ESSAYS IN HEALTH ECONOMICS

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This dissertation consists of three essays, each examining a topic in health economics. These papers are connected by the theme of exploring the ways in which institutions, such as hospitals and pharmaceutical companies, interact with federal and state policies and what that ultimately means for healthcare consumers. In Chapter 1, I estimate the effect of prescription drug coupons on generic drug use, medication adherence, and competition between branded drugs. I take advantage of a law in Massachusetts banning, and then allowing, prescription drug coupons to estimate a series of difference-in-differences and triple-difference regressions. I find that prescription drug coupons decrease own-molecule generic drug use, and that this effect is driven by patients requesting the brand name drug. I find no effect of coupons on medication adherence, competition between branded drugs, or cross-molecule generic substitution. These results are consistent with prescription drug coupons increasing costs without improving health. In Chapter 2, I estimate the effect of for-profit hospital ownership on the probability of admission through the emergency department (ED). I use variation from hospital conversions to estimate difference-in-differences and event study regressions. I find that for-profit hospital ownership increases the probability of inpatient admission, with the effect driven by Medicare and Medicaid patients. However, I also find evidence that increased admission rates occur when hospitals convert *from* for-profit as well as *to* for-profit, indicating that the estimated effect may actually be measuring the effect

of hospital system membership and not ownership. In Chapter 3, I estimate the effect of policies that decrease the time cost to patients of accessing long-acting reversible contraceptives (LARCs) on LARC uptake, birth rates, and birth outcomes. I take advantage of Medicaid policies which were implemented by states at different times to estimate difference-in-differences and event study regressions. I find no evidence that Medicaid coverage for immediate postpartum LARCs affected LARC use or birth outcomes, though our estimates are imprecise.

BIOGRAPHICAL SKETCH

Marisa Briana Carlos was born and raised in Tucson, Arizona. She graduated with a Bachelor of Science in Business Administration degree in Business Economics from The University of Arizona.

This dissertation is dedicated to my Nana and Tata.

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CHAPTER 1

THE EFFECTS OF PRESCRIPTION DRUG COUPONS ON GENERIC DRUG USE, ADHERENCE, AND COMPETITION: EVIDENCE FROM THREE DRUG CLASSES

ESSAYS IN HEALTH ECONOMICS

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Prescription drug coupons—offers from pharmaceutical companies to pay a portion of a patient's out-of-pocket prescription cost—are the subject of a current and growing debate. Insurance companies and governments are concerned that coupons increase costs without improving health by shifting patients away from generic drugs and towards costly, brand-name drugs. Pharmaceutical companies allege that coupons improve medication adherence, thus improving heath and lowering overall healthcare spending. While the debate has continued, coupon use has increased to 18% of prescription claims in 2017 [3]. I use insurance claims from 2007 to 2016 from a large, national insurer to estimate the effect of coupons on generic drug use, medication adherence, and brand-to-brand competition for drugs in three drug classes: statins, antipsychotics, and acne treatments. I take advantage of a law in Massachusetts that barred residents from using coupons, and was amended in 2012 to allow coupons *only* for drugs without a generic equivalent, to estimate difference-in-differences and triple-difference models. I find that coupons decrease generic drug use by shifting patients towards brand-name drugs and away from generic equivalents. I estimate a 6.1 percentage point (10%) decrease in generic drug use and a 9.3 percentage point (35%) increase in the use of "dispense as written" orders. I find no evidence that coupons shift patients away from older, generic drugs and towards newer, brand-name drugs. Additionally, I find no evidence that coupons affect medication adherence or brand-to-brand competition. A back of the envelope calculation suggests that coupons increase insurer spending by $25 million in my sample alone. These results are consistent with prescription drug coupons increasing costs without improving health.

1.1 Introduction

One of the biggest challenges health insurers face in controlling spending is effectively designing insurance plans that minimize moral hazard while allowing access to necessary care. Insurers and pharmacy benefits managers have addressed this by designing tiered formularies where patients are subjected to higher copays for expensive, branded drugs, and lower copays for generic drugs. The tiered formulary has helped facilitate the sharp decrease in spending that occurs upon generic entry, at which point the branded firm loses their monopoly and generic competitors drive down the price of the drug. Recent estimates find that branded pharmaceutical firms lose close to 88% of the market within 12 months [58]. Pharmaceutical companies have employed multiple strategies to retain market share upon generic entry, including pay-for-delay strategies and me-too drugs, among others [112]. A relatively recent strategy has been for pharmaceutical companies to offer copay coupons, which cover all or a portion of a patient's copay. In many cases, copay coupons make the copay for a branded drug less than the copay for the generic equivalent. Coupons are increasingly popular among patients and were used in 18% of all prescription claims in 2017 [3]. Offering copay coupons can be very profitable for the pharmaceutical company: for example, in exchange for paying a portion of the (relatively) small copay, they receive the negotiated rate from the insurance company. Of course, this strategy is only profitable if, absent the coupon, the patient would have filled a competitor's drug or a generic alternative. For the insurer, this strategy can be costly, especially for patients who would have had similar clinical outcomes with a generic drug. In addition to the ability of coupons to affect demand, coupons can also increase costs by diminishing the ability of

insurers to use tier placement as a means for securing discounts and rebates during price negotiation [11].

Not surprisingly, copay coupons are controversial, with different parties holding vastly different views of the practice. Pharmaceutical companies claim that copay coupons lower overall spending because they increase medication adherence. Insurers argue that copay coupons shift patients away from generic alternatives and needlessly drive up costs; federal and state governments hold similar views. Coupons are a violation of the federal anti-kickback statute, thus beneficiaries of programs funded in any part by the federal government, including Medicare and Medicaid, cannot use coupons [98]. Similarly, the Massachusetts False Health Care Claims Act prohibits Massachusetts residents from using copay coupons, regardless of insurance type [87]. While the Massachusetts and federal laws were not written specifically with copay coupons in mind, other states have been actively considering passing laws that ban copay coupons.[1].

In light of the surrounding debate and the policy relevance to states considering coupon bans, I estimate the effect of copay coupons on generic drug use, medication adherence, and brand-to-brand competition. Using insurance claims data from a large, national insurer from 2007-2016 and information on coupon availability collected from the Internet Archive, I provide the first estimates of the effect of copay coupons on therapeutic substitution (substitution between different drugs in the same class), medication adherence, and brand-to-brand competition. Additionally, I estimate the effect of coupons on generic efficiency—the ratio of generic to total fills of a given drug—and provide sup-

[1]On October 9, 2017, Assembly Bill No. 265 was signed into California law, prohibiting coupons for drugs with generic equivalents. As of February 2018, New Jersey was considering similar legislation [108]

porting estimates to those obtained by Dafny, Ody, and Schmitt 2017. The estimates provided in this paper use a larger sample of patients than the existing literature, allowing me to compare patients across the entire U.S. for a longer period of time.

I use three different outcomes to determine the extent to which coupons affect generic drug use: the generic efficiency rate, the proportion of fills marked "dispense as written," and the therapeutic substitution rate. The generic efficiency rate is the proportion of fills for a given drug that are generic, for example, the ratio of generic Lipitor fills to brand-name Lipitor fills. This measure is used to determine whether coupons shift patients away from generic drugs and towards their more expensive brand-name equivalents. The second measure indicates whether the patient requests the brand-name drug, or the prescriber writes "dispense as written," which ensures that the pharmacy does not substitute a generic drug in place of a brand-name drug. Drug coupons often contain directions to prescribers instructing them to write "dispense as written" when prescribing a drug and giving their patient a coupon. Similarly, the patient can request the pharmacy to dispense the brand-name drug instead of the generic version. I can differentiate between who requests the brand-name drug, the patient or the prescriber, which allows me to determine whether a decrease in generic efficiency is driven by prescriber behavior, patient behavior, or both. The third outcome measuring generic drug use, the therapeutic substitution rate, is the ratio of generic fills to total fills in a given drug class, for example the number of generic statin fills divided by the total number of statin fills. This measure allows me to determine whether coupons affect competition between newer, patent-protected drugs and older, generic drugs in the same drug class, i.e., drugs that are imperfect substitutes. Taken together, these three outcomes

allow me to determine the extent to which coupons affect generic drug use, and the mechanisms through which they operate.

While it is an important piece of the puzzle, estimating the effect of coupons on generic drug use does not paint a complete picture of whether drug coupons increase or decrease drug spending. Even if we observe a decrease in generic drug use in response to coupons, it could also be that coupons increase medication adherence, and thus improve health and decrease other health care spending. To investigate this, I estimate the effect of coupons on medication adherence by measuring the "proportion of days covered" (PDC), which is the proportion of days in a month for which a patient possesses prescribed medication. This measure will pick up any gaps in prescription coverage that result from missed doses or delayed fills, which often arise from inability to afford medication [83].

Finally, I determine whether drug coupons affect brand-to-brand competition, that is, competition between different patent-protected drugs in a class for which there is no equivalent generic. There is suggestive empirical evidence, in addition to theoretical evidence, that coupons increase drug pricing as they erode the incentive for the pharmaceutical company to offer a discount in exchange for a more favorable place on the tiered formulary [32]. Given this, it is important to understand whether coupons shift people to these potentially more expensive drugs. To investigate this, I estimate the effect of coupons on the market share of patent-protected drugs. This allows me to determine whether market share is shifted towards patent-protected drugs that offer coupons and away from drugs that do not.

My empirical strategy hinges on a Massachusetts law barring residents from using coupons. Between 2007 and 2016, Massachusetts was the only state to

ban prescription drug coupons; privately insured residents of all other states were allowed to use coupons. Massachusetts amended the law in 2012 to allow coupon use for drugs without a generic equivalent. I take advantage of this variation—both in who can use coupons for drugs *with* generic equivalents and the variation induced by the 2012 law change—to estimate the effects of coupons on generic drug use, medication adherence, and brand-to-brand competition. I address issues of endogeneity by using this variation in multiple difference-in-differences and triple-difference regressions.

I find that coupons decrease generic drug use by shifting patients towards brand-name drugs and away from generic equivalents. I estimate that coupons decrease generic efficiency by 6.1 percentage points (10%) in the first 14 months following generic entry, which is equivalent to a 16% increase in brand-name drug use. Consistent with this result, I find that coupons increase the use of "dispense as written" by 9.3 percentage points (35%). This effect appears to be driven by patients and not prescribers: coupons increase the rate at which patients request the brand-name drug by 17 percentage points and *decrease* prescriber use of "dispense as written" by 7.9 percentage points. While I find a decrease in generic efficiency, I find no effect of coupons on therapeutic substitution, that is coupons do not shift patients away from older, generic drugs and towards newer, patent-protected drugs. I also find no effect of coupons on medication adherence or brand-to-brand competition. A back-of-the-envelope calculation suggests that these effects correspond to an approximately $25 million increase in insurer spending in my sample alone. Taken together, the results imply that coupons are primarily used as a tool to protect market share of the incumbent brand drug upon generic entry.

1.2 Background

Over the last decade, pharmaceutical companies have increasingly used prescription drug coupons as a strategy to increase profits on brand-name drugs.[2] In 2009, only 86 brand-name drugs offered coupons; this increased to 700 drugs by 2014 [66, 84]. Similarly, the share of total claims associated with a coupon increased from 2.4% in 2010 to 18% in 2017 [3, 65]. While there are many microeconomic theories for why manufacturers offer coupons, the canonical example of which is 3rd-degree price discrimination, coupons issued by pharmaceutical companies have the additional purpose of counteracting the design of the tiered formulary [94, 95]. Tiered formularies are used by insurance companies and pharmacy benefits managers to decrease costs by steering patients away from expensive drugs for which there exists a similarly or equally effective substitute. An example of this occurs for drugs that are "off-patent," i.e., drugs for which there is a generic equivalent on the market. The FDA considers AB-rated generic drugs as therapeutically equivalent to the brand-name drug [44]. Generic drugs tend to have significantly lower costs—30 months after generic entry they are on average 90% cheaper than the brand-name equivalent [67]. In a tiered formulary, drugs are placed on tiers which represent different out-of-pocket copayment amounts that patients are required to pay. In many cases, if a patient wants to get a drug that is more expensive for their insurer, they are required to pay a higher copayment. As generic drugs tend to be the least expensive drugs, they usually have low out-of-pocket costs. The tiered formulary, paired with generic substitution laws which allow or require pharmacists to substitute the generic in place of the brand-name drug, have been instrumental in facilitating the de-

[2] Prescription drug coupons are not offered for generic drugs. Patients generally do not have a choice over which specific generic manufacturer's product they receive, as this is a decision made by the pharmacy [32, 112, 126].

crease in market share of the branded drug following generic entry: in the first month following generic entry, brand-name drugs maintain less than 30% of the market [58]. While insurance companies use the tiered formulary to facilitate a shift from brand to generic following patent expiration, pharmaceutical companies issue coupons to protect market share and bypass the tiered formulary. In the extreme, coupons allow pharmaceutical companies to place their drugs on their preferred tier without having to negotiate with the insurance company.

When a patient uses a drug coupon, the pharmaceutical company pays for some, or all, of the patient's copayment. For many patients, the coupon makes the out-of-pocket cost for the brand-name drug equal to, or less than, that of its generic equivalent. The coupon does not change the amount paid by the insurance company. Figure 1.1 provides a visual example of how a Lipitor coupon (shown in Figure 1.2) works for a patient with a $60 copay for branded Lipitor and a $10 copay for the generic equivalent, atorvastatin. The first bar shows the amount paid for brand-name Lipitor without the coupon. The second bar shows the amount paid for the generic.[3] The third bar shows the amount paid when the patient uses the Lipitor coupon. If the patient uses a coupon, the out-of-pocket cost is $4, which is less than the cost of the generic. The manufacturer covers the remaining $56 of the copayment and receives the negotiated price of $380 from the payer. This strategy can be profitable for the manufacturer if, absent the coupon, the patient would have chosen a generic or competing drug.

Patients can obtain coupons through two main channels: from their prescriber, or directly from the manufacturer. Coupons obtained directly from the manufacturer are usually found by going to the manufacturer's web-

[3]It is not always the case that the insurer does not incur a cost for the generic drug: approximately 28% of generic drug fills in a commercially insured population involve overpayment on the part of the patient, i.e. no cost to the insurer [125].

site, links to which can be found on coupon aggregator websites such as www.internetdrugcoupons.com or www.goodrx.com. Traditionally, insurers have not been able to observe whether a patient uses a coupon in the claim received from the pharmacy. This is because the coupon is run as "secondary insurance," which is not a standard field in the claim sent to the insurer. When the patient fills a prescription and uses a coupon, the pharmacy first bills the insurance to determine the copayment amount. Instead of collecting the entire copayment from the patient, the pharmacy bills the coupon provider for the amount of the copay (up to the limit of the coupon). The pharmacy collects whatever is leftover from the patient, and submits the claim to the patient's insurance company, indicating that the copayment was paid but not showing who paid it. The insurer is only able to observe the coupon if the patient fills a prescription at the insurer's in-house pharmacy, which is usually limited to patients filling specialty drugs. The inability of insurers to observe coupon use limits the tools that insurers have at their disposal to combat coupons, allowing coupon use to persist and the debate to continue.

The markets in which coupons have traditionally been studied differ significantly from the market for prescription drugs. In the market for prescription drugs, patients do not face the full cost of the drug because of insurance. Furthermore, the use (or lack of use) of prescription drugs can affect the demand for other healthcare, for example, non-adherence to cholesterol-lowering medication may result in increased use of hospital care [16, 19]. The unique aspects of prescription drug markets, increased popularity of coupons, and increased government scrutiny of coupon programs make prescription drug coupons an interesting and relevant topic for empirical research.

The literature on the effects of prescription drug coupons is sparse. Dafny, Ody, and Schmitt 2017 present the first and only other estimates of the causal effect of coupons on generic drug use and find that coupons increase branded drug use by over 60% following generic entry. They also provide suggestive evidence that coupons diminish the ability of insurers to extract discounts from pharmaceutical companies in exchange for formulary placement. Other work surrounding prescription drug coupons has provided descriptive evidence of the increased availability of coupons, as well as commentary on the potential effects of prescription drug coupons [84, 107, 126]. Additionally, there are few studies quantifying coupon use because it is difficult for researchers to get access to linkable pharmacy and claims data to identify coupon use. Starner and coauthors 2014 use pharmacy and claims data from a subset of patients who use a specialty pharmacy to show that coupons decreased out-of-pocket costs to patients taking specialty drugs by over 60%, saving the average coupon user over $1,000 in 2013.

A related literature includes the estimated elasticity of demand for prescription drugs, and other healthcare services, which finds that lower out-of-pocket costs leads to greater use (and vice-versa) [53, 55, 69, 77, 85]. Furthermore, this elasticity can lead to spillover effects: increased copayments for prescription drugs can decrease medication use and induce substitution to other (often more costly) healthcare use such as hospitalizations [16].

In this paper, I add to the literature by estimating the effects of prescription drug coupons on generic drug use, within-class drug switching, medication adherence, and competition between branded drugs. Similar to Dafny, Ody, and Schmitt 2017, I take advantage of cross-state variation in coupon le-

gality. Throughout the timespan of my data, coupons were legal for patients with private insurance in all states except for Massachusetts.[4] Prior to 2012, the Massachusetts False Health Care Claims Act (M.G.L. 175H §3) prohibited all Massachusetts residents from using prescription drug coupons. In 2012, the law was amended to allow the use of coupons *only* for drugs *without* an AB-rated generic.[5] Beginning in 2012, Massachusetts residents with private insurance were, for the first time, allowed to use coupons for patent-protected drugs. Importantly for my identification strategy, the law was only amended to allow coupon use for patent-protected drugs; Massachusetts residents have never been allowed to use coupons for drugs that have a generic equivalent (i.e., "off-patent" drugs). I take advantage of this law change to estimate the effect of coupons on within-class drug switching (i.e., therapeutic substitution), medication adherence, and brand-to-brand competition, which has not been done in the previous literature.

1.3 Data

To analyze the effects of prescription drug coupons on generic drug use, medication adherence, and brand-to-brand competition, I use two main sources of data. The first is insurance claims data from a large, national insurer from 2007 to 2016. The data include privately insured individuals who have both prescription drug and medical coverage provided by the insurer. The data do not include anyone enrolled in Medicare or Medicaid plans. The data include

[4]Coupons are illegal in Medicare and Medicaid as they are a violation of the anti-kickback statute and are considered a false claim under the False Claims Act [98]. This law does not apply to anyone without government-funded insurance (i.e., private insurance).

[5]This amendment was originally set to expire on July 1, 2015, but was extended twice; first to July 1, 2017 and then to July 1, 2019 [87].

claims for any individual who filled at least one statin, antipsychotic, or acne prescription during the data time period, amounting to over 11 million unique individuals and 75 million prescription fills.[6] I observe patient characteristics including age, gender, zip code of residence, and a HIPAA-compliant patient identifier. I also observe information about the prescription including brand name, generic name, days supplied, dose, dosage form, copayment, exact date of fill, and a "dispense as written" indicator, which indicates whether the prescriber or patient asked the pharmacy to dispense the brand-name drug over the generic equivalent. Table 1.1 contains descriptive statistics of the insurance claims data. Importantly, I am unable to observe whether a coupon is used in a given prescription claim. The insurer is not able to observe the use of coupons in claims data because the coupon transaction occurs at the pharmacy level and is not recorded on the claim sent to the insurer. Because of this, the estimates presented in this paper are intent-to-treat estimates.

In addition to prescription drug claims, I collect information on coupon availability using webpages archived on the Internet Archive, which maintains a point-in-time history of webpages through the Wayback Machine. I use information from archived versions of www.internetdrugcoupons.com, www.goodrx.com, and individual manufacturer webpages (e.g., www.lipitor.com). I use these archived pages to determine which drugs had coupons available at which points in time between 2007 and 2016, as well as the value of the coupons.

There are two main types of coupons offered. The first is an instant rebate, for example, "up to $25 off your copay". In this case, someone with a $50 copay would have a coupon value of $25, while someone with a $10 copay would have a coupon value of $10. The second type of coupon brings the out-of-pocket cost

[6]The three drug classes used in this study are a function of data availability.

down to a pre-determine minimum, for example, "$4 out-of-pocket cost". In this case, someone with a $50 copay would have a coupon value of $46 and someone with a $10 copay would have a coupon value of $6.

As I cannot observe coupon use in the claims data, I combine the data on coupon availability with the claims data to determine what the out-of-pocket cost *would have been* if someone who filled a brand prescription had used a coupon when available. By doing this, I construct a distribution of coupon values based on the observed brand drugs fills. This distribution is shown in Figure 1.3. Overall, the average coupon value is approximately $15, with the majority of values falling below $40.

To understand the coupon value, it is important to compare it to the copay for the brand drug, as well as the copay for the equivalent generic (if generic entry has occurred). Figures 1.4 through 1.6 provide this context in the situation where a drug has experienced generic entry, and Figures 1.7 and 1.8 provide this context in the situation where a drug is patent-protected.

Figure 1.4 shows the distribution of copays for the brand version of the drug, not taking into account the available coupons. Figure 1.5 shows what happens to the out-of-pocket costs for the brand drug if the coupon is applied to all fills. As we can see, the distribution shifts left, with the out-of-pocket costs for the brand drug significantly decreasing when the coupon is applied. In this case, the majority of fills have an out-of-pocket (OOP) cost of less than $5. Importantly, the distribution of OOP costs for a brand drug filled using a coupon is shifted left compared to the distribution of copays for generic drugs that have coupons available, which is shown in Figure 1.6.[7] This is consistent with Figure

[7]Note that in the claims data I cannot observe an individual's plan design. I am only able to observe the copayment amount for the drug they filled, I cannot observe the copayments for all

1.1, which shows an example of the case where the brand drug filled using a coupon has a lower OOP cost than the generic equivalent.

While the above three graphs show the relative OOP costs for drugs offering coupons after generic entry, Figures 1.7 and 1.8 show the costs for patent-protected drugs that offer coupons before generic entry. Figure 1.7 shows the distribution of copays for branded drugs that offer coupons before they experience generic entry, which has an average copay is around $30. Figure 1.8 shows the OOP cost for the brand fills when the coupon is applied. As expected, the distribution is shifted left compared to that in Figure 1.7, with the average OOP cost shifting from around $30 to approximately $12.

In both situations where a coupon is offered, either before or after generic entry, the coupon shifts the distribution of OOP costs towards zero. The coupons provide more value after generic entry compared to before, which is to be expected given the nature of competition in the post-generic-entry market.

1.4 Empirical Framework

1.4.1 Generic Drug Use

A main point of contention between coupon supporters and non-supporters is whether coupons affect generic drug use. I estimate the effect of coupons on generic drug use using three distinct outcomes: 1) generic efficiency, 2) the rate of "dispense as written," and 3) within-class drug switching (i.e., therapeutic

of the drugs on their formulary.

substitution). Combined, these outcomes provide information on the extent to which coupons affect generic drug use, and the mechanisms through which they operate.

Generic Efficiency

Generic efficiency, the first outcome I use to measure generic drug use, addresses the situation that coupon opponents are most worried about: do coupons shift people towards brand-name drugs and away from their generic equivalents? To estimate the effect of coupons on generic efficiency, I follow Dafny, Ody, and Schmitt 2017 and estimate difference-in-differences models comparing differences in generic efficiency rates between Massachusetts, which bans coupon use for off-patent drugs, and all other states. I compare the differences across drugs that offer coupons and drugs that do not offer coupons. Specifically, I estimate the following equation:

$$\text{Outcome}_{dts} = \alpha + \beta * (\text{notMA} * \text{coupon})_{ds} + \tau_{dt} + \sigma_s + \varepsilon_{dts} \tag{1.1}$$

for drug d, event-time t (i.e., months since generic entry), and state s. The outcome is measured for months following generic entry, as generic drug use is zero in the months prior to generic entry. As this regression is relevant to "off-patent" drugs, the sample only includes drugs that experienced generic entry between 2007 and 2016, a list of which is contained in Table 1.3. The sample consists of a balanced panel of drugs observed for 14 months following generic entry.

When this equation is used to estimate the effect of coupons on generic efficiency, Outcome_{dts} is the generic efficiency rate for a given drug, event-time,

state, i.e., the number of generic Lipitor fills divided by the total number of Lipitor fills in Massachusetts two months after generic entry. The indicator variable notMA$_s$ is equal to one for individuals who live outside of Massachusetts (who are allowed to use coupons), and coupon$_d$ is an indicator for whether the company offers a coupon for drug d after patent expiration. τ_{dt} and σ_s are drug-event-time and state fixed-effects. State fixed-effects are included to account for the fact that states may have different levels of generic efficiency, due to, for example, different generic substitution laws. Drug-event-time fixed-effects are included to account for the fact that drugs have different trends in generic efficiency following generic entry, for example, Lipitor might experience a more rapid decrease in market share in the first three months after generic entry than Crestor. Note that coupon$_d$ is not included as a variable in the regression as it is accounted for by the drug-event-time fixed-effects. The coupon$_d$ indicator does not vary over time, it simply measures whether a firm offers a coupon for a given drug when that drug experiences generic entry.

The variation in coupon$_d$, i.e., the firm's decision of whether to offer a coupon when their drug is about to experience generic entry, is not exogenous. Firms may decide to offer a coupon if, for example, they expect a rapid decline in market share following generic entry. However, the identification strategy does not require that the firm's decision to offer a coupon is exogenous, it only requires that the drug-state variation is exogenous. That is, absent the coupon, the difference in generic efficiency between Massachusetts and the other states would be equal to the difference for drugs that do not offer coupons. The identification strategy relies on the drugs without coupons to accurately control for things that vary across states and affect the outcome. For example, states have different laws dictating the way in which pharmacists can substitute generics.

Because these things vary across state, but do not vary across drug, they are differenced out in the analysis. The strategy will not control for things that vary across state and affect the outcome *if* these things are also correlated with the decision to offer coupons; this would violate the identifying assumption. One potential concern which can be tested would be if copays for generic drugs varied across both state and coupon status. Figure 1.9 shows the distribution of generic copays in Massachusetts compared to other states, plotted separately for drugs that offer coupons and drugs that do not offer coupons. The relative difference in copays between Massachusetts and other states for drugs that do not offer coupons (panels a and b) must be similar to the relative difference in copays for drugs that offer coupons (panels c and d), which is observed in the graphs.

In equation 1.1, the coefficient of interest is β, which is the estimated effect of coupons on generic efficiency. This estimate is an intent-to-treat effect because the use of coupons is not observed in the data. As the outcome variable is aggregated by drug, state, event time, the regressions are weighted by the total number of fills for the drug (brand + generic) in the state-event-time (the denominator). Standard errors are estimated using two-way clustering by drug and state.

Table 1.4 shows the results from the estimation of equation 1.1 using generic efficiency as the outcome variable. The estimate in column one shows the preferred specification, which includes drug-event-time and state fixed-effects. The estimate indicates that coupons decrease generic efficiency by 6.1 percentage points. Given an average generic efficiency rate of 0.62, this estimate corresponds to a 10% decrease in generic efficiency and a 16% increase in branded

drug use after generic entry. Column two shows the estimated effect when state-event-time and drug fixed-effects are included in the regression. State-event-time fixed-effects allow each state to have its own generic efficiency path, which may be important if generic efficiency paths are determined by state laws regarding generic substitution. This estimate shows a similar effect of coupons on generic efficiency.

In a difference-in-differences analysis where only one state is treated and the rest are not, conducting inference using asymptotic approximations can lead to overstated precision. To address this concern, I follow Buchmueller et al. 2011 and Cunningham and Shah 2018 and implement a variant of Fisher's permutation test [49]. I estimate equation 1.1 an additional 50 times, each time substituting a different state as the treatment state (i.e. the state that cannot use coupons). From these regressions I generate a distribution of placebo estimates to which I compare the true estimate from Table 1.4. The distribution of placebo estimates is shown in Figure 1.10. The black dashed lines are the 5^{th} and 95^{th} percentiles, which represent the critical values for rejecting the null hypothesis that the Massachusetts effect is zero with 10% significance. As shown in the graph, the Massachusetts effect falls within the dashed lines, indicating that its not significant at the 10% level. In fact, the Massachusetts effect is ranked 6th from the bottom, which corresponds to a significant effect only at the 25% level in a two-tailed test.

Figure 1.11 provides visual evidence of the difference-in-differences estimation and shows the generic efficiency rate in the months following generic entry, for drugs that offer coupons compared to drugs that do not offer coupons. The difference in generic efficiency between Massachusetts and the other states is

largest for drugs that offer coupons (the graph on the right). The figure shows that the effect of coupons is largest during the first 6 months after generic entry, and begins to taper off after that.

