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Essays on Health Care Payment Incentives

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Essays on Health Care Payment Incentives
by
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Table of Contents

List of Figures

List of Tables

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David Adam Schwartzman

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ABSTRACT OF THE DISSERTATION

Essays on Health Care Payment Incentives

by

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Professor Barton Hamilton, Chair

This dissertation studies how payment incentives in health care change clinician and patient behavior and how different "value-based" payment models may impact outcomes. In Chapter 1, I explore optimal design of healthcare payment models in the presence of market frictions to targeting benefits. I leverage a natural experiment where an employer introduces Direct Primary Care, a form of care delivery where clinicians are paid a capitated monthly fee for high-touch access to a bundle of primary care services. This model modifies moral and behavioral hazard incentives for both patients and clinicians while changing the trade-off between preventive investments and downstream care. I document selection into Direct Primary Care by younger and less costly employees, who have lower primary care spending and shorter job tenures, suggesting the importance of switching costs. Using an instrumental variables approach that leverages plan inertia and a difference-in-difference strategy, I examine the impact on costs of care and demand for preventive care and low-value cardiac imaging. Patient out-of-pocket costs increase for those choosing the Direct Care Plan, and total costs increase for lower spenders but

not for more expensive patients. I also find a decrease in potentially low-value imaging and an increase in high-value mammography screening, suggesting that quality improves.

Chapter 2, joint with Ross Klosterman and Namrata Ramakrishna, provides the first data on Direct Primary Care practices nationally and the characteristics related to practice pricing and location decisions. Average adult price charged by a DPC practice is $81.33 per month. Median income and not accepting children as patients is associated with higher prices. Offering more services is not associated with higher prices, and medication dispensing is associated with lower prices. Lower poverty rate, lower percentages of black residents, and higher education status is associated with more physical locations, with broader implications for access to care and organizational care delivery structure in health care.

In Chapter 3, I examine health system responses to changes in payment incentives, studying the Maryland global hospital budget model, which previous evaluations have shown promise in reducing costs. However, the mechanisms by which improved outcomes were achieved have not been studied. Using a dynamic difference-in-difference model and testing the sensitivity of results to the presence of pre-trends, I examine changes in health system investments following the adoption of the global hospital budget model. I find a decrease in volume, a shift from clinical to non-clinical employees in Maryland relative to other states, and an increase in the administrative share of spending, suggesting a shift to lower-skilled workers and a change in care functions. Heart failure readmissions fall and pneumonia mortality increases, while technology adoption is unchanged.

Preface

The United States faces growing health care costs higher than any other country while having health outcomes worse than most other Organization for Economic Cooperation and Development countries. This dissertation studies the potential of novel payment and delivery models to shift patient and clinician incentives with the goal of improving patient quality of care while decreasing price. I study how patients and clinicians respond to changes in incentives from these new delivery models in several different contexts.

In Chapter 1, I examine the outcomes of employer contracting with physician organizations that change clinician and patient payment incentives through a payment and delivery structure called Direct Primary Care in a context where payment models are mixed with patient choice. In Chapter 2, I turn to clinician organizations and further study Direct Primary Care practices nationally. In Chapter 3, I examine government payment regulation of health systems to study how payment incentives influence hospital staffing and investment decisions. In all chapters, I examine value-based reimbursement models and their effects in varying contexts.

Chapter 1: The Interaction of Patient and Clinician Incentives: Evidence from Direct Primary Care*

1.1 Introduction

Health care spending in the United States has grown rapidly, outpacing inflation and wage growth. In light of these increases, insurance plan designs have targeted demand incentives, aiming to balance moral hazard concerns about over-utilization due to patients not bearing the full cost of care with behavioral health hazard concerns about patient underinvestment in preventive care due to behavioral biases (Baicker, Mullainathan, and Schwartzstein, 2015; (Brot-Goldberg, Chandra, Handel, and Kolstad, 2017; Chandra, Flack, and Obermeyer, 2021; Ho and Lee, 2022). Supply side incentives have also received attention, as clinicians play a central role in determining health care utilization through treatment and referral choices. A variety of organizational models, such as health maintenance organizations and accountable care organizations, have attempted to shift clinician incentives from "fee-for-service" to "value-based" reimbursement because of concern that fee-for-service payment creates clinician moral hazard, as clinicians may induce demand for procedures regardless of their clinical value. However, the interaction of patient and clinician incentives has not been considered in the context of market frictions to targeting payment models.

I study a growing alternative model of clinician organization known as Direct Primary Care (DPC) that changes payment incentives for patients and clinicians. DPC practices charge a monthly fee for a bundle of primary care and chronic disease management services and typically offer shorter waits and longer appointment times, with many offering ancillary services at some cost (such as medication dispensing or labs). This model changes incentives for both clinicians and patients, as clinicians receive capitated

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payments while patients face zero cost-sharing for primary care visits. In addition to the compensation model, these practices trade off increased access to care for increased primary care cost. Despite the rapid growth of this form of clinician organization, little is known about the effects of Direct Primary Care.

I use a novel proprietary dataset from an employer that contracted with a Direct Primary Care practice to understand its effect on spending and treatment choices. The introduction of Direct Primary Care allows me to study a simultaneous shift to several trade-offs: a shock to clinician and patient incentives and to the trade-off between preventive health investment requiring up-front costs and downstream utilization and spending. This change could increase or decrease health care costs depending on the effect on downstream utilization and spending. Increased access to DPC may induce additional primary care visits, which could lead to additional specialty care. Alternatively, DPC access could lead patients to substitute away from specialty care to primary care, or could change clinician substitution patterns between time and referrals (Freedman et al., 2021). Finally, the trade-off between preventive health investment and reductions in downstream utilization depends on the health status of the patients who sign up and how DPC changes their health status.

The Direct Care plan introduced by the employer featured zero out-of-pocket costs for primary care at the DPC practice and unlimited appointments. The contracted practice advertised same or next-day appointments, 24/7 communication with physicians via phone and email, in-house prescriptions at lower prices, virtual specialist consultations at no cost, and discounted lab and imaging services. There are no explicit bonuses or penalty payments in the contract between the employer and the practice, and practice incentives are aligned with the employer only through the dynamic nature of the contract – if the employer does not achieve the desired outcomes, the contract may not be renewed, and the clinic will lose future revenue. The introduction of DPC provides a natural experiment that allows me to estimate both demand for DPC and selection in plan choice and how the increased investment in primary care access impacts overall and specialist costs.

In the commercially insured health care setting, where the spending distribution is right-skewed, employers are particularly interested in impacts for the small portion of employees with the highest medical expenses, so they may have heterogeneous weights on patient preferences when designing benefits. However, market frictions in targeting benefits from regulatory constraints and systematic choice errors suggest that these heterogeneous weights may create welfare losses for patients with lower spending (Samek and Sydnor, 2021; Liu and Sydnor, 2022; Tilipman, 2022). Therefore, I study how the demand for and the impact of DPC varies by health spending level. My work informs policy design about the optimal set of incentives for healthcare payment in the presence of market frictions for targeting.

I first offer descriptive evidence of selection driven by incumbent employees which is not present for employees hired after the introduction of DPC, suggesting a potential role of plan inertia in determining plan choice. Next, with a plan choice model, I find that job tenure and out-of-pocket primary care spending are negatively associated with choosing the Direct Care plan for pre-DPC hires, while median zip code income and partial year enrollment is positively associated with choosing the plan. These findings suggest an important role for primary care switching costs in the demand for DPC, especially since out-of-pocket specialist spending has no significant impact on plan choice. Longer distance of DPC practices is associated with more demand for DPC for pre-existing hires, suggesting that distance is not driving DPC choice for these employees. For new hires, being closer to a DPC practice is associated with more demand for Direct Care, and being female and being an administrator or professional employee is associated with less demand.

Motivated by descriptive evidence of plan inertia, I restrict the sample to those hired a few years prior to the introduction of DPC and those hired a few years after, and examine the impact on the outcomes of interest by instrumenting for whether an employee is hired after the introduction of the Direct Care plan. I then estimate the impact on the total cost of care and patient cost of care, examining potential mechanisms for any changes in cost. I also do a propensity score weighted difference-in-difference analysis, restricting the sample to those who never switch to the Direct Care plan and those who choose the Direct Care plan in the first year, when most patients switch. In addition to spending outcomes, I measure the effect on non-primary care and emergency room (ER) utilization. To examine quality effects, I also estimate the effect on preventive mammography screenings and potentially low-value cardiac imaging. For identification of the difference-in-difference model, I assume that conditional on patient fixed effects and the propensity to choose the Direct Care plan, patient costs would have evolved similarly between patients who switched to the Direct Care plan and those that did not switch to the Direct Care plan.

I find no change in mean medical spending once Direct Care is implemented, although there is a noticeable increase in median medical spending, suggesting that the effects of DPC may differ based on the level of previous spending. There is no significant effect of DPC on total medical spending with both the instrumental variables and the difference-in-difference models. However, with a quantile difference-in-differences model, I find that spending increases for those on lower levels of the spending distribution, with spending increasing by 118% for the median spender. Patient out-of-pocket costs increase, driven by the lack of health reimbursement account (HRA) access. I find no effect on overall non-primary care spending, suggesting that increased primary care spending alone can be insufficient to reduce specialty spending. Finally, I provide evidence of a decrease in emergency room utilization and in potentially low-value cardiac imaging along with an increase in likely high-value mammography screening, suggesting that treatment changes from payment incentives may lead to improved allocative efficiency, which I use as a proxy for quality (Ho and Pakes, 2014; Eliason et al., 2020; Ding and Liu, 2021). My results suggest that while quality improvements may be present, the model is poorly targeted, as DPC is a higher-touch model with more demand from healthier patients. Weak incentive alignment between clinicians and employers because of the lack of an explicit incentive to reduce costs or to use non-primary care resources less intensively may contribute to the expenditure effects.

Similar to other work, I find evidence of the importance of primary care relationships, finding that travel distance may not be as important a factor in PCP choice when there is a pre-existing primary care relationship (Sabety, 2022). My paper also closely relates to work on selection and outcomes in concierge care (Leive, David, and Candon; 2022) and direct employer care provision through worksite clinics (Chen, 2016; Engberg et al., 2018). I find an increase in spending concentrated among lower-cost employees, similar to Chen (2016), who reports utilization increases for low-utilizers. Like Leive, David, and Candon (2022), I find no evidence that sicker patients have higher demand for high-touch care or that high-touch care lowers spending, although there is evidence of improved allocative efficiency when payment incentives also shift. My paper also contributes to the literature on choice offerings, inertia, and implications for selection in health plan choice (Einav et al., 2013; Handel, 2013; Ericson, 2014; Abaluck and Gruber, 2020; Hua, 2022), studying demand for plans with non-standard attributes and potential implications for adverse selection. Unlike this literature on inertia and plan choice, I am not using choice as an outcome variable; I instead use inertia's impact on plan choice as identifying variation for the impacts of the plan itself.

Alternative payment models have been more common in Medicare than among the commercially insured (Milad et al., 2022), and studies on their effects have mainly concentrated on Medicare as well (Alexander, 2020; Gupta, Martinez, and Navathe, 2022; Einav et al, 2022). My results suggest that this may be because of frictions to targeting higher-touch models, as costs increase for cheaper patients, and the average Medicare patient has higher health needs compared to the commercially insured. These results are relevant to policymakers, payers, employers, and others designing payment and delivery models in settings with patient choice and market frictions, particularly in settings where larger up-front investments are required.

The remainder of the paper is organized as follows. Section 1.2 provides additional context regarding Direct Primary Care. Section 1.3 contains additional information about the employer's health care offerings and presents descriptive statistics about medical costs and plan choice. Section 1.4 analyzes who chooses DPC. Section 1.5 presents the impact of Direct Care on costs and on the distribution of costs, while Section 1.6 presents the impact of Direct Care on preventive screenings and allocative efficiency. Section 1.7 concludes.

1.2 Background on DPC Practices

1.2.1 History of Direct Primary Care

There is a long history of capitation models in the United States, including Kaiser Permanente, an integrated managed care delivery system which evolved out of an employer-provided health care clinic in the 1930s. Capitated reimbursement increased in prevalence after the Health Maintenance Organization Act in 1973. HMOs with capitated models increasingly grew from the 1980s onward. Evidence suggests that they decreased costs, although they also attracted healthier patients (Manning et al, 1984; Glied, 1999; Cutler, McClellan, and Newhouse, 2000; Gaynor, Rebitzer, and Taylor, 2004). However, HMO models with capitated payments were associated with unpopular "managed care" approaches that included barriers to accessing care. Capitated models decreased in prominence from the 2000s on as managed care receded. (Zuvekas and Cohen, 2010; Pinkovskiy, 2020).

DPC practices were an outgrowth of the concierge care movement, where high-income individuals paid membership fees for enhanced primary care access, in addition to insurance visits on a fee-for-service basis. Qliance Medical Group in Washington was one of the first practices to popularize the DPC model in the mid-2000s, charging a monthly membership fee that varied by age in place of fee-for-service insurance payments at a more accessible price point than concierge practices (Wu, Bliss, Bliss, and Green 2010). DPC practices charge a monthly fee for a bundle of primary care services and offer shorter waits and longer appointment times, with some offering additional services at some cost (such as medication dispensing or labs). Eskew and Klink (2015) define Direct Primary Care as follows:

"a primary care practice that:

(1) charges a periodic fee for services,

(2) does not bill any third parties on a fee-for-service basis, and

(3) any per-visit charges are less than the monthly equivalent of the periodic fee."

Initially, most DPC practices contracted with individuals or families, discounting bundled prices for families. Increasingly, DPC practices have started to contract with employers as well, and most patient growth over the last several years has come from patients receiving access to DPC through their employer (Hint Trends Report, 2022). DPC practices also differ in their contracting behaviors and their organizational structure. Some practices operate in a hybrid model, seeing some patients on a fee-for-service basis and others as part of a membership model. Some practices are single-location practices with one or two clinicians, while other practices employ a larger number of clinicians or affiliate with larger practices to increase the geographic area in which they can provide services to employers. The average monthly price for an adult DPC membership is $81.33 (Klosterman, Schwartzman and Ramakrishna, 2023).[1]

1.2.2 Practice Growth

While exact numbers are not available, most growth in DPC has been recent, as there were 21 practices in 2010, 273 known practices as of 2015, and 2,018 known practices listed as of April 2023 on the DPC Mapper website (Eskew and Klink, 2015; DPC Mapper, 2023). One company providing administrative

[1] For more detail about how practice and geographic characteristics relate to pricing, see Klosterman, Schwartzman, and Ramakrishna, (2023).

services to DPC practices noted 3,500 clinicians and 800,000 patients on their platform, with 51% of practice memberships paid for by an employer as of December 2021. (Hint Trends Report, 2022). The continued rapid growth of the DPC practice model suggests the importance of understanding the demand for this care delivery model and its effects.

1.2.3 Theoretical Framework for Incentive Differences from Previous Models

Contracting with a DPC practice has a theoretically ambiguous impact on costs. While primary care spending increases, there could be an impact on non-primary care costs through several channels. While other plan designs increasing in prevalence, such as high deductible health plans (HDHPs), may lessen concerns about moral hazard through primary care cost-sharing, they increase behavioral moral hazard concerns about patient underinvestment in preventive care due to behavioral biases (Baicker, Mullainathan, and Schwartzstein, 2015; Chandra, Flack, and Obermeyer, 2021). Increased access through DPC should reduce the impact of behavioral health hazard and may induce patients to visit a primary care physician (PCP) instead of not interacting with the health care system (Gruber, Sabety, Sood, and Bae, 2022). Additionally, primary care access may lead patients to substitute away from specialty care or from emergency room use that is driven by a lack of access to preventive care. Alternatively, more frequent interactions with a PCP could induce demand for care of varying degrees of clinical benefit, (Kaestner and Lo Sasso, 2015; Cliff, Hirth, and Fendrick, 2019), especially since clinicians have no explicit incentive to reduce specialty care. However, longer appointment times could lead clinicians to substitute additional time for a referral or prescription (Freedman et al., 2021). While clinicians working for the DPC practice are constrained by the implicit incentive to lower costs so the employer contract is renewed, practice treatment costs are lower when referring outside the practice instead of treating within the practice, so the financial incentives may be insufficient to reduce specialty utilization.

DPC guarantees a minimum level of medical spending, so it is likelier to lead to an increase in spending at levels of medical spending below that threshold. Market frictions make it difficult to target DPC, as price and benefit differences between plans based on health status are illegal. While the DPC clinic would have the incentive to limit care or frequently refer to specialists instead of providing care within the practice with a one-period contract, the desire for a contract with the employer in future periods helps align the incentives between the employer and the practice. However, employer plan menu switching costs may lessen the incentive alignment between the practice and the employer. Finally, there may be an impact on costs through changes in health status due to increased investments in preventive health. However, to the extent that increased investments in preventive care could improve long-term health outcomes, employers have an under-incentive to invest in preventive care because employees may switch jobs or retire before long-term costs are realized.

Also, it is unclear who would most value increased DPC practice access. While primary care access increases, this is only for the clinicians that have contracted with the employer, which will not be the existing PCP for most patients. While older patients and higher users of health care could potentially derive more benefit from a high-touch model, they are also likelier to have existing relationships with primary care physicians that make switching costs higher. Switching costs could also vary based on practice distance. Given the non-traditional plan design, a DPC plan may be seen as further away in the product space from a traditional plan, so approximate inertia could play a large role (Abaluck and Gruber, 2022). At lower levels of health need, DPC is likelier to mechanically lead to increased spending due to the membership fee. Ideally, a more expensive high-touch primary care model like DPC would be targeted towards individuals that could most benefit from the model, likely to be higher-need patients. Given the potentially heterogeneous impacts of the benefit of DPC, plan choice has implications for total welfare, depending on whether demand for Direct Care is higher among those who would benefit more from DPC.

