scores are calculated using inpatient Medicare FFS volume in year $t-4$ through $t-2$, outpatient Medicare FFS volume in year $t-4$ through $t-2$, share of patients who travel more than 15 miles to see the provider, zipcode median income of the average patient, and the elixhauser comorbidity score of the average patient.

Figure A.7: *Event study of log of outpatient procedure by specialty*



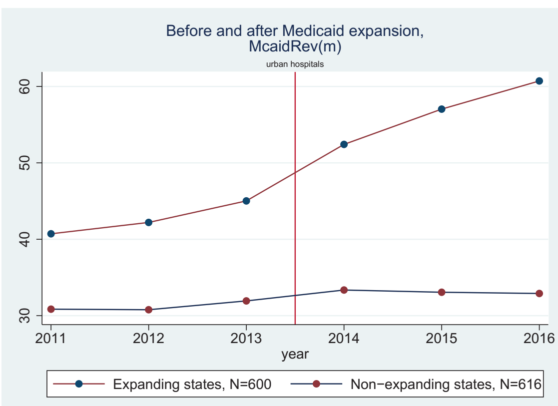
(a) *All Specialties* **(b)** *General Surgery* **(c)** *Neurosurgery* **(d)** *OB/GYN* **(e)** *Orthopedic Surgery* **(f)** *Cardiac Surgery* **(g)** *Urology* **(h)** *Vascular Surgery*

Note: Event study of log Medicare FFS outpatient volume by specialty. Treatment sample includes SuperDoctor winners who first won the award between 2004 and 2014, is active in Medicare claims data in both 2000 and 2014, and has never won another major "top doctor" award. Propensity scores are calculated using inpatient Medicare FFS volume in year $t-4$ through $t-2$, outpatient Medicare FFS volume in year $t-4$ through $t-2$, share of patients who travel more than 15 miles to see the provider, zipcode median income of the average patient, and the elixhauser comorbidity score of the average patient.

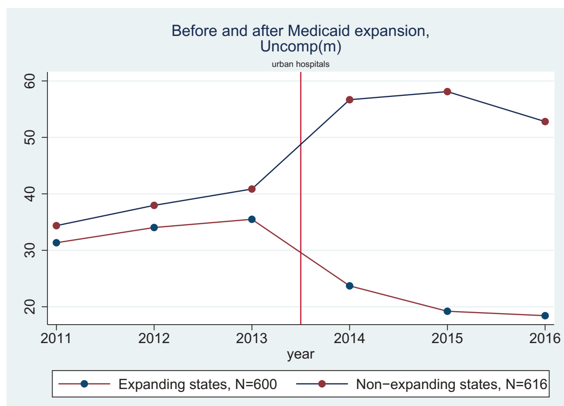
Appendix B

B.1 Change in Medicaid revenue and Cost of Uncompensated Care, Alternative Samples

Figure B.1: *Medicaid Revenue and Cost of Uncompensated Care, Urban Hospitals*

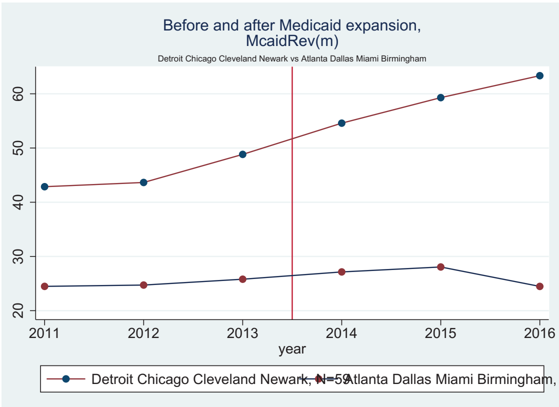


Medicaid revenue (millions)

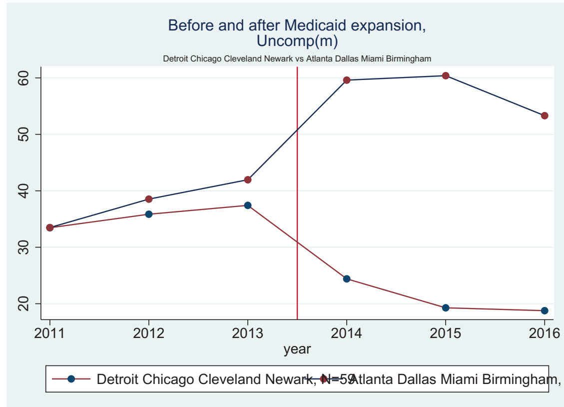


Cost of uncompensated care (millions)

Figure B.2: *Medicaid Revenue and Cost of Uncompensated Care,hospitals in Detroit, Chicago, Cleveland, Newark, Atlanta, Dallas, Miami, and Birmingham*



Medicaid revenue (millions)



Cost of uncompensated care (millions)