As a robustness check I estimate equation 1.1, excluding drugs one at a time to ensure the effect is not driven by a single drug. The results of this are shown in Appendix Table 1.A1. In all cases, the effects are significant using two-way clustered standard errors and the estimated effects remain relatively the same.

Use of "Dispense as Written"

In the previous section, I showed that coupons decrease generic drug use following generic entry. Given this, it is important to understand who drives the decreased generic drug use, patients or prescribers? To investigate this, I use the proportion of fills marked "dispense as written." Drug coupons often contain notes to prescribers instructing them to write "dispense as written" when prescribing a drug and providing their patient with a coupon. Similarly, the patient can request the pharmacy to dispense the brand-name drug instead of the generic. The data allow me to differentiate between who requests the brand, the prescriber or the patient, which allows me to determine whether decreases in generic efficiency are driven by prescribing behavior and/or patient behavior.

For this outcome, I estimate equation 1.1 using the "dispense as written" rate (DAW_rate$_{dts}$) as the outcome variable. DAW_rate$_{dts}$ is the proportion of fills indicating "dispense as written" in a given drug, state, event-time (months since generic entry). In this regression, β measures the intent-to-treat effect of coupons on the use of "dispense as written." As with the generic efficiency

regression, the sample consists of drugs which experienced initial generic entry between 2007 and 2016, and includes observations up to 14 months following generic entry. The list of drugs used to estimate this regression can be found in Table 1.2.

Table 1.5 shows the results from the estimation of equation 1.1 using the "dispense as written" rate as the outcome variable. The first column shows the effect of coupons on use of "dispense as written," either by the prescriber or the patient. The second column shows the effect of coupons on the prescriber's use of "dispense as written," and the third column shows the effect on the patients' probability of requesting the brand. The results indicate that coupons increase the overall use of "dispense as written" by 9.1 percentage points, but this effect is driven by patients whose probability of requesting the brand increases by 17.1 percentage points in response to coupons. The estimated effect of coupons on the use of "dispense as written" by prescribers is negative, indicating that coupons *decrease* the use of "dispense as written" among physicians by 7.9 percentage points.

As with the previous outcome, I construct distributions of placebo estimates to compare the estimated coefficients to. These distributions are shown in Figures 1.12 through 1.14. Figure 1.12 shows the placebo distribution for the effect of coupons on any use of "dispense as written". The solid red line corresponds to the Massachusetts effect shown in column one of Table 1.5. The dashed lines represent the 5^{th} and 95^{th} percentiles, which are the critical values beyond which the estimate would be considered significant at the 10% level. While the Massachusetts estimate falls within the dashed lines, it is ranked 3^{rd} from top, indicating significance at around the 15% confidence level. Figure 1.13 shows the

placebo distribution for the effect of coupons on the probability that the patient requests the brand, and Figure 1.14 shows the distribution for the effect of coupons on the prescriber use of "dispense as written". In both of these graphs, the Massachusetts effect falls within the dashed lines, indicating significance at the 10% level.

These relationships are visualized in Figures 1.15 through 1.17, which show the share of fills indicating "dispense as written" in the months following generic entry for drugs that offer or do not offer coupons. Figure 1.15 shows the share of fills for which the prescriber or patient instructed to "dispense as written." Consistent with the estimates in column 1 of Table 1.5, the difference in the "dispense as written" rate between Massachusetts and the other states is larger for drugs that offer coupons than drugs that do not offer coupons. Figure 1.16 shows the relationship for the use of "dispense as written" among prescribers, and Figure 1.17 shows the relationship for patients. Consistent with the regression estimates, the relationship between coupons and the use of "dispense as written" is opposite for prescribers compared to patients. The prescriber use of "dispense as written" is significantly *higher* in Massachusetts than other states for drugs that offer coupons compared to drugs that do not offer coupons. This could be the result of pharmaceutical companies relying on coupons to drive their marketing strategy in states where coupons are legal, and employing heavier physician detailing in Massachusetts to make up for the fact that they cannot offer coupons. From the graph, it appears that this relationship is decreasing over time, which could be due to pharmaceutical companies ceasing detailing of prescribers, or insurance companies implementing formulary tools to counteract the effects of the coupon.

As with the previous outcome I perform a robustness check by excluding drugs one a time from the regression to ensure the effect is not driven by a single drug. The results of this are shown in Appendix Table 1.A2. In all cases, the effect remains significant using two-way clustered standard errors and the estimated effects remain relatively the same.

Therapeutic Substitution

The third outcome I use to investigate the extent to which coupons affect generic drug use is therapeutic substitution, i.e., substitution between different brand-name and generic drugs within the same drug class, for example, generic Mevacor with brand-name Crestor. When a company offers a coupon for a drug that does not have a generic equivalent, it may induce people to shift away from older, generic drugs in the same class and towards the patent-protected drug offering the coupon. I estimate the extent to which coupons affect therapeutic substitution by measuring the share of fills in a given drug class that are dispensed as generic.

Similar to the previous regression, I estimate a difference-in-differences model. However, in this regression I take advantage of the change in Massachusetts state law in which coupons went from banned to allowed for drugs *without* a generic equivalent (i.e., patent-protected drugs). I use a regression framework to compare the share of prescriptions in a class that are dispensed as generic across states, before and after July 2012, the month the policy change went into effect. Specifically, I estimate

$$\text{Outcome}_{cts} = \alpha + \beta * (\text{MA} * \text{post})_{ts} + \tau_{ct} + \sigma_s + \varepsilon_{cts} \tag{1.2}$$

for drug class c, month t and state s. To measure the effect on therapeutic substitution, Outcome_{cts} is the share of fills in a given drug class that are dispensed as generic in a state-month, e.g., the number of generic statins divided by the total number of statins filled in Massachusetts in January 2010. MA_s is an indicator equal to one for Massachusetts and zero elsewhere, and post_t is an indicator equal to one for months on or after July 2012, when the coupon ban was lifted. τ_{ct} and σ_s are class-month and state fixed-effects. The class-month fixed-effects account for any patent expirations that occur in the drug class over time, as this necessarily affects the share of fills that are dispensed as generic. The coefficient of interest is β, which measures the intent-to-treat effect of coupons on therapeutic substitution. As the claims data are aggregated up to the drug class-state-month, the regressions are weighted by the total number of fills in the drug class-state-month (the denominator). Standard errors are clustered at the state level. Unlike the regressions for generic efficiency and "dispense as written", the sample used to estimate equation 1.2 contains all fills from 2007 to 2016.

In order to interpret β as the causal effect of coupons on therapeutic substitution, the parallel trends assumption must hold, i.e., if Massachusetts did not lift the ban on coupons for drugs without a generic equivalent, the generic share of fills in a drug class would trend with the other states. Figure 1.18 provides visual evidence of the relationship between coupons and therapeutic substitution and provides support for the parallel trends assumption. The figure plots the outcome variable, the share of drugs in a class dispensed as generic, over time, in Massachusetts compared to the other states. With the exception of the earliest year of the data, Massachusetts seems to follow a parallel trend with the other states.

Table 1.6 shows the results from the estimation of equation 1.2 using the share of fills in a given drug class that are dispensed as generic as the outcome variable. The coefficient estimate, while negative, is not statistically significant, which is consistent with the graph in Figure 1.18. The point estimate corresponds to less than a 1 percent decrease in the share of drugs in a class that are dispensed as generic. This estimate is a precisely estimated null: the 95% confidence interval allows me to rule out effects larger than a 1.5 percentage point decrease in therapeutic substitution, and anything larger than a 0.5 percentage point increase in therapeutic substitution.

Combined with the previously discussed results, these estimates indicate that the effect of coupons on generic drug use is limited to shifting patients away from generic equivalents following generic entry.

1.4.2 Medication Adherence

While the effect of coupons on generic drug use is an important component, it does not give a complete picture as to whether coupons increase or decrease healthcare spending. Pharmaceutical companies argue that prescription drug coupons increase medication adherence, and thus improve health and keep people out of the hospital. To investigate this claim, I estimate equation 1.2 using the proportion of days covered (PDC), a commonly used measure of adherence, as the outcome variable. The PDC measures the proportion of days in a given month for which an individual has prescribed medication on hand. This measure of adherence can be used to determine whether patients skip doses, split single doses over multiple days, or delay filling prescriptions.

The first step in calculating the PDC is to use the exact date of fill and patient identifier to trace out each individual's prescription history. From the prescription history, I calculate the PDC for each individual in every month of data. I make adjustments to take into account the fact that individuals move in and out of the data, for example if they change insurers. I also account for medication changes, as well as for the fact that some patients refill prescriptions before they have run out of the previous month's supply. More detailed information on the calculation of PDC can be found in section 1.8.2 of the appendix. I calculate PDC for statins and antipsychotics, but not acne medications. The majority of acne medications fills are topical creams or gels, the days supplied of which is not binding as it is with, for example, a 30-days supply of Lipitor pills. Further, there is evidence that adherence to statins and antipsychotics can reduce other healthcare use such as hospitalizations; it is unlikely that this would occur with adherence to acne medications [16, 19, 119].

To estimate equation 1.2, I aggregate the individual PDCs so that the outcome variable is the average PDC in a given drug class-state-month. In this difference-in-differences regression, I compare the average PDC across states before and after July 2012 when Massachusetts lifted its coupon ban for drugs without a generic equivalent. As the outcome is aggregated, I weight the regressions by the number of unique patients in the state-class-month (the denominator). The coefficient of interest is β, which measures the intent-to-treat effect of coupons on the PDC. To interpret β as the causal effect of coupons on medication adherence, the parallel trends assumption must hold, i.e., absent the policy change in Massachusetts, the average PDC in Massachusetts would follow a parallel trend to the other states.

Table 1.7 shows the results from the estimation of equation 1.2 using the measure of medication adherence, average proportion of days covered (PDC), as the outcome variable. The coefficient estimate indicates a precisely estimated null effect. The 95% confidence interval allows me to rule out effects larger than a 0.2 percentage point (0.3%) decrease, or a 0.4 percentage point (0.6%) increase, in the average proportion of days covered. Figure 1.19 plots the average PDC over time in Massachusetts compared to other states and provides visual evidence of the effect of coupons on medication adherence. Consistent with the regression estimates, the graph shows no effect of coupons on adherence. Furthermore, the lines are consistently parallel, providing evidence in support of the parallel trends assumption.

In sum, the results of this analysis show that the ability to use coupons does not appear to have an effect on medication adherence. Without an effect on medication adherence, it is unlikely that coupons would lead to any meaningful improvements in health outcomes.

1.4.3 Brand-to-Brand Competition

The final outcome I investigate is the extent to which coupons affect brand-to-brand competition, that is, competition between patent-protected, brand-name drugs within the same drug class. This outcome is important for two reasons. The first is that even if coupons do not decrease generic drug use, they could still increase spending if they move people towards drugs that offer coupons and away from drugs that do not. This is because offering a coupon erodes the incentive of the pharmaceutical company to offer a discount in exchange for a

place a more favorable spot on the tiered formulary. The coupon essentially allows the pharmaceutical company to choose their own formulary tier, removing some of the bargaining chips of the insurer. Dafny, Ody and Schmitt (2017) provide suggestive evidence that this leads to higher prices in the form of faster branded price growth following generic entry. The second reason to analyze the outcome is to gain a better understanding of why pharmaceutical companies offer drug coupons: is it to steal market share from equivalent generics, different generics, or other brand-name drugs?

To estimate the effect of coupons on brand-to-brand competition, I estimate a triple-difference regression, taking advantage of the July 2012 policy change in which Massachusetts began allowing coupons for drugs *without* a generic equivalent. I compare two different groups before and after the policy: patent-protected drugs offering coupons at the time the policy changed compared to patent-protected drugs that did not, and Massachusetts to other states. Specifically, I estimate

$$\begin{aligned}\text{BrandShare}_{dts} = \alpha + \beta * (\text{post} * \text{MA} * \text{Coupon})_{dts} + \gamma_1 * (\text{post} * \text{MA})_{ts} \\ + \gamma_2 * (\text{MA} * \text{coupon})_{ds} + \tau_{dt} + \sigma_s + \varepsilon_{dts} \quad (1.3)\end{aligned}$$

for drug d in month t and state s. BrandShare_{dts} is the market share of drug d, i.e., the number of fills of drug d divided by the total number of fills in the drug class, in a given state-month. Post_t is an indicator equal to 1 if the month is July 2012 or later, Coupon_d is an indicator for whether drug d offers a coupon in July 2012, and MA_s is an indicator equal to 1 for Massachusetts and 0 for other states. τ_{dt} and σ_s are drug-time and state fixed-effects, respectively. Note that the interaction term $(\text{Post} * \text{Coupon})_{dt}$ and the variables Coupon_d and Post_t are not included as they are collinear with the drug-time fixed-effects.

This outcome is calculated for all drugs in the sample that have not experienced generic entry by July 2012. Table 1.3 contains the list of drugs in the sample and indicates which drugs had coupons available. The outcome is aggregated to the drug-state-month and therefore the regressions are weighted by the number of fills for that drug-state-month. Standard errors are clustered at the drug level. The outcome of interest is β, which measures the intent-to-treat effect of coupons on the market share of patent-protected drugs.

Table 1.8 shows the results from the estimation of equation 1.3. The point estimate corresponds to a 0.6 percentage point (5%) increase in market share, though this effect is not statistically significant. However, the coefficient is imprecisely estimated and the 95% confidence interval does not allow me to rule out meaningful effects as large as a 2.3 percentage point (20%) increase, or a 1 percentage point (9%) decrease in brand market share.

Figure 1.20 provides visual evidence of the relationship between coupons and the market share of patent-protected drugs. The figure shows that for drugs that offer coupons, there is a difference in market share between Massachusetts compared to other states (right panel), and no difference for drugs that do not offer coupons (left panel). However, in both graphs, Massachusetts and the other states follow a similar trend. When Massachusetts lifts the ban on coupons, the difference between Massachusetts and the other states does not appear to change in either graph, which is consistent with the estimated null effect of the triple-difference regression.

1.4.4 Effect of Coupons on Spending

In Table 1.4, I show that coupons decrease generic efficiency by 6.1 percentage points during the first 14 months following generic entry, which corresponds to a 16% increase in brand-name drug use.[8] As I find no effect of coupons on adherence, within-class drug switching, or brand-to-brand competition, I use the estimated effect of coupons on generic efficiency to perform a back-of-the-envelope calculation of the effect of coupons on insurer spending. First, I sum up the total post-generic-entry brand spending by adding up the wholesale acquisition cost (0.8 $*$ average wholesale price) for all brand-name fills that occur in the 14 months after generic entry ($t \in [0, 14]$) in all states ($s \in S$):

$$\text{Total post-generic-entry brand spending} = \sum_{\substack{f \in \{\text{brand fills}\} \\ d \in \{\text{all drugs}\} \\ s \in S \\ t \in [0,14]}} \text{WAC}_{fdst} \tag{1.4}$$

I then multiply the total post-generic-entry brand spending by the effect of coupons on brand-name drug use due to coupons (16%):

$$\text{Increase in } \textit{brand} \text{ spending due to coupons} = 16\% * \text{post-generic-entry brand spending} \tag{1.5}$$

To get the net spending increase, which takes into account the corresponding decrease in generic spending, I multiply this quantity by 0.66, which is the difference in cost between generic and brand-name drugs 1 year after generic entry

[8] The average generic efficiency rate is 0.62 and the estimated effect of coupons on generic efficiency is a 6.1 percentage point decrease, so the percent increase in branded drug use is equivalent to $\frac{0.061}{1-0.62} = 16\%$

[67].

Increase in *total* spending due to coupons =

$$0.66 * \text{Increase in } \textit{brand} \text{ spending due to coupons} \quad (1.6)$$

The result of this calculation is shown in Table 1.9. The first column shows the increase in spending for the entire sample, and columns two through four show the increase separately for each drug class. The first row shows the increase in spending calculated using all 22 drugs in the sample that experience generic entry between 2007 and 2016, while the second row uses only the subset of drugs that offer coupons at generic entry.[9] The estimates show that in my sample, coupons increased spending by approximately $24.9 million, which is equivalent to 6.8% of total spending in the 14 months following generic entry. The absolute spending increase is greatest for statins, while the largest relative increase occurs in the acne class, for which the increase in spending accounts for 8.7% of total post-generic-entry spending. Importantly, this calculation does not take into account discounts and rebates negotiated between the payer and the manufacturer. Additionally, it does not include decreases in consumer out-of-pocket spending due to coupon use.

1.5 Discussion

In this paper, I address the debate surrounding prescription drug coupons. I use prescription drug claims from a large, national insurer from 2007 to 2016 to estimate the effect of coupons on generic drug use, medication adherence, and

[9] Using only the subset of drugs that offer coupons at generic entry is equivalent to modifying equation 1.4 so that the sum over d is for $d \in$ {drugs that offer coupons at generic entry}.

brand-to-brand competition for drugs in three drug classes: statins, antipsychotics, and acne medications. I use a variety of difference-in-differences and triple-difference regressions to estimate the causal relationships. My empirical strategy hinges on a Massachusetts law which barred residents from using coupons for all drugs (regardless of whether they had a generic equivalent), and was amended in 2012 to allow coupons for drugs *without* a generic equivalent.

I use three outcomes to measure the effect of coupons on generic drug use: the generic efficiency rate, the share of prescriptions indicating "dispense as written," and the therapeutic substitution rate. I find that coupons decrease generic efficiency by 6.2 percentage points (10%). Consistently, I find that coupons increase the share of prescriptions indicating "dispense as written" by prescribers and patients by 9.1 percentage points (34%). Interestingly, this relationship is driven by patients requesting the brand and not prescribers: coupons increase the probability that the patient requests the brand by 17.1 percentage points and *decrease* the use of "dispense as written" by prescribers by 7.9 percentage points. I do not find evidence that coupons affect therapeutic substitution, i.e., coupons do not shift patients away from generics and towards *different* brand-name drugs. Taken together, these results imply that coupons decrease generic drug use primarily by shifting patients towards brand-name drugs for which there is a generic equivalent.

While a decrease in generic drug use might lead to an increase in drug spending, it does not necessarily lead to an increase in total health spending if it is coupled with an improvement in health. Medication adherence is the primary mechanism through which coupons might improve health. An additional mechanism is therapeutic substitution in which patients shift towards newer,

more effective drugs. However, I find no evidence that coupons affect either of these outcomes. Furthermore, I find no evidence that coupons are used for brand-to-brand competition among patent-protected drugs. These findings are consistent with the idea that coupons are primarily issued to protect the market share of the branded drug upon generic entry. This is consistent with the beliefs of insurers and policymakers, who are concerned that coupons increase spending without improving health.

An important caveat is that the null effects of coupons may have occurred if the repeal of the coupon ban in Massachusetts was not widely publicized, and residents were not aware that they were now allowed to use coupons. While I lack the ability to observe coupon use in the data and therefore cannot answer this question directly, articles from local news outlets including the Boston Globe suggest that this policy was publicized [1, 76]. Furthermore, pharmaceutical companies and firms that administer coupon programs have an incentive to inform physicians and patients of the policy change.

While these results provide evidence that coupons decrease generic drug use without improving medication adherence, the results are limited to other small-molecule drug classes that are similar in nature to those used in this study. Importantly, these results might not translate to coupons issued for specialty or biologic drugs. As many insurers require members to fill prescriptions for specialty drugs at the insurer's in-house pharmacy, the insurer is able to observe, and therefore restrict, the use of copay coupons for these drugs. A recent development has been for insurers to implement "copay accumulator programs." These programs change the benefit design so that copay coupons no longer count towards the patient's deductible, resulting in patients facing the

full amount of their deductible, often over $5000, after copay assistance runs out, which can happen within the first few months of the year for many expensive specialty drugs [45]. The results of this study cannot speak to whether limiting copay coupons through the use of accumulator programs will affect generic drug use, medication adherence, or overall health.

As policymakers consider enacting laws that ban or limit the use of copay coupons, the results from this study provide evidence that banning coupons for drugs that have a generic equivalent may be an effective way to decrease healthcare spending. For drugs without a generic equivalent, there is no evidence that coupons have a significant effect, and therefore banning them might not lead to realized cost savings. Additionally, given that many of the coupons for patent-protected drugs are for expensive specialty drugs for which there are few available substitutes, banning coupons for patent-protected drugs might have an unintended consequence of worsening health. The effect of coupons in specialty and biologic drug classes is an important avenue for future research, and the dearth of evidence on these effects should be taken into account when considering legislation and benefit design to limit coupon use.

1.6 Tables

Table 1.1: Summary Statistics, Claims Data

	Massachusetts	**Other States**
# statin fills	723,493	59,616,281
# antipsychotic fills	80,165	6,023,796
# acne fills	110,831	8,760,650
# unique patients	75,906	6,499,801
average copay	$18 (21.4)	$21 (24.2)
average days supplied	40.1 (23.6)	38.5 (21.8)
% DAW by prescriber or patient (post generic entry, drugs experiencing entry 2007-2016)	4.40%	8.42%
% DAW by prescriber (post generic entry, drugs experiencing entry 2007-2016)	3.86%	1.99%
% DAW by patient (post generic entry, drugs experiencing entry 2007-2016)	0.54%	6.43%
% of fills on formulary	92.4%	89.6%
% generic fills (full sample)	67.0%	58.2%
% generic fills (post generic entry, all drugs)	96.5%	94.1%
% generic fills (post generic entry, drugs experiencing entry 2007-2016)	93.5%	89.5%

Standard deviations in parentheses

Table 1.2: Drugs Experiencing Generic Entry, 2007 - 2016

Brand Name	Drug Class	Generic Entry	Coupon
Solodyn	Acne	February 2009	
Lipitor	Statin	November 2011	
Caduet	Statin	December 2011	
Geodon	Antipsychotic	March 2012	Coupon
Doryx	Acne	April 2012	Available at
Retin-A Micro 0.1% gel	Acne	March 2013	Generic Entry
Retin-A Micro 0.04% gel	Acne	March 2013	
Metrogel	Acne	July 2013	
Differin 0.3% gel	Acne	April 2014	
Abilify	Antipsychotic	April 2015	
Atralin	Acne	August 2015	
Risperdal	Antipsychotic	June 2008	
Risperdal M-Tab	Antipsychotic	February 2009	
Differin 0.1% gel	Acne	June 2010	
Differin 0.1% cream	Acne	June 2010	
Zyprexa	Antipsychotic	October 2011	
Seroquel	Antipsychotic	March 2012	No Coupon
Lescol	Statin	April 2012	Available at
Invega	Antipsychotic	August 2015	Generic Entry
Lescol XL	Statin	September 2015	
Fazaclo ODT	Antipsychotic	September 2015	
Orap	Antipsychotic	September 2015	

Table 1.3: Patent-Protected Drugs by Coupon Availability

Brand Name	Drug Class	Coupon
Abilify	Antipsychotic	
Abilify discmelt	Antipsychotic	
Acanya	Acne	
Aczone	Acne	
Atralin	Acne	
Crestor	Statin	
Differin 0.1% lotion	Acne	
Differin 0.3% gel	Acne	Coupon
Epiduo 0.1%/2.5% gel	Acne	Available
Fanapt	Antipsychotic	in July 2012
Finacea	Acne	
Livalo	Statin	
Metrogel	Acne	
Oracea	Acne	
Saphris	Antipsychotic	
Seroquel XR	Antipsychotic	
Solodyn (55, 65, 80, 105, 115 mg)	Acne	
Vytorin	Statin	
Advicor	Statin	
Altoprev	Statin	
Azelex	Acne	
Fazaclo	Antipsychotic	
Invega	Antipsychotic	
Juvisync	Statin	No Coupon
Latuda	Antipsychotic	Available
Lescol XL	Statin	in July 2012
Noritate	Acne	
Orap	Antipsychotic	
Simcor	Statin	
Tazorac 0.01% gel	Acne	
Tazorac 0.05% cream	Acne	
Tazorac 0.05% gel	Acne	

Table 1.4: Effect of Coupons on Generic Efficiency

notMA · coupon	-0.0612***	-0.0608***
	[-0.073,-0.049]	[-0.073,-0.049]
N (drug-state-event time)	13,320	13,313
Average Generic Efficiency	0.620	0.620
Drug-Event-Time FEs	Yes	No
State-Event-Time FEs	No	Yes
Drug FEs	No	Yes
State FEs	Yes	No
Timeframe	14 months after generic entry	14 months after generic entry

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

Table presents estimates of β from equation 1.1: the intent-to-treat estimate of the effect of coupons on generic efficiency (ratio of generic fills to total fills). The sample includes a balanced panel of drugs observed for 14 months following generic entry. The estimate can be interpreted as a $\beta * 100$ percentage point change in generic efficiency as the result of coupons. 95% confidence intervals in brackets. The outcome is aggregated by drug, state, event-time, and regressions are weighted by the number of fills in a drug-state-event time. Standard errors are estimated using two-way clustering by drug and state.

Table 1.5: Effect of Coupons on "Dispense as Written" (DAW) Rate

	Prescriber or Patient	Prescriber	Patient
notMA · coupon	0.0913***	-0.0793***	0.171***
	[0.081,0.102]	[-0.093,-0.066]	[0.158,0.183]
N	12,460	12,460	12,460
Average DAW Rate	0.269	0.056	0.213

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

Table presents estimates of β from equation 1.1: the intent-to-treat estimate of the effect of coupons on the "dispense as written" rate. The sample includes a balanced panel of drugs observed for 14 months following generic entry. The estimate can be interpreted as a $\beta * 100$ percentage point change in the "dispense as written" rate as the result of coupons. 95% confidence intervals in brackets. The outcome is aggregated by drug, state, event-time, and regressions are weighted by the number of fills in a drug-state-event time. Standard errors are estimated using two-way clustering by drug and state.

Table 1.6: Effect of Coupons on Therapeutic Substitution

MA · Post	-0.00481
	[-0.015,0.005]
N (drug class-state-months)	18,541
Average Generic Share	0.583

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

Table presents the estimate of β from equation 1.2: the intent-to-treat effect of coupons on therapeutic substitution (share of fills in a given drug class that are dispensed as generic). The estimate can be interpreted as a $\beta * 100$ percentage point change in therapeutic substitution as the result of coupons. 95% confidence intervals in brackets. The outcome is aggregated by drug class, state, month, and regressions are weighted by the number of fills in a class-state-month. Standard errors are clustered by state.

Table 1.7: Effect of Coupons on Adherence (Proportion of Days Covered)

MA · Post	0.000650
	[-0.002,0.004]
N (drug class-state-months)	11,804
Average PDC	0.706

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

Table presents the estimate of β from equation 1.2, the intent-to-treat effect of coupons on medication adherence (proportion of days covered). The estimate can be interpreted as a $\beta * 100$ percentage point increase in proportion of days covered. 95% confidence intervals in brackets. The outcome is aggregated by drug class, state, month, and regressions are weighted by the number of unique patients in a class-state-month. Standard errors are clustered by state.

Table 1.8: Effect of Coupons on Brand-to-Brand Competition

MA · Post · Coupon	0.00639
	[-0.010,0.023]
N (drug-state-months)	141,059
Average Brand Share	0.114

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

Table presents the estimate of β from equation 1.3: the intent-to-treat effect of coupons on the market share of patent-protected drugs. 95% confidence intervals in brackets. The outcome is aggregated by drug, state, month, and regressions are weighted by the number of fills in a drug-state-month. Standard errors are clustered by drug.

Table 1.9: Increase in Spending Due to Coupons

		Drug Class			
		All	Statins	Acne	Antipsychotics
All drugs	Increase in spending	$24.9	$16.7	$5.39	$2.77
	(% of post-generic-entry spending)	(6.82%)	(7.29%)	(8.65%)	(3.73%)
Drugs that offer coupons	Increase in spending	$22.5	$16.7	$5.18	$0.624
	(% of post-generic-entry spending)	(7.20%)	(7.29%)	(9.16%)	(4.01%)

Dollar values are in millions. "All drugs" includes the 22 drugs that experienced generic entry between 2007 and 2016. "Drugs that offer coupons" include only the 11 drugs that had a coupon available at generic entry. Estimates are calculated by first calculating the increase in brand spending, which is the sum of all post-generic-entry branded fills multiplied by the percent increase in branded drug use due to coupons from column 1 of Table 1.4 ($\frac{0.061}{1-(\text{average generic efficiency} = 0.62)} = 16\%$). To get the net increase in spending, I multiply the increase in brand-name spending by 0.66, which is the difference in cost between generic and brand-name drugs 1 year after generic entry [67].