Figure 1.1 illustrates the trade-offs between different plan designs and clinician compensation arrangements. High-deductible health plans increase patient cost-sharing and reduce moral hazard, leading patients to cut back on all care and increasing behavioral hazard, potentially worsening health outcomes and future health costs. Alternatively, plan design can vary cost-sharing by specialty or by the clinical value of the care, such as by waiving primary care co-pays. Such models lower behavioral moral hazard while incentivizing choices for particular types of care. This increases the moral hazard for these specific types of care but may decrease moral hazard concerns about other types of care. Meanwhile, capitated payments decrease clinician moral hazard concerns but increase the up-front investment in preventive health. The value of this

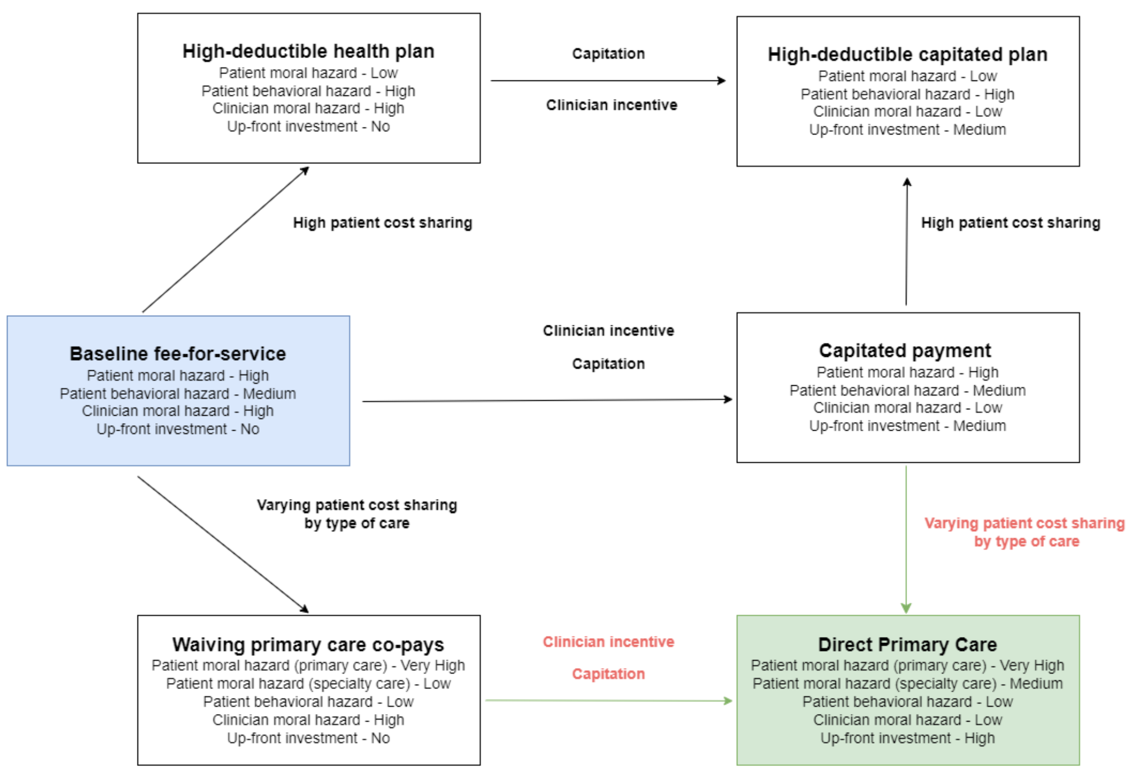


Figure 1.1: Differences in incentives between plan designs and clinician compensation arrangements.

preventive health investment depends on the health of the patient population. DPC changes patient and clinician incentives, waiving co-pays for primary care while making an up-front investment in preventive health by capitating clinician payments.

Despite the continuing growth of the DPC model, there are currently no studies that measure the effect on the clinical impact of the model. A 2020 study by the actuarial firm Milliman analyzed spending outcomes by comparing patients who did and did not sign up for DPC using risk adjustment methods to compare them (Busch, 2020), finding no reduction in costs from DPC. Previous studies do not use data from before the introduction of DPC or account for endogenous selection, concerns which this work addresses.

1.3 Institutional Context and Summary Statistics

1.3.1 Study Setting

I examine individuals employed by a suburban public school district in Colorado. In 2016 and 2017, the school district offered a fully-insured and a self-insured plan option.[2] Before the initial treatment period in 2018, the employer offered one self-insured HDHP with an HRA contribution, referred to as the Choice Plus plan. For the Choice Plus plan, the employer contributed $750 towards an HRA for employee-only plans, up to a maximum of $2,000 balance in the account (contributing $1,500 with up to a $4,000 balance for plans that include dependents). In 2018, the school district introduced the Direct Care plan, which was also a self-insured plan. The Direct Care plan has the same deductible as the Choice Plus plan but does not include any HRA contribution. Instead of an HRA contribution, the employer pays the monthly membership fee for DPC practice access for employees and their dependents on the Direct Care plan. Employees who switch from the Choice Plus plan to the Direct Care plan lose their HRA balance.

[2] In fully-insured plans, the employer passes the risk of medical spending to the insurer, while in self-insured plans, the employer itself takes on the risk of medical spending.

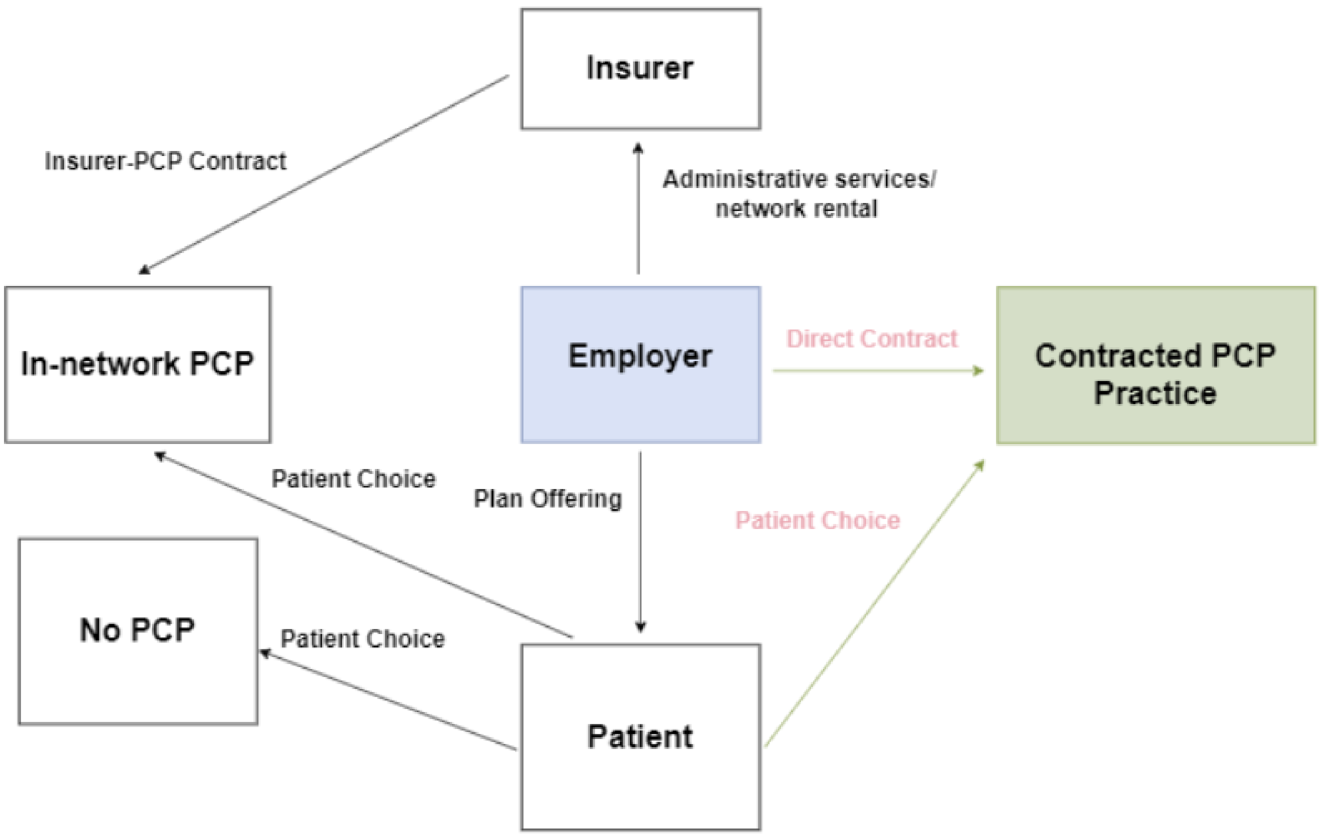


Figure 1.2: Contracting structure for the employer-DPC relationship.

I focus on the approximately 1,600 employees who never choose the fully insured plan option and do not leave the employer before the introduction of DPC in 2018, including new hires after the introduction of DPC. I drop patients with missing plan information during employment, those located more than 100 miles from the nearest DPC practice, and all dependents.[3]

Figure 1.2 shows the contracting structure of the employer-DPC relationship. The employer contracts with an insurer for administrative services and uses the insurer's pre-existing negotiated network of hospitals and clinicians, including PCPs. The employer additionally contracts with a primary care practice for high-touch primary care services. The employer then offers multiple plans to its employees. Patients can choose a plan with access to the contracted DPC practice for high-touch primary care services without a co-pay, or they can choose another plan, where they can use a PCP in the insurance network or not use any PCP. The

[3] I drop dependents because I do not have a family identifier, so I cannot tie dependents to the employee.

options in red are only available if the employer contracts with a DPC practice. Otherwise, patients do not have DPC practice access through an employer and can choose from in-network PCPs.

Panel A of Table 1.1 shows the monthly fee the DPC practice received for patients on the Direct Care plan. The Employee Only monthly fee is $92, the Employee and Spouse Fee is $154, the Employee and Children fee is $170, and the Family plan fee is $232. These fees remain constant for the duration of the sample. These fees represent employer payments to the DPC practice for employees who signed up for the Direct Care plan. Panels B and C show the trends in premiums throughout the sample period. Employee Only premiums do not differ between the Direct Care plan and the Choice Plus plan, with both plans being $180 a year through 2019 and then increasing to $564 a year in 2020. Meanwhile, all plans with dependents have lower premiums for the Direct Care plan than for the Choice Plus plan.[4]

[4]Communications with the benefit manager indicate that the employer subsidized the premiums for the Direct Care plan for those with dependents to incentivize uptake of the Direct Care plan.

Table 1.1: DPC Membership Fee and Employee Premiums by Year

	Panel A: Direct Care Monthly Membership Fee				
Plan members	2016	2017	2018	2019	2020
Employee Only	92	92	92	92	92
Employee + Spouse	154	154	154	154	154
Employee + Children	170	170	170	170	170
Family	232	232	232	232	232
	Panel B: Choice Plus Employee Premiums by Year				
Plan members	2016	2017	2018	2019	2020
Employee Only	180	180	180	180	564
Employee + Spouse	7691	7691	7354	8133	6502
Employee + Children	6103	6103	5598	6191	5547.48
Family	13449	13449	13722	15172	10973
	Panel C: Direct Care Employee Premiums by Year				
Plan members			2018	2019	2020
Employee Only			180	180	564
Employee + Spouse			5804	6443	4816
Employee + Children			4124	4578	3938
Family			11899	13210	9011

Notes: This table reports monthly membership fees the employer pays to the clinician organization and the premium levels for the health care plans with and without Direct Primary Care access.

Figure 1.3 shows the cost-sharing attributes of employee-only plans. Premiums do not differ between the Choice Plus plan and the Direct Care plan. Conditional on the same level of non-DPC practice spending, the Choice Plus plan will have lower out-of-pocket expenses than the Direct Care plan, as the employer provides $750 towards medical costs on the Choice Plus plan and not on the Direct Care plan. However, this does not account for primary care utilization having zero cost to the employee and to the employer on the Direct Care plan but having standard cost-sharing on the Choice Plus plan.

Several patterns emerge from the plan choice structure. One implication is that one might expect a preference for the Choice Plus plan for someone with low demand for care, as the value of the HRA contribution would outweigh access to increased care. The Direct Care plan guarantees a minimum level

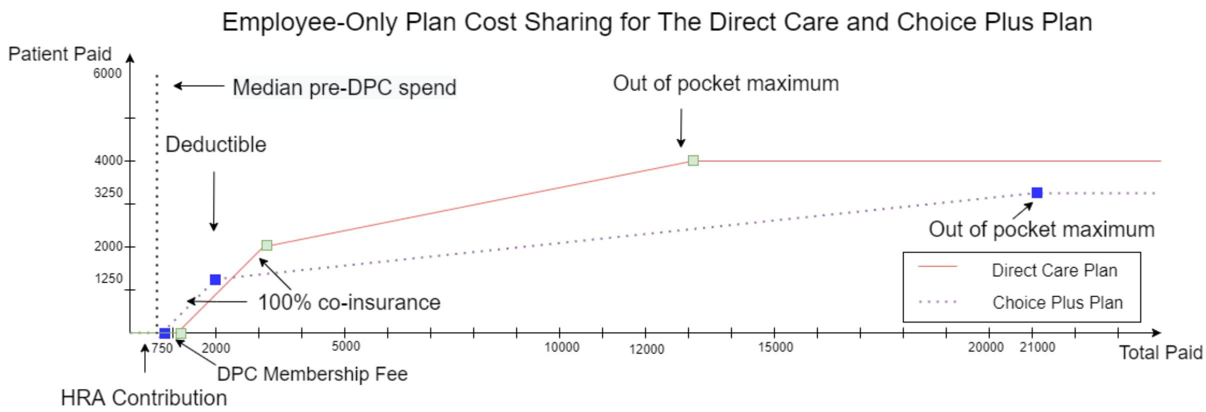


Figure 1.3: The relationship between total medical spending and patient out-of-pocket spending for the Choice Plus and the Direct Care plans for employee-only plan holders. Since premiums are identical for the two plans, I only include patient medical expenditures for comparisons of patient spending. Regardless of patient medical utilization, the employer bears the cost of contributing to HRA accounts and DPC membership fee for the Choice Plus and the Direct Care plans, respectively.

of medical spending due to the membership fee. For lower spending levels, the likelihood of increased costs may be higher if the spending is below the membership fee threshold. This is relevant because of the right-skewed nature of medical spending; the DPC membership fee is $1,104 yearly, while median medical spending before the introduction of DPC is $635.37. While primary care access increases, this is only for the clinicians who have contracted with the employer, likely not the existing PCP for most patients, limiting the potential benefit for patients with higher demand for medical care. While older patients and higher users of health care could potentially derive more benefit, they are also likelier to have existing relationships with primary care physicians that make switching costs higher. If higher need patients are less likely to choose DPC, this may suggest that the model is poorly targeted to the patients with the highest potential benefit.

1.3.2 Data and Summary Statistics

I use a novel administrative dataset from a self-insured school district in a suburban area of Colorado. The data consists of member eligibility files and medical claims for employees from 2016, two years prior to the introduction of the Direct Care plan, to 2020. I observe eligibility files for each employee for every year between 2016 and 2020. I also observe line-item claims for all employees, including the payments paid by the employee and the employer, diagnosis codes, procedure codes, and service codes associated with the claim. I use the claim data to construct chronic condition status at the yearly level.[5] One major limitation of this data is that it does not include such data for any of the DPC visits, so I cannot observe the frequency of primary care DPC usage.[6] However, I observe all non-DPC visits for patients enrolled in the Direct Care plan. Finally, I observe demographic data on all plan enrollees, including age and gender, in addition to a broad job classification and zip code of residence.

Table 1.2 provides pre-treatment summary statistics for the sample. The average level of spending in 2016 and 2017 is $4,373, while the spending winsorized at the 2% and 98% level is $3,108.[7] Meanwhile, the median pre-DPC spending level is $635.37. The difference between mean and median spending points to the right-skewed nature of medical spending, which may lead employers to overweight higher-spending individuals in crafting plan design. The average patient paid amount is $778. The average employee age is 45.2 years old, and 76% of employees are female. The average job tenure is 9.0 years, and 77% of total plans only cover the employee. In 26% of pre-treatment member years, an employee chooses a Direct Care plan at some point. 70% of employees have no chronic condition.

Columns B and C display summary statistics before the introduction of the Direct Care plan at the patient-year level for those who never choose the Direct Care plan and for those who later choose the Direct Care plan. The data suggest that patients who later choose the Direct Care plan are healthier than those who never choose the Direct Care option on average. Patients who later choose the Direct Care plan are younger, have less medical spending and out-of-pocket spending, and are less likely to have a chronic condition. For those on employee-only plans, medical spending and out-of-pocket spending tend to be lower as well. Job tenure for those who choose the Direct Care plan is shorter than for those who never choose the Direct Care plan. There is a notable difference in the proportion of employees who choose DPC on employee-only plans as opposed to plans that have dependents, with 80% of those who never choose a Direct Care plan having

[5]The chronic conditions included are asthma, anemia, depression, acquired hyperthyroidism, hypertension, hyperlipidemia, diabetes mellitus, chronic kidney disease, rheumatoid osteoarthritis, ischemic heart disease, COPD, breast cancer, and prostate cancer.

[6]Since I do not directly observe the medical care that patients receive from the DPC practice, I cannot study the impact on primary care utilization and on preventive care investments that are taking place in the DPC clinic, nor can I distinguish between Direct Care plan-holders who use the DPC practice and those that do not use the practice and may have signed up for the plan without using DPC. Furthermore, I cannot separate the impact of the Direct Care plan from the impact of DPC in general.

[7]All spending information is adjusted to 2016 medical CPI levels.

employee-only plans, and 68% of those who later choose the Direct Care plan having employee-only plans.

Table 1.2: Pre-DPC Summary Statistics

Variable	**N**	**All employees**	**Never DPC**, N = 2,230	**Ever DPC**, N = 770
Total Paid (unwinsorized)	3,000	4,373 (21,507)	4,883 (24,354)	2,894 (9,046)
Total Paid (Excluding DPC fee)	3,000	3,108 (6,954)	3,314 (7,261)	2,511 (5,940)
Patient Paid	3,000	778 (965)	808 (985)	692 (902)
Total Paid (Employee-only plan)	2,313	3,046 (6,979)	3,184 (7189)	2577 (6194)
Patient Paid (Employee-only plan)	2,313	391 (732)	403 (753)	348 (652)
Median zip code income	2,994	86,682 (18,066)	86,295 (18,239)	87,799 (17,521)
Age	3,000	45.21 (11.38)	45.78 (11.53)	43.55 (10.78)
Female	3,000	2,282 (76%)	1,700 (76%)	582 (76%)
Job Tenure	3,000	9.00 (7.56)	9.38 (7.73)	7.92 (6.94)
Employee-Only Plan	3,000	2,313 (77%)	1,787 (80%)	526 (68%)
Ever Direct Care Plan	3,000	770 (26%)		
Chronic Condition Count	3,000			
0		2,093 (70%)	1,518 (68%)	575 (75%)
1		551 (18%)	419 (19%)	132 (17%)
2		208 (6.9%)	162 (7.3%)	46 (6.0%)
3		102 (3.4%)	87 (3.9%)	15 (1.9%)
4+		46 (1.5%)	44 (2.0%)	2 (0.3%)

Notes: Chronic conditions included for chronic condition status are asthma, anemia, acquired hyperthyroidism, hypertension, hyperlipidemia, depression, ischemic heart disease, diabetes, COPD, lung cancer, colorectal cancer, and breast cancer. Total medical spending is winsorized at the 2% level unless otherwise noted.