B.2 Graphic illustration of revenue components

Figure B.3: *Revenue details*

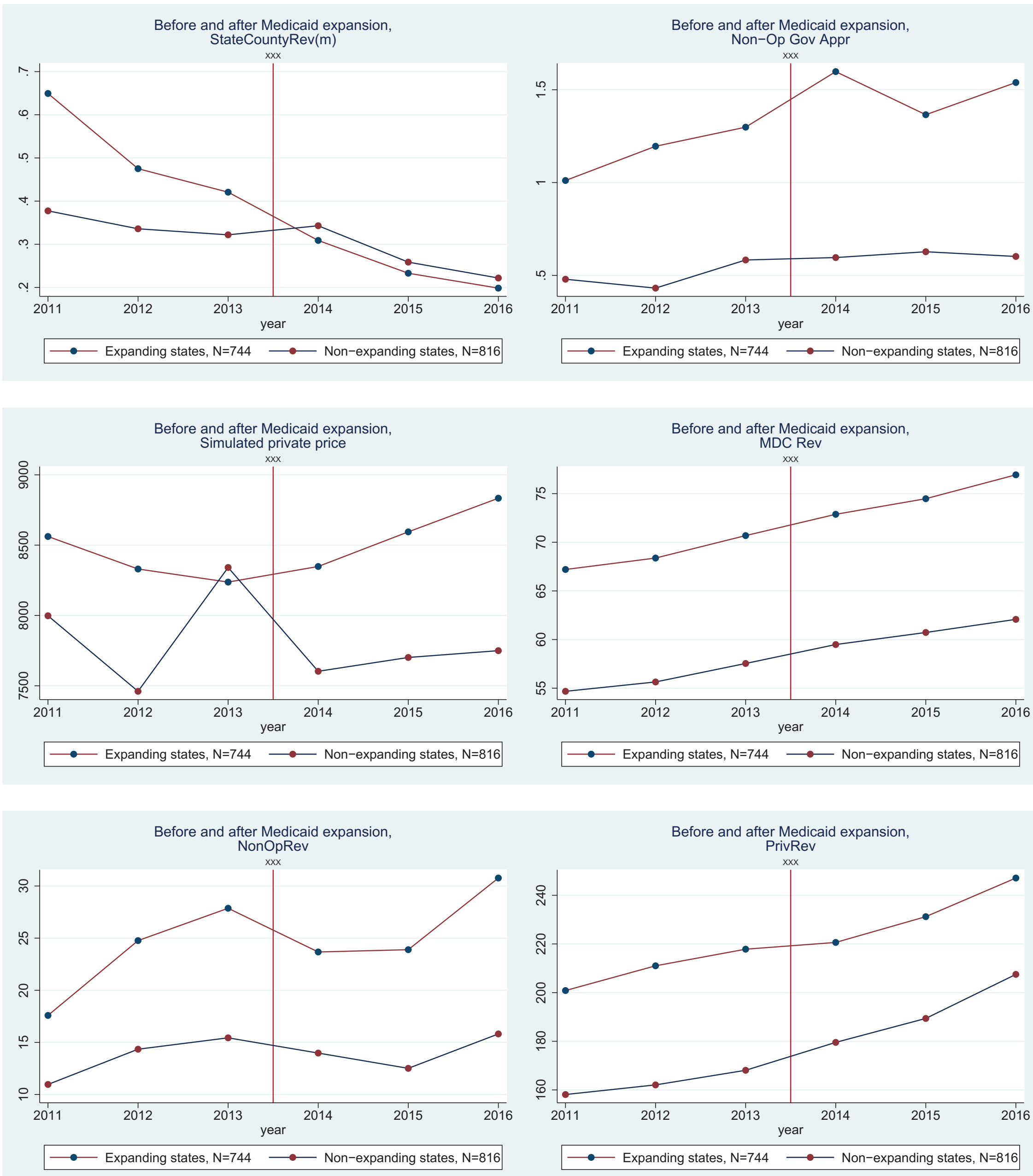