1.7 Figures

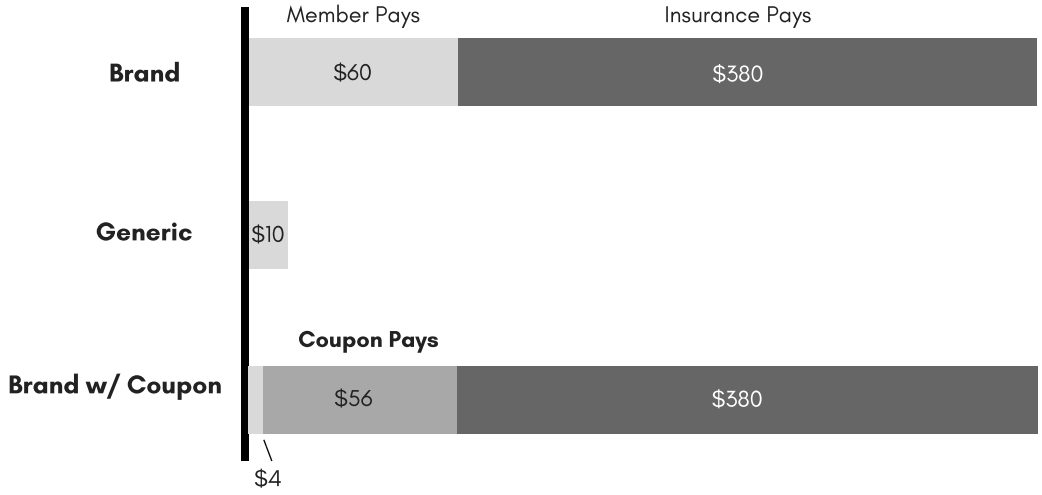

Figure 1.1: Coupon Costs

Figure presents an example of patient out-of-pocket costs and insurer costs in the presence of a coupon for a branded drug with a generic equivalent. "Member Pays" represents the patient's out-of-pocket costs; "Coupon Pays" represents the amount subsidized by the pharmaceutical company through the coupon. In this example, the copayment for the generic drug represents an overpayment, in which the patient out-of-pocket cost exceeds the cost to the insurer. Van Nuys and coauthors 2018a estimate that this occurs in 28% of generic drug fills. While it is not always the case that the insurer cost for the generic is $0, in most cases the insurer cost for the generic is significantly less than the cost for the brand-name equivalent.

Figure 1.2: Lipitor Coupon

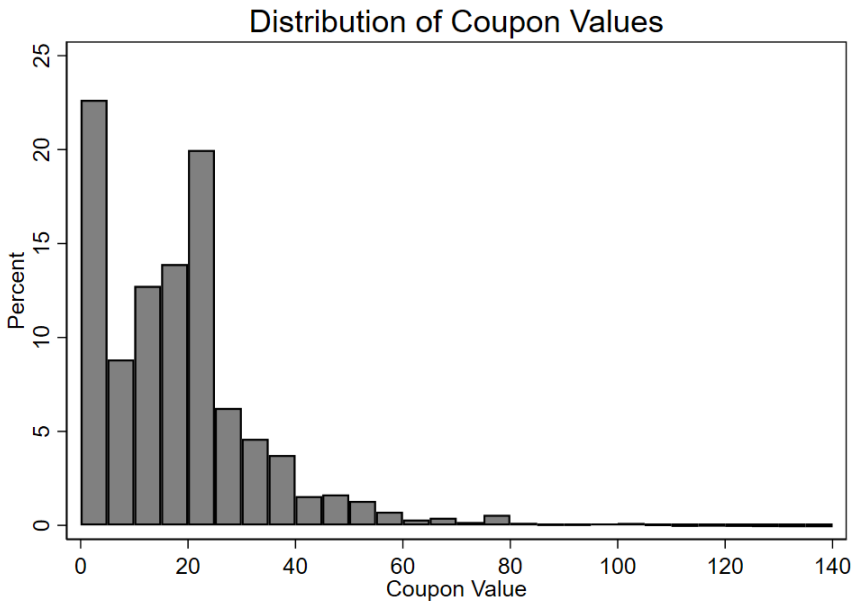

Figure 1.3: Distribution of Coupon Values

Figure shows the distribution of coupon values. As redeemed coupons are not visible in the claims data, coupon values are calculated based on the copayment amounts for brand drug fills and the corresponding available coupon. The figure represents the distribution of coupon values that would occur if brand fills in the data were always used in conjunction with the drug's coupon.

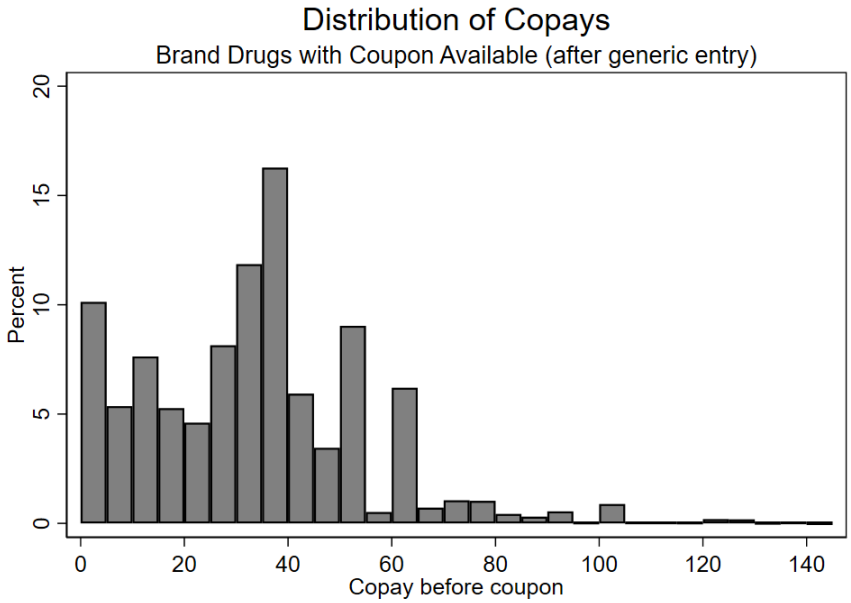

Figure 1.4: Distribution of Copays, Brand Drugs with Coupon Available (after generic entry)

Figure shows the distribution of copayments for post-generic-entry brand drug fills for drugs that offer coupons. The copayment amount does not take into account any available coupons.

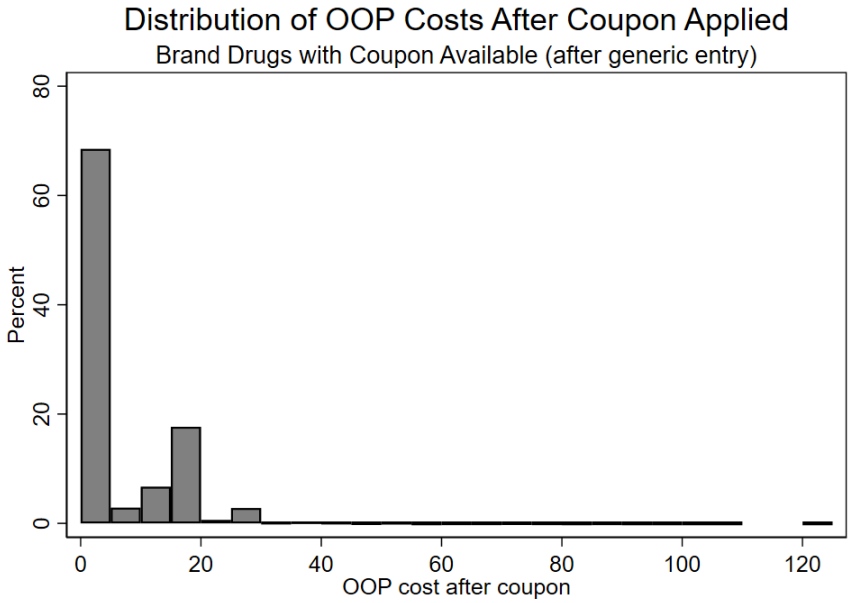

Figure 1.5: Distribution of OOP Costs After Coupon Applied, Brand Drugs with Coupon Available (after generic entry)

Figure shows the potential distribution of final out-of-pocket (OOP) costs for post-generic-entry brand drug fills, subtracting the available coupon amount from the copayment. This represents the distribution of OOP costs that would occur if everyone in the sample who filled a prescription for a post-generic-entry brand drug used a coupon when it was available.

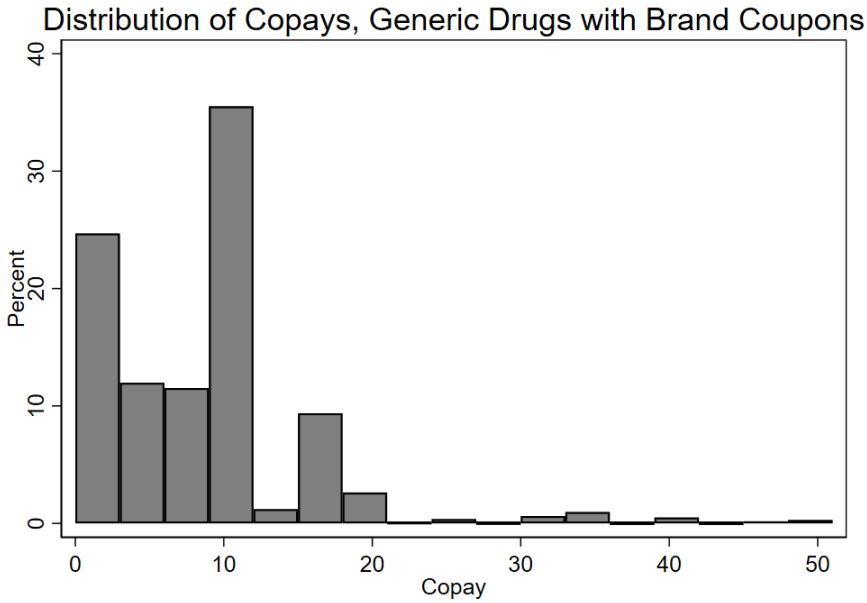

Figure 1.6: Distribution of Copays, Generic Drugs with Brand Coupons

Figure shows the distribution of copayments for generic fills when the equivalent brand drug has a coupon available.

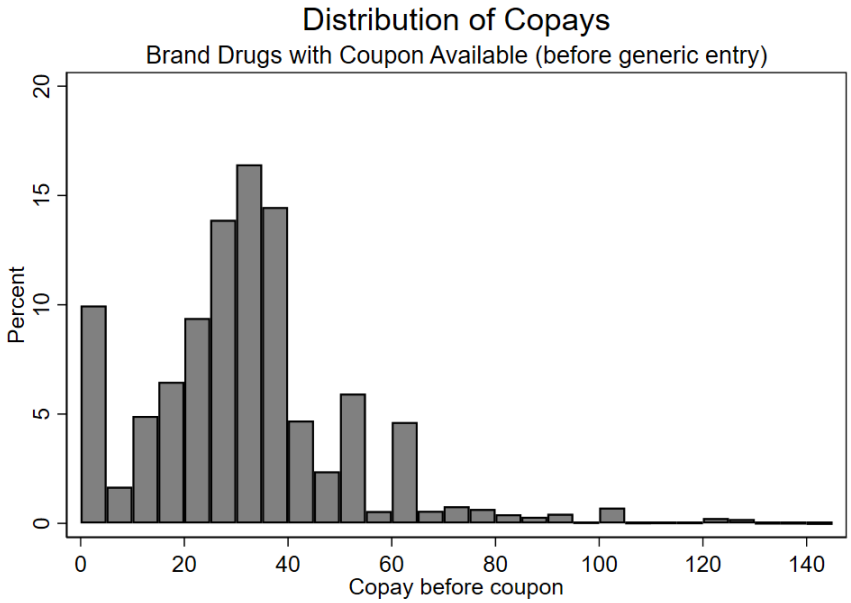

Figure 1.7: Distribution of Copays, Brand Drugs with Coupon Available (before generic entry)

Figure shows the distribution of copayments for pre-generic-entry (patent-protected) brand drug fills for drugs that offer coupons. The copayment amount does take into account any available coupons.

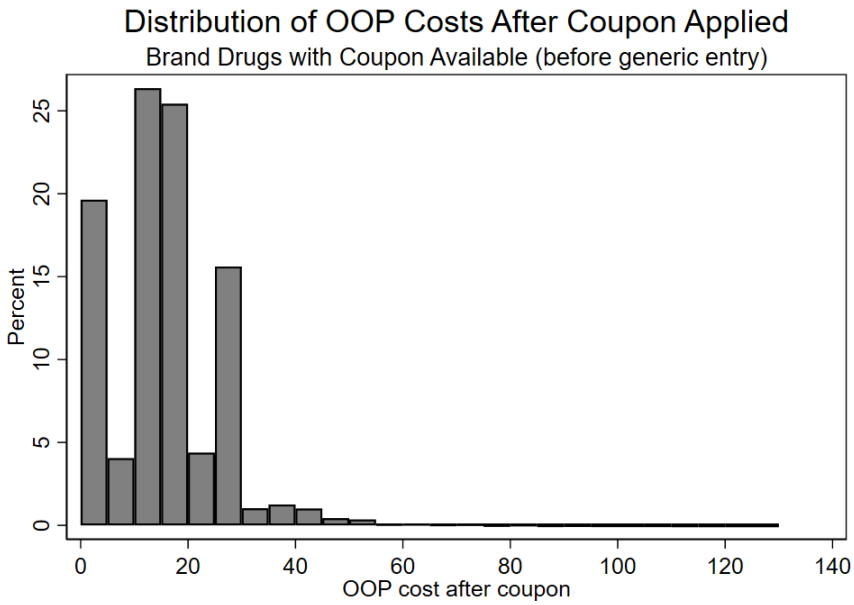

Figure 1.8: Distribution of OOP Costs After Coupon Applied, Brand Drugs with Coupon Available (before generic entry)

Figure shows the distribution of final out-of-pocket (OOP) costs for pre-generic-entry (patent-protected) brand drug fills, subtracting the available coupon amount from the copayment. This represents the distribution of OOP costs that would occur if everyone in the sample who filled a prescription for a patent-protected brand drug used a coupon when it was available.

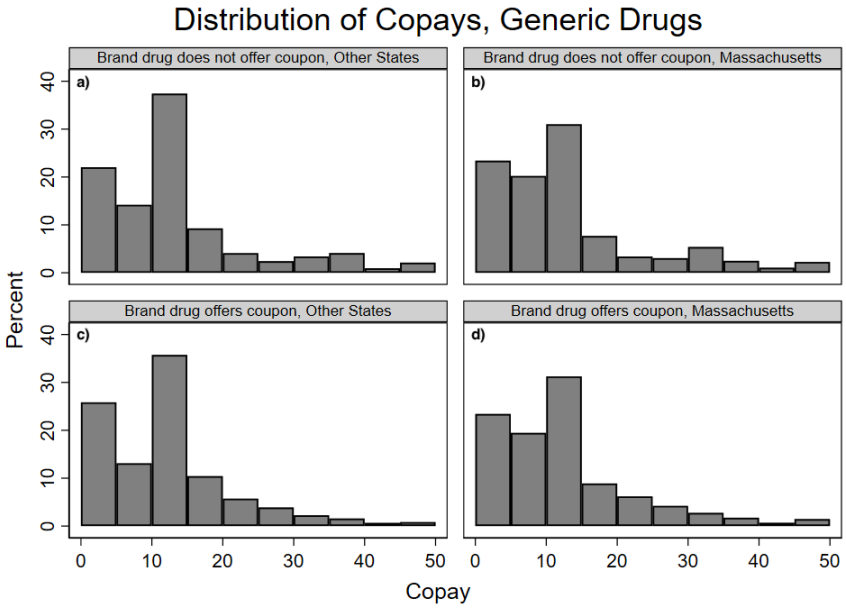

Figure 1.9: Distribution of Copays, Generic Drugs

Figure shows the distribution of generic copayments, stratified by state (Massachusetts compared to other states) and coupon status (drugs that offer coupons compared to drugs that do not).

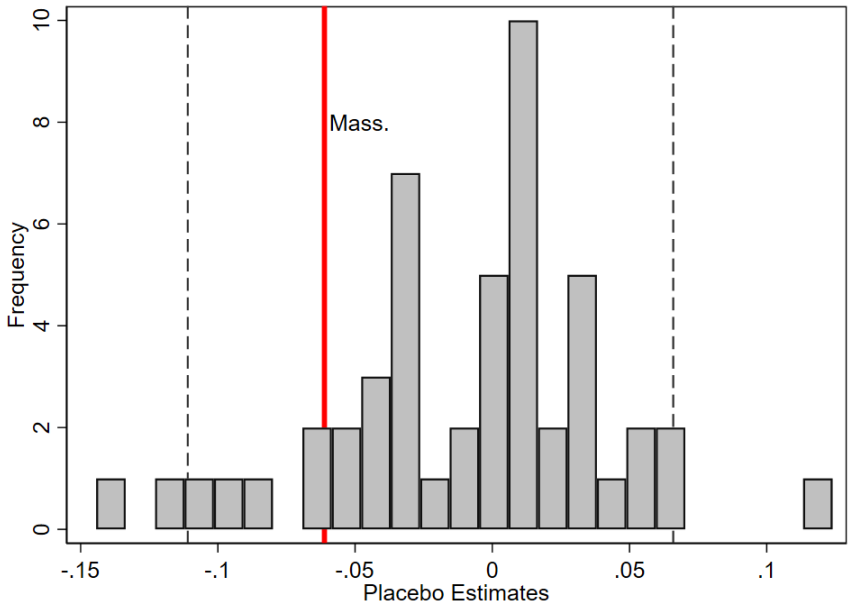

Figure 1.10: Placebo Estimates of the Effect of Coupons on Generic Efficiency

The figure above plots the distribution of 50 placebo estimates generated by estimating equation 1.1, simulating states other than Massachusetts as the treatment state. The dashed lines indicate the 5^{th} and 95^{th} percentiles, which represent the critical values for rejecting the null hypothesis that the Massachusetts effect is different from zero at a 10% significance level. The red solid line indicates the estimated effect using the actual treatment state (Massachusetts).

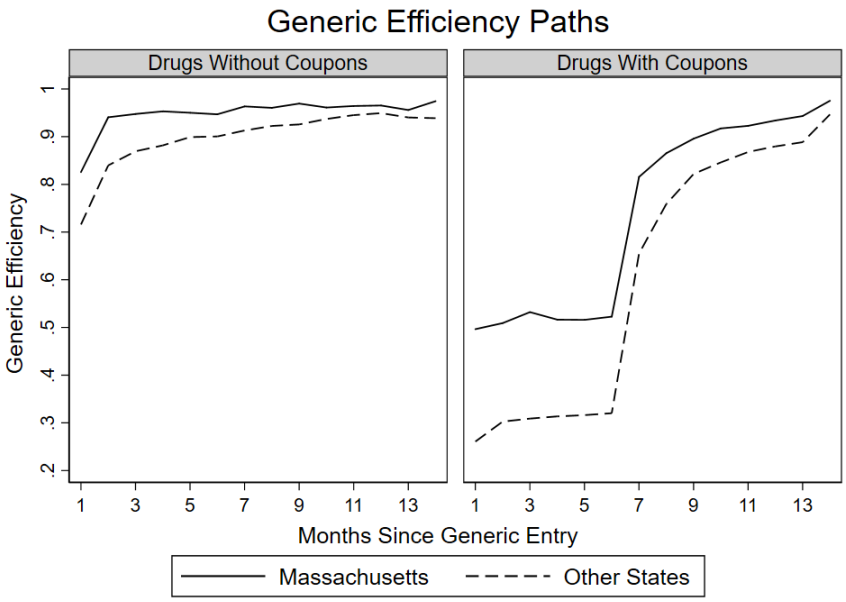

Figure 1.11: Generic Efficiency Paths by Coupon Availability

Figure above plots the average generic efficiency rate in each month following generic entry, separately for drugs that offer coupons (right panel) compared to drugs that do not (left panel). The solid line plots the average for Massachusetts, where coupons are illegal for drugs with a generic equivalent, and the dotted line plots the average for other states, where coupons are allowed.

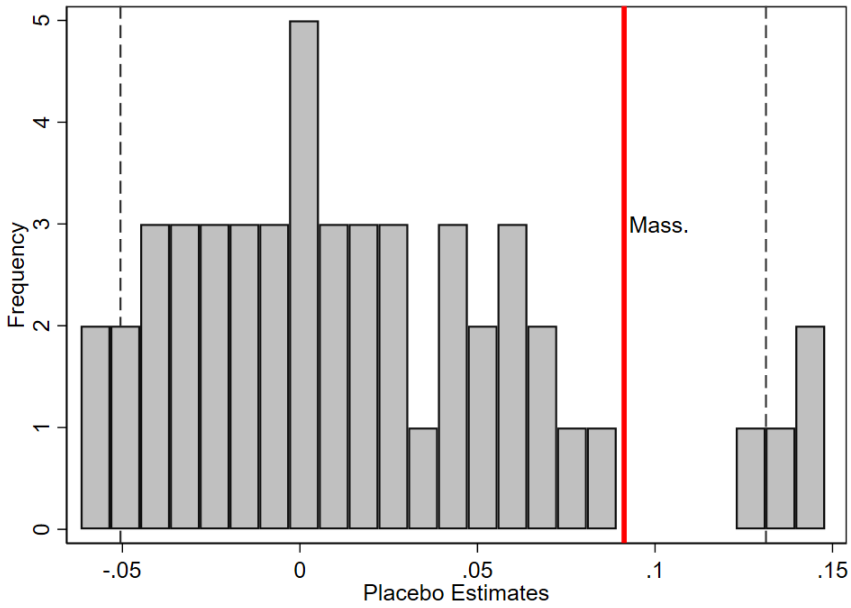

Figure 1.12: Placebo Estimates of the Effect of Coupons on Any Use of "Dispense as Written"

The figure above plots the distribution of 50 placebo estimates generated by estimating equation 1.1, simulating states other than Massachusetts as the treatment state. The dashed lines indicate the 5^{th} and 95^{th} percentiles, which represent the critical values for rejecting the null hypothesis that the Massachusetts effect is different from zero at a 10% significance level. The red solid line indicates the estimated effect using the actual treatment state (Massachusetts).

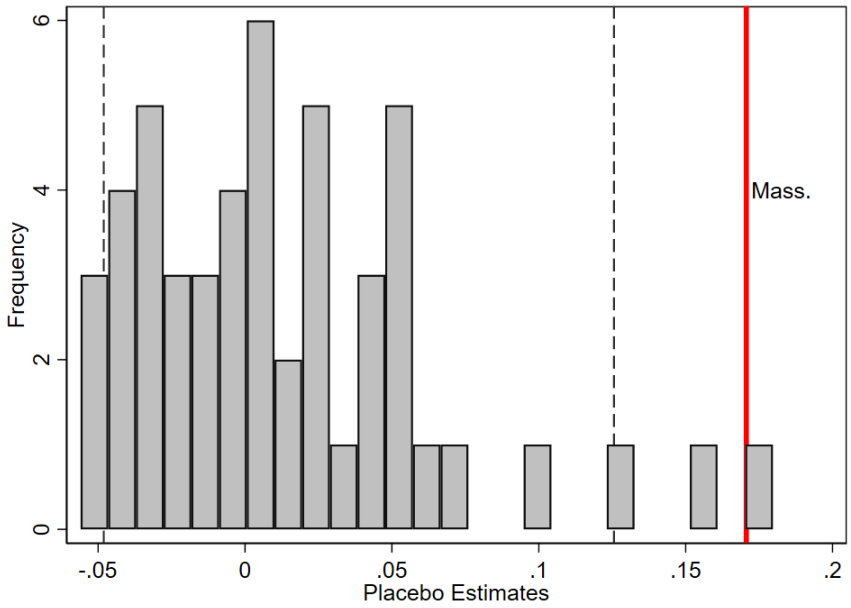

Figure 1.13: Placebo Estimates of the Effect of Coupons on Patient Request for Brand Drug

The figure above plots the distribution of 50 placebo estimates generated by estimating equation 1.1, simulating states other than Massachusetts as the treatment state. The dashed lines indicate the 5^{th} and 95^{th} percentiles, which represent the critical values for rejecting the null hypothesis that the Massachusetts effect is different from zero at a 10% significance level. The red solid line indicates the estimated effect using the actual treatment state (Massachusetts).

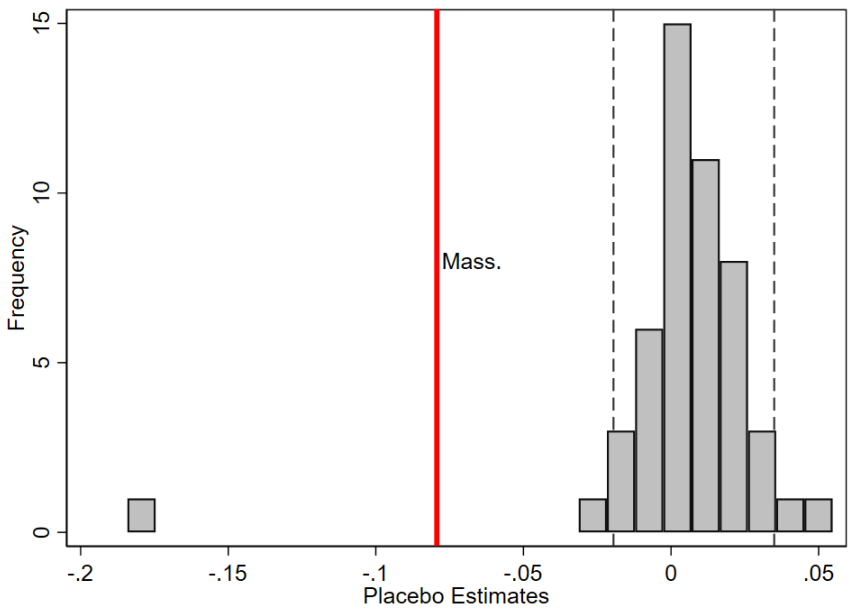

Figure 1.14: Placebo Estimates of the Effect of Coupons on Prescriber Use of "Dispense as Written"

The figure above plots the distribution of 50 placebo estimates generated by estimating equation 1.1, simulating states other than Massachusetts as the treatment state. The dashed lines indicate the 5^{th} and 95^{th} percentiles, which represent the critical values for rejecting the null hypothesis that the Massachusetts effect is different from zero at a 10% significance level. The red solid line indicates the estimated effect using the actual treatment state (Massachusetts).

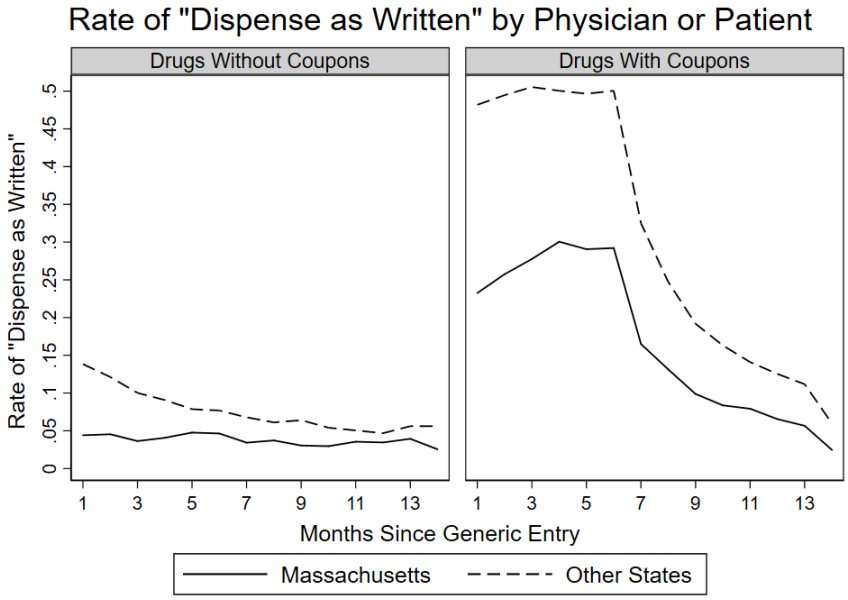

Figure 1.15: Use of "Dispense as Written" by Prescriber or Patient Following Generic Entry

The figure above plots the average proportion of fills where either the patient or physician indicates "dispense as written" on the prescription in each month following generic entry, separately for drugs that offer coupons (right panel) compared to drugs that do not (left panel). The solid line plots the average for Massachusetts, where coupons are illegal for drugs with a generic equivalent, and the dotted line plots the average for other states, where coupons are allowed.