Table 1.3 describes trends in plan selection over time. Most individuals remain on the incumbent Choice Plus plan. However, the Direct Care plan becomes more prevalent over time, increasing from 22% of plan choices in the first year to 33% of plan choices in 2020. Panels B and C show how plan selection differs between those hired before the introduction of the Direct Care plan and those hired after plan introduction. While 20% of employees hired before the introduction of DPC choose the Direct Care plan in 2018 (25% in 2020), 50% of employees hired after the introduction of DPC choose the Direct Care plan in 2018 (56% in 2020). While the Direct Care plan is becoming more popular over time, there is a notable difference in takeup between those hired before the introduction of Direct Care and those hired after Direct Care is introduced.

Table 1.3: Plan Choice by Year

Year	Choice Plus Plan	Direct Care Plan
	Plan Choice	
	Panel A: Plan Choice for all Employees	
2016	1,466 (100%)	0 (0%)
2017	1,610 (100%)	0 (0%)
2018	1,345 (78%)	388 (22%)
2019	1,258 (71%)	519 (29%)
2020	1,178 (67%)	570 (33%)
	Panel B: Plan Choice for those hired before DPC being introduced	
2016	1,466 (100%)	0 (0%)
2017	1,610 (100%)	0 (0%)
2018	1,267 (80%)	310 (20%)
2019	1,098 (78%)	322 (22%)
2020	983 (75%)	326 (25%)
	Panel C: Plan Choice for those hired after DPC is introduced	
2018	78 (50%)	78 (50%)
2019	160 (45%)	197 (55%)
2020	195 (44%)	244 (56%)

Table 1.4 provides suggestive evidence about how plan selection interacts with inertia by using the average age of Choice Plus and Direct Care choosers as a proxy for selection for those hired before the introduction of DPC and those hired after. For those hired before DPC is introduced, Choice Plus enrollees have a higher average age than Direct Care enrollees. Pre-DPC hire Choice Plus enrollees have an average age of 47.0 in 2018 and 48.6 in 2020, while pre-DPC hire Direct Care enrollees have an average age of 45.7 in 2018 and 47.0 in 2020. Meanwhile, we do not observe this difference for post-DPC hires, as post-DPC hire Choice Plus enrollees have an average age of 36.4 in 2018 and 39.1 in 2020, while post-DPC hire Direct Care enrollees have an average age of 38.0 in 2018 and 40.2 in 2020. While these results suggest the presence of selection for those who were employed before the introduction of Direct Care, they also imply

that adverse selection effects may differ based on whether someone is hired before or after Direct Care is introduced. These results suggest further exploring the interaction between adverse selection and inertia.

Table 1.4: Average Age at DPC Hire

	Plan Choice	
Year	Choice Plus Plan	Direct Care Plan
	Panel A: Pre-DPC hires	
2018	46.8	45.5
2019	47.9	46.0
2020	48.6	47.0
	Panel B: Post-DPC hires	
2018	36.4	38.0
2019	38.4	39.4
2020	39.1	40.2

1.4 Who Chooses DPC?

1.4.1 Preprogram Regression for Medical Spending

To formally estimate selection in plan choice, I run a pre-program regression, as described in Heckman and Hotz (1989). I use medical spending as the dependent variable and the year someone first chooses DPC as the main variables of interest for selection. I also control for other factors related to medical spending, such as age, job tenure, and gender. I examine how selection varies based on the year that someone first chooses a Direct Care plan. I use quantile regression to test how selection differs throughout the distribution of spending.

Results of the pre-treatment regression are in Table 1.5. Those who choose Direct Care have lower medical spending.[8] However, selection differs based on the first year that someone chooses the Direct Care plan. My results also suggest that individuals at the extremes of the spending distribution are less likely to choose Direct Care, while this selection is not present for those closer to median spending levels.

[8]Estimates are similar without controlling for co-variates. Results are available upon request.

Furthermore, most switchers on employee-only plans switch to the Direct Care plan in 2018, with 210 employees first choosing Direct Care in 2018 and 75 employees first choosing Direct Care in 2019 or 2020.

Table 1.5: Impact of future Direct Care participation on medical spend

	Quantile			
	(1)	(2)	(3)	(4)
	25th	50th	75th	90th
DPC Group 2018	-70.14 **	-52.61	-204.85	-3631.64**
	(25.49)	(57.86)	(220.71)	(1515.49)
DPC Group 2019	-107.26*	-20.42	-200.16	6547.99
	(49.57)	(131.80)	(734.39)	(5607.32)
DPC Group 2020	-96.66	-87.86	128.29	-3726.52
	(86.26)	(136.12)	(771.77)	(4597.60)
Age	4.155**	20.369***	56.324***	66.550
	(1.521)	(2.877)	(11.803)	(65.838)
Job Tenure	14.981***	12.666**	32.295	106.188
	(2.766)	(5.019)	(24.206)	(141.082)
Male	-100.310***	-426.561***	-919.832***	-775.312
	(27.401)	(48.060)	(198.868)	(1864.638)
Median zip code income (000s)	1.206	2.410	4.980	22.655
	(0.618)	(1.237)	(4.995)	(38.087)

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Bootstrapped standard errors using 1,000 replications. I restrict the sample to employee-only plans for those under 65. Of those hired prior to DPC being introduced, 958 individuals on employee-only plans never choose the Direct Care plan, while 210 individuals first switch in 2018, 37 first switch in 2019, and 38 first switch in 2020.

1.4.2 Determinants of Plan Choice

While the previous section documents selection in plan choice for pre-DPC hires, the factors driving selection in plan choice are unclear. Additionally, Table 1.4 suggests that selection may differ between pre-DPC and post-DPC hires, so determinants of plan choice may differ between the two groups. To extend the pre-treatment selection analysis in the previous section, I estimate binary logit models for whether the employee chooses to enroll in the Direct Care plan for each year between 2018 and 2020.[9]

[9] Results for linear probability models are included in the Appendix.

The probability that an individual i picks a plan j in a given year t is:

$$DPC_{ijt} = \frac{\exp\{\beta_{ijt} v_{ijt} + \xi_{ijt}\}}{\sum_{k \in J} \exp\{\beta_{ikt} v_{ikt} + \xi_{ikt}\}} \tag{1}$$

Here, v_{ijt} represents patient characteristics and ξ_{ijt} is an extreme value distributed error term, and J represents the set of plan choices. I estimate the impact of median zip code income, job tenure, job type, age, gender, plan type, and relative practice distance.[10] The preferred specification includes results for the employee-only plan choice sub-sample only, as I do not have information about utilization and chronic condition status for non-employee members of the plan, which could impact plan choice.[11]

I separately estimate logit models for pre-Direct Care hires and post-Direct Care hires. I use different explanatory variables in the choice model for pre-Direct Care hires than in the choice model for post-Direct Care hires, as there is more health consumption and health status information available about the pre-Direct Care hire group than about the post-Direct Care hire group. For the pre-Direct Care hires, I also estimate the impact of pre-DPC out-of-pocket primary care spending and pre-DPC out-of-pocket non-primary care spending to test the role of primary care switching costs in explaining plan choice. Including non-primary care spending helps separate the impact of primary care switching costs from other selection on health status in determining plan choice.

Plan choice estimates are presented in Table 1.6. For pre-DPC hires, longer job tenure and higher out-of-pocket primary care spending are associated with a lower probability of enrollment. Higher median zip code income and being enrolled in the health plan for part of the year are associated with an increased probability of enrollment. For new hires, those enrolled in the health plan for part of the year are more likely to select the Direct Care plan, while administrators and other professional employees are less likely to choose a Direct Care plan. There is also a statistically significant association between shorter relative distances to a DPC practice and a higher likelihood of choosing the Direct Care plan. These results differ from the estimates for the relationship between relative distance and plan choice for the pre-DPC hires, where shorter

[10]To construct relative practice distance, for each individual, I calculate the minimum distance to any primary care clinician that sees at least two patients of the employer a year and the closest practice location of the DPC with which the employer contracts. I then subtract the DPC practice distance from the closest practice location distance.

[11]Results including other employees are included in the Appendix.

relative practice distance is surprisingly associated with a lower probability of Direct Care enrollment. Given the association between job tenure and primary care spending and plan choice, this suggests that primary care switching costs influence demand for DPC. Since the employer is contracting with a specific practice, patients with more established primary care relationships may be unwilling to switch to a DPC doctor due to higher switching costs from terminating their existing primary care relationship. The lack of statistical significance for the association between non-primary care spending and Direct Care plan choice is also suggestive of this mechanism.

Table 1.6: Determinants of Plan Choice

	(1)	(2)
	Pre-DPC hires	Post-DPC hires
Age	0.001	0.006
	(0.002)	(0.004)
Partial Year	0.529***	0.211***
	(0.048)	(0.048)
Female	-0.043	-0.477*
	(0.031)	(0.200)
Relative DPC practice distance	0.024***	-0.013**
	(0.004)	(0.004)
Median zip code income (000s)	0.006***	0.003
	(0.001)	(0.003)
Hourly Employees	0.020	0.266
	(0.073)	(0.205)
Professional Employees	-0.043	-0.690***
	(0.078)	(0.209)
Job Tenure	-0.015***	
	(0.002)	
2017 primary care OOP spend (000s)	-0.179*	
	(0.072)	
2017 non-primary care OOP spend (000s)	-0.031	
	(0.039)	
Observations	2985	736

Notes: Chronic condition and year fixed effects are included. Chronic condition status is 2017 for pre-DPC hires and previous year status for post-DPC hires. I restrict the sample to those who choose employee-only plans. For the impact of employee type on plan choice, the baseline category is teachers. Other categories of job type are hourly employees and administrators and professional employees. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors in parentheses.

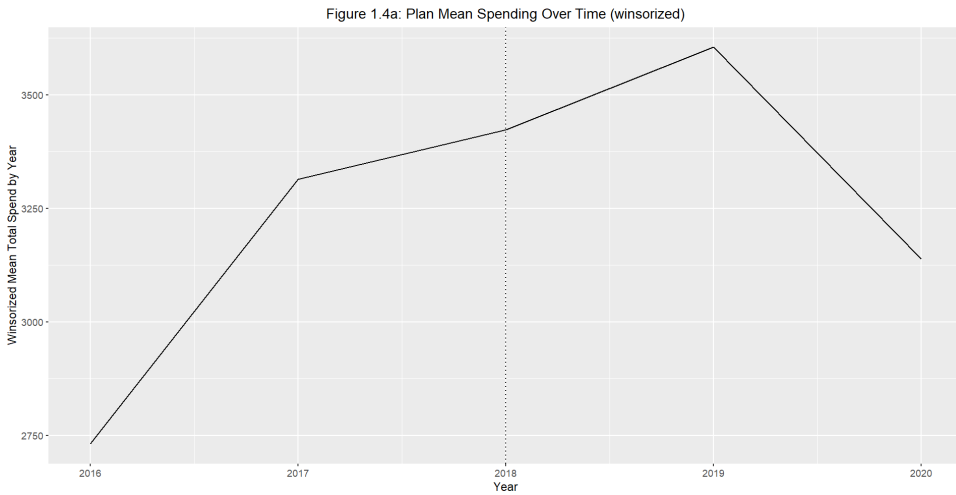
Taken together, the results suggest that selection in plan choice is driven by higher switching costs for those with higher utilization of primary care. A priori, this group could be expected to have greater benefit from DPC. Meanwhile, plan choice patterns differ for those hired after DPC is introduced, where switching costs are less of a factor. These results may explain why selection patterns differ between pre-DPC hires and post-DPC hires.

1.5 Does Direct Primary Care Reduce Costs?

I next turn to the impact of the Direct Care plan on expenditures. I first show spending time trends and how they change in 2018, when Direct Care is introduced. I examine changes in both mean spending and median spending over time to estimate whether the introduction of Direct Care has impacts that vary based on spending level. This analysis does not separate the impact on spending based on plan choice, but instead looks for changes in spending trend when Direct Care is introduced.

Figure 1.4a provides descriptive evidence of changes in spending following the implementation of the Direct Care plan in 2018, showing the average total yearly medical spending adjusted for medical CPI for those on employee-only plans.[12] While there is no notable change in the mean spending trend following the introduction of DPC in 2018, there is a trend change for median spending following the introduction of the Direct Care plan, with an increase in median spending shown in Figure 1.4b. This suggests that the impact of the Direct Care plan may differ based on the level of spending.

[12] Similar figures for all employees and with medical spending not winsorized at the 2% level are in the Appendix.



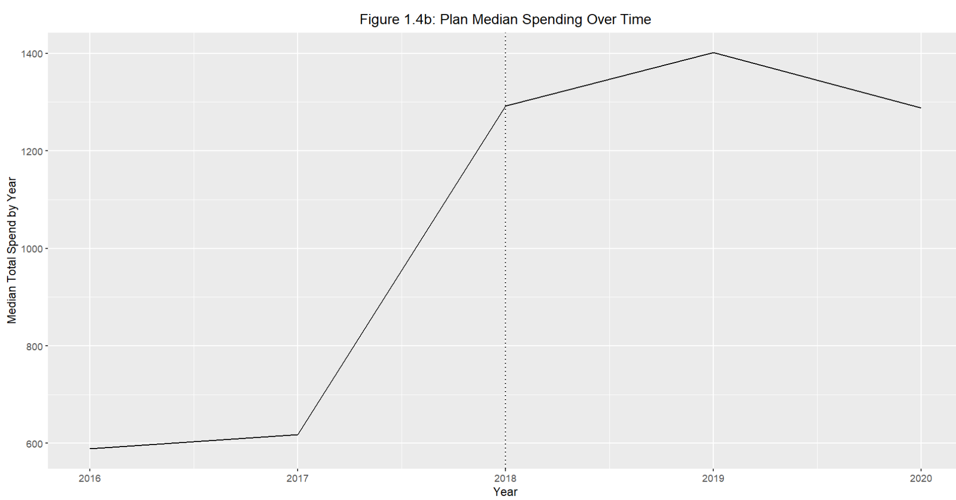


Figure 1.4: Figure a plots mean yearly medical spending by year. Winsorized results are presented for consistency, but unwinsorized results, excluded for brevity, are similar. Figure b plots median yearly medical spending by year. All numbers are adjusted to 2016 levels of medical CPI.

1.5.1 OLS Estimates

I next examine the relationship between choosing the Direct Care plan and cost outcomes. I estimate the following equation:

$$y_{it} = \gamma_1 \times PreDPCYear_{it} + \gamma_2 \times PostDPCYear_{it} + \gamma_3 DPC_{it} + \gamma_4 PartialYear_{it} + \gamma_5 Age_{it} + \gamma_6 JobTenure_{it} + \varepsilon_{it} \tag{2}$$

.

y_{it} represents the cost outcome variables for individual i in year t. The *PreDPCYear* and *PostDPCYear* terms capture some of the selection into Direct Care plans and selection in terms of those who exit from a Direct Care plan. γ_3 is the main variable of interest, showing the association between those who choose a Direct Care plan and outcomes, while ε_{it} is a normally distributed error term. Due to Medicare eligibility, I restrict the sample to those under the age of 65. I also restrict the sample to those who do not leave employment one calendar year or less after being hired to only include employees with at least two full years of data. In my main specification, I also restrict the sample to those who never choose Direct Care and those who switch to Direct Care in 2018, since this is the year in which most employees switch, and to avoid two-way fixed effects with differential treatment timing issues.[13]

I first examine total spending, which includes medical spending and the DPC membership fee. To explore how DPC impacts the incidence of medical spending, I analyze total employer spending and total patient spending.[14] I decompose medical spending into primary care service spending and non-primary care service spending. I examine non-primary care service spending to empirically estimate the impact of DPC on specialty spending, since it is theoretically ambiguous. The magnitude of the primary care service spending (which excludes the DPC fee) indicates that savings through reduced specialty care spending are needed for there to be a null impact on total cost, as primary care spending will increase from the DPC

[13] I also include a specification that does not exclude later switchers to Direct Care in the Appendix. I also include a Poisson model for expenditures in the Appendix.

[14] Total employer spending consists of the employer share of medical spending and the DPC membership fee. Total patient spending consists of the patient share of medical expenditures subtracted by the employer HRA contribution for those on the Choice Plus plan.

membership fee.

Estimates are displayed in Table 1.7. Selection into Direct Care is associated with an increase in total patient spending and a decrease in primary care service spending. There are no significant effects of the Direct Care plan on total spending. There is evidence of selection into Direct Care by those who have less spending, as in the pre-program regressions. I also do not detect any selection out of the Direct Care plan. Enrolling in the Direct Care plan is associated with lower primary care service spending as well as lower non-primary care service spending.

Table 1.7: OLS Regression for Direct Care and Change in Spending

	(1) Total Spend	(2) Total Patient Spend	(3) Total Employer Spend	(4) Primary Care Service Spend	(5) Non-primary Care Service Spend	(6) ER Spend
Age	27.885	3.087	24.666	4.913*	22.461	-2.876
	(12.920)	(1.695)	(11.233)	(1.335)	(11.300)	(2.579)
Job Tenure	22.931	1.513	20.533	2.020	19.260	-1.093
	(16.816)	(2.211)	(14.987)	(2.126)	(15.391)	(1.250)
Median zip code income (000s)	5.661	0.337	5.309	0.579	5.096	-0.921
	(5.220)	(0.759)	(4.673)	(0.485)	(4.868)	(0.601)
Direct Care Plan	0.007	152.911*	-120.125	-319.718***	-710.346*	-36.991
	(174.444)	(52.277)	(163.057)	(37.011)	(164.206)	(51.800)
Pre DPC Year	-476.512*	-28.730	-447.601*	-3.633	-474.320*	-11.659
	(134.694)	(19.737)	(111.598)	(15.310)	(128.625)	(7.921)
Post DPC Year	541.173	9.718	560.080	-5.418	587.699	-92.809
	(508.218)	(48.353)	(437.040)	(46.906)	(476.541)	(124.937)
Partial Year	-900.992	-132.756	-775.473	-203.500***	-731.543	-94.441
	(372.202)	(50.350)	(327.010)	(16.038)	(354.116)	(62.803)
Lagged chronic condition status	1606.888*	202.256*	1398.686*	202.280*	1386.249*	106.275*
	(505.633)	(64.741)	(441.151)	(55.221)	(461.799)	(36.392)
Observations	5225	5225	5225	5225	5225	5225
R^2	0.040	0.050	0.038	0.143	0.036	0.034

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors in parentheses. Year, employee type, and gender fixed effects are included. I restrict the sample to those who do not have missing plan information and to those who never switched to Direct Care plans or those who switched to the Direct Care plan in 2018. Standard errors are clustered at the year-member level.