Figure B.4: *Revenue details, urban hospitals*

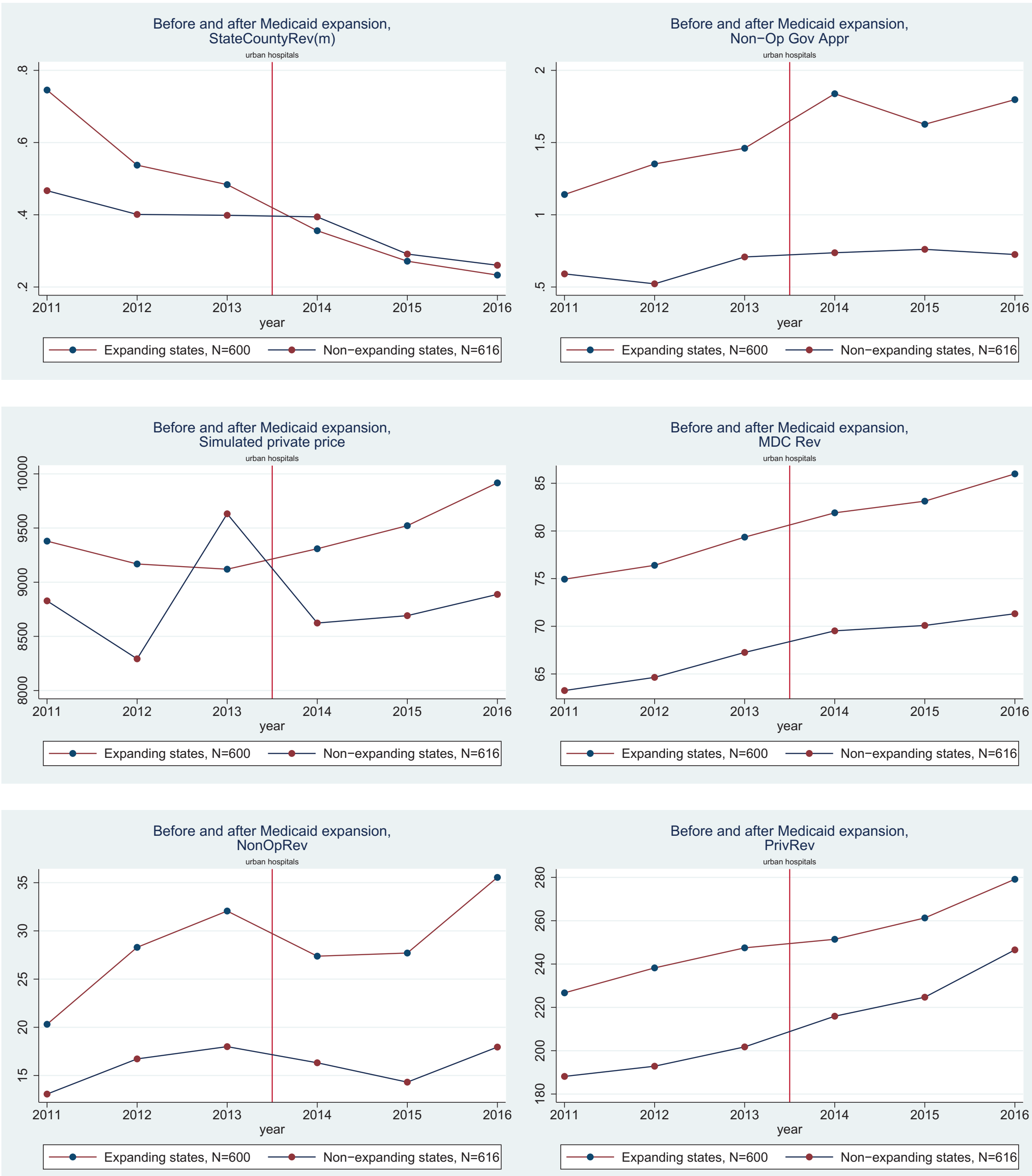
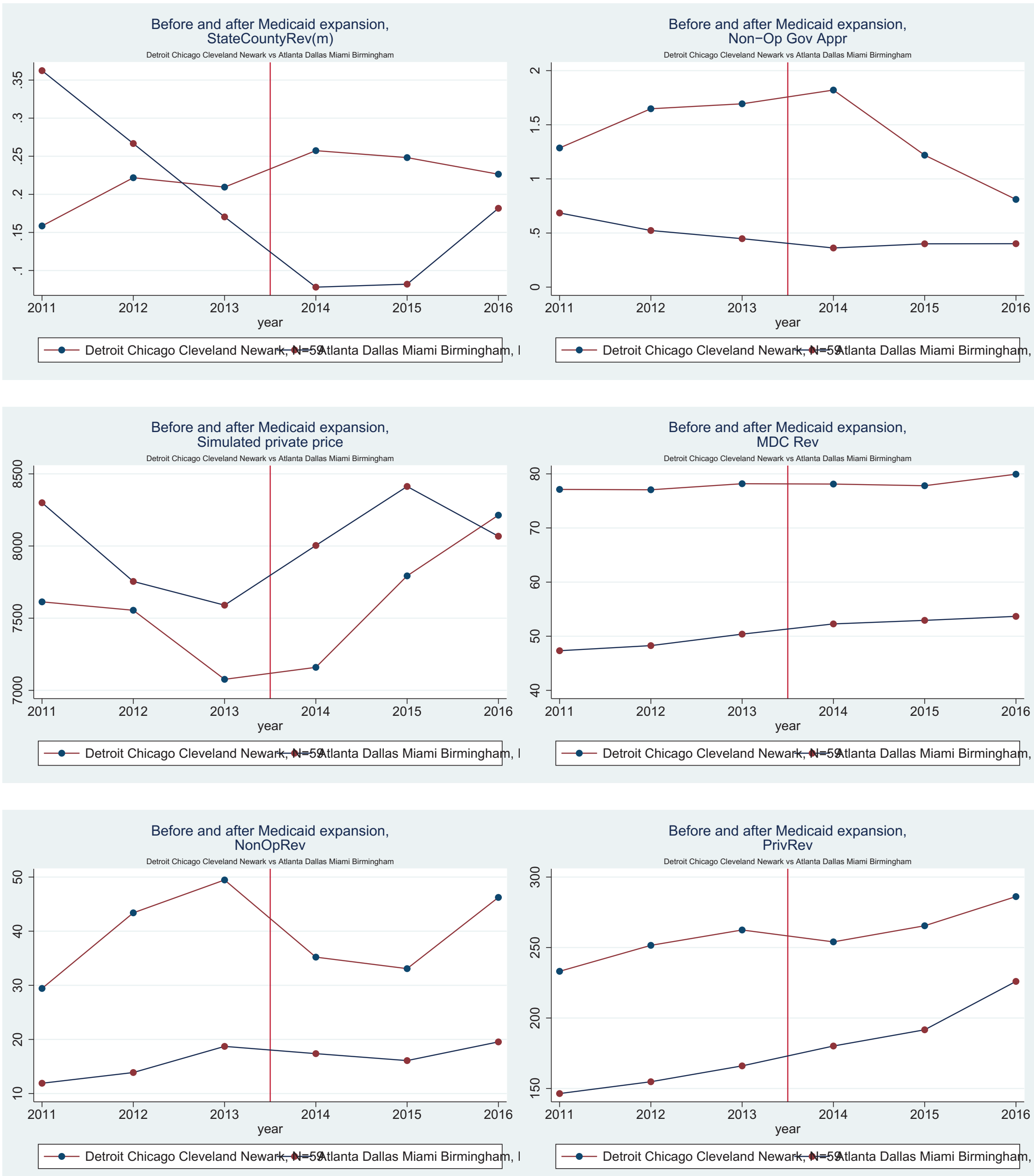