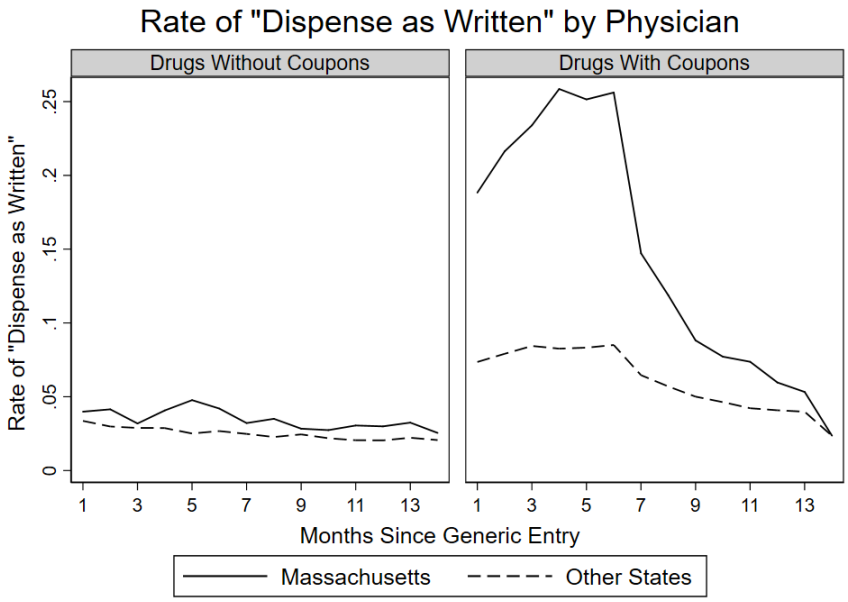

Figure 1.16: Use of "Dispense as Written" by Prescriber Following Generic Entry

The figure above plots the average proportion of fills where the physician indicates "dispense as written" on the prescription in each month following generic entry, separately for drugs that offer coupons (right panel) compared to drugs that do not (left panel). The solid line plots the average for Massachusetts, where coupons are illegal for drugs with a generic equivalent, and the dotted line plots the average for other states, where coupons are allowed.

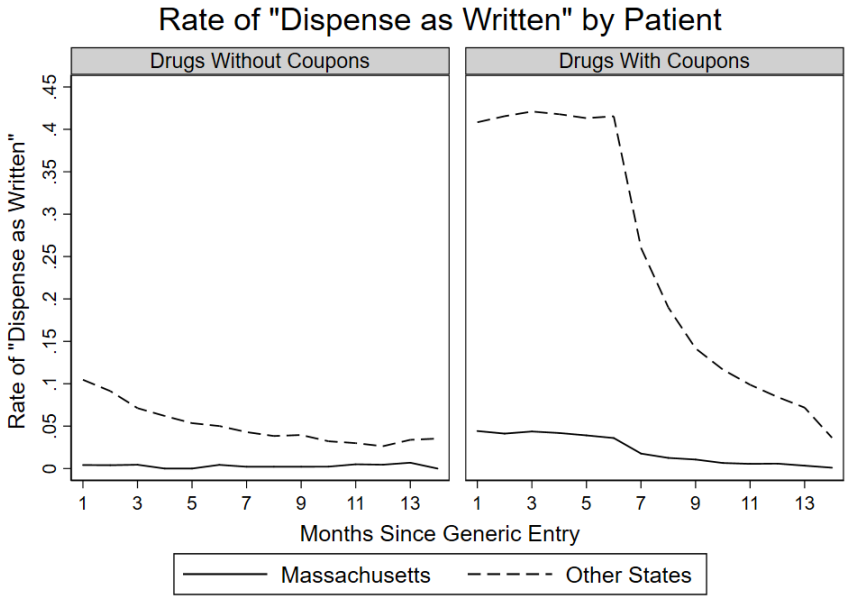

Figure 1.17: Use of "Dispense as Written" by Patient Following Generic Entry

The figure above plots the average proportion of fills where the patient requests the brand in each month following generic entry, separately for drugs that offer coupons (right panel) compared to drugs that do not (left panel). The solid line plots the average for Massachusetts, where coupons are illegal for drugs with a generic equivalent, and the dotted line plots the average for other states, where coupons are allowed.

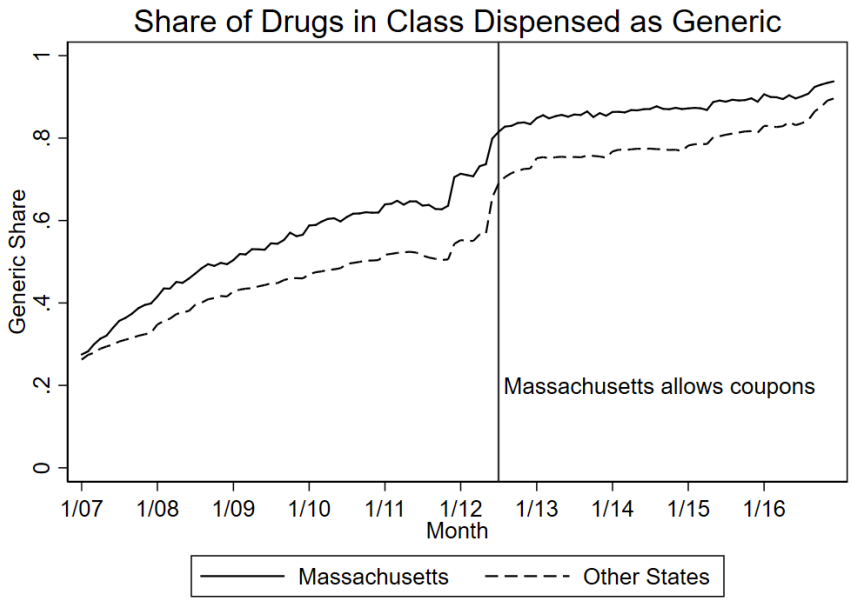

Figure 1.18: Share of Drugs in Class Dispensed as Generic

The figure above plots the average share of drugs in a given drug class that are dispensed as generic in each month. The solid line plots the average for Massachusetts, where coupons were allowed for drugs without a generic equivalent starting in July 2012; the dotted line plots the average for other states, where coupons have always been allowed.

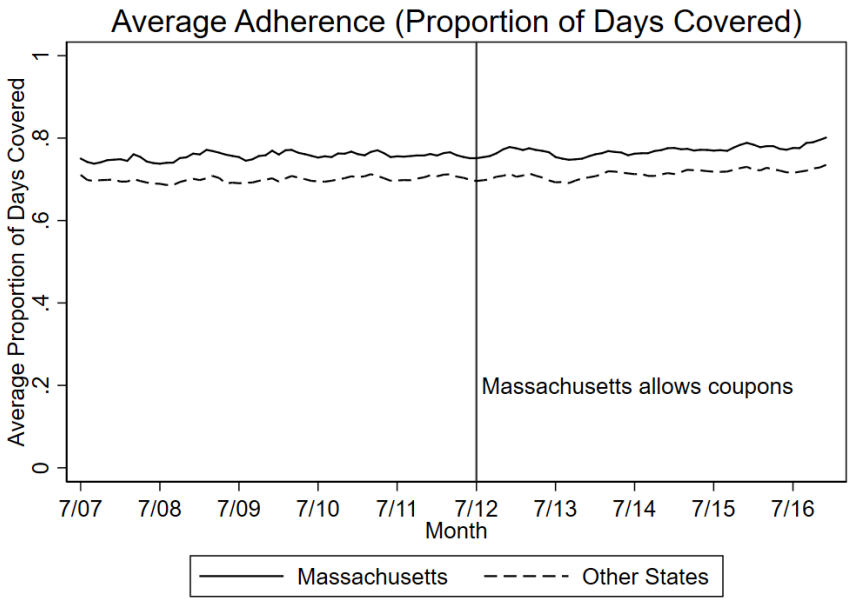

Figure 1.19: Average Proportion of Days Covered

The figure above plots the average medication adherence (measured by the proportion of days covered) over time for Massachusetts, where coupons went from banned to allowed for drugs without a generic equivalent starting in July 2012, and all other states, where coupons have always been allowed.

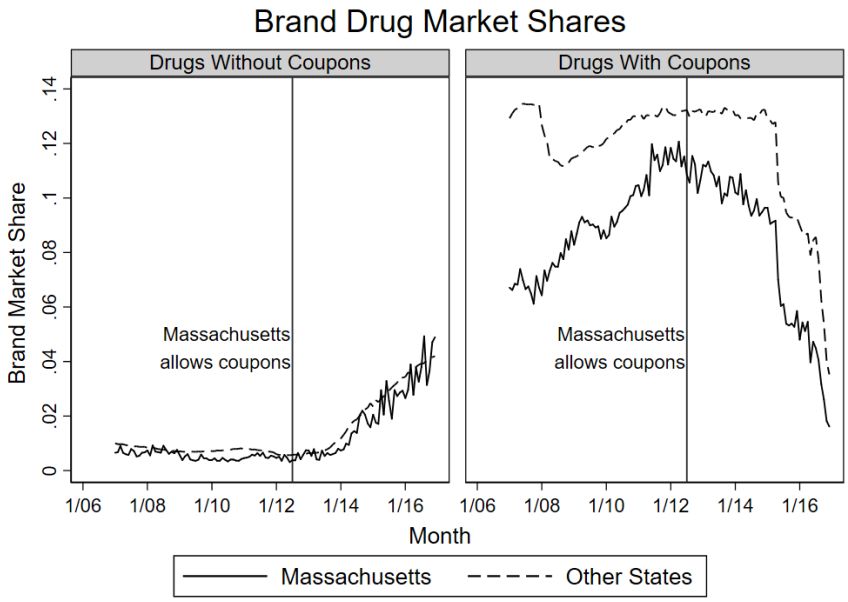

Figure 1.20: Market Share of Patent-Protected Drugs by Coupon Availability

The figure above plots the average market share of patent-protected drugs over time, measured by the number of fills of drug d divided by the total number of fills in the given drug class. The figure on the right plots the average for drugs that have coupons, and the figure on the left plots the average for drugs that do not have coupons. The solid line plots the average for Massachusetts, where coupons became legal for drugs without a generic equivalent starting in July 2012; the dotted line plots the average for other states, where coupons have always been allowed.

1.8 Appendix

1.8.1 Appendix Tables

Table 1.A1: Effect of Coupons on Generic Efficiency, Dropping Drugs

Drug Dropped	Coef	S.E.
ADAPALENE 0.1 CREAM	-0.0627***	0.00583
ADAPALENE 0.1 GEL	-0.0654***	0.0058
ADAPALENE 0.3 GEL (coupon drug)	-0.0594***	0.00561
AMLODIPINE ATORVASTATIN (coupon drug)	-0.0607***	0.00582
ARIPIPRAZOLE (coupon drug)	-0.0641***	0.00713
ATORVASTATIN CALCIUM (coupon drug)	-0.0462***	0.00464
CLOZAPINE ODT	-0.0612***	0.00589
DOXYCYCLINE 150 DR (coupon drug)	-0.0610***	0.00588
FLUVASTATIN ER	-0.0616***	0.00589
FLUVASTATIN SODIUM	-0.0622***	0.00573
METRONIDAZOLE 1 GEL (coupon drug)	-0.0612***	0.0059
MINOCYCLINE ER (coupon drug)	-0.0621***	0.00605
OLANZAPINE	-0.0624***	0.00579
PALIPERIDONE ER	-0.0613***	0.0059
PIMOZIDE	-0.0607***	0.00592
QUETIAPINE FUMARATE	-0.0416***	0.0055
RISPERIDONE	-0.0652***	0.00602
RISPERIDONE M TAB	-0.0615***	0.00587
TRETINOIN 0.04 MICRO GEL (coupon drug)	-0.0614***	0.00593
TRETINOIN 0.05 GEL (coupon drug)	-0.0610***	0.00587
TRETINOIN 0.1 GEL (coupon drug)	-0.0614***	0.00593
ZIPRASIDONE HCL (coupon drug)	-0.0613***	0.00597

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

Table presents estimates of β from equation 1.1, the intent-to-treat estimate of the effect of coupons on generic efficiency (ratio of generic fills to total fills), dropping drugs on at a time. The sample includes a balanced panel of drugs observed for 14 months following generic entry. The outcome is aggregated by drug, state, event-time, and regressions are weighted by the number of fills in a drug-state-event time. Standard errors are estimated using two-way clustering by drug and state.

Table 1.A2: Effect of Coupons on DAW Rate, Dropping Drugs

	Prescriber or Patient		Patient		Prescriber	
Drug Dropped	**Coef**	**S.E.**	**Coef**	**S.E.**	**Coef**	**S.E.**
ADAPALENE 0.1 CREAM	0.0936***	0.00502	0.174***	0.00599	-0.0805***	0.00645
ADAPALENE 0.1 GEL	0.0959***	0.0051	0.174***	0.00611	-0.0783***	0.00636
ADAPALENE 0.3 GEL (coupon)	0.0897***	0.00479	0.172***	0.00619	-0.0823***	0.00626
AMLODIPINE ATORVASTATIN (coupon)	0.0911***	0.00515	0.170***	0.00609	-0.0794***	0.00637
ARIPIPRAZOLE (coupon)	0.0945***	0.00623	0.182***	0.00726	-0.0872***	0.00593
ATORVASTATIN CALCIUM (coupon)	0.0787***	0.00643	0.0477***	0.00576	0.031	.
CLOZAPINE ODT	0.0913***	0.00518	0.171***	0.00609	-0.0793***	0.00637
DOXYCYCLINE 150 DR (coupon)	0.0911***	0.00516	0.171***	0.00614	-0.0794***	0.00639
FLUVASTATIN ER	0.0913***	0.00524	0.171***	0.00612	-0.0796***	0.00637
FLUVASTATIN SODIUM	0.0915***	0.00508	0.171***	0.00604	-0.0799***	0.00635
METRONIDAZOLE 1 GEL (coupon)	0.0915***	0.0052	0.171***	0.00608	-0.0795***	0.00638
MINOCYCLINE ER (coupon)	0.0904***	0.00509	0.171***	0.00613	-0.0804***	0.00622
OLANZAPINE	0.0929***	0.0048	0.170***	0.00575	-0.0769***	0.00596
PALIPERIDONE ER	0.0915***	0.00524	0.171***	0.00607	-0.0798***	0.00639
PIMOZIDE	0.0910***	0.00524	0.171***	0.00613	-0.0800***	0.00636
QUETIAPINE FUMARATE	0.0794***	0.00532	0.159***	0.00591	-0.0794***	0.00555
RISPERIDONE	0.0888***	0.00505	0.167***	0.00553	-0.0786***	0.00639
RISPERIDONE M TAB	0.0911***	0.00521	0.171***	0.00608	-0.0798***	0.00636
TRETINOIN 0.04 MICRO GEL (coupon)	0.0913***	0.0052	0.171***	0.00612	-0.0795***	0.00637
TRETINOIN 0.05 GEL (coupon)	0.0911***	0.00514	0.171***	0.00608	-0.0795***	0.00637
TRETINOIN 0.1 GEL (coupon)	0.0915***	0.00524	0.171***	0.00613	-0.0794***	0.00638
ZIPRASIDONE HCL (coupon)	0.0920***	0.00536	0.173***	0.00623	-0.0808***	0.00631

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

Table presents estimates of β from equation 1.1, the intent-to-treat estimate of the effect of coupons on the "dispense as written rate", dropping drugs one at a time.

1.8.2 Measurement of Adherence

Medication adherence is defined as adhering to a medication regimen recommended by the prescriber. However, in working with claims data, I cannot see the physician's prescribed regimen. For this reason, I must make some assumptions when calculating adherence. Adherence is measured by calculating the number of days supplied a patient has on hand in any given month. Because a patient doesn't always fill a prescription every 30/90 days on the dot, I make adjustments for fills that overlap to make sure that the proportion of days covered is not over- or under-estimated. Additionally, I have to make assumptions when an individual has overlapping or adjacent fills of different medications within the same class. The adjustments I make are:

- If a person fills a 30-day supply of Lipitor on 7/1 and another 30-day supply on 7/29, I adjust the fill date on the second prescription so that it looks like it was filled on the day the person ran out of the first prescription (7/1 + 30 days = 7/31).
- If a person fills a 30-day supply of Lipitor on 7/1 and a 30-day supply of Crestor on 7/15, I assume they are switching therapies and begin their Crestor prescription on the day it is filled, throwing out the remaining Lipitor pills. The days-supplied of the 7/1 Lipitor prescription is adjusted to account for the therapy switch so as not to double-count the Lipitor and Crestor prescriptions in the adherence calculations.
- Adherence is only measured for person-months where they:
  - have an eligible plan for the entire month

  - have previously filled a prescription in the given class in the last 6 months

- The month in which the first prescription is filled is omitted from regressions so as to not over- or under-inflate adherence in that month.

1.8.3 Determining Dates of Generic Entry

The process for determining dates of generic entry was as such:

1. For each molecule (active ingredient)-dosage form (molecule-dosage form-strength for the acne drug class), the dates of first generic approval were determined from the FDA's "First Generic Drug Approvals" list (see here).
2. The data contain an indicator variable for whether a filled drug is multi-sourced. This variable allows me to imprecisely determine whether a generic version of a given drug is available at a certain point in time. Using this variable, I determined the date at which a given drug (molecule-dosage form) becomes multi-sourced.
3. To determine whether a prescription fill is for a generic drug or a brand name drug, I compare the name in the field "brnd_nm" with the name in the field "gnrc_nm." If the names are equal (or highly similar, i.e., a reordering of words, etc.), the fill is considered generic. If they are not the same, the fill is considered a brand fill. Using this method, I determine when the first generic fill occurs in the data for each drug.
4. I compare the dates calculated in steps 1, 2, and 3.

(a) If the dates are close to each other, I assign the date of generic entry as the FDA generic approval date.

(b) If the dates are not close to each other, I manually search the web to determine the date of generic entry.

CHAPTER 2

HOSPITAL OWNERSHIP AND ADMISSION THROUGH THE EMERGENCY DEPARTMENT

ESSAYS IN HEALTH ECONOMICS

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As the number of for-profit hospitals has increased over the last few decades, the debate over whether for-profit hospitals are any different from nonprofit hospitals has continued. Many opponents of for-profit hospitals point to a number of whistleblower lawsuits in which the Department of Justice alleges that certain for-profit hospital chains pressure physicians to admit patients through the emergency department when it is not medically necessary [34–37]. There has been a similar debate within economics, with numerous studies providing largely mixed results. Most of the previous studies have focused on patients in the inpatient setting; we do not whether ownership affects the likelihood that an emergency department patient becomes an inpatient, which is the route of admission for approximately 50% of inpatients [59]. In this paper, I estimate the extent to which for-profit hospital ownership affects the probability that a patient who visits an emergency department is admitted to the hospital. Leveraging within-hospital variation induced by ownership changes, I find that for-profit ownership increases the probability of inpatient admission by 2.3 percentage points (13%) for Medicare patients and 1.4 percentage points (8.2%) for Medicaid patients, with the largest increases occurring in diagnosis categories such as abdominal pain (2.8 percentage points/90%), other injuries and conditions due to external causes (1.5 percentage points/47%) and nonspecific chest pain (8.3 percentage points/46%). However, I also find evidence that increased admission rates occur when hospitals convert *from* for-profit as well as *to* for-profit, indicating that the estimated effect may actually be measuring the effect of hospital system membership and not ownership.

2.1 Introduction

Over the past two decades there has been a steady increase in the number of for-profit hospitals: between 1999 and 2014 the number of for-profit hospitals increased by 77% , from 747 to 1,322 [71]. At the same time, government scrutiny of for-profit hospitals has increased, with the Department of Justice intervening in lawsuits against for-profit hospital chains, alleging that they pressure physicians to admit emergency department (ED) patients to the hospital when it is not medically necessary [25]. While the inherence of asymmetric information in the market for healthcare provides a ripe opportunity for provider-induced demand, it is unclear whether the decision to act on this opportunity differs between for-profit, nonprofit, and government owned hospitals. In this paper, I investigate whether patients are more likely to be admitted to the hospital from the ED if they arrive in a for-profit hospital's ED compared to a nonprofit or government ED.

While the effect of hospital ownership on the probability of admission has not been studied in the previous literature, there has been much other work looking at the effects of hospital ownership on patient outcomes and costs. These papers have presented largely mixed results, with some studies finding significant differences by ownership [17, 21, 40, 50, 54, 63, 102, 109] and others finding no differences [9, 73, 88, 97, 110, 113, 116]. Sloan and coauthors 2001, 2003 find that for-profit hospitals increase inpatient Medicare costs without improving outcomes such as mortality or readmission. Additionally, for-profit hospitals are more likely to employ profitable procedures, many of which are expensive and high-tech [63, 117]. While these studies have shown an increase in costs and no difference in outcomes, other work has shown that hos-

pitals converting to for-profit status experience increased mortality rates, with no increase in costs but increased profitability [102]. Additionally, researchers have shown that for-profit hospitals are more likely to "upcode" patients into higher reimbursing diagnosis-related groups (DRGs) [33, 114]. While most of this previous research has focused on Medicare patients, there is some evidence that for-profit hospitals have different preferences for insurance types than non-profit hospitals. Norton and Staiger 1994 show that for-profit hospitals avoid the uninsured by locating in better-insured areas. Additionally, there is evidence that for-profit hospitals located in markets with nonprofit hospitals are more likely to avoid uninsured patients following an increase in the local uninsured rate [52].

The majority of the previous research examining differences between hospital ownership types has focused on Medicare patients in the inpatient setting. To my knowledge, no studies have examined whether hospital ownership affects the decision to admit a patient from the emergency department to the hospital. The emergency department is the source of inpatient admission for roughly half of patients [59]. Furthermore, inpatient admissions tend to produce profits, while ED-only visits ("treat and release"), tends to produce loses [90]. Given this, in addition to the allegations against for-profit hospitals, understanding whether for-profit hospital ownership affects the decision to admit patients through the ED is an important avenue for empirical research.

In this paper, I estimate the effect of hospital ownership on the probability that a patient is admitted as an inpatient in the same hospital in which they present to the ED. I use hospital discharge records from AHRQ's Healthcare Cost and Utilization Project in conjunction with data from the American Hos-

pital Association's annual survey to estimate linear probability models of the probability of inpatient admission. I use two methods to estimate the effects: the first takes a subset of patients arriving at the emergency department for injuries and estimates models conditioning on injury severity to see whether hospital ownership affects the probability of inpatient admission. The second method takes advantage of variation induced by hospitals changing ownership status and estimates two-way fixed effect difference-in-differences regressions and event studies.

Among patients visiting the ED for injuries, I find no significant difference in the probability of admission between nonprofit and for-profit hospitals after conditioning on the severity of a patient's injuries. In this sample, government hospitals have the highest probability of admitting patients. As these results require the assumption that hospital ownership is exogenous conditional on control variables, the more rigorous evidence comes from difference-in-differences analyses using variation induced by hospitals changing ownership. In these analyses, I find that for-profit hospitals have a 0.9 percentage point (5%) increase in the probability of admission compared to nonprofit hospitals, and a 2.4 percentage point (14%) increase compared to government hospitals. These results appear to be driven by Medicare and Medicaid patients, with no significant differences between ownership categories for privately insured or uninsured patients. The largest increases in admission occur in diagnosis categories such as abdominal pain, nonspecific chest pain, and other injuries due to external causes. However, separating the regressions by different types of hospital conversions (e.g. nonprofit to for-profit versus for-profit to nonprofit) and generating event studies shows that the increase in probability of admission is observed in both conversions *to* for-profit and *from* for-profit. As most of the

ownership changes are induced by hospitals becoming part of a larger chain, these results indicate that the estimated increase in probability of admission due to hospital ownership could instead be due to the effects of being part of a hospital chain. Future research should include hospitals becoming part of a chain, but not changing ownership, to determine whether these effects are observed among those hospitals as well. Finally, I find mixed results with respect to the effect of hospital ownership on other outcomes such as average DRG weight, probability of a surgical DRG, probability of a transfer, and the number of diagnoses. There is some evidence that for-profit ownership increases the probability of transfer from the inpatient setting, but more research is needed to make any strong conclusions.

2.2 Background

Hospital ownership, which can be either for-profit, nonprofit, or government owned, has implications for profit motives stemming from legal requirements on profit distribution, tax treatment, and financing. Nonprofit hospitals are barred from distributing profits to shareholders and must provide "community benefit", a vaguely defined requirement that includes things such as providing uncompensated care as well as physician education. In return, nonprofit hospitals enjoy tax exemptions on revenue, property, and donations, and have access to tax-exempt financing. There is no limit on the amount of profit nonprofit hospitals can earn; the requirement is that any profit be reinvested into hospital operations, which includes things such as executive reimbursement [24]. In contrast, for-profit hospitals are not tax-exempt but are allowed to distribute profits to shareholders or owners. They are not required to provide commu-

nity benefits. Government hospitals are similar to nonprofit hospitals in that they cannot distribute profits to the governing board, but any profit generated is reinvested in the hospital or returned to the sponsoring entity, which is often the local government. Usually, government hospitals have more regulations regarding employee compensation than nonprofits [115].

These legal distinctions have the potential to influence executive behavior in different ways. A CEO whose job security is determined by a Board who is beholden to shareholders may act very differently from a CEO who is judged by a board whose main interest is providing community benefit. While there has been a debate within economics about whether hospitals of different ownership types behave differently, there has also been a public debate over whether hospitals should be allowed to operate as for-profit entities. Opponents of for-profit hospitals point to the many different lawsuits that have been brought against for-profit hospital chains alleging illegal misconduct. While there are many different types of misconduct that have been alleged, the lawsuits relevant to this paper are those alleging that certain for-profit hospital chains pressure physicians into admitting ED patients to the hospital when it is not medically necessary.

Many of these lawsuits have resulted from whistleblower cases filed under the False Claims Act, which prohibits defrauding government programs, including Medicare and Medicaid. These cases are usually brought by current or former employees with knowledge of the misconduct, who are oftentimes employees who have been fired for speaking out against it. Since at least 2012, there have been many cases brought against multiple different for-profit hospital chains alleging medically unnecessary hospital admissions for patients orig-

inating in the ED. While some investigations are ongoing and/or not public, many have resulted in settlements ranging up to $260 million [34–37]. Furthermore, these practices have been the subject of investigative reporting, with CBS' 60 minutes featuring the case of Health Management Associates (HMA), a company that recently settled False Claims Act allegations for $260 million [35, 91]. In the lawsuits and the 60 minutes piece, emails from hospital administrators to ED staff were presented as evidence, with lines such as, "Only 14 admits so far!!!!!!! Act accordingly....", as well as emails to physicians who are "underperforming" in terms of admission rates: "Every time a 65 year old or older comes in, I am already thinking, do they have some condition I can admit them for? [...] I have been told to replace you if your numbers do not improve." Additionally, HMA is said to have implemented electronic health record software that prompted physicians to reconsider their decisions to send patients home, alerting them with popups if the patient has a condition that qualifies for admission. In these lawsuits, the company is accused of having admission targets of 20% overall, and 50% for Medicare patients, which physicians have said is medically unnecessary and potentially dangerous as unnecessary hospitalizations can lead to hospital-acquired infections.

While much of the public debate over for-profit hospitals has focused on legal actions and anecdotal evidence of misconduct, the economics literature has much to contribute to the debate. Seminal articles include those by Norton and Staiger 1994 as well as Sloan and coauthors 2001, 2003, which have shown that for-profit hospitals avoid uninsured patients by locating in areas with low uninsurance rates, use more expensive procedures, and have higher costs, but similar outcomes. Follow up work by Picone and coauthors 2002 finds opposite results: hospitals converting from nonprofit to for-profit have higher rates

of mortality than hospitals converting away from for-profit status, with no differences in costs. In addition to these studies, researchers have looked at differences in rates of "upcoding", in which hospitals code patients into diagnosis categories with higher reimbursement rates, finding that for-profit hospitals are more likely to engage in the process than nonprofit hospitals [33, 114].

Other work looking at differences in hospital behavior across ownership type has also found mixed evidence. Many studies have found that nonprofit and for-profit hospitals behave similarly [9, 73, 88, 97, 110, 113, 116], while others have found that for-profit hospitals offer more high-tech and profitable services [63] and less uncompensated care [21, 50, 63, 109]. While most of these articles focus on differences between nonprofit and for-profit hospitals, Duggan 2000 compares all three hospital ownership types and finds that nonprofits and for-profits are both less likely to treat indigent patients than government hospitals. Researchers have also looked at whether hospital ownership differences are influenced by market structure, finding that nonprofits and for-profits behave similarly when they are competing in the same local hospital market, though nonprofits absorb the bulk of increases in demand from uninsured patients [31, 41, 52, 64, 74].