1.5.2 Instrumental Variables Approach

While I have found suggestive results about the impact of DPC on spending, precisely quantifying the impact requires an identification strategy that credibly recovers a causal effect. My prior results suggest that there is selection in Direct Care plan choice, so there may be endogeneity and reverse causality concerns in interpreting previous results. To overcome the empirical challenges to recovering a causal estimate, I leverage the inertia that I previously document in this setting, as evidenced by descriptive analysis of plan choice for hires shortly before the implementation of DPC and following its introduction, in addition to the estimates in Table A7 of the Appendix showing that being hired after DPC is associated with higher rates of DPC plan choice. I restrict the sample to those hired by the employer between 2016 and 2019 to make pre-DPC and post-DPC hire groups more comparable.

Table 1.8 presents descriptive evidence for changes in plan choice likely driven by inertia. Those hired in the two years before the introduction of the Direct Care plan appear observationally similar to those hired in the years after the introduction of the Direct Care plan. The average age at hire is 39.50 for those hired in the years before Direct Care, as opposed to 38.15 for those hired after DPC is introduced. The percentage of employees who choose employee-only health plans is similar across groups, with 81% of the pre-DPC hires and 77% of the post-DPC hires being employee-only. There is also a similar percentage of female employees across groups, with 74% of pre-DPC hires and 77% of post-DPC hires being female. Pre and post-DPC employees also are similarly represented by job category, with 52% of pre-DPC hires being teachers and 41% being hourly employees, while 52% of post-DPC hires are teachers and 40% are hourly workers. There are also similar chronic condition levels, with 69% of pre-DPC hires and 72% of post-DPC hires having no chronic conditions.

We also see similar median zip code income levels, with a median zip code income of $85,546 for pre-DPC hires and $88,554 for post-DPC hires. However, there is a notable difference in the proportion of those signing up for Direct Care plans after the introduction of Direct Care, with 27% of pre-DPC hires and 56% of post-DPC hires signing up for the Direct Care plan. Given the similarities between observables for the two groups, I assume that a hire date between 2016 and 2019 is as good as random after conditioning on covariates. Implicit in this assumption is that those hired after DPC do not choose to work for the employer as a result of the DPC benefits offered for any reasons that would impact their health spending.

Table 1.8: Post-DPC Summary Statistics

		Hire after DPC	
Variable	**N**	**Pre-DPC Hire**, N = 495	**Post-DPC Hire**, N = 376
Median zip code income	870	85,546 (18,217)	88,554 (18,609)
Age	871	42.00 (11.13)	39.52 (10.20)
Age at Hire Year	871	39.50 (11.14)	38.15 (10.22)
Female	871	366 (74%)	288 (77%)
Employee Only Plan	871	399 (81%)	289 (77%)
Teachers	871	256 (52%)	197 (52%)
Hourly Employees	871	201 (41%)	152 (40%)
Direct Care Plan	**871**	**132 (27%)**	**210 (56%)**
Chronic Condition Count	871		
0		341 (69%)	272 (72%)
1		96 (19%)	73 (19%)
2		43 (8.7%)	23 (6.1%)
3		12 (2.4%)	5 (1.3%)
4+		3 (0.6%)	3 (0.8%)

After restricting the sample to those hired between 2016 and 2019, I instrument for Direct Care plan choice with whether the individual is hired after the introduction of the Direct Care plan, in 2018 or later. Because I have a binary first-stage outcome, I use a probit first-stage model.[15] The exclusion restriction requires that whether an employee is hired after DPC is introduced only impacts outcomes through its impact on plan choice, conditional on controls. For this to be satisfied, employees must not choose to work for the employer due to the DPC offering due to unobserved characteristics.

I use a three-stage estimation procedure as described in Adams, Almeida, and Ferreira (2009).[16] In the first stage, I estimate a probit model of plan choice, with whether an employee is hired after DPC serving as the instrument. In the second stage, I generate predicted plan choice from the probit model. In the third stage, I use the predicted plan choice probabilities from step (2) as instruments for plan choice. I use the

[15] I also do a linear first stage with whether an employee is hired after DPC as the instrument for robustness and find similar results.

[16] Heebsh (2020) also uses a similar approach.

same covariates for outcomes in the third stage as I use for plan choice in the first stage. I use the fitted probabilities as an instrument instead of directly using $HireAfterDPC$ because of the binary nature of the endogenous regressor, as discussed in Wooldridge (2010). The linear probability model has a worse fit than a binary dependent variable regression. Using a probit first stage strengthens the instrument, improving the precision of the estimate.[17]

I estimate regressions for the probability of choosing Direct Care and for the outcome variables for individual i in year t of the following form:

$$\phi^{-1}(P(DPC_{it} = 1)) = \gamma_0 + \gamma_t + \gamma_1 \mathbb{1}(HireAfterDPC) + \gamma_2 X_{it} + \xi_{it} \tag{3}$$

$$y_{it} = \beta_0 + \beta_t + \beta_1 \widehat{DPC}_{it} + \beta_2 X_{it} + \varepsilon_{it} \tag{4}$$

In the first stage, γ_t represents year-level fixed effects, while $\mathbb{1}(HireAfterDPC)$ takes value 1 if the individual is hired after the introduction of Direct Care in 2018. X_{it} includes patient characteristics, such as age, relative DPC practice distance, gender, median zip code income, chronic condition status, and employee type. I assume the ξ and ε error terms are normally distributed and independent from patient characteristics and whether an employee is hired after DPC. $\widehat{DPC}_{it}$ is the predicted probability that someone chooses Direct Care from the first equation. In the third-stage outcome equation, I use the same covariates as in the first-stage probit equation. I only use years after the introduction of DPC for the model, when the probability of choosing the DPC plan is positive.

To test the quasi-randomization assumption of the design, I also formally examine the effect of the instrument on the balance of relevant variables for selection, such as age, gender, median zip code income, and practice distance. If individuals make job choices in response to the DPC offering, I would expect to find evidence of these strategic choices. As shown in Appendix Table 1.A.11, there is no statistically significant imbalance in these variables.

I estimate the IV model restricting the sample to those who choose employee-only plans, since these

[17] I do not need to adjust standard errors in this procedure despite regressors being generated in the first stage, as standard errors are asymptomatically valid, as noted in Wooldridge (2010).

plans have no premium differences between the Direct Care plan and the Choice Plus plan. I consider this to be the main specification.[18]

1.5.3 IV Results

In this section, I present results about changes in spending outcomes from signing up for a Direct Care plan. I present results about effects on total costs and patient costs. Table 1.9 presents the estimates for Equation (3), the probit model for plan choice that serves as the first stage. Being hired after DPC is strongly related to choosing the Direct Care plan, with a p-value under 0.001. This suggests that there is little concern about a weak instrument in this setting. In the sample of hires between 2016 and 2019, distance from a DPC practice relates to plan choice.

Table 1.9: Probit Model for Plan Choice

	(1)	(2)
	Employee-Only Plan	All Employees
Hire after DPC	0.695***	0.721***
	(0.018)	(0.066)
Age	0.005	0.004
	(0.006)	(0.006)
Median zip code income (000s)	0.000	0.001
	(0.001)	(0.001)
Female	-0.370***	-0.275***
	(0.033)	(0.043)
Relative DPC Practice Distance	-0.023***	-0.028**
	(0.006)	(0.010)
Employee Only Plan		-0.693***
		(0.091)
Observations	689	870
FE: Chronic Conditions	X	X
FE: Program	X	X
FE: Year	X	X

Notes: Estimates of γ from Equation (3). Observations are from employees hired between 2016 and 2019 who choose employee-only benefit plans. Partial years are excluded. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

[18] Since newer hires are likelier to have partial plan years after the introduction of DPC due to being hired partway through the year, which may bias the results, I exclude all partial years from the analysis.

Table 1.10 presents the instrumental variable estimation regression results for cost. Total spending results are presented in column 1. Total patient spending outcomes are presented in column 2. Total employer spending is presented in column 3. Primary care service spending is presented in column 4. Non primary-care service spending results are presented in column 5, and emergency spending results are presented in column 6. There is a non-significant increase in total spending, and there does not appear to be a significant effect on most outcomes from the Direct Care plan. Notably, I do not detect any decrease in non-primary care service spending, suggesting that increased preventive investment is not resulting in decreased specialist utilization.

Table 1.10: IV Estimates for Medical Spending

	(1) Total Spend	(2) Total Patient Spend	(3) Total Employer Service Spend	(4) Primary Care Service Spend	(5) Non-primary Care Service Spend	(6) ER Spend
Direct Care Plan	738.762	-36.505	786.102	-453.852	96.061	-487.799
	(1889.738)	(281.300)	(1629.201)	(223.150)	(1758.958)	(316.927)
Age	9.400	2.748	7.060	1.285	8.878	-9.698
	(21.685)	(2.881)	(18.969)	(3.080)	(19.171)	(3.615)
Median zip code income (000s)	-2.382	-1.881	-0.934	-0.306	-3.423	1.814
	(9.725)	(0.823)	(9.083)	(1.368)	(7.646)	(1.877)
Female	958.888	143.799	828.456	142.187	857.933	-24.070
	(557.031)	(74.821)	(532.220)	(51.841)	(479.270)	(156.001)
Relative DPC practice distance	7.936	5.914	-1.892	-1.184	-0.042	12.425
	(69.992)	(7.695)	(59.265)	(5.974)	(58.178)	(12.387)
Observations	689	689	689	689	689	689
F-test	40.9	40.9	40.9	40.9	40.9	40.9

Notes: Estimates of β from Equation (4). Observations are from employees hired between 2016 and 2019 who choose employee-only benefit plans without missing plan information during employment. Partial years are excluded. Chronic condition, employee type, and year fixed effects are used. Standard errors are clustered on the individual-year level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

1.5.4 Difference-in-Difference Estimation

I use a difference-in-differences approach to estimate how choosing a Direct Care plan may change health utilization for those who worked for the employer before the introduction of DPC. I examine only those already employed before the introduction of the Direct Care plan. Even with the inclusion of patient fixed effects, which necessitate that estimates are driven by within-individual variation, there may be concerns regarding endogenous selection into the Direct Care plan, especially given the descriptive evidence of selection presented earlier. I use an inverse propensity score weighting approach to model selection into Direct Care to mitigate selection concerns. Since a misspecified propensity score model could lead to biased estimates, I use a covariate balancing propensity score for the likelihood of treatment to weigh the observations. This approach uses a generalized method of moments (GMM) framework, which combines score conditions and covariate balance moment conditions to balance the conditional probability of treatment assignment with balanced covariates between treatment groups. (Imai and Ratkovic, 2014).[19] The underlying assumption for the strategy is that changes in outcomes would have evolved similarly between those who choose the Direct Care plan and those who never choose the Direct Care plan in the absence of the introduction of the DPC plan, conditional on covariates and on year and patient fixed effects. For covariates that I use to generate propensity scores, I use dummies for chronic condition status for the set of chronic conditions listed in Table 1.2: age, gender, median zip code income, teacher status, hourly employee status, and out-of-pocket primary care spending in 2017.

Since a large majority of patients who sign up for Direct Care first sign up in 2018, and because the impact of DPC may vary with time and I am interested in having multiple post-treatment periods, I use a difference-in-difference model restricting the sample to those who never sign up for Direct Care and those who first sign up in 2018.[20]

I estimate the following equation:

$$Y_{it} = \alpha_i + \lambda_t + \sum_{t=2016}^{2020} \beta_t \mathbb{1}\{E_i = D\} + \varepsilon_{it} \tag{5}$$

[19]Results are similar if observations are unweighted.

[20]An analysis using a staggered difference-in-difference approach with the full sample is included in the Appendix.

.

β_t can be interpreted as the impact of enrolling in the Direct Care plan in 2018 on the outcome in year t, and $\mathbb{1}\{E_i = D\}$ is an indicator for treatment status in a given year. α_i and λ_t are individual and year fixed effects, respectively, and ε_{it} is a normally distributed error term. I omit 2017, the year before the introduction of the Direct Care plan, to capture baseline differences between those who never choose Direct Care and those who choose Direct Care in 2018.

1.5.5 Difference-in-Difference Results

In this section, I present difference-in-Difference estimates about changes in outcomes from signing up for a Direct Care plan. I examine the same outcomes as in the instrumental variables analysis, looking first at the effects on spending outcomes.

Figure 1.5 presents the difference-in-difference estimates for different spending outcome variables. Total spending results are presented in plot (a). Total patient spending outcomes are presented in plot (b). Total employer spending is presented in plot (c). Primary care service spending is presented in plot (d). Non primary-care service spending results are presented in plot (e), and emergency spending results are presented in plot (f).

There is no statistically significant effect of Direct Care on total spending. However, the positive coefficients are the same sign as in the instrumental variable estimates, suggesting a potentially positive association between choosing the Direct Care plan and spending. Total patient spending increases after the introduction of the Direct Care plan, while primary care service spending decreases. There is no significant effect on non-primary care service spending, while emergency spending is lower in the first year after the Direct Care plan is chosen; however, this effect dissipates.

The difference-in-difference estimates suggest that while patients may substitute between receiving primary care services at the DPC clinic and receiving them at fee-for-service providers, the reductions in primary care service spending do not lead to lower costs. As with the IV estimates, I do not find evidence for any decrease in non-primary care service spending, and the DPC fee exceeds the reduction in primary care service spending. Furthermore, these changes also result in an increase in patient out-of-pocket spending,

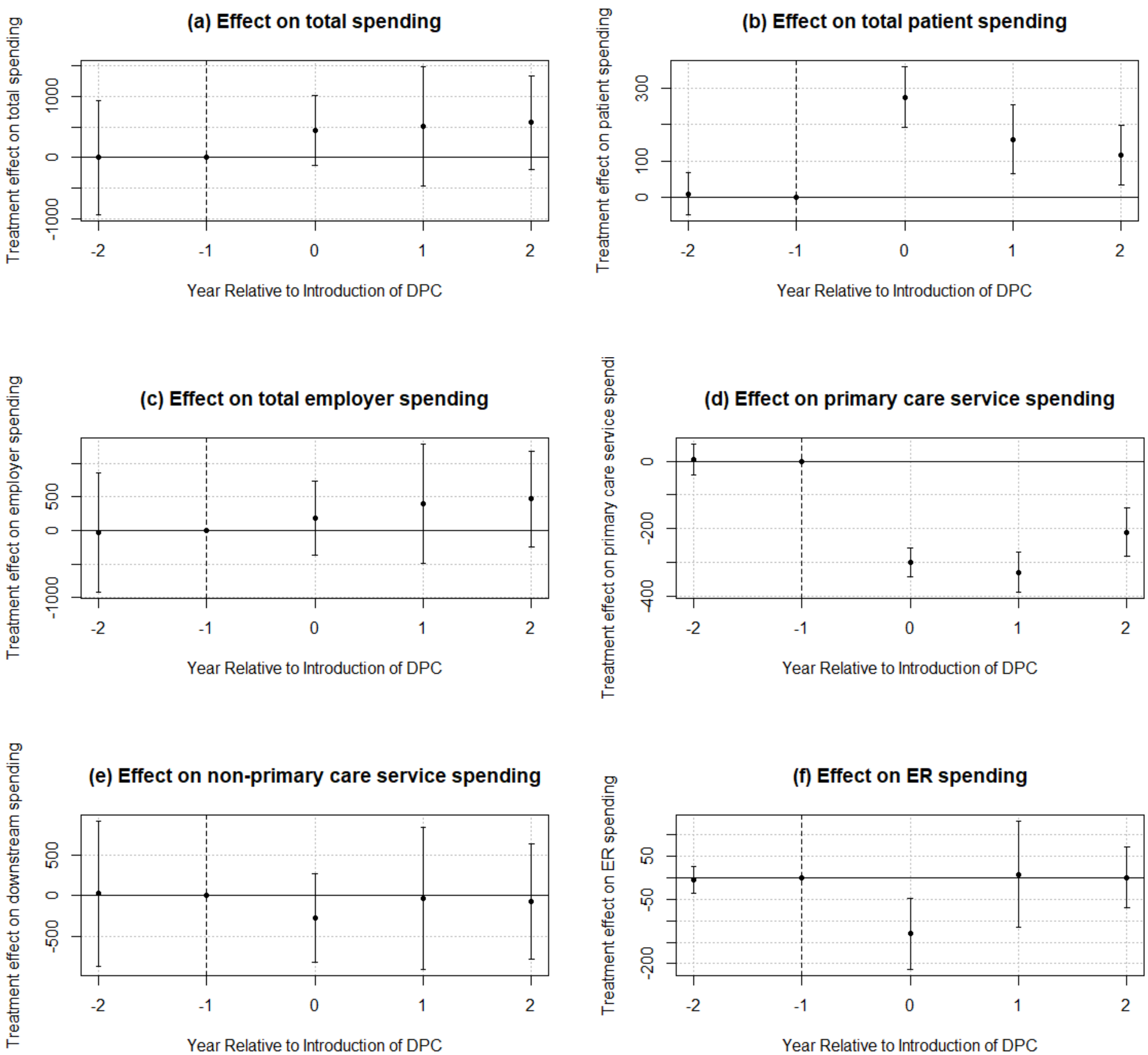


Figure 1.5: Estimates for Equation (5). 95% confidence intervals are shown. The sample includes all employees on employee-only plans who were hired before the introduction of DPC who either never choose the Direct Care plan or first choose it in 2018 without missing plan information during employment. Year and individual fixed effects are used. Standard errors are clustered on the individual-year level.

likely driven by the lack of HRA savings account access in the Direct Care plan. The decrease in ER spending in the first year and the negative insignificant point estimate for ER spending in the IV estimate suggest that patients may substitute from ER use to primary care use. However, there is no evidence of substitution away from specialty care.

Since there may be heterogeneous effects from choosing Direct Care, I also estimate a quantile difference-in-difference regression, as described in Callaway and Li (2019). Quantile difference-in-difference estimation requires a stronger distributional assumption about parallel trends throughout the distribution instead of solely in the mean and a copula assumption that the distribution of changes in outcomes does not change over time. The panel quantile difference-in-difference model allows me to recover the quantile treatment effect on the treated (QTET).[21]

Results are displayed in Figure 1.6. There is a statistically significant increase in total spending at lower levels of the spending distribution. Meanwhile, point estimates become negative at higher levels of total spending, although the results are not significant. The Direct Care Plan caused yearly medical expenditure to increase by \$1132.31, \$1010.39, and \$741.49 in the 25th, 40th, and 50th quantile, respectively. Compared to average pre-treatment values, these estimated effects represent increases of 1113% at the 25th percentile, 251% at the 40th percentile, and 118% at the 50th percentile.

[21] See Callaway and Li (2019) for more details on this method. The method is implemented using the qte package in R.