Figure B.5: *Revenue details, hospitals in Detroit, Chicago, Cleveland, Newark, Atlanta, Dallas, Miami, and Birmingham*



B.3 Definition of variables

B.3.1 Private Price

The calculation of private price is based on Lewis and Pflum (2017). In an ideal world, private prices is total private revenue divided by total private quantity with a case-mix adjuster. There are a few limitations in the Cost Report data that deviates from this calculation. First, case mix is not available from Cost Report data alone. Non-adjusted data has be calculated in previous literature. Second, outpatient quantity is difficult to qualify. For this reason, conventional calculation has focused on inpatient alone. Third, it is actually not possible to deduce private inpatient from the Cost Report. What is available is only total inpatient revenue, Medicare inpatient revenue, and Medicaid total revenue (all services and DSH payment). For this reason, the convention calculation in the literature has been:

$$\text{Discharge Price} = \frac{[\text{Gross Inpatient Revenue} \times (1 - \text{discount})] - \text{Medicare Payments}}{\text{Non-Medicare Discharges}} \tag{B.1}$$

where

$$\text{discount} = \frac{\text{Total contractual adjustments}}{\text{Gross Inpatient Revenue} + \text{Gross Outpatient Revenue}}$$

This calculation has the limitation that it is actually a linear combination of Medicaid prices and private prices, weighted by the share of uninsured discharges. Previous literature has argued that when Medicaid prices is relatively stable and share of Medicaid discharges is orthogonal to the variation of interest, this is a fairly good measure of prices. However, this is not a good assumption in this study. The following section illustrates why:

Let P_p denotes average private payer price for inpatient services, let Q_p denotes the number of privately-insured inpatient discharges. Let P_m denotes the average private medicaid price, and Q_m denotes the number of Medicaid-insured inpatient discharges. Let Q_u denotes the number of uninsured inpatient discharges. For simplicity. We assume that the price of uninsured discharges is 0. What equation B.1 calculates is essentially

$$\hat{P} = \frac{Q_m P_m + Q_p P_p}{Q_p + Q_m + Q_u} \tag{B.2}$$

when we want to know P_p or Q_pP_p/Q_p. Lewis and Pflum uses Equation B.1 as a proxy for P_p because Q_pP_p, Q_p, and Q_u cannot be isolated from the cost reports. However, since the Medicaid expansion likely increased Q_m and decreased Q_u, we may see an increase in the value of Equation B.1 even if there has been no change in P_p. If we think that the additional Medicaid revenue may lead to a decrease in P_p, and if we test the null hypothesis that $\hat{P} >= 0$ against the alternative hypothesis that $\hat{P} < 0$, we are testing the null hypothesis that the magnitude of cost-shifting through changes ins P_p is not large enough to counterbalance the positive effect on $\hat{P}$ from changes in payer mix. While this test does not inform us whether there's any cost-shifting at all, it does inform us whether cost-shifting fully explains why gains in Medicaid revenue is washed out.

Appendix C

C.1 Risk-Adjusted Estimates

C.1.1 Utilization Rates in MEPS

Table 3.1 reports unadjusted rates of ED use by insurance status in the MEPS. In Table 3.2 we show how these differences by insurance status are affected by adjusting for differences in demographics and self-reported health. Specifically, we adjusted for gender, age, marital status, race/ethnicity, education, poverty status, and self-reported health.

As seen in Appendix Table C.3, there are pronounced differences across insurance groups in these characteristics. Compared to the privately insured, the uninsured are younger, more likely to be single, more likely to be Hispanic, lower income, lower education, and in worse health. There are also differences between the uninsured and the Medicaid population, although they are less pronounced. Complementing the regression-adjusted differences reported in Table 3.2 Appendix Table C.7 reports the analysis from Table 3.1 separately by demographic group; the basic findings persist within these stratifications.

Another difference across insurance groups in the MEPS is that the uninsured have lower rates of diagnoses for various conditions than any of the insured populations. This is shown in Appendix Table C.4. Differences in diagnoses may reflect differences in underlying health and/or differences in diagnosis rates conditional on underlying health; it has been argued that lack of insurance may lead to substantial underdiagnosis of health conditions. (1) Nonetheless, for completeness Appendix Table C.6 shows that the demographically-adjusted results in Table 3.2 are robust to adding health diagnoses as risk adjustors.