The bulk of the early work on differences between hospitals by profit status focused on patient-level outcomes among Medicare recipients. A related and more recent literature has examined differences between for-profit and nonprofit hospitals and their responses to exogenous fixed-cost shocks. Analyzing hospital responses to a seismic retrofitting mandate in California, Chang and Jacobson 2012 find that nonprofit hospitals increased their provision of profitable services, government hospitals decreased their offering of charity care, and for-

profit hospitals did not change their service offering.

While all of this work has informed the debate over whether for-profit hospital ownership results in meaningful differences compared to nonprofits, we have yet to determine whether ownership affects the likelihood of admission into the hospital. As the emergency department is the source of approximately 50% of inpatient admissions, focusing on the emergency department is an important avenue for empirical research [59]. In this paper, I estimate whether hospital ownership affects the probability of inpatient admission for patients arriving in hospital emergency departments. I find some evidence that for-profit ownership increases the probability of inpatient admission for Medicare and Medicaid patients who arrive in the ED, however more research is needed to determine whether this result is due to the effects of for-profit ownership as opposed to the effect of being part of a large hospital chain.

2.3 Data

In this paper I use two main sources of data: hospital discharge records from the Healthcare Cost and Utilization Project from the Agency for Healthcare Research and Quality (AHRQ), and the American Hospital Association (AHA) Annual Survey. The hospital discharge data come from two sources within HCUP: the State Emergency Department Databases (SEDD) and the State Inpatient Databases (SID). The SEDD contains information on patient encounters that originate in the emergency department and do not result in hospital admission, while the SID contains information on all inpatient encounters and indicates where they originated (e.g. the emergency department, transfer, etc.).

Both the SID and the SEDD contain a near universe (approximately 97%) of all hospital discharge records occurring in a given state. Combined, the SEDD and the SID contain all discharge records for patients visiting the emergency department. 19 of the AHRQ partner states release AHA identifiers, which allows linkage to the AHA Annual Hospital Survey. I use discharge data merged with AHA data from six states between 2005 and 2013: Arizona, California, Florida, Kentucky, Maryland, and New Jersey. Table 2.2 shows the included years for each state. In these states, the AHA identifiers allow me to determine the ownership type of each hospital in the discharge data. I use the AHA identifiers to determine hospital ownership changes and confirm these changes by manually collecting news articles related to the hospital ownership changes. I use these articles to both confirm that the hospital changed ownership and determine the quarter in which the change took place.

The hospital discharge data contains information on a patient's encounter in the hospital, including demographic information such as age, gender, and insurance type, as well as information specific to the hospital visit, such as diagnoses, procedures, and whether the patient was admitted to the hospital as an inpatient, or treated and released from the emergency department. The discharge data contains an AHA identifier that identifies the hospital a patient was treated at, which allows me to link the data with the AHA Annual Survey and determine hospital characteristics such as ownership and capacity.

Table 2.3 presents summary statistics for the hospital discharge data merged with the AHA annual survey data. On average 17% of patients who show up in the emergency room are admitted as inpatients to the same hospital. Approximately 32% of the discharges were paid for by private insurance, 21% were

paid for by Medicare, 24% were paid by Medicaid and 18% were uninsured. The average patient was 38 years old and slightly more than half were female. Overall, about 30% of visits occurred on the weekend. Among the 18% of patients who were admitted as inpatients, the average DRG weight was 1.29. The second panel of table 2.3 shows hospital variables. Consistent with the national breakdown of hospital ownership types, 68% of visits took place in a nonprofit hospital, 18% in a for-profit hospital, and 13% in a government hospital. The table also shows some additional hospital characteristics such as the average daily census, number of residents and RNs, and number of hospital beds.

2.4 Methods & Results

2.4.1 Controlling for Injury Severity

I take two approaches to estimate the effect of hospital ownership on admissions through the ED. In the first approach, I only include patients who visited the ED for an injury. In this sample I can control for the severity of the injury using the Injury Severity Score (ISS). The ISS provides a single score, ranging from 1 to 75, that takes into account the number, severity, and body region of a patient's injuries. It was developed empirically in the trauma literature to correlate linearly with mortality [8]. This variable is created using a publicly-available Stata program from AHRQ [20]. Patients with injuries are determined from the ICD-9 code of the first-listed diagnosis, which is the main reason for the ED visit or hospital admission. The list of ICD-9 codes identifying injuries can be found in Table 2.1.

In this first approach, I estimate two linear probability models for patient i who goes to the ED at hospital h in county c and year t. The first is:

$$\text{admit}_{ihtc} = \beta_0 + \beta_1 \cdot \text{forprofit}_h + \beta_2 \cdot \text{govt}_h + \boldsymbol{\beta}_3 \mathbf{X_i'} + \boldsymbol{\beta}_4 \mathbf{Z_{ht}'} + \delta_t + \gamma_c + \varepsilon_{ihtc} \quad (2.1)$$

where admit_{ihtc} is an indicator for whether the patient was admitted versus treated and released and forprofit_h and govt_h are indicators for hospital ownership. $\mathbf{X_i}$ is a vector of patient characteristics which include age dummies, gender, a weekend visit indicator, injury severity score, and insurance type. Dummy variables for categories of diagnoses occurring in more than 5% of the discharges are also included. $\mathbf{Z_{ht}}$ is a vector of time-varying hospital characteristics including average daily census, number of full-time residents and nurses, and number of beds. δ_t and γ_c are quarter-year and county fixed effects. Standard errors are clustered by hospital identifier.

With nonprofit hospitals as the omitted category, β_1 is the difference in admission probability in a for-profit hospital compared to a nonprofit hospital. Similarly, β_2 is the difference in admission probability in a government hospital compared to a nonprofit hospital, and $\beta_1 - \beta_2$ is the difference in admission probability in a for-profit compared to a government hospital.

The above equation indicates whether, conditional on injury severity, hospital ownership is correlated with the probability of inpatient admission. While this is an interesting question on its own, it is also useful to determine whether this association differs by insurance type. Previous work has indicated that for-profit hospitals are able to avoid uninsured patients by locating in well-insured areas and allowing nonprofit hospitals in the same hospital market to absorb increases in demand from uninsured patients [52, 97]. To answer this question, I estimate a similar linear probability model to that in equation 2.1, adding in

interactions between hospital ownership type and insurance status. Specifically, I estimate,

$$\text{admit}_{ihtc} = \alpha_0 + \boldsymbol{\alpha}_1[\mathbf{owner_h} \times \mathbf{payer_i}]' + \boldsymbol{\alpha}_2\mathbf{X_i'} + \boldsymbol{\alpha}_3\mathbf{Z_{ht}'} + \delta_t + \gamma_c + \varepsilon_{ihtc} \tag{2.2}$$

where $[\mathbf{owner_h} \times \mathbf{payer_i}]'$ is a vector of ownership and payer interactions. Payer consists of Medicare, Medicaid, private insurance, uninsured, and other insurance. $\mathbf{X_i}$, $\mathbf{Z_{ht}}$, δ_t, and γ_c are the same as in equation 2.1. The coefficients of interest are in the vector $\boldsymbol{\alpha}_1$. For example, if the omitted category is $\text{govt}_h \times \text{Medicaid}_i$, then the coefficient on $\text{forprofit}_h \times \text{Medicaid}_i$ is the difference in admission probability for a Medicaid patient in a for-profit hospital compared to a Medicaid patient in a government hospital.

Table 2.4 shows the summary statistics for the sample of patients who visit the ED for an injury, which is the sample used to estimate equations 2.1 and 2.2. Compared to the overall sample (Table 2.3), patients visiting the ED for an injury have a lower rate of admission: 5.6% compared to 17.5%. The highest admission rates occur in government owned hospitals, who admit approximately 7.7% of patients who come to the ED with an injury. This is consistent with the fact that the Injury Severity Score is highest in government hospitals. The majority of discharges in the sample occur at nonprofit hospitals, which is consistent with the distribution of hospital ownership in the U.S.. The breakdown of patients by payer is similar in the three different types of hospitals, though nonprofit hospitals see a higher proportion of privately insured patients and for-profit hospitals see more Medicare patients. Furthermore, for-profit hospitals tend to be smaller than nonprofit or government hospitals, with a lower average daily census, and fewer beds, residents, and nurses.

Table 2.5 includes the results from the regression of equation 2.1. While the coefficient on forprofit_h is positive, it corresponds to a less than 0.2 percentage point increase in the probability of admission and is not statistically significant, indicating that there is no significant difference between the probability of admission between nonprofit and for-profit hospitals after conditioning on injury severity. The standard errors indicate that this estimate is a somewhat precisely estimated null, with the 95% confidence interval allowing me to rule out effects greater than a 0.4 percentage point (7.5%) decrease, and a 0.9 percentage point (15.7%) increase in the likelihood of admission. Compared to government hospitals, for-profit hospitals are 0.9 percentage points *less* likely to admit patients to the hospital when they show up in the ED with an injury, and this estimate is significant at the 10% confidence level. These results indicate that after conditioning on injury severity and other patient and hospital characteristics, government hospitals have the highest probability of inpatient admission for patients who go to the emergency department for an injury.

Table 2.6 presents the results from comparing the interacted coefficients from equation 2.2 to determine the association between hospital ownership and the probability of admission for each different type of insurance. The results of this analysis are relatively similar to those in Table 2.5 and show that nonprofit hospitals and for-profit hospitals have relatively similar admission rates after conditioning on injury severity for patients who come to the ED with an injury. For privately insured, Medicaid, and other insured patients, government hospitals are *more* likely than both for-profit and nonprofit hospitals to admit a patient to the hospital conditional on injury severity. For Medicare and uninsured patients, there is no statistically significant difference in admission rates between any of the three hospitals types.

While these results provide important descriptive work, interpreting the results in Tables 2.5 and 2.6 as the causal effect of hospital ownership requires the strong assumption of unconfoundedness, that is, conditional on the covariates I control for, hospital ownership is exogenous. There are many plausible reasons why this assumption may not hold, including that patients who choose to go to a for-profit ED may differ from patients who choose to go to a nonprofit or government ED, in ways that are systematically related to the likelihood of hospital admission. They may be higher income or have fewer comorbidities, among other things, which could both be unobserved confounders. In the next section, I address this concern by taking advantage of variation induced by hospitals changing ownership.

2.4.2 Hospital Conversions

To relax the assumption of unconfoundedness required to interpret the results in Tables 2.5 and 2.6 as the causal effect of hospital ownership, I use hospital conversions to estimate the effect of ownership on the probability of admission in a difference-in-differences design. I take advantage of within-hospital variation induced by hospitals converting between nonprofit, government, and for-profit ownership status. Using this variation, I first estimate two-way fixed-effect difference-in-differences regression. Specifically, I estimate the following equation for patient i at time t in hospital h,

$$\text{admit}_{ith} = \beta_0 + \beta_1 \text{ForProf}_{ht} + \beta_2 \text{Govt}_{ht} + \boldsymbol{\beta_3} \boldsymbol{X}_i' + \boldsymbol{\beta_4} \boldsymbol{Z}_{ht}' + \delta_t + \gamma_h + \varepsilon_{ith} \tag{2.3}$$

where $admit_{ith}$ is 1 if the patient is admitted to the hospital. ForProf_{ht} takes a

value of 1 if hospital h is a for-profit hospital at time t. $\mathbf{X}_i'$ is a vector of individual characteristics including payer, age, gender, weekend admission indicator, and admission diagnosis category. $\mathbf{Z}_{ht}$ is a vector of time-varying hospital characteristics including average daily census, number of hospital beds, number of residents, and number of RNs. δ_t and γ_h are year by quarter and hospital fixed effects, respectively.

As with the injury subset, I run a similar regression to equation 2.3, interacting payer with hospital ownership to determine whether the effect of hospital ownership varies by payer. Specifically, I estimate,

$$\text{admit}_{ith} = \alpha_0 + \boldsymbol{\alpha_1}[\mathbf{owner_h} \times \mathbf{payer_i}]' + \boldsymbol{\alpha_2} \boldsymbol{X}_i' + \boldsymbol{\alpha_3} \boldsymbol{Z}_{ht}' + \delta_t + \gamma_h + \varepsilon_{ith} \tag{2.4}$$

where $[\mathbf{owner_h} \times \mathbf{payer_i}]'$ is a vector of ownership and payer interactions. Payer consists of Medicare, Medicaid, private insurance, uninsured, and other insurance. $\mathbf{X_i}$, $\mathbf{Z_{ht}}$, δ_t, and γ_h are the same as in equation 2.3. The coefficients of interest are in the vector $\boldsymbol{\alpha}_1$. For example, if the omitted category is $\text{govt}_h \times \text{Medicaid}_i$, then the coefficient on $\text{forprofit}_h \times \text{Medicaid}_i$ is the difference in admission probability for a Medicaid patient in a for-profit hospital compared to a Medicaid patient in a government hospital. In both equations 2.3 and 2.4, standard errors are clustered by hospital identifier.

Table 2.7 shows the results from the estimation of equation 2.3. The first row shows the coefficient on ForProf_{ht}, which represents a 0.9 percentage point increase in admission probability in for-profit hospitals compared to nonprofit hospitals, which is significant at the 10% confidence level. Compared to the mean admission rate of 0.174, this represents an approximately 5% increase in admission probability. Compared to government hospitals, for-profit hospitals are 2.4 percentage points (13.5%) more likely to admit patients to the hospital

when they show up in the ED. The results indicate no difference between government and nonprofit hospitals.

Table 2.8 shows the results from estimating equations 2.3 and 2.4. The first column shows the estimated coefficient on the for-profit$_{ht}$ variable from equation 2.3. Columns 2 through 4 show the estimated effect of for-profit ownership on the probability of admission from tests of linear combinations of the coefficients on the interacted payer and ownership variables in equation 2.4. The first row shows the results from a regression using the entire sample, while the remaining rows show the results for categories of diagnoses representing the reason for the hospital visit. Diagnoses are grouped together from ICD-9 codes using Clinical Classifications Software (CCS) from the Agency for Healthcare Research and Quality [43]. The diagnosis groups listed in the table represent the 20 most frequent CCS categories in the data.

The results show that while there is a marginally significant increase in admission probability between for-profit and nonprofit hospitals for the entire sample (row 1), there are only a few groups of diagnoses in which the increased probability of admission is statistically significant. The effect of for-profit ownership appears to be driven by Medicare and Medicaid patients, with no increase in probability for privately insured or uninsured patients. Medicare patients experience a 2.3 percentage point (13%) increase, and Medicaid patients a 1.4 percentage point (8.2%) increase, in admission probability due to for-profit hospital ownership. For Medicare patients, the majority of coefficients for each CCS category are positive and significant. For Medicare patients, the largest increases occur within the CCS categories of abdominal pain (2.8 percentage points/90%), other injuries and conditions due to external causes (1.5 percent-

age points/47%) and nonspecific chest pain (8.3 percentage points/46%). If for-profit hospitals increase their admission rates above what is medically necessary, it would be easiest to do this within diagnosis categories that have wiggle room to justify whether the admission was medically necessary, compared to categories where it would be easy to determine whether the patient did or did not have said condition with, e.g., clinical tests.

These results are the opposite of that found in the previous analysis, which found that government hospitals were more likely to admit patients than for-profit and nonprofit hospitals, and that for-profit hospitals were no more likely to admit patients that nonprofit hospitals, conditional on the injury severity score. However, given the potential confounders and strong assumptions required to estimate the effects in Tables 2.5 and 2.6 as the causal effect of hospital ownership on the probability of admission, the estimates obtained using variation induced by hospital ownership changes provide stronger evidence.

Event Studies

In addition to the difference-in-differences analysis, I use the variation induced by hospital conversions to estimate and graph the results from event studies. The estimating equation for the event studies is,

$$\text{admit}_{ith} = \alpha + \sum_{q=-8}^{12} \beta_q \cdot (\text{qtr\_to\_convert}_h) + \gamma_h + \delta_t + \boldsymbol{\zeta}\mathbf{X}_i' + \boldsymbol{\eta}\mathbf{Z}_{ht}' + \varepsilon_{ith} \tag{2.5}$$

where admit_{ith} is 1 if the patient is admitted to the hospital and qtr_to_convert_h is the number of quarters before/after the hospital changes ownership. $\mathbf{X}_i'$ is a vector of patient variables including payer, dummies for age and diagnosis category, gender, and weekend visit indicator. $\mathbf{Z}_{ht}$ is a vector of time-varying hospi-

tal characteristics including average daily census, number of hospital beds, and number of residents and RNs. δ_t and γ_h are year by quarter and hospital fixed effects. I estimate Equation 3.1 separately for three different types of conversions: nonprofit or government to for-profit, nonprofit to for-profit, and for-profit to nonprofit or government. Standard errors are clustered by hospital identifier.

Figure 2.1 presents the event studies for three different conversion types. Figure 2.1a shows the event study for hospitals converting from government or nonprofit to for-profit, which occurs in 18 hospitals in the data. The figure plots the β_q estimates from equation 3.1, which are the coefficients on the event time variables (qtr_to_convert_h). The whiskers represent 95% confidence intervals. The figure shows a jump in the probability of admission in the quarter in which the hospital changes ownership, which is consistent with the difference-in-differences estimates in Table 2.7. Figure 2.1b shows the event study if we limit the sample to the hospitals converting from nonprofit to for-profit, which occurs for 16 hospitals in the sample. The results are similar to those in Figure 2.1a. However, in Figure 2.1c, which shows the event study for hospitals converting *from* for-profit, we also observe an increase in the probability of admission following conversion. The result from these event studies tells us that the effect of for-profit ownership estimated in Table 2.7 may be simply due to an effect from *any* hospital conversion.

In the difference-in-differences regressions in equations 2.3 and 2.4, the coefficient on ForProf_{ht} is estimated using variation induced by hospitals converting *to* for-profit as well as hospitals converting *from* for-profit. To see whether the effect of ownership might be different when hospitals are converting *to* for-profit compared to *from* for-profit, it is instructive to estimate the difference-in-

differences regressions in a similar manner to the event studies. That is, only including hospitals that change ownership, and only including hospitals that convert in a single direction (e.g., nonprofit to for-profit). To do this, I estimate the following regression,

$$\text{admit}_{ith} = \beta_0 + \beta_1 + \beta_2 \text{owner\_change}_{ht} + \boldsymbol{\beta_3} \boldsymbol{X}_i' + \boldsymbol{\beta_4} \boldsymbol{Z}_{ht}' + \delta_t + \gamma_h + \varepsilon_{ith} \tag{2.6}$$

where owner_change_{ht} equals 1 if hospital h has converted at time t and 0 otherwise. As in the previous regressions, $\boldsymbol{X}_i'$ contains patient characteristics, $\boldsymbol{Z}_{ht}'$ contains time-varying hospital characteristics, and δ_t and γ_h are quarter-year and hospital fixed effects. To determine whether the effects differ across payer, I estimate equation 2.6 interacting the owner_change_{ht} variable with payer. Standard errors are clustered by hospital identifier.

Table 2.9 presents the results from the estimation of equation 2.6. The first row represents the results from the estimation of equation 2.6 for all payers. The remaining rows present the estimates generated from linear combinations of the interacted payer and ownership variables to determine whether there are heterogeneous effects across payers. Each column is a individual regression using hospitals that undergo a specific type of conversion (e.g., nonprofit to for-profit).

In the first row we observe that the effect of changing ownership on the probability of admission is only positive and marginally significant for the hospitals converting from government or nonprofit to for-profit. The effect for hospitals converting in the opposite direction, from for-profit to nonprofit, is also positive, but not insignificant. However, for Medicare patients we see a slightly dif-

ferent story: when converting from nonprofit or government to for-profit, the probability of admission increases by approximately 3 percentage points. However, we also observe an increase in admission following the conversion *from* for-profit to government or nonprofit. However, when we limit the sample to hospitals converting from for-profit to nonprofit, the result is no longer statistically significant. The same pattern holds for Medicaid patients, but we observe an opposite pattern for uninsured patients: a decrease in the probability of admission among uninsured patients for *all* conversions in the data: both from for-profit and to for-profit, with the effect largest in hospitals converting *from* for-profit. This result is inconsistent with the hypothesis that for-profit hospitals cherry pick patients and avoid uninsured patients. Because ownership conversions are often induced by chains buying individual hospitals, the decreased treatment of uninsured patients may be a strategy used to improve the finances of struggling hospitals, which would be consistent with the conclusions made in previous research examining responses to fixed-cost shocks [39].

The outcomes presented in Table 2.9 are somewhat consistent with what is observed in the event studies: while we observe an increase in the probability of admission following a hospital gaining for-profit status, we also observe an increase in admission for hospitals *losing* for-profit status. This pattern is observed among the Medicare and Medicaid patients in the sample. Given this evidence, it may be the case that the ownership effects estimated in the difference-in-differences analysis using all hospital conversion presented in Table 2.8 are due to an effect caused by a hospital conversion itself, and not just an ownership change. As many of the conversions are the result of hospitals being purchased by a hospital chain, the increased probability may actually be the effect of being a part of a hospital chain. Future work should address this by identifying

and comparing with hospitals that become part of a hospital chain but do not change ownership (hospitals which are not currently identified in my data).

2.4.3 Additional Outcomes

In the results presented so far, we have seen that there is some evidence that for-profit hospital ownership increases the probability of inpatient admission for Medicaid and Medicaid patients. However, it is useful to know whether changes in hospital ownership lead to changes in other outcomes that might be affected by an increased focus on profits and/or might be related to the probability of inpatient admission. Using the difference-in-differences regressions in equations 2.3 and 2.4, and the event-study regressions in equation 3.1, I estimate the effect of ownership on four different outcomes: average DRG weight, probability of a surgical DRG, probability of transfer, and the number of diagnoses. The DRG weight is a weight assigned to each DRG that is determined by the average cost of treating patients in that diagnosis group, and determines the amount of payment the hospital receives for that discharge. Higher DRG weights correspond to higher payments. Additionally, surgical DRGs tend to be more costly and thus garner higher weights and payments. By looking at both the average DRG weight and the probability of a surgical DRG, we can see whether ownership changes affect payments received for treating patients.

Additionally, I estimate whether the number of diagnoses on a patient's record changes in response to ownership changes. An increase in the number of diagnoses could indicate a sicker patient population, though it could also indicate more appropriate, or more aggressive, coding by hospitals. Additionally,

it could be directly related to increasing inpatient admissions: if the goal of increasing admission rates is accomplished by admitting marginal patients, then one potential strategy could be to increase the number of diagnoses coded on the record in order to justify inpatient admission.[1]

Finally, I look at the probability of being transferred to a different hospital before or after hospital admission. There is anecdotal evidence of hospitals turning away patients (within the bounds of EMTALA) who they deem to be unprofitable [51]. Any differences in the probability of transfer for different payers could provide evidence of hospitals "cherry-picking" patients of a certain insurance type.

The results from the difference-in-differences regressions are contained in Tables 2.10 and 2.11. Each column in Table 2.10 contains the coefficients from a regression of equation 2.3 using the additional outcomes (Avg. DRG weight, etc.) as the outcome variable. The first row shows the outcome mean, and the remaining rows show the coefficients and standard errors comparing different hospital ownership types.

In the table, we see that for-profit hospital ownership decreases the average DRG weight by 2.1% compared to nonprofit hospitals, and 3.7% compared to government hospitals, though the former result is only marginally significant. Compared to government ownership, for-profit ownership increases the probability of a transfer for inpatients by 0.6 percentage points (24%). In all other outcomes, there is no significant difference between for-profit and nonprofit or government hospitals. There is no difference in any of the outcomes between

[1]For this reason, the number of diagnoses is not included as a control variable in the regressions estimating the effect of ownership on the probability of admission; this would introduce collider bias.

government and nonprofit hospitals.

When we separate the results by payer as shown in Table 2.11, some new patterns emerge. The decrease in DRG weight in for-profit hospitals compared to nonprofit and government hospitals is driven by medicaid and uninsured patients, with no difference for Medicare or privately insured patients. Additionally, while there were no effects of ownership on the probability of a surgical DRG in the entire sample, among uninsured and privately insured patients, for-profit ownership decreases the probability of a surgical DRG by approximately 1 percentage point (6%) compared to nonprofit hospitals.

Similar to the average DRG weight, the increased probability of inpatient transfer in for-profit hospitals compared to government hospitals is driven by Medicaid and privately insured patients, whose probability of transfer is 0.9 percentage points (35%) higher in for-profit compared to government hospitals. Additionally, we observe that for-profit ownership increases the probability of inpatient transfer among uninsured patients by 0.6 percentage points (22%) compared to nonprofit hospitals. While there are no effects of ownership on the probability of transfer from the ED among the entire sample, for Medicaid patients, for-profit ownership increases the probability of transfer from the ED by 0.5 percentage points (45%) compared to nonprofit hospitals, with no difference compared to government hospitals or among other payers, though this effect is only marginally significant.

Finally, while there are no effects of ownership on the number of diagnoses in the entire sample, among Medicare patients, for-profit ownership decreases the number of diagnoses by 0.35 (9%) compared to nonprofit hospitals, and *increases* the number of diagnoses by 0.37 (10%) compared to government hospi-

tals.

In addition to the difference-in-differences regressions presented above, Figure 2.2 and 2.3 show the results from event studies created by estimating equation 3.1 using the additional outcomes. In the difference-in-differences regressions, we observed that for-profit ownership decreases DRG weight compared to nonprofit and government hospitals. However, in the event studies we observe a slight increase in DRG weight two years after conversion for hospitals gaining for-profit status, compared to no change in DRG weight for hospitals losing for-profit status.

In the difference-in-differences regressions we also observed that for-profit hospitals had a higher probability of inpatient transfer compared to government hospitals. A similar pattern is observed in the event studies, which show the probability of transfer (any, inpatient, and ED) increases slightly following conversion to for-profit status. For hospitals converting away from for-profit status, we observe the opposite: a slight decrease in transfers following a hospital losing its for-profit status.

With respect to the number of diagnoses, we also observe opposite effects in the event studies: in hospitals gaining for-profit status, there is a decrease in the number of diagnoses, and for hospitals losing for-profit status, there is an increase in the number of diagnoses. This could be the result of for-profit hospitals being less likely to admit patients with many comorbidities than government or nonprofit hospitals. Finally, for hospitals losing for-profit status, we observe a decrease in the probability of a surgical DRG, but observe no significant change for hospitals gaining for-profit status.

Given the mixed evidence observed in the difference-in-differences analyses and the event studies, it is difficult to make any strong conclusions about how hospital ownership affects these additional outcomes. However, from both the difference-in-differences analysis and the event studies, there appears to be an increase in the probability of inpatient transfer in for-profit hospitals. This could be due to a change in service offering in hospitals gaining for-profit status, or it could also be the result of hospitals transferring patients who may be less profitable. More research is needed to determine whether either of these is the case.

2.5 Discussion

In this paper, I explore the question of whether for-profit hospital ownership affects the probability that a patient is admitted to the hospital when they visit the emergency department (ED). While there has been a significant amount of work in the economics literature comparing hospitals of different ownership types, there has been no worth thus far determining whether patients experience an increased probability of admission if they go to an ED in a for-profit hospital compared to a nonprofit hospital. This work is relevant to the many lawsuits that the Department of Justice has been involved in, some of which have been settled, alleging that for-profit hospitals pressure physicians to admit patients to the hospital when it is not medically necessary. In many of the lawsuits, the for-profit hospital chains have been ordered to pay fines up to $260 million, with some fines personally paid by hospital executives.

To determine whether for-profit ownership affects the probability of admis-

sion through the ED, I use a near universe of hospital discharge data from HCUP/AHRQ for patients who visited the ED in six states between 2005 and 2013, paired with the American Hospital Association's Annual Survey. I conduct two main analyses. In the first analysis I estimate linear probability models for a subset of patients who visit the ED for injuries, estimating a model of the probability of admission as a function of hospital ownership, conditioning on the severity of a patient's injury in addition to patient and hospital characteristics. In the second analysis I use the entire sample of patients to estimate difference-in-differences and event study regressions taking advantage of variation in hospital ownership induced by hospitals changing ownership. I find conflicting results between the two analyses: in the analyses using the subset of injury patients, I find no difference in the probability of admission between for-profit and nonprofit hospitals, and find that government hospitals are the most likely to admit patients through the ED. In the analysis using hospital conversions, I find evidence that for-profit ownership increases the probability of admission through the ED, and this is driven by Medicare and Medicaid patients, which is consistent with the DOJ lawsuits. Furthermore, the diagnosis categories in which the effect is largest are in categories such as abdominal pain and non-specific chest pain. These diagnosis categories may be more likely to include the set of marginal patients whose inpatient admission may be easier to justify as the diagnoses rely largely on symptoms rather than test results.