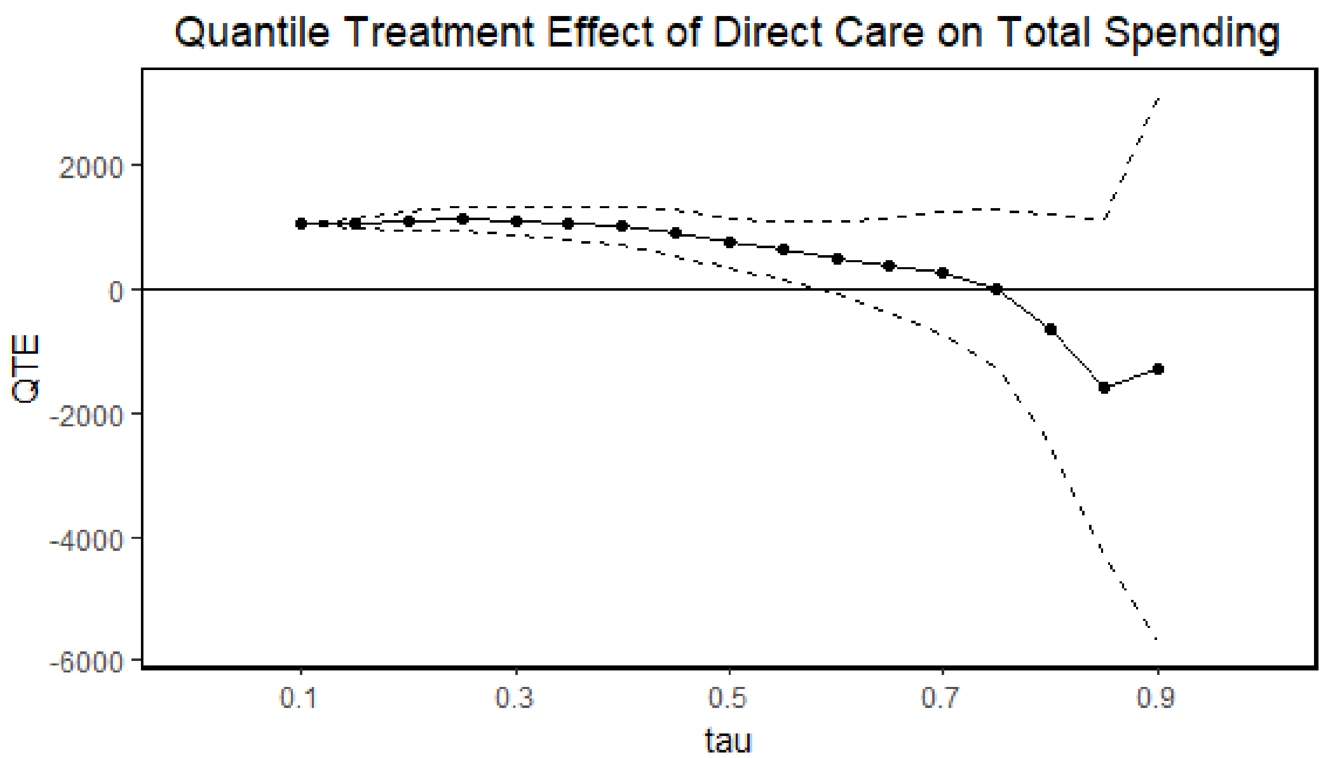


Figure 1.6: Estimates for a quantile difference-in-difference model, as described in Callaway and Li (2019). I calculate outcomes from the 10th to the 90th percentile with 5 percentile intervals. 95% confidence intervals are shown. Bootstrapped standard errors with 500 draws. I include all employees on employee-only plans without missing plan information during employment who were hired before the introduction of DPC who either never choose the Direct Care Plan or first choose it in 2018. I exclude those over age 65. Tau represents the quantile of spending.

The heterogeneous effects of DPC highlight how the right-skewness of medical spending influences the importance of targeting, in this case, the targeting of preventive health investments. While total spending increases from Direct Care for lower spenders, lower spenders are likelier to choose Direct Care than higher spenders are. Due to the distribution of health care costs, most attempts to reduce employee health care costs are aimed at a small portion of employees. However, market frictions can make insurance programs difficult to target, as price and benefit differences between plans based on health status are illegal, and there may be behavioral frictions from low health and health insurance literacy (Samek and Sydnor, 2021; Liu and Sydnor, 2022). As in Tilipman (2022), my results suggest that these frictions may reduce employer and employee welfare.

Overall, I do not find evidence that DPC reduces total medical spending or non-primary care spending. I also find mechanical increases in spending at lower levels of the spending distribution and increases in patient out-of-pocket medical spending. The results are driven by the increased preventive health spending incurred from the DPC fee without evidence of an offsetting impact on downstream spending. My findings may be partially explained by selection into the plan, as patients with lower prior healthcare spending are likelier to choose Direct Care; however, these patients have less specialty spending that can be reduced with increased preventive investment.

1.6 Impact on Quality

Finally, I turn to the impact of Direct Care on quality, proxied by changes to the allocative efficiency of testing as a result of increasing primary care access. I use mammography screening in clinically appropriate populations to proxy for increased preventive care. Mammography screenings have straightforward guidelines in certain age groups and are often ordered in the primary care setting rather than in the specialist setting, making them an appropriate outcome.

Additionally, I study whether increased primary care access would increase the frequency of low-value care. Previous research has shown that waving primary care co-pays can increase the utilization of both high-value and low-value screening (Cliff, Hirth, and Fendrick, 2019). With DPC, there is a change to clinician financial incentives, and the payment model is tied to a specific practice, so results may differ in this context. I examine effects on low-value care in the context of cardiac imaging for lower-risk patients. I use this measure as a proxy for low-value care because cardiac imaging is a common procedure identified in previous research and in Choosing Wisely guidelines on medical overuse.[22] Cardiac imaging is also often primary care driven rather than specialist-driven; therefore, the primary care payment model may impact use.

Additional time could lead a clinician to substitute away from imaging, especially low-value imaging. A previous study of low-value utilization from waived primary care co-pays did not include any changes to clinician incentives for appointment length. However, increased primary care access could increase clinician opportunities to order imaging.

Table 1.11 examines IV estimates for changes in screening and imaging behavior due to the Direct

[22] Choosing Wisely is a clinician-led effort to lower the overuse of low-value care.

Care plan. For mammography screening, I restrict to females over age 50 so that results are not driven by changes in screening recommendations during this time period. (Kowalski, 2021). For cardiac imaging, I restrict to patients who do not have ischemic heart disease, hypertension, or COPD, and are not over 40 with diabetes mellitus to focus on changes for low-risk patients. The impact on mammography screening is a non statistically-significant increase, while I find a significant decrease in cardiac imaging utilization.

Table 1.11: IV Estimates for Imaging and Screening

	(1)	(2)
	Mammography Screening	Cardiac Imaging
Direct Care Plan	0.151	-0.052*
	(0.093)	(0.004)
Age	0.015	-0.001
	(0.011)	(0.001)
Median zip code income (000s)	-0.003	0.000
	(0.003)	(0.000)
Relative DPC practice distance	-0.016	0.000
	(0.010)	(0.002)
Female		0.003
		(0.027)
Observations	130	608
F-test	23.8	40.3

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Estimates of β from Equation (4). Observations are from employees hired between 2016 and 2019 without missing plan information during employment. Partial years are excluded. For mammography screenings, the sample includes females on employee-only plans above age 50 on January 1, 2021. For cardiac imaging, the sample includes those who do not have ischemic heart disease, hypertension, or COPD, and are not both over 40 and having diabetes mellitus. The outcome variable for cardiac imaging is whether a patient has had an electrocardiogram, echocardiogram, advanced cardiac imaging, or a stress test in a given year. Employee type, chronic condition, and year fixed effects are included. Standard errors are clustered on the individual-year level.

Figure 1.7 displays difference-in-difference estimates for mammography screening and potentially low-value cardiac imaging. Results are similar to the IV estimates, with evidence of increased mammography screening and decreased cardiac imaging in certain years, indicating that allocative efficiency may be increasing. These results indicate that a change in clinical decision-making occurs when DPC is introduced that may correspond to improved quality of care.

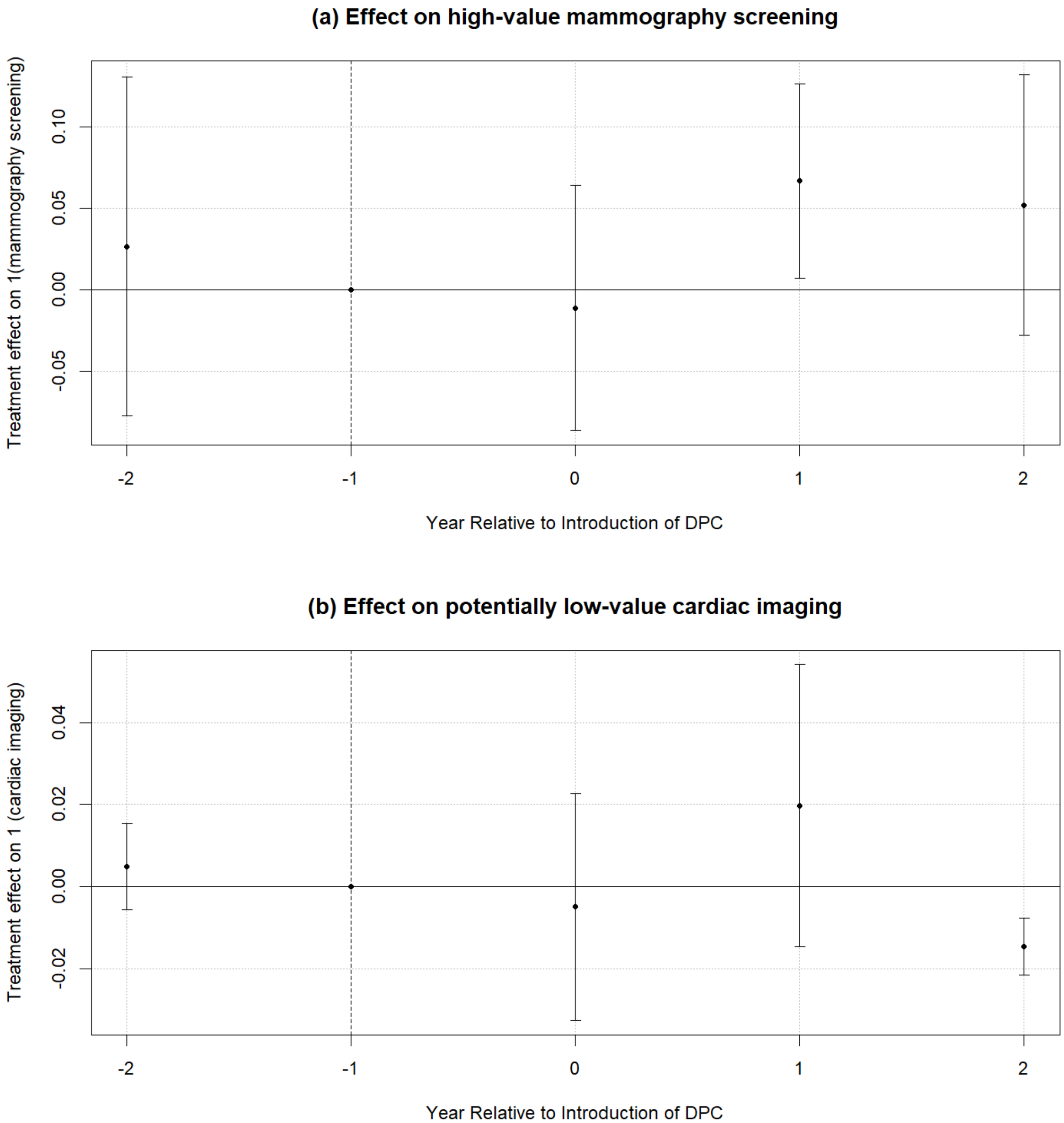


Figure 1.7: Quality difference-in-difference estimates for Equation (5). 95% confidence intervals are shown. I include all employees on employee-only plans without missing plan information during employment who were hired before the introduction of DPC who either never choose the Direct Care plan or first choose it in 2018. Year and individual fixed effects are used, and standard errors are clustered on the individual-year level. I restrict the sample for mammography screening to females on employee-only plans above age 50 on January 1, 2021, and under age 65. For cardiac imaging, the outcome variable is whether a patient has had an electrocardiogram, echocardiogram, advanced cardiac imaging, or a stress test in a given year, restricting to those who do not have ischemic heart disease, hypertension, or COPD, and are not both over 40 and having diabetes mellitus.

1.7 Conclusion

Clinician and patient incentives influence the type of care that patients receive. This study provides new evidence about how incentives shape outcomes in a growing practice structure type, Direct Primary Care, where clinicians are paid a capitated monthly fee for high-touch access to a bundle of primary care services. I present a variety of evidence suggesting the presence of selection in the demand for DPC, resulting in challenges to targeting.

Patients with more prior primary care use and with longer job tenures are less likely to choose Direct Care, suggesting one mechanism for selection is that higher primary care attachment increases switching costs. I find no evidence that Direct Care decreases total costs, combined with evidence of increasing out-of-pocket patient expenditures and total spending for those with lower spending levels, with a 118% increase in total medical spending at the median. While primary care service spending decreases, other specialty spending does not decrease as a result of DPC. However, I do find some indications that the value of health consumption may increase, as potentially low-value cardiac screenings decrease in frequency and preventive mammography screenings may increase. My findings contrast with previous work on increased primary care access, which found an increase in both high-value care and low-value care due to the increased access (Cliff, Hirth, and Fendrick; 2019). Future work may further explore mechanisms by which similar delivery models can improve quality. My findings also suggest that selection into plan offerings must be a prime consideration when designing and targeting benefit design strategies. In my setting, individuals with higher levels of primary care spending are less likely to sign up for Direct Care. However, this may be partially driven by inertia, since those with higher spending tend to be older and have lengthier job tenure. My results suggest the importance of understanding the relationship between inertia and selection and how treatments may differ in their short-term and long-term outcomes because of this relationship. Investments in plan offerings must be well-targeted to incentivize participation by the desired participants. Inertia and market frictions to targeting need to be considered when designing incentives.

DPC is notable in simultaneously changing the incentives that patients and clinicians face instead of only changing incentives for one group. Because of this, it can be difficult to decompose which effects are driven by clinician incentives and which effects are driven by patient incentive changes. Future work should attempt to decompose such effects further. In my setting, the payment model is bundled with a narrow network model for primary care. While patients may choose to use their previous primary care provider after choosing DPC, this results in increased employer spending and patient cost-sharing. While research exists on patient steering, more work is needed on the demand and effects of payment models explicitly tied to provider choice, especially in settings with market frictions.

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A Appendix

A.1 Family plan cost-sharing

Figure 1.A.1 shows cost-sharing attributes for family plans in 2020 for illustration. Employee premiums for Direct Care plans are lower than the premiums for Choice Plus plans. The co-insurance rate is higher on the Direct Care plan than the Choice Plus plan for medical spending categories. As a result, there is a level of non-DPC medical spending after the deductible is met for which the cost sharing on the Direct Care plan exceeds the cost sharing on the Choice plan.

Conditional on the same level of non-DPC health care utilization, costs are at least as high in the Employee Only plan, and there are spending levels for which costs are lower in the Direct Care plan and other levels where costs are higher in the Direct Care plan. However, analysis of the impact on cost-sharing is complicated by the potential substitution between non-DPC care and DPC care.

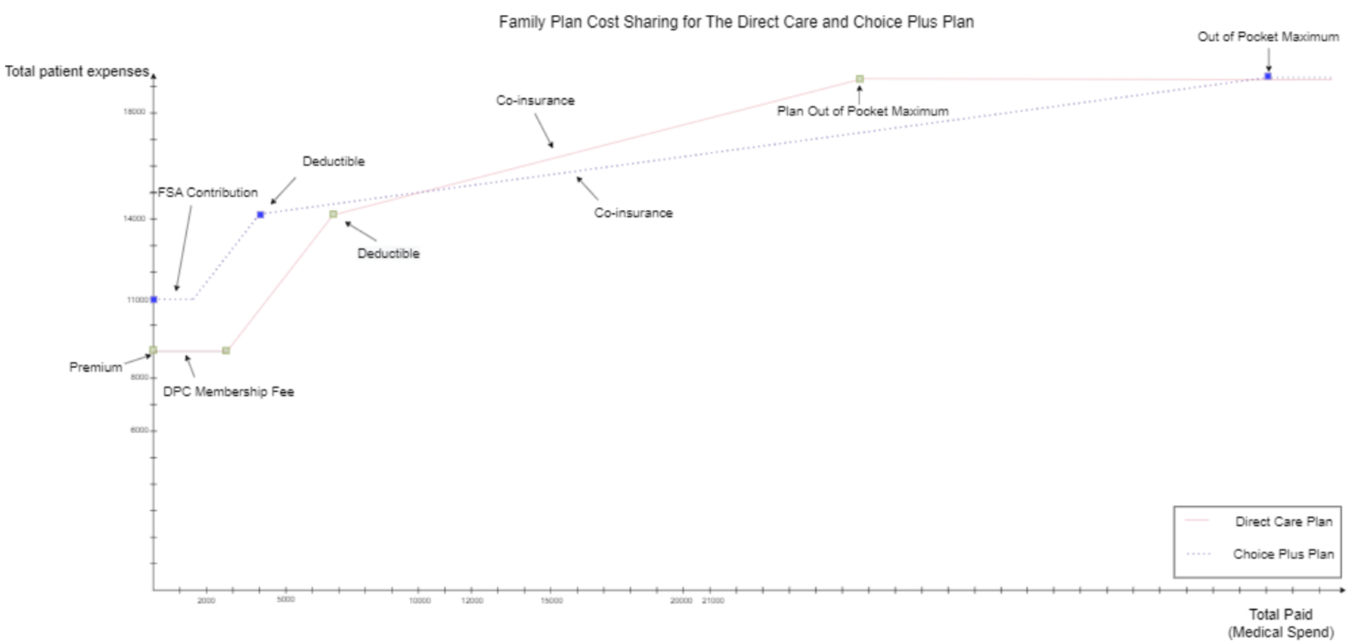


Figure 1.A.1: Family plan cost sharing for 2020. I use a marginal federal income tax rate of 24%, which is the tax rate for household earnings at least $86,350 a year, and a 4.55% tax rate in Colorado, the state where the employer is located, to factor in the premium tax advantage.

Given differences in premium subsidies between employee-only enrollees and those with dependents, we would expect to see differences in enrollment between the two groups. Table 1.A.1 shows that enrollees with dependents enroll in the Direct Care plan at a higher rate than enrollees with employee-only plans. While Direct Care plans become more common over time, non-employee-only plan choosers select the Direct Care plan at a higher rate than employee-only choosers.