C.1.2 Types of Visits in NHAMCS

Table 3.3 reports how the circumstances of ED visits varied by insurance status. In Table 3.4 we show how these differences by insurance status are affected by adjusting for differences in demographics. Specifically, we adjusted for gender, age, and race/ethnicity. These represent fewer adjustors than available in the MEPS; Appendix Table C.5 shows that using the sparser NHAMCS adjustors in the MEPS yields similar results to using the richer MEPS adjustors in the MEPS in Table 3.2.

Appendix Table C.8 shows the NHAMCS results from Table 3.3 persist when looking separately by age group.

C.2 Undercounting of ED visits in the MEPS

The advantage of the MEPS is that it is a nationally-representative dataset from which a direct calculation of utilization rates by insurance status is possible. (Many of the national databases on emergency utilization, such as the NHAMCS and NEDS, are event-based rather than population-based âĂŞ so one cannot directly calculate visit rates for a given group.) However, it has been noted that MEPS may undercount ED visits and may do so differentially by insurance type). For example, Zuvekas and Olin (2009) focuses on participants in MEPS with Medicare coverage during 2001-2003 and compared household reports of Medicare-covered services to Medicare claims. They found that households accurately reported inpatient stays and number of nights, but underreport ED visits by one-third and office visits by 19 percent. They also found that the individuals who were dual eligible on Medicaid are more likely to underreport ED visits, and those who also have private insurance is less likely to underreport. While Zuveskas and Olin (2009) does not address the uninsured in this validation, their results do suggest that the undercount of ED visits in MEPS may differ by insurance status.

To investigate this issue, we followed Tang et al. (2010) in constructing an alternative measure of ED utilization by combining ED visit counts by insurance status in the NHAMCS, an event-based dataset, and population counts by insurance status from the Census. (4) We follow their approach, using the 2013 NHAMCS and the 2013 Census data on population counts by insurance status (from Smith and Medalia 2014) for the denominator. (5) This is a somewhat crude exercise, since insurance status in the NHAMCS is measured based on recorded payment for the visit and insurance status in the Census is defined based on self-report at the time of interview.

Appendix Table C.1 shows the results. This confirms the prior literature that ED visits are undercounted in the MEPS. Moreover, the results suggest that ED visits are disproportionately undercounted in the MEPS for the uninsured: Our estimate of the number of ED visit per person from the NHAMCS/Census is twice the estimate from MEPS for the uninsured, about 1/3 higher for the privately insured, and about Â¡ higher for the publicly insured.

As a result, the adjusted estimates suggest that the uninsured may use the ED somewhat more than the insured (0.49 visits per year compared to 0.35 visits per year). This makes even more pronounced the fact that the uninsured use the ED for a much larger share of their care than the insured. Moreover, the fact that the uninsured use the ED much less than the publicly insured continues to hold: even in the NHAMCS/Census approach, ED use for the uninsured is only about half that of the publicly insured.

We also employed an additional data source to more directly measure rates of ED use and how they differ by insurance status. Specifically, we drew on data used by Taubman et al. (2014) for analyzing the Oregon Health Insurance Experiment. (6) Those data contain a well-specified denominator (approximately 19,000 low income adults (ages 19-64) in the Portland area who signed up for the Oregon Medicaid lottery), matched to hospitalsâĂŹ administrative records of ED visits between March 2008 and September 2009 for 12 Portland-area EDs; more detail on the data construction is presented in Taubman et al. (2014). While the data were previously used to analyze the causal effects of Medicaid on ED use, here we use them for a different, descriptive purpose. We use the data to estimate the number of ED visits for our population for those with Medicaid and those without Medicaid. The advantage of these data is that (unlike the NHAMCS data) we have a well-measured population denominator (individuals in the Portland area who signed up for the Oregon lottery) and (unlike the MEPS data) the ED visits are measured in

Table C.1: *Estimating Under-Counting of ED Visits in MEPS, by Insurance Status*

	Uninsured Adults	Insured Adults	Privately Insured Adults	Publicly Insured Adults
Average Visits per person (MEPS 2013)	0.177	0.202	0.149	0.521
Number of Visits, 000s (NHAMCS 2013)	17,191	54,443	26,040	24,698
Population Counts, 000s (Census 2013)	35,353	155,364	129,813	25,551
Implied Average Visits per person (NHAMCS visits / Census counts)	0.486	0.350	0.201	0.967
Ratio of estimated average Visits per person in NHAMCS / MEPS	2.741	1.733	1.348	1.854

SOURCE Author's analysis of data from the Medical Expenditure Panel Survey, National Hospital Ambulatory Medical Care Survey, and the U.S. Census Bureau Current Population Survey Annual Social and Economic Supplement. NOTES First row reproduces the MEPS estimates of average number of visits per person from Exhibit 1. Second row reproduces NHAMCS estimates of total number of visits from Exhibit 3. Third row shows population counts from Census. Fourth row shows implied average number of visits per person from combining counts in NHAMCS and Census.

administrative data therefore not subject to biases from self-reports such as under-reporting. We therefore view them as a useful complement for corroborating our national analysis in MEPS and NHAMCS of rates of ED use for those with and without Medicaid.