While these results provide some evidence that for-profit ownership increases the probability of inpatient admission, when I conduct the analyses separately for each type of hospital conversion (nonprofit to for-profit versus for-profit to nonprofit), I find that the increased probability of admission is also observed when hospitals *lose* their for-profit status. Given that many of the

ownership conversions are due to hospital chains purchasing individual hospitals, this result indicates that the estimated effect may be the result of hospitals becoming part of a chain instead of the effect of for-profit ownership. Future research should identify hospitals that become part of a chain but do not change ownership and compare them to the hospitals that become part of a chain and change ownership to see whether the effect is driven by hospital system membership.

Finally, while this paper does show the effects of hospital ownership on the probability of admission through the ED, it does not speak to whether the increased admissions are medically necessary or not. It could be the case that prior to the hospital conversion, admission rates were too low, and the conversion restored them to the appropriate level. This could certainly be the case if hospitals that are more likely to be purchased are hospitals with poor management and financial trouble. Future research should look at patient outcomes such as mortality, readmission, or quality indicators to determine whether ownership conversions lead to meaningful changes in patient outcomes. As the previous literature on this topic is mixed, this would be an important avenue for future research.

Table 2.1: ICD-9 Codes Identifying Injuries

ICD-9 Codes	Description
800 - 909.2, 909.4, 909.9	Fractures; dislocations; sprains and strains; intracranial injury; internal injury of thorax, abdomen, and pelvis; open wound of the head, neck, trunk, upper limb, and lower limb; injury to blood vessels; late effects of injury, poisoning, toxic effects, and other external causes, excluding those of complications of surgical and medical care and drugs, medicinal or biological substances.
910 - 994.9	Superficial injury; contusion; crushing injury; effects of foreign body entering through orifice; burns; injury to nerves and spinal cord; traumatic complications and unspecified injuries; poisoning and toxic effects of substances; other and unspecified effects of external causes.
995.5 - 995.59	Child maltreatment syndrome.
995.80 - 995.85	Adult maltreatment, unspecified; adult physical abuse; adult emotional/ psychological abuse; adult sexual abuse; adult neglect (nutritional); other adult abuse and neglect.

The list of ICD-9 codes identifying injuries is consistent with that used in the AHRQ national databases and originated in a 2003 report by the State and Territorial Injury Prevent Directors Association.

Table 2.2: State Inpatient and Emergency Department Databases

State	Years
Arizona	2005 - 2012
California	2005 - 2011
Florida	2005 - 2013
Kentucky	2008 - 2013
Maryland	2005 - 2012
New Jersey	2005 - 2012

Table 2.3: Summary Statistics, Full Sample

Variable	Mean	(SD)
a) Patient Variables		
Admission Rate	0.1746	(0.3796)
Private Insurance	0.3154	(0.4647)
Medicare	0.2082	(0.4060)
Medicaid	0.2405	(0.4274)
Uninsured	0.1849	(0.3882)
Other Insurance	0.0510	(0.2200)
Age	38.7	(24.9)
Weekend Visit	0.2841	(0.4510)
Female	0.5466	(0.4978)
DRG Weight (Inpatients Only)	1.2871	(1.2765)
b) Hospital Variables		
For-Profit Hospital	0.1817	(0.3856)
Nonprofit Hospital	0.6875	(0.4635)
Government Hospital	0.1308	(0.3372)
Average Daily Census	255.8	(251.6)
# Full-Time Residents	47.6	(135.3)
# Full-Time RNs	639.6	(751.9)
# Hospital Beds	361.6	(340.0)

Table 2.4: Summary Statistics, Injury Subsample

	All Hosps	For-Profit	Nonprofit	Govt
Admit	0.0585	0.0517	0.0567	0.0774
For-Profit Hospital	0.187			
Government Hospital	0.130			
Nonprofit Hospital	0.683			
Patient/Discharge Variables				
Medicare	0.149	0.172	0.146	0.129
	(0.356)	(0.377)	(0.353)	(0.335)
Medicaid	0.187	0.195	0.180	0.210
	(0.390)	(0.396)	(0.384)	(0.407)
Private Insurance	0.385	0.336	0.414	0.304
	(0.487)	(0.472)	(0.493)	(0.460)
Injury Severity Score	3.817	3.650	3.755	4.381
	(12.037)	(11.715)	(11.831)	(13.468)
# of Diagnoses	2.502	2.370	2.541	2.490
	(2.562)	(2.361)	(2.619)	(2.526)
Weekend Visit	0.306	0.304	0.306	0.304
	(0.461)	(0.460)	(0.461)	(0.460)
Female	0.467	0.479	0.470	0.432
	(0.499)	(0.500)	(0.499)	(0.495)
Age	35.123	36.665	34.835	34.413
	(23.923)	(24.290)	(23.999)	(22.873)
N (Discharges)	43,231,103	8,101,809	29,512,155	5,617,139
Hospital Variables				
Average Daily Census	156.3	108.1	172.3	169.6
	(153.7)	(78.1)	(159.2)	(202.5)
Hospital Beds	230.4	177.6	248.7	241.3
	(204.9)	(108.9)	(210.1)	(279.3)
# Full-Time Residents	23.1	4.00	22.4	58.7
	(89.9)	(8.72)	(76.8)	(171.3)
# Full-Time Nurses	367.5	225.2	421.8	375.4
	(419.0)	(150.5)	(458.1)	(490.8)
N (Hospitals)	901	216	548	137

Standard errors in parenthesis. Patient variables average at the discharge level; hospital variables averaged at the hospital level.

Table 2.5: Association Between Hospital Ownership and Probability of Admission Conditional on Injury Severity

	Outcome = pr(admit)	
For-Profit Hospital	0.00237	For-Profit - Govt
	(0.00346)	= -0.009^{+}
Govt Hospital	0.0114*	
	(0.00548)	
Medicare	0.0278***	
	(0.00200)	
Medicaid	0.0187***	
	(0.00268)	
Private Insurance	0.00861***	
	(0.00193)	
Other Insurance	0.0201***	
	(0.00297)	
Injury Severity Score	-0.000953***	
	(0.0000714)	
Weekend Indicator	0.000627^{+}	
	(0.000320)	
Female	-0.00909***	
	(0.000906)	
Average Daily Census	0.000118***	
	(0.0000267)	
# FT Residents	0.0000727**	
	(0.0000260)	
#FT RNs	0.00000954	
	(0.00000814)	
# Hospital Beds	-0.0000550**	
	(0.0000210)	
N	43,231,103	

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Standard errors in parentheses

Table presents coefficient estimates from the regression of equation 2.1. Standard errors are clustered by hospital ID. Regression includes county and year-quarter fixed effects. Sample includes patients who visited the emergency department for an injury. The coefficient on "For-Profit Hospital" can be interpreted as a 0.237 percentage point increase in the probability of admission.

Table 2.6: Association Between Hospital Ownership and Probability of Admission Conditional on Injury Severity, by Insurance Type

	For-profit vs Nonprofit	**For-profit vs Govt**	**Govt vs Nonprofit**
Private Insurance	0.002	-0.018**	0.020**
Medicare	0.007	0.001	0.006
Medicaid	0.001	-0.017*	0.018*
Uninsured	0.001	0.007	-0.007
Other Insurance	0.002	-0.020+	0.021*

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table presents test results of linear combinations of the payer and ownership interactions from the regression of equation 2.2. Standard errors are clustered by hospital ID. Regression includes patient and hospital controls as well as county and year-quarter fixed effects. Sample includes patients who visited the emergency department for an injury. The first column shows the difference in the probability of admission in a for-profit hospital compared to a nonprofit hospital. The estimates can be interpreted as a $\beta * 100$ percentage point increase in the probability of inpatient admission.

Table 2.7: Effect of Hospital Ownership on Probability of Inpatient Admission

	Coef.	**SE**	
For-Profit Hospital	0.00939^{+}	(0.00553)	For-Profit - Govt
Government Hospital	-0.0142	(0.0112)	= 0.0236**
Medicare	0.123***	(0.00201)	
Medicaid	0.0302***	(0.00228)	
Uninsured	-0.0573***	(0.00318)	
Other Insurance	-0.0147***	(0.00371)	
Weekend Visit	-0.0124***	(0.000408)	
Female	-0.0289***	(0.000629)	
Average Daily Census	0.000112*	(0.0000478)	
# Full-Time Residents	0.0000183	(0.0000131)	
# Full-Time RNs	-0.00000673	(0.00000500)	
# Hospital Beds	-0.0000102	(0.0000119)	
N	215,662,539		

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table presents the coefficient estimates from the regression of equation 2.3. Regression includes hospital and quarter-year fixed effects, as well as dummy variables for the top 10 most frequent CCS diagnosis groups. Average admission rate is 0.17. Standard errors are clustered by hospital ID. The estimate on For-Profit Hospital can be interpreted as a $\beta * 100$ percentage point change in the probability of inpatient admission.

Table 2.8: Effect of For-Profit Ownership on the Probability of Admission by Diagnosis Category

Outcome = Probability of Admit (vs. Treat and Release)						
CCS Category	**All**	**Private**	**Medicare**	**Medicaid**	**Uninsured**	**N**
All CCS Categories (Nonprofit Mean = 0.174; DRG Weight = 1.2871)	0.0094*	-0.0011	0.023***	0.0143**	0.0055	215,662,539
	(0.0055)					
Sprains and strains (Nonprofit Mean = 0.004; DRG Weight = 0.8593)	0.0006*	0.0003	0.0003	0.0011***	0.0009**	9,718,200
	(0.0004)					
Superficial injury; contusion (Nonprofit Mean = 0.0087; DRG Weight = 0.7917)	0.0007	-0.0003	0.0037**	0.0011	0	9,377,841
	(0.0007)					
Other upper respiratory infections (Nonprofit Mean = 0.0123; DRG Weight = 0.6953)	-0.0004	-0.0019	-0.0045	0.0007	0.0027*	9,214,668
	(0.0015)					
Abdominal pain (Nonprofit Mean = 0.0315; DRG Weight = 0.7685)	0.0067*	0.0013	0.0282***	0.0057	0.0015	8,672,733
	(0.0034)					
Nonspecific chest pain (Nonprofit Mean = 0.1791; DRG Weight = 0.6302)	0.0514*	0.0403	0.0828***	0.0595**	0.0245	7,082,199
	(0.0275)					
Skin and subcutaneous tissue infections (Nonprofit Mean = 0.1659; DRG Weight = 0.9534)	0.0015	-0.0057	0.0049	0.0041	0.0066	5,759,826
	(0.0118)					

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CCS Category	All	Private	Medicare	Medicaid	Uninsured	N
Spondylosis/intervertebral disc disorders/other back (Nonprofit Mean = 0.0451; DRG Weight = 1.1136)	0.0021 (0.0027)	-0.0012	0.0102**	0.0047	-0.0003	5,647,095
Open wounds of extremities (Nonprofit Mean = 0.0142; DRG Weight = 1.2637)	0.0015 (0.0011)	0.0012	0.0012	0.0019	0.002	5,206,242
Urinary tract infections (Nonprofit Mean = 0.1803; DRG Weight = 0.8939)	0.0075 (0.0088)	-0.0046	0.0219**	0.0044	0.0115	5,224,804
Other injuries/conditions (external causes) (Nonprofit Mean = 0.0308; DRG Weight = 1.0488)	0.0015 (0.0037)	-0.0014	0.0146***	-0.0015	-0.0014	4,811,623
Headache; including migraine (Nonprofit Mean = 0.0278; DRG Weight = 0.736)	0.0014 (0.0014)	-0.0015	0.0066***	0.0027*	0.0019	4,577,020
Open wounds of head; neck; and trunk (Nonprofit Mean = 0.0155; DRG Weight = 1.0505)	0.0014 (0.0024)	0.0014	0.0032	0.0034	-0.0013	4,282,270
Asthma (Nonprofit Mean = 0.1878; DRG Weight = 0.8189)	0.0112 (0.009)	-0.0079	0.0611***	0.0039	0.0144	3,398,557
Otitis media and related conditions (Nonprofit Mean = 0.0052; DRG Weight = 0.688)	0.0008 (0.0007)	-0.0005	0.0017	0.0014**	0.002***	3,295,999

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CCS Category	All	Private	Medicare	Medicaid	Uninsured	N
Fracture of upper limb (Nonprofit Mean = 0.072; DRG Weight = 1.2492)	0.0072 (0.0057)	-0.0001	0.0194***	0.0096	0.0088	3,263,915
Other connective tissue disease (Nonprofit Mean = 0.056; DRG Weight = 1.0585)	0.0044 (0.0048)	-0.0017	0.0131**	0.0072	0.0023	3,221,528
COPD and bronchiectasis (Nonprofit Mean = 0.3061; DRG Weight = 1.0451)	0.0356** (0.0163)	0.0157	0.048***	0.0292*	0.0251	3,157,755
Other lower respiratory disease (Nonprofit Mean = 0.0547; DRG Weight = 1.1022)	0.0038 (0.0056)	-0.003	0.017**	0.0026	-0.0001	3,140,091
Pneumonia (not caused by tuberculosis or STD) (Nonprofit Mean = 0.5225; DRG Weight = 1.2466)	0.0049 (0.0168)	-0.0171	0.0101	0.0062	0.0249	3,069,854
Other complications of pregnancy (Nonprofit Mean = 0.07; DRG Weight = 0.6132)	0.0003 (0.0112)	-0.0057	-0.0457**	0.0051	-0.0017	2,903,707

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table presents coefficient estimates from equations 2.3 and 2.4. The first column shows the estimated coefficient on the for-profit$_{ht}$ variable from equation 2.3. Columns 2 through 4 show the estimated effect of for-profit ownership on the probability of admission from tests of linear combinations of the coefficients on the interacted payer and ownership variables in equation 2.4. The first row shows the results from a regression using the entire sample, while the remaining rows show the results for categories of diagnoses representing the reason for the hospital visit. Diagnoses are grouped together from ICD-9 codes using Clinical Classifications Software (CCS) from the Agency for Healthcare Research and Quality [43]. The diagnosis groups listed in the table represent the 20 most frequent CCS categories in the data. Standard errors in parentheses. Nonprofit Mean indicates the average admission rate in nonprofit hospitals. DRG weight indicates the average DRG weight among all inpatients in a given category. Regressions include controls for patient and hospital characteristics, as well as hospital and quarter-year fixed effects. Estimates can be interpreted as a β*100 percentage point change in the probability of inpatient admission.

Table 2.9: Effect of Hospital Ownership on Probability of Inpatient Admission Among Hospitals Changing Ownership

	Conversion Types			
	Govt/Nonprofit to For-Profit	**Nonprofit to For-Profit**	**For-Profit to Govt/Nonprofit**	**For-Profit to Nonprofit**
All Insurance Types	0.00924*	0.00763	0.00482	-0.00286
	(0.00401)	(0.00475)	(0.00958)	(0.01573)
Medicare	0.04110**	0.04686**	0.04468+	0.05895
	(0.01271)	(0.01448)	(0.02404)	(0.03325)
Medicaid	0.03325***	0.03151***	0.03045**	0.03415
	(0.00714)	(0.00768)	(0.00914)	(0.01614)
Uninsured	-0.02463*	-0.03122**	-0.07016+	-0.10390*
	(0.01008)	(0.01020)	(0.03267)	(0.03542)
Private Insurance	-0.00010	-0.00298	-0.00711	-0.01577
	(0.00528)	(0.00542)	(0.01331)	(0.01987)
Other Insurance	-0.01369	-0.01676	-0.05614+	-0.08113
	(0.01621)	(0.01839)	(0.03019)	(0.04486)
N (discharges)	4,343,862	3,655,165	1,234,567	824,436
N (hospitals)	18	16	10	5
Average Admission Rate (all payers)	0.182	0.185	0.233	0.284

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Standard errors in parentheses. The first row presents the results from the estimation of equation 2.6 for all payers. The remaining rows present the estimates generated from linear combinations of the interacted payer and ownership variables. Each column is a individual regression using hospitals that undergo a specific type of conversion (e.g., nonprofit to for-profit). The regression includes patient and hospital controls, as well as quarter-year and hospital fixed effects. Standard errors are clustered by hospital ID. The estimates can be interpreted as a $\beta * 100$ percentage point change in the probability of admission.

Table 2.10: Effect of Hospital Ownership on Additional Outcomes (all payers)

	Outcomes						
	DRG Weight	**ln(DRG Weight)**	**Pr(Surgical DRG)**	**Pr(Transfer from ED)**	**Pr(Transfer from IP)**	**Pr(Any Transfer)**	**# of Diagnoses**
Outcome Mean	1.2871	0.0592	0.1771	0.0124	0.0253	0.0147	3.7050
For-profit	-0.03338*	-0.02106*	-0.00716	0.00392	0.00289	0.00383	-0.12588
- Nonprofit	(0.01525)	(0.01039)	(0.00437)	(0.00290)	(0.00263)	(0.00254)	(0.11024)
Govt	0.01207	0.01586	-0.00614	0.00461	-0.00327	0.00370	-0.04632
- Nonprofit	(0.03159)	(0.02439)	(0.01072)	(0.00358)	(0.00361)	(0.00319)	(0.19802)
For-profit	-0.04545	-0.03691+	-0.00102	-0.00069	0.00616+	0.00013	-0.07955
- Govt	(0.02839)	(0.02188)	(0.00968)	(0.00393)	(0.00344)	(0.00341)	(0.17896)
N	37,656,214	37,653,744	35,873,618	161,800,000	34,266,403	196,100,000	215,700,000
N (hosps)	791	791	790	743	744	744	791

Standard errors in parentheses; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Each column contains the coefficients from a regression of equation 2.3 using the outcome in the column header as the outcome variable. The first row shows the outcome mean, and the remaining rows show the coefficients and standard errors comparing different hospital ownership types. The regressions include patient and hospital controls as well as hospital and quarter-year fixed effects. Standard errors are clustered at the hospital ID. Outcomes involving DRGs and inpatient transfers only include admitted patients; outcomes involving transfer from the ED only include non-admitted patients.

Table 2.11: Effect of Hospital Ownership on Additional Outcomes, by payer

Outcome	DRG Weight	ln(DRG Weight)	Pr(Surgical DRG)	Pr(Transfer from ED)	Pr(Transfer from IP)	Pr(Any Transfer)	# of Diagnoses
Mean	1.2871	0.0592	0.1771	0.0124	0.0253	0.0147	3.7050
a) For-profit - Nonprofit							
Medicare	-0.02202	-0.01450	-0.00435	0.00338	0.00311	0.00344	-0.35209**
	(0.01596)	(0.01086)	(0.00457)	(0.00340)	(0.00273)	(0.00278)	(0.13376)
Medicaid	-0.04271*	-0.03040*	-0.00216	0.00541+	-0.00071	0.00486+	-0.06102
	(0.01772)	(0.01212)	(0.00494)	(0.00294)	(0.00287)	(0.00262)	(0.11225)
Uninsured	-0.05591***	-0.03249**	-0.01139*	0.00354	0.00554+	0.00391	-0.08598
	(0.01686)	(0.01109)	(0.00485)	(0.00285)	(0.00295)	(0.00252)	(0.10946)
Private	-0.02279	-0.01310	-0.01569**	0.00344	0.00085	0.00306	-0.04127
	(0.01736)	(0.01176)	(0.00545)	(0.00304)	(0.00279)	(0.00272)	(0.11174)
b) Govt - Nonprofit							
Medicare	-0.01155	0.00198	-0.01472	0.00900+	-0.00263	0.00648+	-0.72920***
	(0.03305)	(0.02487)	(0.01104)	(0.00465)	(0.00365)	(0.00379)	(0.21158)
Medicaid	0.01480	0.02299	-0.00218	0.00366	-0.00929*	0.00193	0.21281
	(0.03448)	(0.02553)	(0.01140)	(0.00362)	(0.00388)	(0.00324)	(0.19889)
Uninsured	0.07989*	0.04309+	0.00298	0.00450	0.00404	0.00474	0.06545
	(0.03667)	(0.02557)	(0.01146)	(0.00367)	(0.00431)	(0.00329)	(0.19495)
Private	0.02081	0.03232	0.00863	0.00375	-0.00847*	0.00262	0.05488
	(0.03446)	(0.02552)	(0.01191)	(0.00374)	(0.00386)	(0.00334)	(0.20246)
c) For-profit - Govt							
Medicare	-0.01047	-0.01648	0.01037	-0.00562	0.00574	-0.00304	0.37711+
	(0.03014)	(0.02255)	(0.01008)	(0.00508)	(0.00351)	(0.00406)	(0.19794)

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	DRG Weight	ln(DRG Weight)	Pr(Surgical DRG)	Pr(Transfer from ED)	Pr(Transfer from IP)	Pr(Any Transfer)	# of Diagnoses
Medicaid	-0.05750^{+}	-0.05339*	0.00002	0.00175	0.00857*	0.00293	-0.27382
	(0.03208)	(0.02370)	(0.01069)	(0.00399)	(0.00382)	(0.00350)	(0.18000)
Uninsured	-0.13581***	-0.07558**	-0.01437	-0.00096	0.00150	-0.00083	-0.15144
	(0.03382)	(0.02333)	(0.01066)	(0.00399)	(0.00428)	(0.00349)	(0.17617)
Private	-0.04361	-0.04541*	-0.02432*	-0.00030	0.00932*	0.00044	-0.09615
	(0.03137)	(0.02307)	(0.01122)	(0.00413)	(0.00370)	(0.00363)	(0.18467)
N	37,656,214	37,653,744	35,873,618	161,800,000	34,266,403	196,100,000	215,700,000
N (hosps)	791	791	790	743	744	744	791

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ Standard errors in parentheses. Each column contains the coefficients from a regression of equation 2.4 using the additional outcomes (Avg. DRG weight, etc.) as the outcome variable. Estimates are derived from linear combinations of coefficient estimates on the (payer)X(ownership) interactions in equation 2.4. Regressions for outcomes involving DRGs and inpatient transfers only include admitted patients; outcomes involving transfer from ED only include non-admitted patients. Regressions include hospital/patient characteristics, year-quarter and hospital fixed effects. Standard errors are clustered by hospital ID.

2.6 Figures

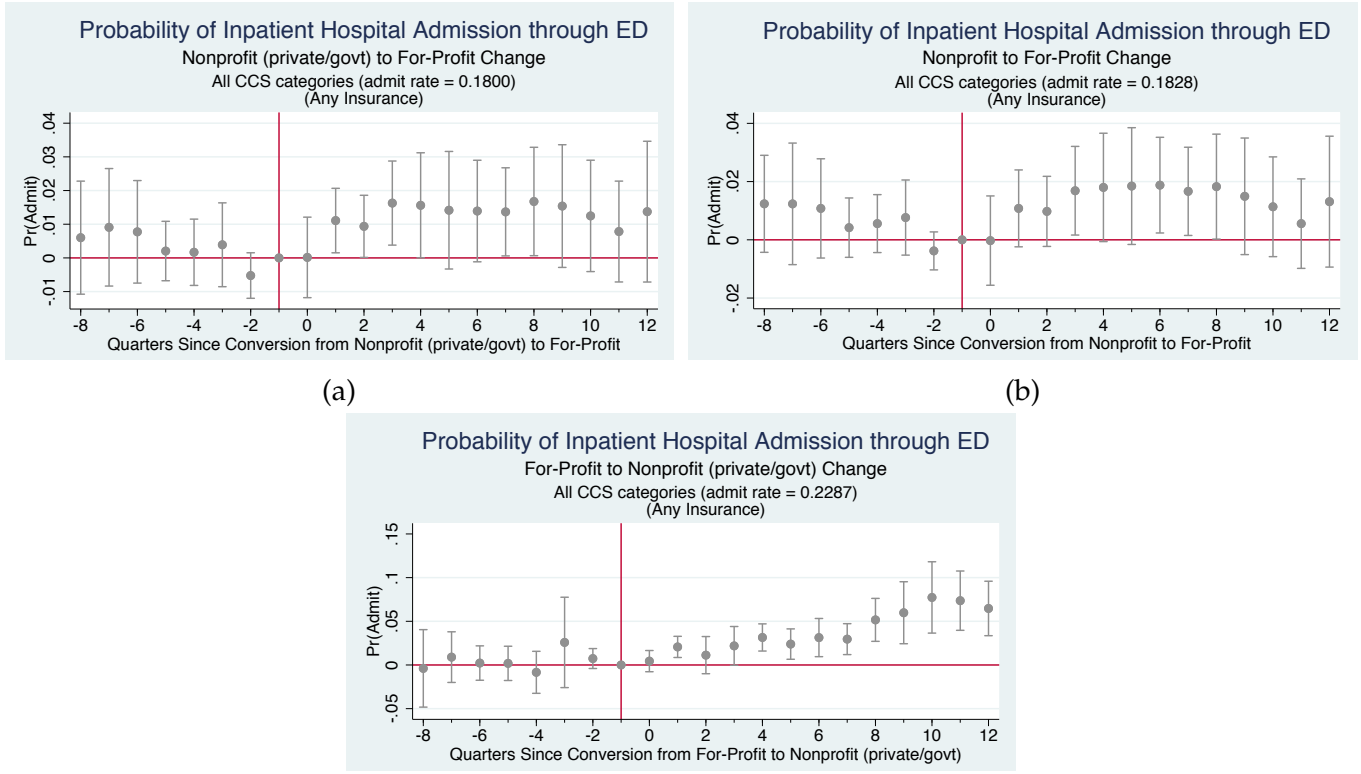
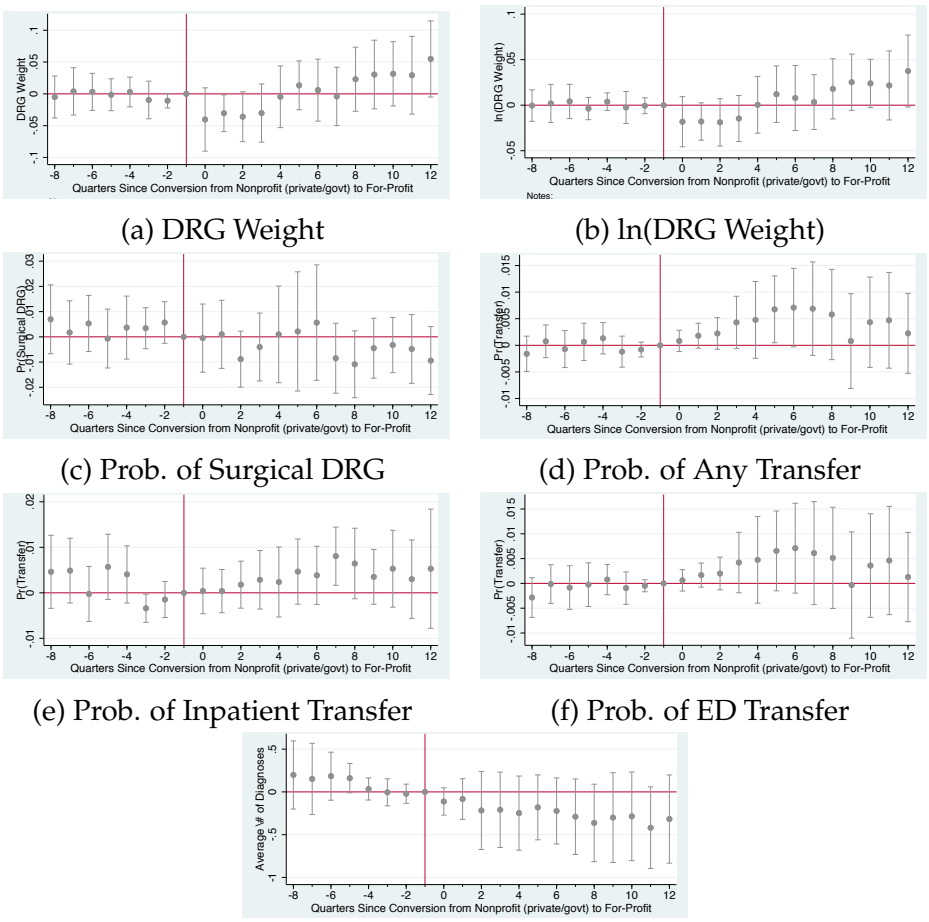


Figure 2.1: Event studies showing the effect of hospital conversion on the probability of inpatient admission

Figures show the event studies estimated using equation 3.1. Whiskers are 95% confidence intervals. Standard errors are clustered by hospital ID. Regressions include controls for patient and hospital characteristics, dummies for the top 10 most frequent diagnosis groups, and hospital and quarter-year fixed effects. Observations more than 8 quarters prior to conversion are included in the t=-8 indicator; observations more than 12 quarters after conversion are included in the t=12 indicator. The three different panels represent different types of ownership changes: nonprofit or government to for-profit, nonprofit to for-profit, and for-profit to nonprofit or government.