Table 1.A.1: Plan Choice by Dependent Status

	Plan Choice	
Year	Choice Plus Plan	Direct Care Plan
	A: Employee only plan choice	
2016	1,130 (100%)	0 (0%)
2017	1,243 (100%)	0 (0%)
2018	1,083 (81%)	259 (19%)
2019	1,024 (74%)	352 (26%)
2020	953 (71%)	390 (29%)
	B: Non-employee-only plan choice	
2016	336 (100%)	0 (0%)
2017	367 (100%)	0 (0%)
2018	262 (66%)	129 (34%)
2019	234 (58%)	167 (42%)
2020	225 (56%)	180 (44%)

A.2 Plan retention

As a potential proxy for plan satisfaction, I use differences in plan retention between the Choice Plus and the Direct Care plans based on 2018 choice in Table 1.A.2. I find relatively similar numbers of plan retention between the plans, with Choice Plus members who leave the plan likelier to switch to Direct Care, and Direct Care members who leave the plan are likelier to do so because of leaving the employer.

Table 1.A.2: Plan Retention by Plan Type Based on 2018 Choice

	Plan Choice		
Year	Choice Plus Plan	Direct Care Plan	Leaving Employer
	A: DPC Plan Retention		
2018	0 (0%)	388 (100%)	0 (0%)
2019	18 (5%)	337 (87%)	33 (9%)
2020	28 (7%)	284 (73%)	76 (20%)
	B: Choice Plus Plan Retention		
2018	1,344 (100%)	0 (0%)	0 (0%)
2019	1,176 (88%)	99 (7%)	69 (5%)
2020	1,033 (77%)	132 (10%)	179 (13%)

A.3 Pre-treatment selection

To investigate trends in pre-treatment selection by year, I estimate the following regressions, with and without controls:

$$Y_{it} = DPCGroup_{2018} + DPCGroup_{2019} + DPCGroup_{2020} + \varepsilon_{it} \tag{6}$$

$$Y_{it} = DPCGroup_{2018} + DPCGroup_{2019} + DPCGroup_{2020} + X_{it} + \varepsilon_{it} \tag{7}$$

where X_{it} represents the controls that may relate to medical spending, such as employee type, age, job tenure, gender, and median zip code income.

I also estimate the following equations:

$$Y_{it} = EverTreated_{DPC} + \varepsilon_{it} \tag{8}$$

$$Y_{it} = EverTreated_{DPC} + X_{it} + \varepsilon_{it} \tag{9}$$

As above, X_{it} represents the controls that may relate to medical spending, such as employee type, age, job tenure, gender, and median zip code income.

The results of the pre-treatment regression are in Table 1.A.3. In Column 1, I present the results for the full sample of individuals who were employed before the introduction of DPC in 2018 including controls that may impact medical spending, such as job tenure, age, gender, and employee job type. In Column 2, I do not use any controls for the full sample. In Column 3, I re-run the regression with controls, restricting the sample to those on employee-only plans. In Column 4, I use no controls while restricting to the employee-only sample. Selection is consistently notable for those who choose the Direct Care plan in 2018, and the coefficient for medical spending is negative across all specifications. Selection for other years is mainly not significant, although lower spenders choose the Direct Care plan across multiple years. In Table 1.A.4, I present selection results for all individuals who ever choose Direct Care in the future instead of separating choices by year. I find similar selection patterns.

Table 1.A.3: Pre-treatment Selection by year

	(1)	(2)	(3)	(4)
	Controls	No Controls	Employee-only plan (Controls)	Employee-only plan (No Controls)
DPC Group 2018	-820.778	-908.167	-665.128	-754.378
	(172.103)	(201.108)	(195.326)	(235.770)
DPC Group 2019	324.720	20.968	1060.907	483.197
	(299.769)	(375.493)	(528.372)	(663.553)
DPC Group 2020	-306.648	-488.225	122.225	-160.065
	(879.138)	(677.518)	(939.373)	(714.337)
Age	43.389		54.287	
	(29.044)		(15.413)	
Male	-255.504		-377.965	
	(485.504)		(554.348)	
Job Tenure	40.891		41.072	
	(24.669)		(42.401)	
Median zip code income (000s)	8.406		8.098	
	(6.697)		(7.166)	
Observations.	2731	2737	2097	2101
R^2	0.013	0.004	0.025	0.003

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Estimates are of equation (6) and equation (7).

Table 1.A.4: Pre-treatment Selection

	(1) Controls	(2) No Controls	(3) Employee-only plan (Controls)	(4) Employee-only plan (No Controls)
Ever Direct Care	-612.486	-736.721	-357.599	-527.504
	(153.726)	(147.434)	(177.008)	(168.857)
Age	42.152		52.041	
	(29.000)		(15.443)	
Male	-274.820		-402.944	
	(475.164)		(546.655)	
Job Tenure	40.839		41.197	
	(24.290)		(42.298)	
Median zip code income (000s)	8.375		7.959	
	(6.636)		(7.162)	
Observations	2731	2737	2097	2101
R^2	0.012	0.003	0.024	0.003

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Estimates are of equation (8) and equation (9).

I next examine the relationship between choosing the Direct Care plan and cost outcomes, as in Table 1.8. I estimate the following equation, as in Section 5.1:

$$y_{it} = \gamma_1 \times PreDPCYear_{it} + \gamma_2 \times PostDPCYear_{it} + \gamma_3 DPC_{it} + PartialYear_{it} + Age_{it} + JobTenure_{it} + \varepsilon_{it} \quad (10)$$

In Table 1.A.5, I include a specification that does not exclude individuals who switch to Direct Care after 2018. Choosing the Direct Care plan is associated with higher out-of-pocket patient spending, lower primary care service spending and lower non-primary care service spending. However, I find no association with total spending.

Table 1.A.5: OLS Regression for Direct Care and change in spending

	(1) Total Spend	(2) Total Patient Spend	(3) Total Employer Spend	(4) Primary Care Service Spend	(5) Non-primary Care Service Spend	(6) ER Spend
Age	23.948	2.995	20.884	4.547*	18.960	-3.297
	(12.066)	(1.482)	(10.607)	(1.292)	(10.463)	(2.633)
Job Tenure	22.914	1.697	20.361	1.819	19.539	-1.153
	(13.758)	(1.763)	(12.312)	(1.870)	(12.485)	(1.001)
Median zip code income (000s)	4.892	0.258	4.622	0.548	4.357	-0.875
	(3.997)	(0.652)	(3.582)	(0.439)	(3.720)	(0.541)
Direct Care Plan	-27.919	143.899*	-138.074	-292.526**	-754.062*	-63.189
	(235.604)	(42.829)	(219.508)	(41.290)	(191.365)	(31.618)
Pre DPC Year	-5.630	13.371	-14.445	-0.267	-8.109	15.817
	(291.250)	(26.230)	(269.457)	(23.602)	(271.565)	(27.988)
Post DPC Year	981.502	6.456	998.794	-29.547	1015.392	13.070
	(638.265)	(44.642)	(565.074)	(56.452)	(605.080)	(177.232)
Partial Year	-917.641	-148.403*	-777.679	-200.908***	-742.633	-109.616
	(361.370)	(49.250)	(322.396)	(18.496)	(345.913)	(39.594)
Lagged chronic condition status	1528.990*	197.684*	1324.250*	189.904*	1323.940*	110.829*
	(427.402)	(62.982)	(365.654)	(51.606)	(389.758)	(36.400)
Observations	5787	5787	5787	5787	5787	5787
R^2	0.037	0.047	0.034	0.144	0.034	0.033

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors in parentheses. Year, employee type, and gender fixed effects are included. I restrict the sample to those who do not have missing plan information. Standard errors are clustered at the year-member level.

A.4 Plan Choice Models

I look at the distribution of plan switchers for those who were employed before the introduction of DPC in 2018 in Table 1.A.6. While most employees do not switch, those who do switch mainly switch in 2018, the first year that the Direct Care plan is available.

Table 1.A.6: First DPC Choice by Year

	First DPC Year	
Year	All Employees	Employee-only plan
Never DPC	1,190	958
2018	313	210
2019	59	37
2020	48	38

In Table 1.A.7, I present the results for linear probability models for plan choice for all employees and when restricting to those in employee-only plans. For both specifications, I estimate the impact of median zip code income, job tenure, age, gender, plan type, relative practice distance, and whether an employee is hired after the Direct Care program was introduced.

While being on an employee-only plan is associated with a lower likelihood of choosing the Direct Care plan, results are generally similar for all employees and restricting to those on employee-only plans. While coefficient estimates indicate that pre-Direct Care primary care spending is associated with a lower likelihood of choosing a Direct Care plan, I do not find evidence for a similar relationship for other medical spending. Also, as suggested by the descriptive evidence, being hired after the introduction of Direct Care is associated with an increased probability of choosing the Direct Care plan, as is having dependents on the health plan. Older employees and those who have been employed for longer periods are less likely to choose Direct Care. I find no effect of practice distance on Direct Care choice and no effect of employee type on plan choice. Higher median zip code income is associated with higher Direct Care enrollment.

Table 1.A.7: Linear Probability Model of Plan Choice (all employees)

Variables	(1) DPC Plan Choice (all)	(2) DPC Plan Choice (Employee-only plan)
Age	-0.001	0.000
	(0.000)	(0.000)
Partial Year	0.047*	0.066***
	(0.007)	(0.001)
Hire After DPC	0.235**	0.230**
	(0.012)	(0.012)
Lagged primary care OOP spend (000s)	-0.379*	-0.336
	(0.082)	(0.117)
Lagged non-primary care OOP spend (000s)	-0.026	-0.025
	(0.018)	(0.009)
Female	-0.010	-0.022
	(0.010)	(0.009)
Relative distance closest DPC	0.000	0.001
	(0.001)	(0.001)
Employee Only Plan	-0.214**	
	(0.012)	
Job Tenure	-0.008**	-0.002**
	(0.000)	(0.000)
Median zip code income (000s)	0.000*	0.001*
	(0.000)	(0.000)
Hourly Employee	0.032	0.015
	(0.015)	(0.014)
Professional Employee	0.038	-0.030
	(0.014)	(0.031)
Employee Only Plan × Job Tenure	0.007***	
	(0.000)	
Observations	5143	3969
R^2	0.129	0.107
FE: Chronic Conditions	Y	Y
FE: Year	Y	Y

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors in parentheses. I restrict the sample to those who do not have missing plan information and within 100 miles of a DPC clinic. Equation (2) includes plan choice for those on employee-only plans.

In Table 1.A.8, I present linear probability models of plan choice for pre and post-DPC hires. Results are broadly similar to the binary logit choice models in Section 4, but several results are not significant in the linear probability specification for post-DPC hires, such as relative DPC practice distance and gender.

Table 1.A.8: Linear Probability Model of Plan Choice

	(1)	(2)
Variables	Pre-DPC hires	Post-DPC hires
Age	0.0002	0.0017
	(0.0003)	(0.0010)
Partial Year	0.0909**	0.0562**
	(0.0126)	(0.0086)
Relative DPC practice distance	0.0040**	-0.0031
	(0.0006)	(0.0011)
Female	-0.0083	-0.1181
	(0.0044)	(0.0458)
Median zip code income (000s)	0.0008**	0.0007
	(0.0002)	(0.0009)
Hourly Employee	0.0036	0.0639
	(0.0112)	(0.0479)
Professional Employee	-0.0066	-0.1531*
	(0.0097)	(0.0397)
Job Tenure	-0.0018**	
	(0.0003)	
2017 primary care OOP spend (000s)	-0.0264*	
	(0.0070)	
2017 non-primary care OOP spend (000s)	-0.0022	
	(0.0052)	
Observations	2986	736
R^2	0.022	0.043
FE: Chronic Conditions	Y	Y
FE: Year	Y	Y

Notes: Chronic condition status is 2017 for pre-DPC hires and previous year status for post-DPC hires. I restrict the sample to those who choose employee-only plans. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors in parentheses.

A.5 Poisson estimates of the impact of Direct Care

In this section, I present Poisson models for spending instead of OLS models of associations between choosing the Direct Care plan and spending outcomes. I present results for all switchers to Direct Care and those who switch in 2018 only in Tables 1.A.9 and 1.A.10, respectively. In both specifications, choosing the Direct Care plan is associated with higher total patient spending and lower primary care service spending. However, there is no statistically significant decrease in non-primary care service spending. In the full sample, I do not detect any selection into the plan based on previous spending, but restricting to 2018 switchers, there is an association between lower previous spending and DPC plan choice.

Table 1.A.9: Poisson Regression for Direct Care and Change in Spending

	(1) Total Spend	(2) Total Patient Spend	(3) Total Employer Service Spend	(4) Primary Care Service Spend	(5) Non-primary Care Service Spend
Age	0.036*	0.008*	0.042*	0.011***	0.042*
	(0.018)	(0.004)	(0.021)	(0.003)	(0.021)
Job Tenure	0.012	0.003	0.012	0.003	0.012
	(0.019)	(0.004)	(0.022)	(0.004)	(0.021)
Median zip code income (000s)	0.006*	0.001	0.007*	0.001	0.007*
	(0.003)	(0.002)	(0.003)	(0.001)	(0.003)
Direct Care Plan	-0.105	0.349***	-0.058	-0.732***	-0.336
	(0.200)	(0.096)	(0.221)	(0.098)	(0.269)
Pre DPC Year	-0.150	0.032	-0.194	-0.018	-0.158
	(0.162)	(0.069)	(0.197)	(0.062)	(0.178)
Post DPC Year	-0.289	0.017	-0.334	-0.059	-0.314
	(0.309)	(0.123)	(0.347)	(0.080)	(0.359)
Partial Year	-0.024	-0.449*	0.050	-0.586***	0.024
	(0.234)	(0.176)	(0.238)	(0.114)	(0.252)
Lagged chronic condition status	0.574***	0.465***	0.613***	0.370***	0.619***
	(0.121)	(0.124)	(0.136)	(0.087)	(0.131)
Observations.	5787	5787	5787	5787	5787

Notes: Year, gender, and employee type fixed effects are included. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 1.A.10: Poisson Regression for Direct Care and change in spending (employee only)

	(1) Total Spend	(2) Total Patient Spend	(3) Total Employer Service Spend	(4) Primary Care Service Spend	(5) Non-primary Care Service Spend
Age	0.039*	0.008	0.046*	0.011***	0.045*
	(0.019)	(0.004)	(0.022)	(0.003)	(0.022)
Job Tenure	0.011	0.003	0.012	0.003	0.011
	(0.021)	(0.005)	(0.024)	(0.004)	(0.024)
Median zip code income (000s)	0.007*	0.001	0.008*	0.001	0.007*
	(0.003)	(0.002)	(0.004)	(0.001)	(0.003)
Direct Care Plan	-0.009	0.356***	0.051	-0.803***	-0.178
	(0.263)	(0.103)	(0.292)	(0.081)	(0.353)
Pre DPC Year	-0.390***	-0.091	-0.487***	-0.043	-0.428***
	(0.089)	(0.053)	(0.115)	(0.047)	(0.096)
Post DPC Year	-0.442	0.029	-0.520	-0.020	-0.497
	(0.277)	(0.134)	(0.309)	(0.051)	(0.321)
Partial Year	0.044	-0.410*	0.123	-0.571***	0.103
	(0.213)	(0.195)	(0.215)	(0.119)	(0.220)
Lagged chronic condition status	0.581***	0.477***	0.623***	0.385***	0.622***
	(0.161)	(0.128)	(0.180)	(0.088)	(0.173)
Observations	5225	5225	5225	5225	5225

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors in parentheses. Year, employee type, and gender fixed effects are included. I restrict the sample to those on employee-only plans.

A.6 IV falsification tests and robustness checks

I test whether there is any statistically significant association between covariates and my instrument in Table 1.A.11 by using the covariates as an outcome variable. I do not find any statistically significant effects, suggesting that endogeneity concerns may not threaten the validity of my instrumental variable strategy.

Table 1.A.11: IV falsification test

	(1) Age at Hire Year	(2) Gender	(3) Median Zip Code Income	(4) Relative DPC Practice Distance
Hire After DPC	-3.847	-0.094	13.358	6.454
	(3.729)	(0.166)	(7.419)	(2.223)
p-value	0.411	0.628	0.214	0.101
Observations	690	690	689	690

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors in parentheses. I restrict the sample to those who are on employee-only plans.

In addition to using the probit first stage approach to instrumental variables, I also use a linear first stage methodology, with $HireAfterDPC$ as the relevant instrument. Results are similar when comparing between the probit approach used in the main part of the paper and the linear first stage approach in the Appendix.

$$DPC_{it} = \gamma_0 + \gamma_t + \gamma_1 \mathbb{1}(HireAfterDPC) + \gamma_2 X_{it} + \xi_{it} \tag{11}$$

$$y_{it} = \beta_0 + \beta_t + \beta_1 P(DPC_{it}) + \beta_2 X_{it} + \varepsilon_{it} \tag{12}$$

Table 1.A.12: IV Estimates for Medical Spending

	(1) Total Spend	(2) Total patient Spend	(3) Total Employer Service Spend	(4) Primary care Service Spend	(5) Non-primary care Service Spend	(6) ER Spend
Direct Care plan	511.986	-117.517	628.872	-507.023	-104.514	-545.053
	(1990.545)	(277.019)	(1728.106)	(226.585)	(1842.349)	(303.680)
Age	9.494	2.782	7.126	1.307	8.961	-9.672
	(22.013)	(3.017)	(19.189)	(3.133)	(19.476)	(3.687)
Median zip code income (000s)	-2.241	-1.831	-0.836	-0.273	-3.298	1.848
	(9.721)	(0.820)	(9.072)	(1.392)	(7.653)	(1.914)
Female	929.119	133.164	807.817	135.207	831.604	-31.503
	(537.769)	(71.963)	(515.665)	(47.877)	(464.970)	(146.964)
Relative DPC practice distance	7.008	5.582	-2.536	-1.401	-0.863	12.184
	(70.309)	(7.667)	(59.590)	(5.900)	(58.414)	(12.272)
Observations	689	689	689	689	689	689
F-test	38.1	38.1	38.1	38.1	38.1	38.1

Notes: Estimates of β from Equation (11). Observations are from employees hired between 2016 and 2019 who choose employee-only benefit plans and never have plan choice missing. Partial years are excluded. Chronic condition, employee type, and year fixed effects are used. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

A.7 Mean spending (unwinsorized)

In Figure 1.A.2, I include an unwinsorized graph of spending over time. Similar to the winsorized version, there is no notable increase in medical spending when the DPC plan is introduced in 2018.