The results are shown in Appendix Table C.2. For interest, we report results for the full sample and separately for the control sample that lost the lottery. "Any Medicaid" is defined, based on state Medicaid records, as the individual having been on Medicaid at any point during the study period (March 2009 through September 2009). "No Medicaid" indicates that the individual was not on Medicaid at any point during the study period. For individuals not on Medicaid, about three-quarters were uninsured. Across all measures, we see markedly higher use of the ED for those on Medicaid than those not on Medicaid.

Table C.2: *Rates of Emergency Room Use in Portland among People in Oregon who Signed up for Medicaid Lottery*

	Any Medicaid	No Medicaid	Any Medicaid Control	No Medicaid Control
Panel A: Percent with Any Visits				
All visits	47.0	30.3	51.2	31.5
Inpatient	10.7	6.0	14.0	6.4
Outpatient	44.4	28.1	47.6	29.3
On-hours	36.3	21.9	40.7	23.0
Off-hours	31.1	19.0	33.1	19.9
Panel B: Number of Visits				
All visits	1.576	0.835	1.762	0.890
	(0.043)	(0.017)	(0.071)	(0.022)
Inpatient	0.193	0.092	0.277	0.099
	(0.010)	(0.004)	(0.020)	(0.004)
Outpatient	1.386	0.742	1.486	0.792
	(0.039)	(0.016)	(0.062)	(0.020)
On-hours	0.892	0.465	0.994	0.499
	(0.026)	(0.010)	(0.041)	(0.013)
Off-hours	0.708	0.381	0.781	0.399
	(0.023)	(0.009)	(0.039)	(0.011)
Nonemergent	0.328	0.163	0.365	0.172
	(0.012)	(0.005)	(0.020)	(0.006)
Emergent				
ED care not needed (primary care treatable)	0.533	0.288	0.556	0.305
	(0.016)	(0.006)	(0.024)	(0.008)
ED care needed, preventable	0.116	0.061	0.135	0.064
	(0.006)	(0.002)	(0.010)	(0.003)
ED care needed, not preventable	0.311	0.178	0.342	0.190
	(0.011)	(0.005)	(0.017)	(0.006)
Unclassified	0.320	0.151	0.381	0.164
	(0.013)	(0.004)	(0.023)	(0.006)
N	6,042	18,604	2,273	12,747

SOURCE Authors' analysis of data from emergency room visits to 12 Portland Area Emergency Rooms during the study period (March 10 2008 - September 30 2009) among individuals living in the Portland area who signed up for the 2008 Oregon Medicaid Lottery. NOTES Taubman et al. (2014) provides more detail on the data and sample. "Medicaid" is defined as any Medicaid coverage during the study period. "Control" refers to the subset of the sample who were not selected in the lottery. Standard deviations (for continuous outcomes) are shown in parentheses.

Table C.3: *Demographic Characteristics by Insurance Status (MEPS 2013)*

	Uninsured Adults	Insured Adults	Privately Insured Adults	Publicly Insured Adults	Adults on on Medicaid
Female (%)	43.6	52.6	51.2	60.9	62.8
Age as of 12/31	38.8 (14.4)	41.8 (12.4)	42.1 (11.5)	40.4 (16.5)	38.6 (16.2)
Married (%)	38.7	56.6	61.1	29.5	28.7
High school degree or more (%)	70.5	87.8	91.7	64.6	62.9
Race					
Non-Hispanic Black (%)	13.7	11.8	10.0	22.7	23.1
Non-Hispanic White (%)	44.2	67.0	70.5	45.8	43.5
Hispanic (%)	34.9	12.6	10.8	23.0	24.3
Non-Hispanic other or multi-race (%)	7.3	8.6	8.7	8.5	9.2
Poverty status					
<100% FPL (%)	26.0	10.4	3.8	49.5	52.9
100-125% FPL (%)	7.7	3.2	1.9	11.1	11.2
125-200% FPL (%)	23.9	10.4	9.1	18.5	17.9
200-400% FPL (%)	28.6	30.3	32.6	16.6	14.9
>400% FPL (%)	13.8	45.7	52.6	4.4	3.1
Self-rated health status					
Excellent/Very Good (%)	35.0	46.2	50.4	20.8	22.6
Good (%)	40.4	35.4	36.1	31.6	32.3
Fair/Poor (%)	24.6	18.4	13.5	47.6	45.1
N (unweighted)	5,853	15,930	12,115	3,815	3,410

SOURCE Authors' analysis of data from the Medical Expenditure Panel Survey. NOTES Table reports characteristics of adults (ages 19-64) in the 2013 MEPS, by insurance status. All results are weighted using final person weights which are designed to be nationally representative of the civilian, non-institutionalized US population. Standard deviations (for continuous values) are shown in parentheses.