(a) DRG Weight

(b) ln(DRG Weight)

(c) Prob. of Surgical DRG

(d) Prob. of Any Transfer

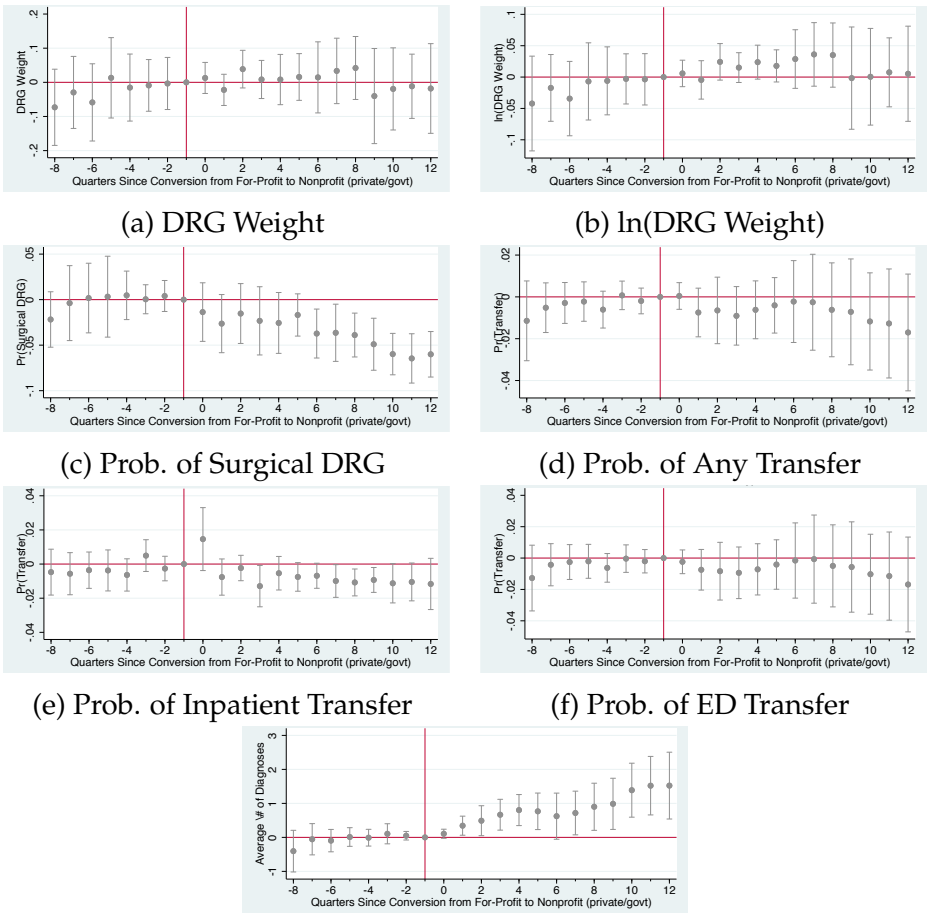
(e) Prob. of Inpatient Transfer

(f) Prob. of ED Transfer

(g) # of Diagnoses

Figure 2.2: Event studies showing the effect of hospitals converting from nonprofit/government to for-profit on additional outcomes

Figures show the event studies estimated using equation 3.1 on additional outcomes for hospitals converting from government or nonprofit to for-profit. Whiskers are 95% confidence intervals. Standard errors are clustered by hospital ID. Regressions include controls for patient and hospital characteristics, and hospital and quarter-year fixed effects. Observations more than 8 quarters prior to conversion are included in the t=-8 indicator; observations more than 12 quarters after conversion are included in the t=12 indicator. Each panel represents an event study for a different outcome variable.



(a) DRG Weight

(b) ln(DRG Weight)

(c) Prob. of Surgical DRG

(d) Prob. of Any Transfer

(e) Prob. of Inpatient Transfer

(f) Prob. of ED Transfer

(g) # of Diagnoses

Figure 2.3: Event studies showing the effect of hospitals converting from for-profit to nonprofit/government on additional outcomes

Figures show the event studies estimated using equation 3.1 on additional outcomes for hospitals converting from for-profit to government or nonprofit. Whiskers are 95% confidence intervals. Standard errors are clustered by hospital ID. Regressions include controls for patient and hospital characteristics, and hospital and quarter-year fixed effects. Observations more than 8 quarters prior to conversion are included in the t=-8 indicator; observations more than 12 quarters after conversion are included in the t=12 indicator. Each panel represents an event study for a different outcome variable.

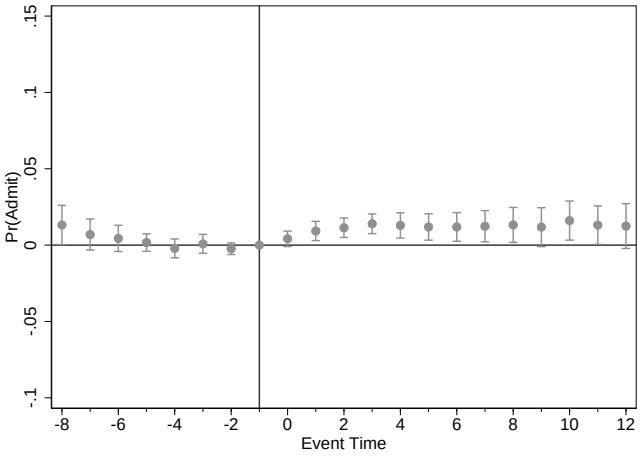
2.7 Appendix

2.7.1 Appendix Tables

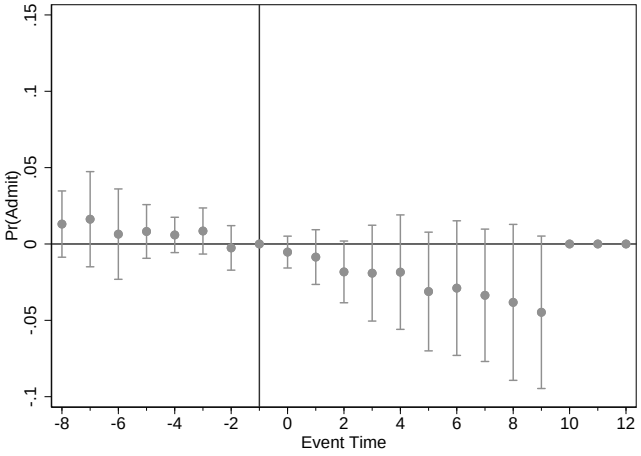
Table 2.A1: Summary Statistics for Hospitals Acquiring Hospitals Compared to Hospitals Being Acquired

	Acquired hospitals	**Hospitals in systems that are acquiring**
Average # Full-Time RNs	176	282
Average # Full-Time Doctors	8	26
Average # Nurses	211	336
Average # Residents	5	18
Average # Hospital Beds	164	213
Average Daily Census	111	148
Average # Inpatient Visits (Annual)	4,241	6,522
Average # ER Visits (Annual)	19,893	29,663
% Nonprofit/Govt Hospitals	67%	49%
% For-Profit Hospitals	33%	51%
Average Admission Rate	16%	17%
Average Age	42	39
Average % Medicare	21%	21%
Average % Medicaid	21%	22%
Average % Private Insurance	35%	33%
Average % Uninsured	18%	19%
Average % Weekend Visits	33%	29%
Average Length of Stay	0.80	0.87
Average # of Diagnoses	3.54	3.41
Average # of Procedures	1.03	1.06
Average % of DRGs Surgical	14%	17%
# Hospitals	66	251

2.7.2 Appendix Figures



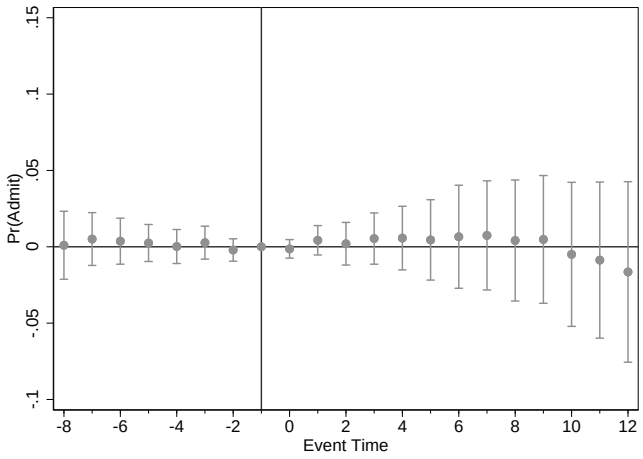
(a) Before 2011 - Unlikely EHR Use



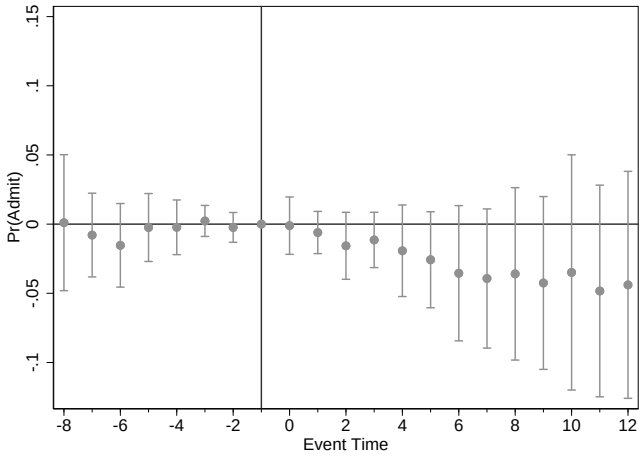
(b) 2011 or After - Likely EHR Use

Figure 2.A1: Event studies showing the effect of hospital acquisition on the probability of inpatient admission

Figures show the event studies estimated using equation 3.1 using all hospital acquisitions regardless of whether the acquisition resulted in a profit status change. The figure on the left shows the acquisitions occurring before 2011, and the figure on the right shows the acquisitions occurring after 2011. Whiskers are 95% confidence intervals. Standard errors are clustered by hospital ID. Regressions include controls for patient and hospital characteristics, dummies for the top 10 most frequent diagnosis groups, and hospital and quarter-year fixed effects. Observations more than 8 quarters prior to conversion are included in the t=-8 indicator.



(a) Did Not Gain EHR



(b) Gained EHR

Figure 2.A2: Event studies showing the effect of hospital acquisition on the probability of inpatient admission

Figures show the event studies estimated using equation 3.1 using all hospital acquisitions regardless of whether the acquisition resulted in a profit status change. The sample contains acquisitions that occurred after 2009 and splits the sample into two groups: hospitals that reported EHR use for the first time following acquisition (right) and hospitals that did not experience a change in EHR use following their acquisition (left). Whiskers are 95% confidence intervals. Standard errors are clustered by hospital ID. Regressions include controls for patient and hospital characteristics, dummies for the top 10 most frequent diagnosis groups, and hospital and quarter-year fixed effects. Observations more than 8 quarters prior to conversion are included in the t=-8 indicator.

CHAPTER 3

NON-MONETARY OBSTACLES TO MEDICAL CARE: EVIDENCE FROM POSTPARTUM CONTRACEPTIVES

ESSAYS IN HEALTH ECONOMICS

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Cornell University 2019

In this paper we use variation in state-level policies to test whether non-monetary costs are meaningful obstacles to health care. Starting in 2012, most state Medicaid agencies made long-acting reversible contraceptives (LARCs, which include IUDs and implants) available in the hospital immediately following delivery of a child, eliminating some of the time-cost and stress associated with getting a LARC. Policymakers and advocates stress that providing access to immediate postpartum LARCs can reduce unintended and short-interval pregnancies, which are associated with adverse neonatal outcomes. We test whether lowering non-monetary costs increases LARC use, decreases birth rates, and improves birth outcomes. We use data from the National Vital Statistics System and quarterly Medicaid claims for LARCs to estimate event studies and difference-in-differences models. We find no evidence that Medicaid coverage for immediate postpartum LARCs affected LARC use or birth outcomes, though our estimates are imprecise.

3.1 Introduction

Access to health care is a growing area of concern, particularly in the United States, where access is often unequal across the income distribution [111]. From a significant body of previous work, we know that lowering the direct financial burden of health care increases demand [10, 14, 27, 28, 47, 70, 85, 96]. However, there is much less evidence on the effect of non-monetary costs. These costs include arranging travel, finding childcare, and lost wages for workers without paid sick leave. Surveys have shown that one in five adults report non-monetary barriers to health care such as lack of transportation and inadequate hours of operation [75]. These costs can be particularly burdensome for people with lower socio-economic status. For instance, only 27% of the bottom decile of wage earners have paid sick leave, compared to 87% in the top decile [13]. Workers without sick leave are 3 times more likely to forego medical care [38]. Furthermore, research on the relationship between distance and use of care has shown that black children who live in urban areas are less likely to receive a check-up when their distance from home to the hospital increases [30].

In this paper we investigate the effect of lowering the non-monetary costs of obtaining health care by leveraging state-level changes in Medicaid policies regarding long-acting reversible contraceptives (LARCs). LARCs include subdermal implants and intrauterine devices (IUDs), which are effective for at least three years but can be removed at any time [5]. Starting with South Carolina in 2012, these policies provided financial incentives to health-care providers to insert a LARC while the mother is still in the hospital ("immediate postpartum"), anytime from immediately after delivery to right before the mother leaves the hospital [93]. While women with Medicaid coverage already had access to

LARCs with little-to-no out-of-pocket cost [127], these policies removed many of the non-monetary costs, such as the requirement to attend a follow-up visit, which patients have cited as a reason for not receiving a LARC [130]. Previous studies have shown that expanding access to reproductive health care decreases unwanted births and birth rates [2, 10, 72, 78, 79]. In the opposite direction, researchers have shown that increasing the barriers to receiving care, such as distance to a women's health clinic, can decrease the use of care [78, 81, 82] and subsequently increase birth rates [48].

To determine whether decreasing the non-monetary costs of LARCs affects use, we take advantage of two sources of variation, which are shown in Table 3.1: thirty-eight states and Washington DC provide separate reimbursement for immediate postpartum LARCs, and states have enacted this policy at different times. Exploiting this across-state and across-time variation, we use event studies and difference-and-differences models to test for effects of access to immediate postpartum LARCs on LARC take up, future fertility, and birth outcomes. Data for birth outcomes come from the National Vital Statistics System (NVSS), and information on LARCs come from an ongoing effort to obtain aggregated claims data from each state's Medicaid office. We do not find evidence that these policies impacted LARC use or births, though we lack the precision to rule out meaningful effects.

In this paper, we make two main contributions to the literature. First, we empirically test for the effect of non-monetary costs on health care use. While two experiments and several quasi-experimental studies have shown that lowering the direct financial cost of care increases use [10, 14, 27, 28, 47, 70, 85, 96], there is much less research on other obstacles to care. This policy targets new

mothers on Medicaid, a group who previously had no out-of-pocket costs for LARCs [127]. Because of this, we can examine the impact of eliminating time and convenience costs.

The second contribution is an evaluation of state-level policies that encourage effective contraception use. These policies are particularly important because they target low-income women, a group with higher than average rates of unintended pregnancy, abortions, and short-interval pregnancies [18, 46, 68]. LARCs are the most effective method for preventing pregnancies after abstinence and sterilization [123]. Immediate postpartum LARCs are an ideal method for reducing short-interval pregnancies, or pregnancies occurring within 18 months of a live birth [18], because the LARC is implanted immediately after a birth [5]. Advocates argue that expanding access to immediate postpartum LARCs will increase take-up as many women who intend to use a LARC postpartum never end up obtaining one, which could be the result of low rates of follow-up care [5]. Because short-interval pregnancies, as well as pregnancies among low-income women, are more likely to result in poor birth outcomes, access to immediate postpartum LARCs may improve average birth outcomes [23, 86].

This paper continues with the following sections. Section 2 provides background information on the previous literature, contraception, and institutional details of Medicaid reimbursement, particularly regarding LARCs. In Section 3, we outline our data sources and empirical strategy. Results are presented in Section 4, and we conclude with a discussion in Section 5.

3.2 Background

This study ties into two main areas of research: 1) the effect of reducing obstacles to health care, and 2) the impact of access to contraception on contraceptive use and birth outcomes. The research on obstacles to health care has focused on direct financial costs, and generally finds that lowering costs increases use and improves health. The most influential study is the RAND Health Insurance Experiment, which randomized households to differing levels of cost-sharing. Manning and coauthors 1987 find that reducing financial obstacles to care increases use of care but has modest-to-no effects on health. More recently, the Oregon Health Insurance Experiment randomized low-income Oregonians the opportunity to apply for Medicaid. Finkelstein and coauthors 2012 find that decreasing the out-of-pocket cost of health care increases health care use and improves self-reported health.

In addition to these randomized experiments, several studies have used the introduction and expansion of Medicaid and the State Children's Health Insurance Program to estimate the effect of insurance/low-cost health care [27–29, 57, 70]. Consistent with the RAND and Oregon experiments, Medicaid increases health care use but has inconsistent effects on health, improving outcomes for some populations and having no impact on others.

There is much less research on the effect of non-monetary costs, such as convenience. One relevant study tests the effect of school-based health centers on teen fertility [80]. By locating health-care providers directly in schools, these health centers reduce the non-monetary costs associated with obtaining care such as missing school and relying on parents for transportation. Lovenheim

and coauthors 2016 find that school-based health centers reduce teen fertility, implying that reducing non-monetary costs can improve health. Looking at the effect in the opposite direction, Currie and Reagan 2003 find that among black children living in urban areas, the likelihood of receiving a check-up decreases as distance to the hospital increases. With respect to access to reproductive care, researchers studying a Texas law which abruptly closed women's health clinics find that increasing the distance to the nearest women's health clinic decreases the use of preventative care such as breast exams, mammograms, and Pap tests [82], as well as abortions [78], which in turn increases the fertility rate [81].

Our study also relates to the broader research on the effects of access to contraception. The evidence consistently finds that improved access to contraception decreases fertility. The introduction of oral contraception in the mid-20th century was a major change in contraception access. Bailey 2010 uses variation in pre-existing state-level anti-obscenity laws, the introduction of oral contraception, and a Supreme Court case, and finds that access to contraception decreases fertility. In the 1960s and 1970s, the US government began funding family planning programs including subsidizing contraception, which reduced fertility especially for low-income women [7]. Kearney and Levine 2009 and Lindrooth and McCullough 2007 find that policies from the 1990s and 2000s which expanded family planning services to low-income women who were not eligible for Medicaid increased contraception use and decreased fertility, particularly among teens. Other studies have also shown that expanding family planning coverage increases the use of preventative care [128] and reduces unwanted births [2]. Leveraging the Affordable Care Act's requirement that insurance cover contraception with no out-of-pocket cost, researchers have found that cheaper contraception increases contraceptive use [10] and lowers fertility

[129].

The studies above, which focus on increasing access to contraceptives by decreasing financial costs, have mostly found that decreasing financial barriers increases use. However, the evidence on non-monetary barriers is more mixed. In randomized controlled trials, researchers find that giving women access to advanced provision of emergency contraceptives (EC) increases use but has no impact on pregnancy rates [103, 105, 106]. Researchers studying the FDA policy change in which pharmacists were permitted to dispense EC without a prescription find that removing the requirement to visit the physician does not increase the use of EC [42, 60], however it does change the setting in which EC is received from the emergency department to the pharmacy [60]. Given this result, it is plausible that providing access to LARCs in the immediate postpartum setting might simply change the location in which LARCs are inserted from the outpatient to the inpatient setting, without affecting the use of LARCs or birth rates.

We add to this literature by examining a policy intervention that removes non-financial barriers to LARCs for low-income mothers. While access to oral contraceptives has helped many women prevent unwanted pregnancies, it requires strict adherence to daily consumption, optimally at the same time of day in order for it to be maximally effective [118, Parenthood]. LARCs, which include intrauterine devices (IUDs) and subdermal implants, minimize user error as they require little to no daily maintenance. LARCs are inserted by a provider and last between 3 and 12 years, depending on the device. IUDs contain either copper or progestin and are inserted in the uterus. Implants contain progestin and are inserted just beneath the skin of the upper arm [5]. With typical use,

LARCs are as effective as sterilization, but can be removed at any time, making them a good option for women who want to delay pregnancy now and conceive in the future [123].

For women with Medicaid, LARCs are provided as a family planning benefit with zero cost-sharing [127]. Traditionally, LARCs were inserted during an outpatient visit, but there has been a recent push by providers and advocates to offer LARCs in the immediate postpartum period, between 10 minutes after delivery and before the mother leaves the hospital. Advocates argue the immediate postpartum period is an ideal time for LARC placement as 40-75% of women who plan to use a LARC postpartum do not obtain one [4], likely due in part to low rates of follow-up visits, which are as low as 21% in some state Medicaid programs [22]. Even for women who do attend follow-up visits, 40-57% of women report unprotected intercourse prior to the 6-week follow-up visit, putting them at risk of short-interval pregnancy, 70% of which are unintended. Short-interval pregnancy is particularly high in adolescent populations, with up to 49% of previously pregnant adolescents experiencing short-interval pregnancy [89]. These pregnancies are more likely to result in adverse birth outcomes such as low birthweight and preterm delivery [18]. Consequently, advocates stress that these issues can be addressed by improved access to immediate postpartum LARCs.

However, a barrier to access of LARCs in the inpatient postpartum setting is the reimbursement system for births. Most state Medicaid programs reimburse for births using a global delivery fee, which includes all pregnancy-related care. Under this system, if a patient wanted to get an immediate postpartum LARC while she was still in the hospital, the hospital or the physician would have to

cover the cost of the device, which can be as much as $900, as Medicaid would not reimburse separately for it. Furthermore, physicians would not receive an additional fee for the insertion procedure. Beginning with South Carolina in 2012, states have begun to unbundle the LARC device and insertion fee from the global delivery fee. In these states, physicians and hospitals are able to receive a payment that covers the cost of the device, in addition to a payment for the insertion procedure. Advocates argue that unbundled reimbursement incentivizes physicians and hospitals to provide immediate postpartum LARCs, which in turn decreases unwanted pregnancies, short-interval pregnancies, and adverse birth outcomes.

To our knowledge, the effect of providing immediate postpartum LARCs has not been studied using a causal framework on a national scale. In a pre/post study, Paul et al. 2018 found that among a sample of 178 women who gave birth at a large, urban hospital, those giving birth after the immediate postpartum payment policy went into effect were 2.5 times more likely to use a LARC as their postpartum birth control. However, this method does not account for that fact that LARC use has likely been increasing over this time period. In another study, Higgins and coauthors 2018 take advantage of pseudo-random exposure to immediate postpartum LARCs due to delayed shipments from the hospital's provider. They find that compared to patients who gave birth when LARCs were available in the hospital, those who did not have the option to receive a LARC while still in the hospital were less likely to use a LARC postpartum and more likely to experience short-interval pregnancy. We contribute to this literature by studying the effects of increasing access to immediate postpartum LARCs on a national scale, taking advantage of variation in Medicaid policy across states and over time.

As of October 2018, 38 states and Washington DC provide unbundled reimbursement for immediate postpartum LARCs separate from the global delivery fee. In this paper, we test whether these policies have been effective at their stated goals: increasing LARC use, decreasing unintended pregnancies, and decreasing adverse birth outcomes such as low birthweight.

3.3 Data & Method

Data come from two sources: information on births come from the National Vital Statistics System (NVSS), and our data set of Medicaid LARC claims is the product of a primary data collection effort. Data from NVSS are available from the CDC WONDER database (https://wonder.cdc.gov/). These data provide counts of all births that occur in the United States regardless of resident status of parents. We queried counts for each calendar quarter and each state, where state is determined by mother's residence [15]. State of mother's residence is of primary importance, because this information allows us to determine if a woman is exposed to additional Medicaid LARC payments and has lower non-monetary obstacles to LARCs.

Because payer-type is not available in NVSS, we stratify our analyses and focus on groups that have high rates of Medicaid enrollment: teen mothers, single mothers, and mothers with a high school education or less. In addition to number of births, we examine birth outcomes, particularly low birth weight. We also use data on parity (number of births a woman has had) in NVSS; this policy should only affect parity greater than or equal to two, because the marginal LARC is implanted immediately after a birth.

In contrast to natality data, there is no centralized database of LARC use in Medicaid. We contacted the Medicaid offices in all 50 states and Washington, D.C. requesting quarterly counts of LARCs paid for by Medicaid. To date, 17 offices have provided us with data (Table 3.1). Ten states refuse to provide the requested information, either because they only release existing reports or only release information to residents of that state. Three states require a prohibitively expense fee. We are currently working with 10 states to obtain data and are continuing to attempt to contact the remaining states.

Our two main empirical strategies are event studies and difference-in-differences. Because states implement immediate postpartum LARC reimbursement at different times, the event studies reveal any trend change that occurs in the treatment year. The estimating equation is:

$$\ln(\text{Outcome}_{st}) = \beta_0 + \sum_{Q=-8}^{-1} \delta_Q \cdot 1(\text{Quarter}_{st} = Q) + \sum_{Q=1}^{6} \beta_Q \cdot 1(\text{Quarter}_{st} = Q) + \text{State}_s + \text{Time}_t \quad (3.1)$$

Observations are at the state-by-quarter of year level. Subscript s indicates state, and subscript t indicates time in quarters of a year. Outcomes include number of LARCs paid for by Medicaid, number of births, and frequency of low birthweight. The fixed effects for state and time are represented by State_s and Time_t. The coefficient δ_Q is the estimated difference in the outcome Q quarters before policy implementation compared to the outcome in the quarter before the policy was implemented. Likewise, the coefficient β_Q is the difference Q quarters after the policy was implemented. Since outcomes are logged, the coefficients represent the percent change in the outcome.

By controlling for state and year fixed effects, the remaining variation is

over-time and within-state. The identifying assumption is that there is no other change that is both correlated with the timing of the policy implementation and the outcomes. The event studies also provide evidence for the difference-in-differences assumptions; flat pre-trends provide evidence in support of the parallel trends assumption.

While the event studies give visual evidence of the effect of the policy, our main empirical strategy is difference-in-differences. We use variation in both the decision to implement separate reimbursement for immediate postpartum LARCs and variation in the timing of implementation. The estimating equation is:

$$\ln(\text{Outcome}_{st}) = \beta_0 + \beta_1 \text{PostbirthLARC}_{st} + \text{State}_s + \text{Time}_t + \varepsilon_{st} \qquad (3.2)$$

The variable $\text{PostbirthLARC}_{st}$ is a binary variable that equals 1 if Medicaid in state s reimburses for immediate postpartum LARCs at time t and equals 0 otherwise. The coefficient of interest is β_1 . By controlling for state and time fixed effects, the identifying variation for β_1 is within-state and over-time.

For β_1 to reflect the causal effect of Medicaid policies for immediate postpartum LARCs, the difference-in-differences assumptions must be met. The first assumption is that states' decision to reimburse for immediate postpartum LARCs is uncorrelated with outcome trends. Specifically, states that will soon start paying for immediate postpartum LARCs should have the same trends in outcomes as states that are not about to implement the policy. For instance, it would be a major concern if states that were already increasing their use of LARCs, or had relatively steep decreases in fertility, were more likely to start reimbursing for immediate postpartum LARCs. In this case, difference-in-differences estimates would be biased towards finding an effect, because the secular trend would be

attributed to the policy. We address this concern using evidence from the event studies to show that states that reimburse for postpartum LARCs do not exhibit a pre-existing upward or downward trend.

A second concern with difference-in-differences is that some other shock or policy is correlated with both Medicaid policies regarding postpartum LARCs and our outcomes of interest. We perform placebo tests using women who are unaffected by this policy. For instance, all mothers are exposed to the same state-time environment, but only mothers who have had at least one previous birth are affected by this policy. As a placebo test, we estimate the difference-in-differences regressions and event studies using women who have not previously given birth.

3.4 Results

The difference-in-differences regressions and event studies indicate that separate reimbursement for immediate postpartum LARCs does not have a meaningful effect on LARC use, births, or low-birthweight births. We also do not find significant effects among populations known to have higher levels of Medicaid coverage—teen mothers, unmarried mothers, or mothers with a high school or lower level of education.