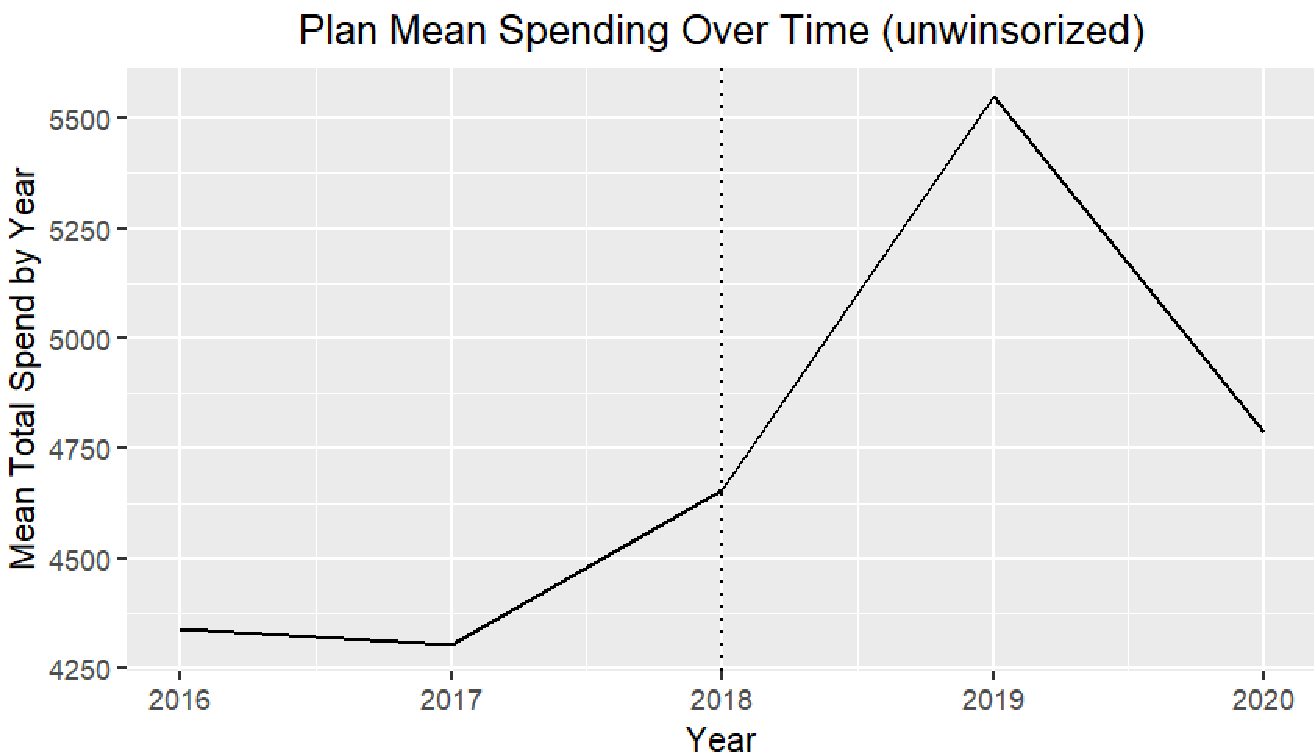


Figure 1.A.2: Unwinsorized medical spending by year. All numbers are adjusted to 2016 levels of medical CPI.

A.8 Difference-in-difference outcomes

In Tables 1.A.13 and 1.A.14, I include the table versions of Figures 6 and 7.

Table 1.A.13: Difference-in-Difference Estimates for Spending

	(1) Total Spend	(2) Total patient Spend	(3) Total Employer Spend	(4) Primary care Service Spend	(5) Non-primary care Service Spend	(6) ER Spend
Year × × -2	-1.520	9.808	-27.005	4.959	26.548	-5.675
	(472.677)	(29.540)	(450.051)	(23.285)	(451.326)	(15.705)
Year × × 0	446.214	275.285***	178.980	-300.302***	-273.719	-130.608**
	(292.994)	(42.596)	(282.450)	(21.158)	(278.996)	(42.040)
Year × × 1	516.107	158.980**	393.970	-329.366***	-34.453	7.859
	(495.979)	(48.409)	(452.929)	(30.095)	(443.790)	(63.171)
Year × × 2	573.869	116.423**	468.301	-211.045***	-64.848	1.010
	(386.794)	(41.401)	(360.815)	(36.887)	(361.233)	(36.003)
Observations	4969	4969	4969	4969	4969	4969
R^2	0.385	0.448	0.372	0.537	0.375	0.274

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Estimates for Equation (5).I include all employees on employee-only plans without missing plan information during employment who were hired before the introduction of DPC who either never choose the Direct Care Plan or first choose it in 2018. Year and individual fixed effects are used. Standard errors are clustered on the individual-year level.

Table 1.A.14: Quality Difference-in-Difference Estimates

	(1)	(2)
	Mammography Screening	Cardiac Imaging
Year × × -2	0.027	0.005
	(0.053)	(0.005)
Year × × 0	-0.011	-0.005
	(0.038)	(0.014)
Year × × 1	0.067*	0.020
	(0.030)	(0.018)
Year × × 2	0.052	-0.015***
	(0.041)	(0.004)
Observations	1846	4269
R^2	0.479	0.327

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Two-way standard errors are clustered at the member-year level. I include all employees on employee-only plans who were hired before the introduction of DPC who either never choose the Direct Care Plan or first choose it in 2018 without missing plan information during employment. For mammography screening, I restrict the sample to females over the age 50 and under age 65 so that my results are not driven by changes in screening recommendations or Medicare eligibility during the sample. For cardiac imaging, I restrict the sample to patients who do not have ischemic heart disease, hypertension, or COPD, and are not over 40 with diabetes mellitus. Individual and year fixed effects are included. Standard errors are clustered on the individual-year level.

A.9 Staggered difference-in-difference estimates

For the staggered treatment difference-in-difference model, I use the Sun-Abraham (2021) method to account for the issues with standard TWFE models discussed in Baker et al. (2021). I have an unbalanced sample, as different treatment groups have different amounts of pre and post-treatment years depending on the year they first receive the treatment. For instance, only those who switch to DPC in 2018 have two years of post-treatment data, and only those who switch in 2020 have four years of pre-treatment data.

I estimate the following equation:

$$y_{it} = \alpha_i + \lambda_t + \sum_{e \notin C} \sum_{\ell \notin -1} \delta_{e\ell} (\mathbb{1}\{E_i = e\} * D^{\ell}_{i,t}) + \varepsilon_{i,t} \tag{13}$$

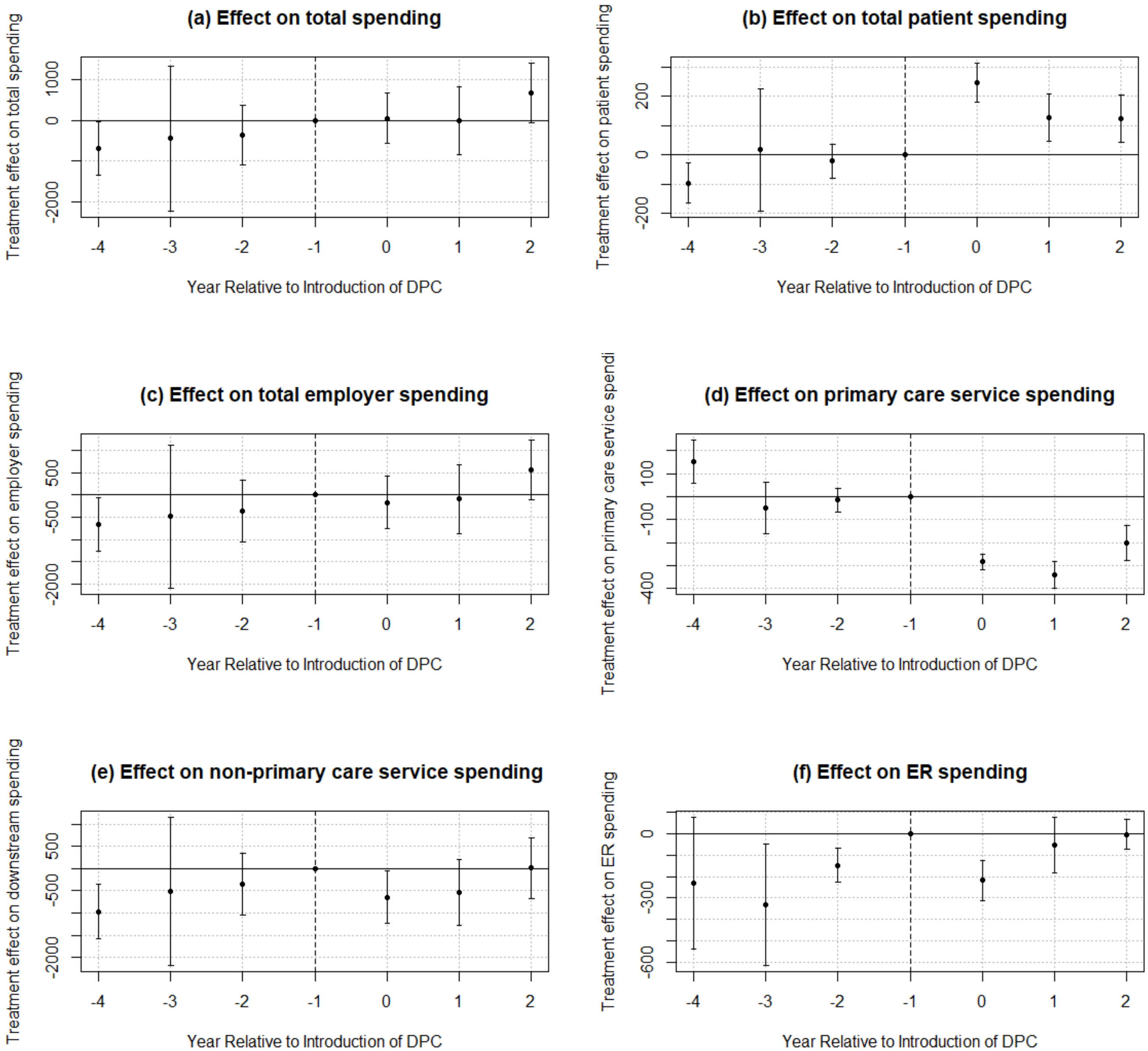


Figure 1.A.3: Estimates for Equation (13). 95% confidence intervals are shown. I include all employees on employee-only plans without missing plan information during employment who were hired before the introduction of DPC who either never choose the Direct Care Plan or first choose it in 2018. Year and individual fixed effects are used. Standard errors are clustered on the individual-year level.

Chapter 2: Direct Primary Care: Practice Distribution and Pricing Across the United States

2.1 Introduction

Trends in physician organization have dramatically shifted in the last decade. While vertical health system integration has increased and the number of independent physicians has declined, there has also been a shift in the organizational structure of physician practices that remain independent, including shifts in the reimbursement models leveraged by these practices (Capps, Dranove, and Ody, 2018). These models can be positioned on a spectrum of reimbursement structures, ranging from pure fee-for-service (FFS) on one end to risk-bearing value-based models on the other (Landon, Weinreb, and Bitton, 2022). Understanding the organizational structures of clinician practices and their effects is especially important in light of the high levels of burnout and practice disruption reported during the COVID-19 pandemic.

We study a little-examined but growing form of physician organization: a direct primary care (DPC) practice. DPC practices charge a monthly fee for a bundle of primary care services and typically offer shorter waits and longer appointment times, with some offering additional services at some cost, such as medication dispensing or labs. Some practices operate in a hybrid model, seeing a portion of patients on a fee-for-service basis and others as part of a membership model. While some practices work exclusively with employers, we concentrate on practices that include a direct-to-consumer option. We provide the first US-wide description of DPC pricing practices by varying patient characteristics, especially age, and document other characteristics of DPC practices. We also investigate the characteristics related to DPC pricing, such as organizational structure, region, rurality, and services offered. We additionally study the characteristics associated with having more DPC practices in a particular county. Finally, we discuss the implications of our results and suggest areas of future research.

The Direct Primary Care practice model has evolved since its first iterations in the mid-1990s. The first known retainer or membership-based practice, MD2 (“MD squared”), catered to high net-worth individuals, and charged $10,000 to $20,000 a year in addition to traditional insurance payments.

Shortly after, several similar practices began with similar business models, often charging less. Over time, these initial practices – typically those that charged monthly fees on top of collecting insurance – began being referred to as concierge medicine. In 2007, Qliance Medical Group began in Seattle, WA and popularized the modern "pure" DPC model – that of pricing monthly fees and removing insurance from the relationship (Wu, Bliss, Bliss, and Green, 2010).

Direct Primary Care growth has continued, with 21 practices noted on the DPC Frontier Mapper in 2010, 273 known practices in 2015, and 1,889 practices as of November 2022. DPC now represents about 2 – 3% of all family medicine practices (AAFP) and a larger portion of independent physician practices. Despite this growth and increased demand for their services by employers, understanding of DPC practices remains limited. Practices are distributed unevenly across the United States, and variation exists in the dollar amount charged to members and the breadth of services provided for the given periodic fee. The DPC model has reached greater practice density in specific geographies, but broad adoption is still nascent compared to the scale of the United States healthcare system.

2.2 Existing literature

We conduct a literature search leveraging the search string ["direct primary care" OR "concierge" OR "concierge medicine" OR "retainer medicine"] in PubMed, yielding 261 results as of June 1, 2022. Only one result yields a comparable study of pricing and geographic variation for DPC practices: Eskew and Klink (2015), who found 116 practices that met their inclusion criteria with an average monthly patient cost was $93.26 and a median monthly cost of $75. A limitation of the Eskew and Klink (2015) study is that DPC physicians often tier their pricing by age, accounting for the clinical intensity required of a given population.

The search term "direct primary care" yielded 47 results, up from the three results noted in the 2015 study. In addition to academic work, a search was also conducted for grey literature, which produced limited results. In 2017 and 2022, Hint Health, a software company that automates the back-office administration of DPC practices, published reports highlighting geographic dispersion of DPC practices on a per capita basis. Busch, Grezkowiak, and Huth (2020) examine the effects of DPC on the total cost of care for an employer sponsored health plan, also surveying 200 DPC physicians who reported monthly DPC membership for adults ages 65 and over, and $150 for families. While the Busch, Grezkowiak, and Huth (2020) survey includes pricing categories by age bracket, the sample size is limited and geographic differences are not accounted for. Besides being more comprehensive in terms of pricing, our study is the first to explore the factors related to the entry of DPC practices.

2.3 Study Data and Methods

2.3.1 Data

We analyze data from public sources to investigate the current landscape of Direct Primary Care practices. We begin with a data extract containing 546 rows of practice data sourced from the DPC Frontier Mapper as of December 2018. We compare this list to existing public data sources in May 2020, including the May 2020 versions of the DPC Frontier Mapper, DPC Alliance Directory, EverMed DPC Network Map, and lists of registered DPC practices from both the State of Washington and State of Oregon. DPC location data from these sources results in 723 additions. With this expanded list of 1,269 practices, we visit each practice website and verify location information. We also record available pricing information, specifically enrollment or startup fees for individuals, partners, and families, individual monthly DPC prices for ages 0 to 100, monthly prices for partner plans (ex: 2 spouses on a single DPC membership), monthly prices for family plans, as well as monthly prices for corporate plans.

Starting with a sample of 1,269 practices, we exclude 104 practices for not having pricing information available and 50 practices for being either pediatric, concierge[1], or specialty care. A further 47 are excluded for charging per visit fees. 61 practices are excluded for having employer-only contracts and having no direct-to-patient access. Finally, after excluding 64 practices that had closed and two duplicate entries, 941 individual practices with available pricing data are included in the analysis. We merge practice-level data with 2019 data from the Agency for Healthcare Research and Quality Social Determinants of Health database. This database includes zip-code and county-level data from a variety of sources, including the American Community Survey and the Area Health Resource File, on local population characteristics, health status, and health workforce.

To map DPC practices, an instance of ArcGIS licensed to Dartmouth College is used. We add a layer to map income by census tract with data for "Median Household Income in past 12 months (inflation-adjusted)" from the Census Bureau's American Community Survey (ACS), for the years 2014-2018. The extrema for heat mapping based on income is <$25,100 at the low end (2018 federal poverty level for a household of 4) and >$100,400 at the high end (400% of FPL for a household of 4). Address information verified on each DPC practice website is uploaded and overlaid on this map to represent DPC practice location by immediate census tract income.

[1] Concierge practices are defined as having a monthly fee of at least $300 or having a subscription fee and a per-visit fee.

DPC practices generally tier pricing by age; only 183 out of 941 (19.4%) practices charge a flat rate for all ages. The practices, however, differ in the cutoffs they use to tier by age. For example, in the <18 year old population, there might be 5-10 age brackets for "children" (0-13 years old, 0-17, 0-18, 1-17, 2-17, 13-17, etc.). We create a table of 101 columns representing ages 0-100 and record the individual pricing for each age to standardize pricing by age. We weigh pricing by age based on demographic information on panel size we receive from a company providing administrative services for many DPC practices, representing 800,000 patients. Patients ages 0 to 17 represent 17% of patient panels, those ages 18 to 44 are 43% of patient panel lives, those ages 45 to 64 are 30% of patient panels, and those 65 and above are 9% of panel lives. We calculate the average pricing within each of these ranges and then weigh by portion of overall panel size to compute average pricing for the panel and average adult pricing. Some practices simply state prices for "children", "adults", and "seniors". In these instances, we assume children are 0-17 years old, adult are 18-64 years, and senior are 65 or older. Stata/SE 15.1 is utilized for all statistical analysis.

Some practices have dual pricing for child members: a price with participating adults on the same plan, and a higher price for the child only to be a member. In these instances, we capture the higher individual member pricing to provide a standardized measure. Some practices require an adult to participate in the membership, in which case we record the child price and flag the practice. Finally, other practices do not have stated requirements for child membership, so we record this data as independent membership pricing. Practices often discount for multiple family members on a single plan, association with certain employers, paying up front in quarterly/semi-annual/annual installments vs. monthly, student status, and certain professions such as military/law enforcement/first responders. To make standardized comparisons, we capture individual pricing only and do not account for discounts of any kind, which are prevalent.

Of the practices sampled, we also record whether the practice is rural or non-rural. Rurality information comes from zip code classifications from the Federal Office of Rural Health and county classifications from the Area Health Resource File.

2.3.2 Measures and Analysis

Outcomes

We have two primary outcomes. First is the average adult price, and second is the number of DPC practices per 10000 in a county. For predictor variables, we use several practice and location characteristics. For pricing information, we include practice characteristics such as whether employer prices are provided, whether the practice has a flat fee across ages, whether a practice provides pediatrics or is only open for adults, chain status, and whether a practice requires a sign-up fee. We use practice weights from a large administrative service company for DPC practices to weigh patient panel prices based on industry-wide weights. As in Pauly and Saterwaite (1981), we include area characteristics to proxy for supply, cost of living, consumer demand, and consumer information. Area characteristics for geographic information are at the zip code level for the pricing analysis and at the county level for the DPC location analysis. We examine the relationship between pricing and practice-specific factors while controlling for other supply, consumer information, and demand factors. We use linear regression models to find the association between area characteristics described above and our two primary outcomes: average adult price, and the number of DPC practices per 10,000.