Table C.4: *Diagnoses by Insurance Status (MEPS 2013)*

	Uninsured Adults	Insured Adults	Privately Insured Adults	Publicly Insured Adults	Adults on on Medicaid
High blood pressure (%)	20.6	27.4	25.6	38.4	36.0
Coronary heart disease (%)	1.4	2.7	2.1	6.8	6.1
Heart attack (%)	1.3	2.1	1.5	5.5	4.9
Stroke (%)	1.0	2.1	1.4	6.1	5.1
High cholesterol (%)	15.2	26.3	25.6	30.7	27.5
Cancer (%)	3.4	6.9	6.6	8.6	7.9
Diabetes (%)	4.8	7.0	5.9	13.1	11.5
N (individuals)	5,853	15,930	12,115	3,815	3,410

SOURCE Authors' analysis of data from the Medical Expenditure Panel Survey. NOTES Table shows percent of adults who report every being diagnoses with a condition, by insurance status, for adults (ages 19-64) in the 2013 MEPS. All results are weighted using final person weights which are designed to be nationally representative of the civilian, non-institutionalized US population.

Table C.5: *Utilization by Insurance Status (MEPS 2013), Adjusted for Gender, Age and Race/Ethnicity*

	Uninsured Adults	Insured Adults	Privately Insured Adults	Publicly Insured Adults	Adults on Medicaid
ED visits					
any (%)	12.6	13.6	10.9	28.4	28.7
number	0.185	0.201	0.145 (0.006)	0.514	0.515
	(0.012)	(0.007)	(0.006)	(0.029)	(0.031)
Outpatient visits					
any (%)	46.4	75.5	75.3	76.9	76.7
number	2.896	6.039	5.463	9.315	9.153
	(0.207)	(0.118)	(0.119)	(0.373)	(0.418)
Hospital admissions					
any (%)	3.3	7.6	6.1	15.7	16.6
number	0.041	0.094	0.073	0.216	0.228
	(0.005)	(0.004)	(0.004)	(0.016)	(0.018)
N (individuals)	5,853	15,930	12,115	3,815	3,410

SOURCE Authors' analysis of data from the Medical Expenditure Panel Survey. NOTES Table repeats the analysis in Table 3.2 but with only a subset of the covariates used there, specifically: gender, age, and race/ethnicity categories. See note to Table 3.2 for more details.

Table C.6: *Utilization by Insurance Status (MEPS 2013), Adjusted for Demographic, Self-Reported Health, and Health Diagnoses*

	Uninsured Adults	Insured Adults	Privately Insured Adults	Publicly Insured Adults	Adults on Medicaid
ED visits					
any (%)	10.2	14.1	13.0	21.1	21.6
number	0.137	0.212	0.186	0.365	0.367
	(0.012)	(0.006)	(0.007)	(0.028)	(0.028)
Outpatient visits					
any (%)	49.0	74.9	75.7	79.6	79.9
number	2.924	6.033	5.775	8.362	8.302
	(0.225)	(0.116)	(0.138)	(0.312)	(0.357)
Hospital admissions					
any (%)	2.5	7.8	7.2	12.4	13.6
number	0.029	0.097	0.088	0.165	0.183
	(0.005)	(0.004)	(0.004)	(0.016)	(0.017)
N (individuals)	5,853	15,930	12,115	3,815	3,410

SOURCE Authors' analysis of data from the Medical Expenditure Panel Survey. NOTES Table repeats the analysis in Exhibit 2 with all the covariates used there and the health diagnoses shown in Table C.4. See note to Table 3.2 for more details.