Table 3.2 , first column and Figure 3.1 show the results of the difference-in-differences analyses and event studies for LARC use. The point estimate indicates that separate reimbursement *decreases* LARC use by 17% , though this effect is not statistically significant and imprecisely estimated. The large confidence interval means our estimate does not allow us to rule out effects as large

as a 50% decrease or a 16% increase in LARC use. The event study in Figure 3.1 shows a slight downward trend in LARC use before the policy is implemented, and the trend continues in the post period, which is consistent with the estimated negative effect in the difference-in-differences regression. Taken together, we interpret the results as indicating that separate reimbursement does not affect the number of LARCs received by women with Medicaid coverage. An important caveat is that the data used in the LARC analyses is incomplete; we are still in the process of receiving data from state Medicaid agencies, which we expect will increase the precision of our estimates.

Columns 2 through 5 of Table 3.2 give estimates of the effect of separate reimbursement on total births among the entire population, as well as specific subpopulations which have higher rates of Medicaid coverage: teen mothers, unmarried mothers, and mothers with a high school or lower level of education. For the entire population, as well as all subpopulations, the estimated effect of separate reimbursement on births is not significantly different from zero. Event studies of the outcomes, shown in Figure 3.2, indicate flat pre-trends, providing evidence in support of the parallel trends assumption. As with the regression results, the event studies do not indicate a meaningful effect of the policy on births, either overall or in any subpopulation. The coefficient in column 2 corresponds to a 0.5% increase in births as a result of separate reimbursement. The confidence interval, while relatively large, does allow us to rule out decreases in births larger than 1.7% and increases larger than 2.6%. The coefficients in columns 3 through 5, which give the estimated effects on subpopulations of particular interest, indicate that the policy has no effect on the number of births, with specific point estimates ranging from a 0.3% decrease to a 1% increase.

Table 3.3 and Figure 3.3 show the difference-in-differences coefficient estimates and the event studies for the effect of the policy on low-birthweight births. Column 1 of Table 3.3 shows the regression using the entire sample, while columns 2 through 4 show the estimates using subpopulations with higher levels of Medicaid coverage. The regression estimates and event studies indicate that the policy does not have a significant effect on the number of low-birthweight births. The event studies show flat pre-trends, lending support to the parallel trends assumption. The point estimates are all negative and correspond to decreases of between 0.7% and 3.4% in the number of low-birthweight births. As with total births, the effects are imprecisely estimated and do not allow us to rule out a null effect.

Because we are concerned about the potential for two-way fixed-effects difference-in-differences to be biased in the presence of time-varying treatment effects [56], we use the coefficients from the event studies to estimate the effect of the policy by averaging the coefficients on the event-time dummies in the post-period. These results are shown in Tables 3.4 and 3.5. Table 3.4 shows the estimated effect of separate LARC reimbursement on LARC use and all births, as well as births among subpopulations with higher rates of Medicaid coverage, while Table 3.5 shows the estimated effect on low-birthweight births. Consistent with our other results, these estimates indicate that the policy did not have a significant effect on LARC use, total births, or low-birthweight births.

The effects of separate LARC reimbursement on birth outcomes shown in Tables 3.2 through 3.5 use births occurring to women who have had at least one previous birth. This is because the separate LARC reimbursement policy only applies to women immediately after they give birth; first-time mothers are not

affected by this policy. We use this group of first-time mothers as a placebo test on which to estimate equations 3.1 and 3.2. The results of these placebo tests are shown in Tables 3.6 and 3.7 and Figures 3.4 and 3.5. The difference-in-differences coefficients in Tables 3.6 and 3.7 show that among the group of women untreated by the policy, separate LARC reimbursement does not have a significant effect on total births or low-birthweight births, overall or among any subpopulation. The event studies in Figures 3.4 and 3.5 are consistent with this result. While there do appear to be some slight decreases in births in the event studies, averaging the post-policy event-time coefficients does not yield statistically significant results. These placebo tests provide some evidence that there were no concurrent policy shocks.

3.5 Conclusion

We leverage state-level policies that provide Medicaid reimbursement for immediate postpartum LARCs to test the effect of non-monetary costs on use of medical care. These policies reduce non-monetary costs by eliminating the time and stress associated with arranging a doctor's appointment, traveling to a health clinic, and obtaining childcare. In contrast to economic intuition, we consistently find null results for the effect of these policies on both Medicaid payments for LARCs and birth outcomes. However, since our estimates are not precise, we are unable to rule out meaningful effects. As many of these state-level policies are recent, precision will likely increase as more post-period data becomes available.

If these state-level policies did affect medical care and births, we would

know that hospitals, health-care providers, and patients all respond to changing incentives. In order for a patient to choose an immediate postpartum LARC, her health-care provider must first offer the procedure; and for the provider to offer immediate postpartum LARCs, hospitals must stock and allow immediate postpartum LARCs. If either hospitals or doctors refuse to increase availability of immediate postpartum LARCs to Medicaid patients, the non-monetary costs for these new mothers would not decrease. A possibility is that some hospitals are unwilling to stock LARCs for inpatient use, either because this benefit is mostly unavailable for privately insured women [92] or because of religious objections [62, 124]. There is some evidence that this might be partially the case: a survey of physicians who treat Medicaid patients in Colorado found that after Colorado implemented immediate postpartum LARC reimbursement, 35% of physicians who did not provide the service cited hospital policies restricting contraceptive provision. Other reasons cited included patients' disinterest in LARCs (27%) and concerns about safety (14%) [122]. Incorrect beliefs about safety have been documented in other studies as well [101, 104]. There is also some evidence that physicians may be unaware of the policy change, as less than half of physicians surveyed in Colorado were aware of the policy change 1 year after implementation [121]. All of these are possible reasons why we find no effect of the policy on LARC use or births.

The data we currently have do not allow us to determine whether physicians and/or hospitals are unwilling to provide the service. A next step is to investigate the availability of immediate postpartum LARCs to determine if pregnant women have the option to get a LARC immediately after giving birth. To get a more detailed picture of hospital, provider, and patient behavior, we plan to obtain hospital and outpatient discharge data for at least one state. Such data

will give insight about which, if any, hospitals and doctors provide access to immediate postpartum LARCs. If only certain providers insert immediate postpartum LARCs or only certain hospitals stock LARCs (for example, only non-catholic hospitals), we can more accurately define treatment and control groups to identify whether this policy affects LARC use. Furthermore, discharge data will allow us to measure births among Medicaid patients, which is a major limitation of the NVSS data.

Any increase in immediate postpartum LARC use can have a meaningful impact on number and timing of births, both of which are important outcomes. However, non-monetary costs might be less salient than financial costs to patients, and many new mothers might not respond to changing availability of immediate postpartum LARCs. Limited data currently prevent the precision needed either to reject null effects or to rule out meaningfully large estimates, but we are hopeful that future data collection will help us shed additional light on this question.

3.6 Tables

Table 3.1: Implementation Dates for Separate Reimbursement for Immediate Postpartum LARC

State	Date	LARC Data	State	Date	LARC Data
South Carolina	Mar-12		Missouri	May-16	
New Mexico	Nov-13	✓	Kentucky	Jul-16	
Iowa	Mar-14	✓	Mississippi	Jul-16	
Georgia	Apr-14		South Dakota	Jul-16	
New York	Apr-14		Arizona	Oct-16	
Louisiana	Jun-14	✓	Florida	Oct-16	✓
Maryland	Sep-14		Hawaii	Nov-16	
Oklahoma	Sep-14	✓	Pennsylvania	Dec-16	
DC	Oct-14		Oregon	Jan-17	
Montana	Jan-15		Virginia	Jan-17	
Rhode Island	Jan-15	✓	West Virginia	Jan-17	✓
Idaho	Mar-15	✓	Wisconsin	Jan-17	
Wyoming	Mar-15		Colorado	Jul-17	
Indiana	Jun-15		Ohio	Jul-17	
California	Jul-15		Vermont	Oct-17	✓
Illinois	Jul-15		Maine	Nov-17	
Washington	Sep-15		Tennessee	Nov-17	
Delaware	Jan-16		New Hampshire	Jan-18	
Texas	Jan-16		Massachusetts	Mar-18	✓
Connecticut	Apr-16				

Non-implementing states with LARC data: Kansas, Nevada, North Carolina, North Dakota, Utah, Michigan, New Jersey

States with a checkmark in the LARC data column are states for which we have complete data on LARC use

Table 3.2: Difference-in-Differences Estimates, Effect of Separate LARC Reimbursement on LARC Use and Births

	LARC Use, All	**Births, All**	**Births, Teen**	**Births, Unmarried**	**Births, HS or less**
Estimated Effect	-0.169	0.005	-0.003	-0.005	0.010
	[-0.500, 0.162]	[-0.016, 0.026]	[-0.072, 0.066]	[-0.033, 0.023]	[-0.030, 0.050]
# states with data	17	50	50	50	50
# states implementing policy	10	19	19	19	19

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; 95% confidence intervals in brackets

Table presents estimates of β_1 from equation 3.2. Column 1 presents the results using the logged number of LARCs as the outcome variable, and columns 2 through 4 present the results using the logged number of births among different subpopulations as the outcome variable. Effects can be interpreted as a $\beta * 100$ percentage point change in the outcome variable in response to separate LARC reimbursement. The samples used for the birth outcomes only include mothers who have previously given birth (i.e., the mothers exposed to the policy). Regressions include controls for Medicaid expansion and lagged unemployment rate. Standard errors are clustered by state.

Table 3.3: Difference-in-Differences Estimates, Effect of Separate LARC Reimbursement on Low-Birthweight Births

	All	**Teen**	**Unmarried**	**HS or less**
Estimated Effect	-0.007	-0.034	-0.020	-0.013
	[-0.038, 0.024]	[-0.160, 0.090]	[-0.063, 0.024]	[-0.058, 0.031]
# states with data	50	50	50	50
# states implementing policy	19	19	19	19

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; 95% confidence intervals in brackets

Table presents estimates of β_1 from equation 3.2 using the logged number of low-birthweight births as the outcome. Column 1 presents results from the entire sample, while columns 2 through 4 present results from subpopulations with higher rates of Medicaid coverage. Effects can be interpreted as a $\beta * 100$ percentage point change in the outcome variable in response to separate LARC reimbursement. The samples only include mothers who have previously given birth (i.e., the mothers exposed to the policy). Regressions include controls for Medicaid expansion and lagged unemployment rate. Standard errors are clustered by state.

Table 3.4: Effect of Separate LARC Reimbursement on LARC Use and Births, Average Post-Policy Event-Time Coefficients

	LARC Use, All	**Births, All**	**Births, Teen**	**Births, Unmarried**	**Births, HS or less**
Estimated Effect	-0.125	-0.006	-0.07	-0.002	0.027
	[-0.276, 0.026]	[-0.030, 0.019]	[-0.164, 0.022]	[-0.040, 0.035]	[-0.030, 0.085]
# states with data	17	50	50	50	50
# states implementing policy	10	19	19	19	19

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; 95% confidence intervals in brackets

Table presents the effect of separate LARC reimbursement on LARC use and births estimated by averaging the post-policy event-time coefficients from the event study in equation 3.1 (β_q). Effects can be interpreted as a $\beta * 100$ percentage point change in the outcome variable in response to separate LARC reimbursement. The samples used for the birth outcomes only include mothers who have previously given birth (i.e., the mothers exposed to the policy). Regressions include controls for Medicaid expansion and lagged unemployment rate. Standard errors are clustered by state.

Table 3.5: Effect of Separate LARC Reimbursement on Low-Birthweight Births, Average Post-Policy Event-Time Coefficients

	All	**Teen**	**Unmarried**	**HS or less**
Estimated Effect	-0.020	-0.068	-0.038	0.003
	[-0.059, 0.020]	[-0.283, 0.147]	[-0.099, 0.023]	[-0.060, 0.066]
# states with data	50	50	50	50
# states implementing policy	19	19	19	19

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; 95% confidence intervals in brackets

Table presents the effect of separate LARC reimbursement on low-birthweight births estimated by averaging the post-policy event-time coefficients from the event study in equation 3.1 (β_q). Column 1 presents results from the entire sample, while columns 2 through 4 present results from subpopulations with higher rates of Medicaid coverage. Effects can be interpreted as a $\beta * 100$ percentage point change in the outcome variable in response to separate LARC reimbursement. The samples only include mothers who have previously given birth (i.e., the mothers exposed to the policy). Regressions include controls for Medicaid expansion and lagged unemployment rate. Standard errors are clustered by state.

Table 3.6: Placebo Difference-in-Differences Estimates, Effect of Separate LARC Reimbursement on Births Among Women Not Exposed to the Policy

	Births, All	**Births, Teen**	**Births, Unmarried**	**Births, HS or less**
Estimated Effect	-0.009	-0.007	-0.014	-0.024
	[-0.034, 0.017]	[-0.045, 0.031]	[-0.037, 0.009]	[-0.081, 0.033]
# states with data	50	50	50	50
# states implementing policy	19	19	19	19

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; 95% confidence intervals in brackets

Table presents placebo estimates from the sample of mothers who have not previously given birth and are therefore not exposed to the policy. Each column contains the estimate of β_1 from equation 3.2 using the logged number of births among different subpopulations as the outcome variable. Effects can be interpreted as a $\beta * 100$ percentage point change in the outcome variable in response to separate LARC reimbursement. Regressions include controls for Medicaid expansion and lagged unemployment rate. Standard errors are clustered by state.

Table 3.7: Placebo Difference-in-Differences Estimates, Effect of Separate LARC Reimbursement on Low-Birthweight Births Among Women Not Exposed to the Policy

	All	**Teen**	**Unmarried**	**HS or less**
Estimated Effect	-0.009	-0.026	-0.010	-0.014
	[-0.039, 0.022]	[-0.082, 0.030]	[-0.044, 0.024]	[-0.053, 0.024]
# states with data	50	50	50	50
# states implementing policy	19	19	19	19

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; 95% confidence intervals in brackets

Table presents placebo estimates from the sample of mothers who have not previously given birth and are therefore not exposed to the policy. Each column contains the estimate of β_1 from equation 3.2 using the logged number of low-birthweight births among different subpopulations as the outcome. Effects can be interpreted as a $\beta * 100$ percentage point change in the outcome variable in response to separate LARC reimbursement. Regressions include controls for Medicaid expansion and lagged unemployment rate. Standard errors are clustered by state.

3.7 Figures

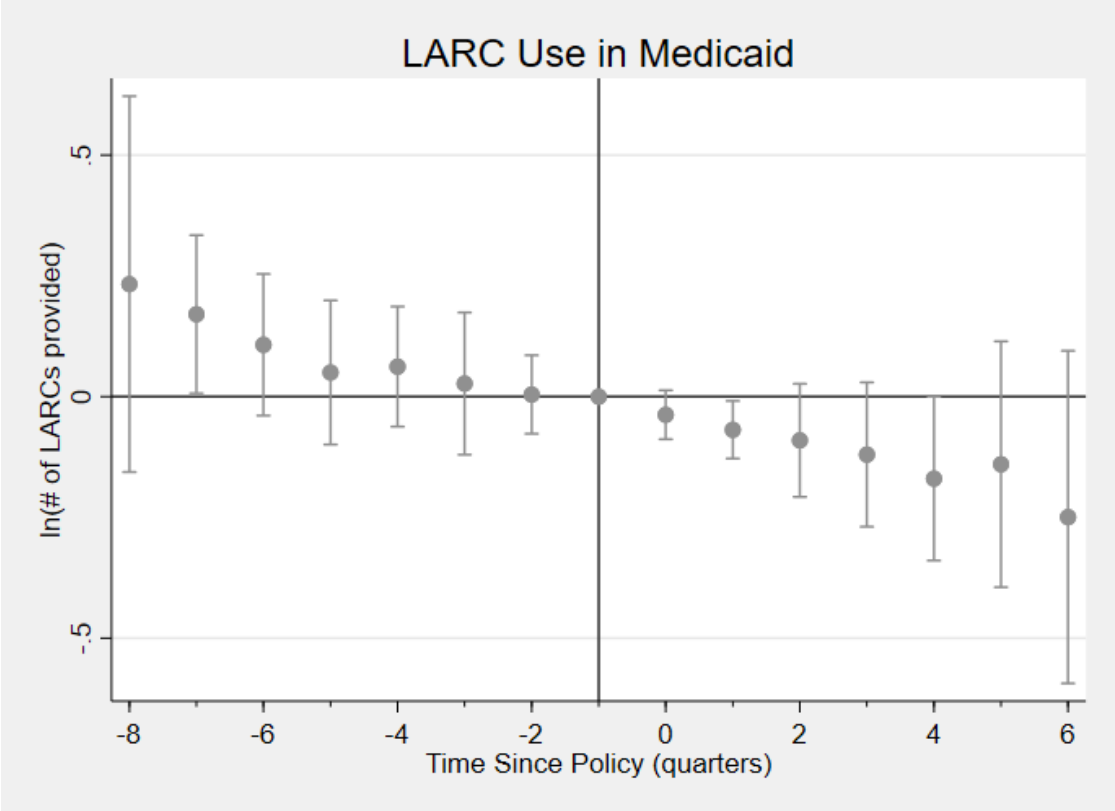

Figure 3.1: Event Study Analysis of Effect of Separate LARC Reimbursement on LARC Use

Figure graphs the estimates of δ_Q and β_Q from equation 3.1 using the logged number of LARCs paid for by Medicaid as the outcome. Regressions include controls for Medicaid expansion and lagged unemployment rate. Sample includes states for which we have data on LARC use (Table 3.1). Standard errors are clustered by state. Whiskers are 95% confidence intervals and time 0 is three quarters after policy implementation.

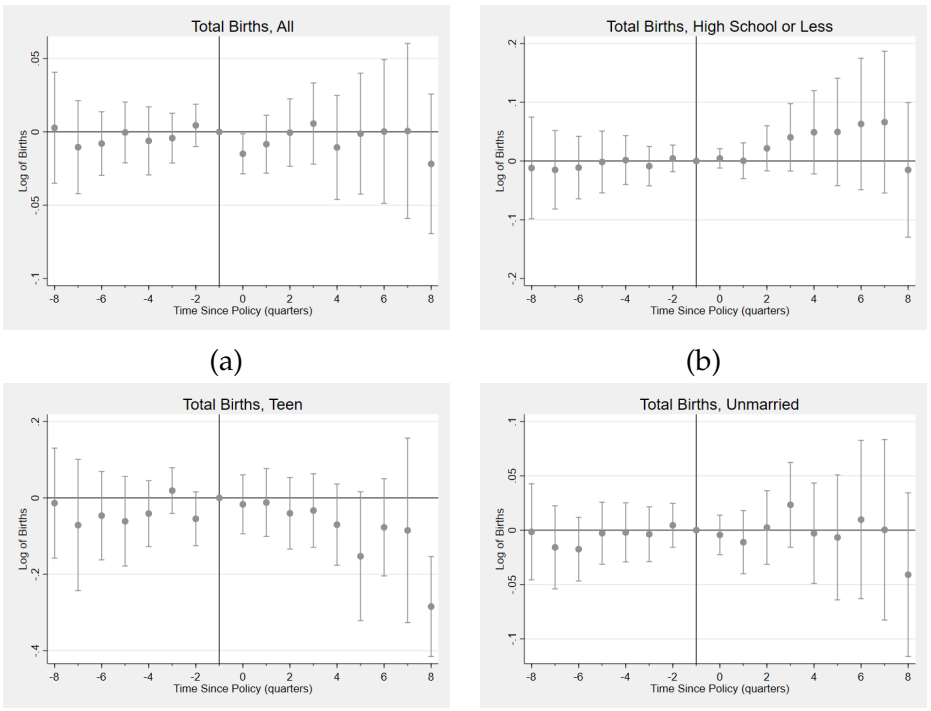

Figure 3.2: Event Study Analysis of Effect of Separate LARC Reimbursement on Births

Figures graph the estimates of δ_Q and β_Q from equation 3.1 using all births as the outcome. Panel A presents results from the entire sample and Panels B, C, and D present results from subsamples with higher rates of Medicaid coverage. Regressions include controls for Medicaid expansion and lagged unemployment rate. Standard errors are clustered by state. The samples only include mothers who have previously given birth (i.e., the mothers exposed to the policy). Whiskers are 95% confidence intervals and time 0 is three quarters after policy implementation.

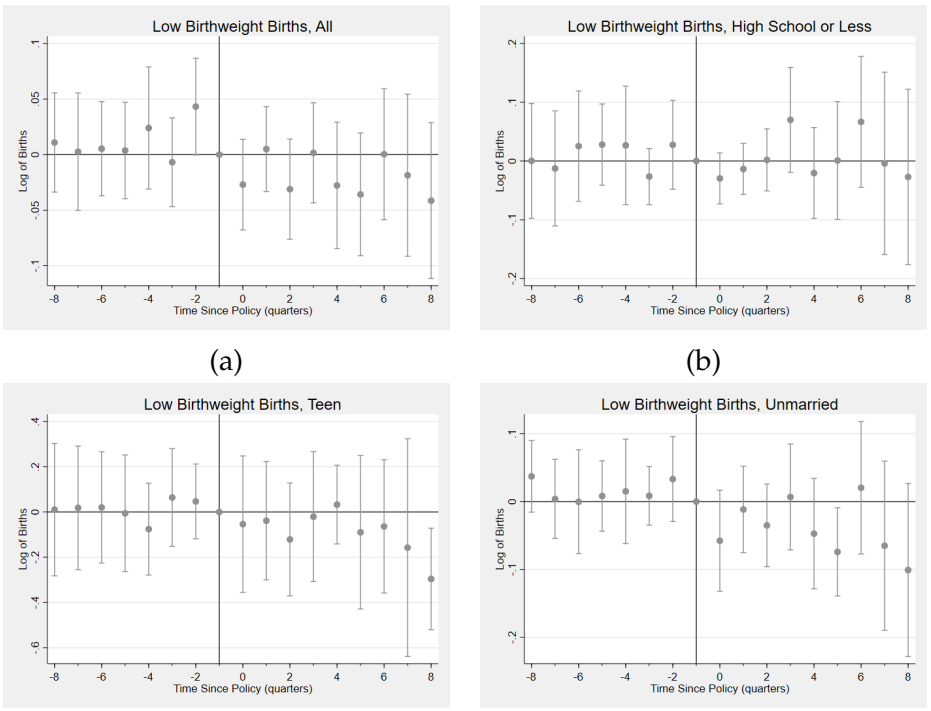

Figure 3.3: Event Study Analysis of Effect of Separate LARC Reimbursement on Low-Birthweight Births

Figures graph the estimates of δ_Q and β_Q from equation 3.1 using low-birthweight births as the outcome. Panel A presents results from the entire sample and Panels B, C, and D present results from subsamples with higher rates of Medicaid coverage. Regressions include controls for Medicaid expansion and lagged unemployment rate. Standard errors are clustered by state. The samples only include mothers who have previously given birth (i.e., the mothers exposed to the policy). Whiskers are 95% confidence intervals and time 0 is three quarters after policy implementation.

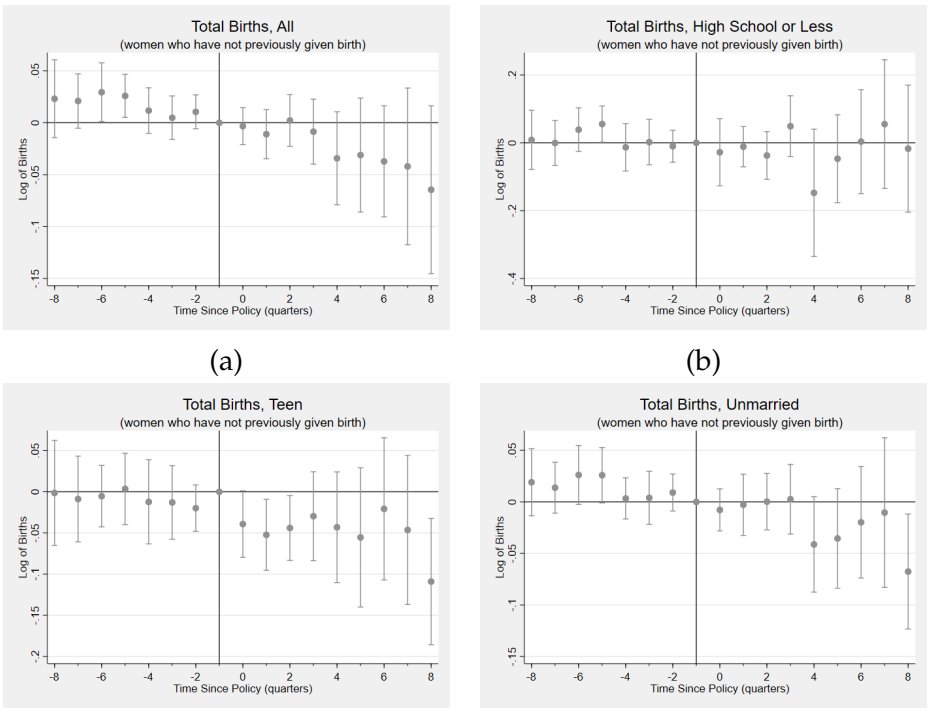

Figure 3.4: Event Study Analysis of Effect of Separate LARC Reimbursement on Births Among Women Not Exposed to the Policy

Figure presents placebo estimates from the sample of mothers who have not previously given birth and are therefore not exposed to the policy. Figures graph the estimates of δ_Q and β_Q from equation 3.1 using all births as the outcome. Panel A presents results from the entire sample and Panels B, C, and D present results from subsamples with higher rates of Medicaid coverage. Regressions include controls for Medicaid expansion and lagged unemployment rate. Standard errors are clustered by state. Whiskers are 95% confidence intervals and time 0 is three quarters after policy implementation.

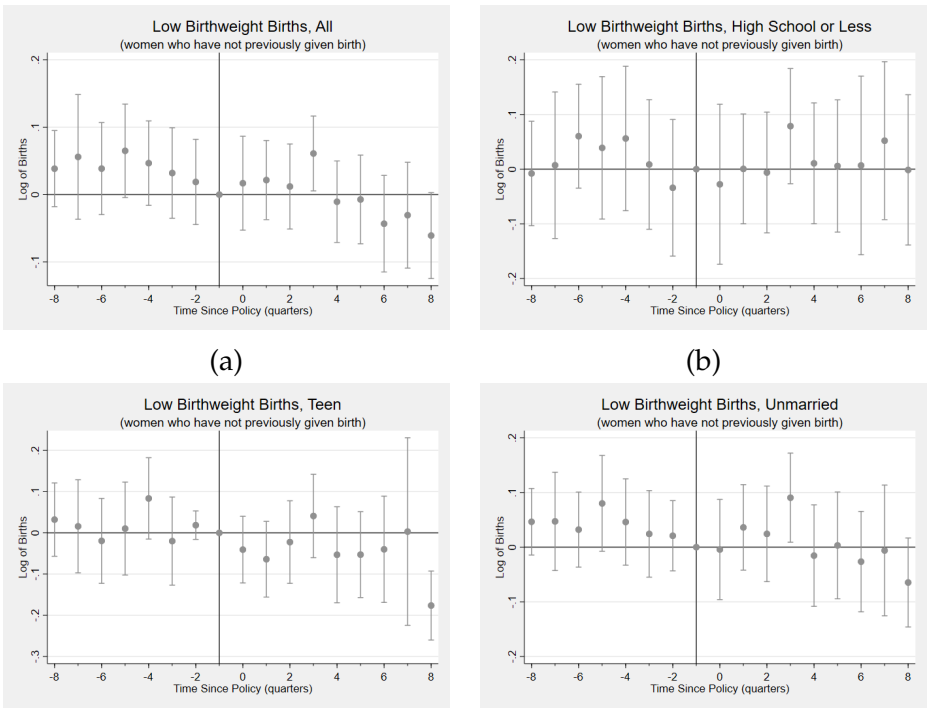

Figure 3.5: Event Study Analysis of Effect of Separate LARC Reimbursement on Low-Birthweight Births Among Women Not Exposed to the Policy

Figure presents placebo estimates from the sample of mothers who have not previously given birth and are therefore not exposed to the policy. Figures graph the estimates of δ_Q and β_Q from equation 3.1 using low-birthweight births as the outcome. Panel A presents results from the entire sample and Panels B, C, and D present results from subsamples with higher rates of Medicaid coverage. Regressions include controls for Medicaid expansion and lagged unemployment rate. Standard errors are clustered by state. Whiskers are 95% confidence intervals and time 0 is three quarters after policy implementation.

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