Table C.7: *Utilization of ED by Insurance Status and Demographics (MEPS 2013)*

	Any (%)					Number (SD)				
	Uninsured Adults	Insured Adults	Privately Insured Adults	Publicly Insured Adults	Adults on Medicaid	Uninsured Adults	Insured Adults	Privately Insured Adults	Publicly Insured Adults	Adults on Medicaid
ED visits	12.2	13.7	11.1	28.9	29.3	0.177 (0.649)	0.202 (0.604)	0.149 (0.443)	0.521 (1.352)	0.523 (1.355)
By age										
19-44	12.1	14.0	11.5	27.6	28.1	0.176 (0.656)	0.206 (0.622)	0.152 (0.462)	0.502 (1.344)	0.508 (1.352)
45-64	12.3	13.3	10.7	30.7	31.3	0.180 (0.630)	0.198 (0.575)	0.145 (0.417)	0.549 (1.353)	0.550 (1.353)
By sex										
male	10.1	11.5	9.8	24.2	24.9	0.139 (0.526)	0.163 (0.503)	0.128 (0.388)	0.425 (1.152)	0.417 (1.083)
female	14.9	15.6	12.3	31.9	31.8	0.227 (0.786)	0.238 (0.683)	0.169 (0.488)	0.584 (1.472)	0.586 (1.505)
By marital status										
married	8.9	11.1	10.1	23.5	22.7	0.123 (0.548)	0.156 (0.478)	0.132 (0.406)	0.445 (1.198)	0.407 (1.072)
not married	14.3	17.0	12.7	31.2	31.9	0.212 (0.697)	0.263 (0.748)	0.175 (0.496)	0.554 (1.411)	0.570 (1.452)
By education										
12 years or more	12.8	13.1	11.2	29.2	30.0	0.186 (0.615)	0.190 (0.556)	0.149 (0.432)	0.540 (1.333)	0.543 (1.324)
<12 years	11.3	17.6	9.9	28.4	28.1	0.163 (0.719)	0.289 (0.941)	0.145 (0.569)	0.492 (1.363)	0.493 (1.396)
By race/ethnicity										
Non-Hispanic Black (%)	14.1	13.3	11.3	32.1	33.0	0.206 (0.481)	0.197 (0.463)	0.151 (0.367)	0.617 (1.032)	0.626 (1.016)
Non-Hispanic White (%)	18.5	19.0	14.0	32.1	32.7	0.273 (0.884)	0.295 (1.057)	0.197 (0.728)	0.556 (1.733)	0.573 (1.799)
Hispanic (%)	8.3	13.7	10.5	22.8	22.6	0.114 (0.643)	0.203 (0.801)	0.140 (0.563)	0.377 (1.342)	0.366 (1.262)
Non-Hispanic other or multi-race (%)	7.6	9.0	7.3	19.5	20.3	0.129 (0.605)	0.117 (0.502)	0.086 (0.349)	0.307 (1.084)	0.323 (1.117)
By self-rated health status										
Excellent/Very Good	7.2	7.1	6.4	17.3	17.6	0.098 (0.429)	0.088 (0.310)	0.076 (0.261)	0.259 (0.881)	0.268 (0.909)
Good	10.4	14.2	12.7	24.5	24.7	0.136 (0.523)	0.187 (0.502)	0.160 (0.428)	0.372 (0.957)	0.372 (0.967)
Fair/Poor	22.2	29.1	24.5	36.9	38.4	0.359 (0.998)	0.520 (1.167)	0.393 (0.857)	0.735 (1.640)	0.759 (1.661)
By poverty status										
<100% FPL	16.7	27.7	16.6	32.7	33.3	0.283 (0.973)	0.490 (1.276)	0.284 (0.857)	0.585 (1.449)	0.605 (1.493)
100-125% FPL	12.1	21.6	16.4	27.1	26.7	0.179 (0.717)	0.396 (1.057)	0.281 (0.823)	0.517 (1.280)	0.515 (1.315)
125-200% FPL	11.9	18.6	16.2	25.9	24.5	0.153 (0.547)	0.292 (0.899)	0.222 (0.622)	0.498 (1.543)	0.412 (1.268)
200-400% FPL	10.7	13.3	12.4	24.4	24.5	0.142 (0.496)	0.189 (0.555)	0.168 (0.504)	0.428 (1.034)	0.420 (1.080)
>400% FPL	7.2	9.0	8.9	20.0	20.0	0.093 (0.342)	0.112 (0.316)	0.110 (0.311)	0.266 (0.575)	0.283 (0.639)

SOURCE Authors' analysis of data from the Medical Expenditure Panel Survey. NOTES Table reports results for adults (ages 19-64) in the 2013 MEPS, by insurance status and demographics. The first row replicates the results from Table 3.1; subsequent rows show results separately by demographics. All results are weighted using final person weights which are designed to be nationally representative of the civilian, non-institutionalized US population. Standard deviations (for continuous outcomes) are shown in parentheses.

Table C.8: *Type of ED Visit (NHAMCS 2013), Stratified by Age Group*

	Ages 19-44					Ages 45-64				
	Uninsured Adults	Insured Adults	Privately Insured Adults	Publicly Insured Adults	Adults on Medicaid	Uninsured Adults	Insured Adults	Privately Insured Adults	Publicly Insured Adults	Adults on Medicaid
Weighted visits (000s)	12,045	31,283	14,409	14,412	11,488	5,146	23,160	11,630	10,285	4,994
N (visits)	2,088	6,053	2,706	2,793	2,285	885	4,359	2,042	2,022	1,068
By triage status										
Immediate/ Emergent (%)	5.0	6.0	5.6	6.3	6.5	8.9	10.0	10.7	9.1	6.1
Urgent (%)	31.7	35.0	35.0	34.1	33.2	31.7	37.5	37.1	37.2	35.1
Semiurgent/Nonurgent (%)	39.1	34.0	32.1	35.3	35.7	32.5	24.2	22.1	26.3	32.1
No triage/Unknown (%)	24.2	25.0	27.3	24.4	24.6	26.9	28.2	30.1	27.4	26.7
By eventual diagnosis										
Nonemergent (%)	24.3	24.6	23.8	25.8	26.4	19.9	19.6	17.9	21.3	21.5
Emergent										
ED care not needed (primary care treatable) (%)	34.2	33.9	35.0	32.9	33.2	30.8	31.3	32.6	30.4	31.5
ED care needed, preventable (%)	5.1	5.1	4.4	5.9	5.9	5.3	6.5	5.4	7.6	7.9
ED care needed, not preventable (%)	19.5	21.5	23.7	19.0	18.4	25.6	25.6	27.9	23.2	21.8
Unclassified (%)	16.9	14.8	13.0	16.4	16.1	18.3	17.0	16.1	17.5	17.2
By condition										
Chronic (%)	13.3	13.4	11.6	15.3	14.6	19.0	18.6	16.0	21.3	23.2
Ambulatory-care sensitive condition (%)	4.1	5.2	3.9	6.7	6.5	4.6	6.4	5.4	8.1	8.4

SOURCE Authors' analysis of data from the National Hospital Ambulatory Medical Care Survey. NOTES Table reports results for ED visits by adults aged 19-64 from the 2013 NHAMCS, by insurance status and by age. We omit visits for childbirth.