2.3.3 Limitations

There are several limitations to note for our study, particularly for the pricing analysis. Firstly, we do not observe the patient volume of each practice, so we cannot weigh by practice patient volume. Furthermore, given the non-standardized nature of DPC pricing, there are several limitations to the information gathered and analyzed: There was an inability to capture a *weighted* price based on a practice or provider's particular patient panel. If, for example a physician charged $0 for children, $100 for adults, and $200 for seniors and had a patient panel of 1 each, the average price would be $100 per patient per month (PPPM). We mitigate this by using national data on overall DPC patient age weights. Additionally, the DPC practice websites do not always explicitly state whether the child pricing required an adult. If nothing positively indicates a necessary adult, we assume the child pricing is reasonable as an independent member price, which may not be the case. Because we are not accounting for any discounts, our prices may represent upper bounds of the actual prices paid.

DPC practices may differ in the scope of services provided for the monthly fee. Some practices may include basic x-rays and lab work in their monthly fee, while others may charge additional cash prices. While we analyze the relationship of some services to pricing for a sub-sample, not all services are considered as they relate to pricing. Finally, for some practices that fall under managed service organization (MSO) type structures such that the practices remain independent but remit some fee to the MSO in exchange for marketing and management of back office functions, prices listed on the MSO website did not agree with pricing listed on the practice's website itself. In these instances, we default to the prices on the practice website. If the practice does not list prices or has no dedicated web presence, we record prices as listed on the MSO's website.

2.4 Results

2.4.1 Descriptive Results

Table 2.1: Practice Summary Statistics

Variable	N	Mean (SD)
Average adult price	940	81.33 (31.86)
Flat fee	941	0.228 (0.420)
Chain Status	941	0.131 (0.337)
Only teen/adult patient	941	0.162 (0.368)
Enrollment fee	941	0.397 (0.490)
Employer business	941	0.203 (0.402)
DPC law	941	0.724 (0.447)
Northeast	941	0.094 (0.291)
West	941	0.256 (0.437)
South	941	0.416 (0.493)
Midwest	941	0.235 (0.424)
Hybrid practice	553	0.145 (0.352)
Home visits	552	0.525 (0.500)
Radiology discounts	550	0.618 (0.486)
Medication dispensing	542	0.450 (0.498)

Table 2.1 summarizes relevant practice characteristics. Practices are more prevalent in the South, with 41.6% of all practices in that region. DPC practices do not uniformly impose registration or enrollment fees. Of the 941 practices included, 161 (17.1%) have explicitly no startup fee while 374 (39.7%) do. The remaining 406 (43.1%) practices provide ambiguous information on whether a fee is charged. Data gathered on single adult registration fees indicate that practices that charge these fees have lower monthly fees. The average fee across all age groups for practices with registration fees is \$77.77 (95% CI \$75.25-\$80.29). For practices without registration fees, the average monthly price is \$85.95 (95% CI \$79.78-92.14). The difference in these means is \$8.19 ± 2.83 (95% CI).

191 practices (20.3%) explicitly state support for employer customers, often with a discount to their typical list prices. 128 of these practices transparently disclose prices or price ranges, while the remaining 63 prompt the site visitor to contact the practice to discuss. Several other practice characteristics are available for the subset of practices that are listed on the DPC Frontier Mapper

website. We explore the relationships between these characteristics and pricing. Of the 552 practices with available information, 80 (14.5%) are hybrid practices. The average monthly DPC fee for these practices is $77.89 (95% CI $72.94 - $82.85). For pure DPC practices, the average fee is $80.91 (95% CI $77.93 – 83.89). The difference in means is $3.01 ± 7.66 (95% CI).

Of the 549 practices with available information, 339 (61.7%) offer radiology discounts. The average monthly DPC fee for these practices is $78.09 (95% CI $75.14 - $81.03). The average fee for those not offering radiology discounts is $84.53 (95% CI $79.39 – 88.68). The difference in means is $6.44 ± 5.59 (95% CI). Of the 541 practices with available information, 243 (44.9%) offer medication dispensing. The average monthly DPC fee for these practices is $73.37 (95% CI $71.00 - $75.76). The average fee for those not offering medication dispensing is $85.86 (95% CI $81.56 – 90.17). The difference in means is $12.48 ± 5.31 (95% CI). The finding of lower DPC fees among practices that dispense medications holds across different geographic sub-regions.

Finally, there are 123 practice chains (13.1%) in our data, where a chain is defined as any practice with four or more locations. This definition of chain includes some practices that are independent but affiliated with a larger DPC group (likely for the purpose of employer-based contracts). Chains are likelier to list employer pricing than non-chains (37.6% compared to 16.4%). There is no difference in pricing between DPC chains and other DPC practices ($0.38 ± 3.10 (95% CI)).

We also examine price differences based on practice rural status. 786 practices (83.5%) are in “non-rural” zip codes while the remaining 155 (16.5%) are rural. In Table 2.2, we show that rural practices exhibit significantly lower prices than non-rural practice prices. However, price differences may not be driven by rural status, so we examine whether these rural price differences are driven by rurality or other factors in a later section of the paper.

Table 2.2: Pricing Characteristics of DPC Practices in rural and non-rural areas

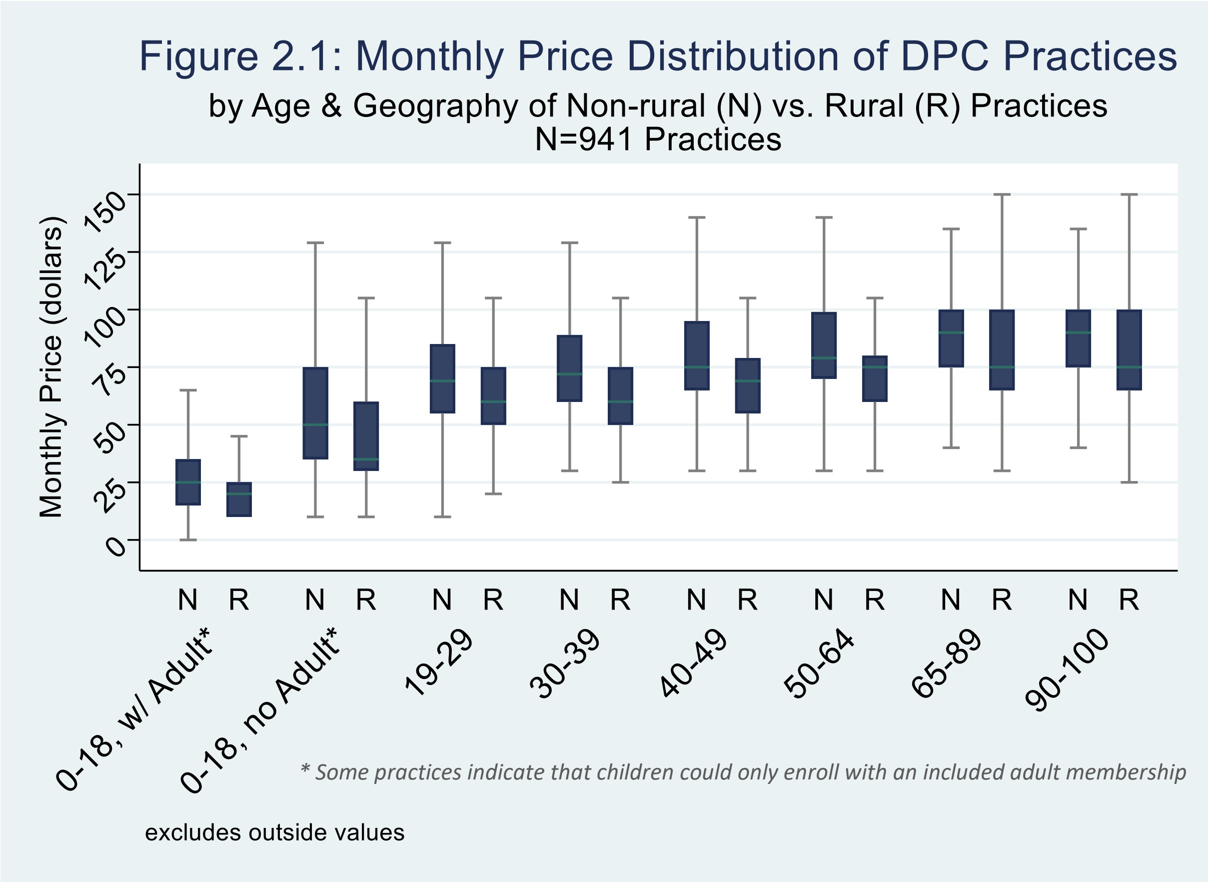
Characteristics	Not Rural^	Rural	Diff. in means (95% CI)	p-value*
Age Bracket				
0-17, Individual Monthly Price; Adult required for price				
Number of practices with pricing	191	37		
Mean monthly price, (SD)	28.2 (20.9)	20.5 (9.7)	7.7 (1.0 - 14.4)	0.0155
Median monthly price	25	22.9		
0-17, Individual Monthly Price; Adult not required for price				
Number of practices with pricing	514	107		
Mean monthly price, (SD)	58.6 (34.1)	44.6 (23.5)	14.1 (7.3 - 20.8)	<0.0001
Median monthly price	50	39		
18-29, Individual Monthly Price				
Number of practices with pricing	790	150		
Mean monthly price, (SD)	75.6 (34.4)	63.7 (23.1)	11.9 (6.1 - 17.6)	<0.0001
Median monthly price	67.6	60		
30-39, Individual Monthly Price				
Number of practices with pricing	790	150		
Mean monthly price, (SD)	80.0 (34.8)	67.7 (22.7)	12.3 (6.5 - 18.2)	<0.0001
Median monthly price	70	60		
40-49, Individual Monthly Price				
Number of practices with pricing	790	150		
Mean monthly price, (SD)	84.2 (34.3)	71.4 (22.2)	12.8 (7.1 - 18.5)	<0.0001
Median monthly price	75	66.5		
50-64, Individual Monthly Price				
Number of practices with pricing	791	150		
Mean monthly price, (SD)	88.5 (34.1)	74.9 (23.2)	13.6 (7.9 - 19.3)	<0.0001
Median monthly price	79	75		
65-89, Individual Monthly Price				
Number of practices with pricing	772	141		
Mean monthly price, (SD)	96.5 (35.0)	81.7 (24.0)	14.7 (8.7 - 20.8)	<0.0001
Median monthly price	90	75		
90-99, Individual Monthly Price				
Number of practices with pricing	766	141		
Mean monthly price, (SD)	96.7 (35.7)	82.0 (24.1)	14.8 (8.6 - 20.9)	<0.0001
Median monthly price	90	75		

**Continuous variables are tested with the 2 Sample t-Test.*

^Rurality is determined by zip code matching to the Federal Office of Rural Health Policy Eligible ZIP Code file. There are ZIP codes with urban population included in the file and ZIP codes with rural population that are not included. Any ZIP code where more than 50% of its population resides in either a Non-Metro County or a rural Census Tract is included in the file as a rural zip code.

We also study whether census tract median income relates to the location of DPC practices. See **Figures A2-A5** in the appendix for a selection of DPC practice locations overlaid on income levels by census tract in four metro areas that exhibit a comparatively higher level of DPC density (Indianapolis, IN/Cincinnati, OH; Denver, CO; Chicago, IL; Seattle, WA). Across all geographies, practices are more often located in or near higher income tracts, with the exception of the rural tracts outside of Indianapolis and Cincinnati.

In **Figure A6**, we compare state level average prices across all age groups for all 50 states against a national price index. In general, the coastal states tend to exhibit higher prices than the national averages, with notable outliers Hawaii at 1.64 times the national average and Alabama being at 0.69 times the national average. Figure 2.1 shows the wide range of DPC practice prices across most age groups.[2]



[2] An additional set of graphical representations of total price distribution by age bracket can be found in the histograms depicted in **Figure A1** in the Appendix.

The average fee across all age groups for practices with registration fees is $77.77 (95% CI $75.25-$80.29). For practices without registration fees the average monthly price is $85.95 (95% CI $79.78-92.14). The difference in these means is $8.19 ± 2.83 (95% CI). Practices open only to teenagers and adults have an average adult price of $106.09 (95% CI $98.65-113.54). Practices also open to children have an average adult price of $82.38 (95% CI $80.51-84.26). The difference in these means is $23.71 ± 5.47 (95% CI).

2.4.2 Regression Results

Table 2.3: Association between adult price and area characteristics

DPC practices per 10000	*Coef.*	*St.Err.*	*t-value*	*p-value*	*[95% Conf*	*Interval]*	*Sig*
Practice Specific Factors							
Employer price listed	-7.229	2.92	-2.48	.013	-12.961	-1.498	**
Enrollment fee noted	-6.342	2	-3.17	.002	-10.267	-2.418	***
No children in practice	18.624	2.706	6.88	0	13.313	23.935	***
DPC chain	4.509	3.111	1.45	.148	-1.596	10.615	
Flat fee structure	5.251	2.385	2.20	.028	.569	9.932	**
Supply factors							
Rural zip code	-.588	3.498	-0.17	.867	-7.454	6.278	
Northeast	-2.484	4.637	-0.54	.592	-11.584	6.616	
West	3.491	4.238	0.82	.41	-4.827	11.81	
South	.891	3.377	0.26	.792	-5.736	7.518	
DPCs per 10000	-2.668	6.711	-0.40	.691	-15.838	10.502	
DPC law passed	.674	2.705	0.25	.803	-4.635	5.983	
Population	-.369	1.228	-0.30	.764	-2.778	2.04	
Median rent (00s)	.997	.53	1.88	.06	-.044	2.038	*
Consumer information							
% public transit to work	.32	.191	1.67	.095	-.055	.695	*
% with bachelors or more	-.186	.283	-0.66	.511	-.742	.37	
Demand Determinants							
Median income (000s)	.148	.084	1.77	.076	-.016	.312	*
PCPs per 10000	.539	.481	1.12	.262	-.404	1.483	
% black	.047	.12	0.39	.698	-.19	.283	
Medicaid %	-.053	.254	-0.21	.834	-.551	.445	
% above 65	.532	.257	2.07	.039	.027	1.037	**
Uninsured % under 64	.365	.304	1.20	.23	-.231	.962	
Proportion with income over $100,000	.219	.205	1.07	.287	-.184	.622	
Poverty rate	.162	.247	0.66	.511	-.323	.648	
Mean dependent var		81.224	SD dependent var			31.776	
R-squared		0.202	Number of obs			934.000	
F-test		6.911	Prob > F			0.000	
Akaike crit. (AIC)		8967.479	Bayesian crit. (BIC)			9132.021	

**** p<.01, ** p<.05, * p<.1. Population variables are standardized. Other variables controlled for are percentage with no Internet access, percentage with kids, percentage employed in administrative work, percentage employed in manufacturing, percentage of adults getting a checkup in the last 12 months, average household size, labor force participation rate, and Gini inequality index.*

Table 2.3 displays the association between adult price and practice and area characteristics. For practice characteristics, not seeing children is associated with higher adult prices, as is having a flat fee structure. Meanwhile, listing a price for employers is associated with a lower price for direct-to-consumer memberships, as is having an enrollment fee required before joining the practice. For area characteristics, more individuals working in health, education, or social services is associated with higher prices, as is a larger portion of individuals above 65. After controlling for other characteristics, there is no relationship between rural status and price.

Table 2.4: Association between number of DPCs and county characteristics (population weighted)

DPC practices per 10000	*Coef.*	*St.Err.*	*t-value*	*p-value*	*[95% Conf*	*Interval]*	*Sig*
Supply factors							
Metro	-.0013	.0041	-0.30	.7618	-.0093	.0068	
Northeast	-.0102	.0047	-2.18	.0293	-.0194	-.001	**
West	.0216	.0062	3.50	.0005	.0095	.0337	***
South	.0058	.0047	1.23	.2183	-.0034	.015	
DPC law passed	.009	.0037	2.44	.0147	.0018	.0161	**
PCPs per 10000	.0005	.0006	0.90	.3686	-.0006	.0016	
Population	-.0008	.0003	-2.74	.0062	-.0013	-.0002	***
Median rent (00s)	-.0026	.0012	-2.13	.033	-.0051	-.0002	**
Home health agencies per 1000	-.024	.017	-1.41	.1583	-.0573	.0093	
Mental health centers per 1000	.037	.0884	0.42	.6759	-.1364	.2103	
Consumer Information							
% public transit to work	.0001	.0003	0.32	.7515	-.0004	.0006	
% with bachelors or more	.002	.0004	5.30	0	.0013	.0028	***
Demand-specific factors							
Population density	.0003	.0006	0.61	.5415	-.0007	.0014	
Poverty rate	-.0004	.0008	-0.56	.5788	-.0019	.0011	
% Medicaid	0	.0005	0.07	.9407	-.001	.0011	
% uninsured under 64	.0009	.0004	1.98	.0476	0	.0017	**
% household income over 100000	-.0002	.0007	-0.38	.7055	-.0015	.001	
% black	-.0003	.0001	-2.73	.0063	-.0006	-.0001	***
% above 65	-.0005	.001	-0.47	.6362	-.0024	.0015	
Median distance to ED (per 10 miles)	-.0062	.0031	-2.02	.0438	-.0123	-.0002	**
Median distance to FQHC (per 10 miles)	.0039	.0029	1.34	.1797	-.0018	.0096	
Mean dependent var		0.0273	SD dependent var			0.1057	
R-squared		0.1229	Number of obs			2742.0000	
F-test		6.4703	Prob > F			0.0000	
Akaike crit. (AIC)		-8424.7638	Bayesian crit. (BIC)			-8182.1896	

**** p<.01, ** p<.05, * p<.1. Population and population density variables are standardized. Counties with at least 4800 residents are included. Other variables controlled for are percentage employed in administrative work, percentage employed in government, percentage employed in education, health, or social services, percentage employed in manufacturing, percentage with no Internet access, percentage with kids, ambulatory surgery centers per 1,000, FQHCs per 1,000, average household size, labor force participation rate, advanced practice nurses per 1,000, dentists per 1,000, nurse anesthetists per 1,000, nurse practitioners per 1,000, mental health providers per 1,000, Gini equality index, unemployment rate, and median age.*

Table 2.4 displays results for the association between DPC practices per 10,000 and county characteristics. Being in the West is associated with more DPC practices and the Northeast is associated with less practices (with the Midwest as the baseline). Counties with higher populations are associated with less practices, as are counties with higher black percentage, higher median rent, and greater distance to the closest emergency department. Meanwhile, a higher percentage of individuals with at least a bachelor's degree is associated with more practices, as is being in a state that has passed a DPC law and a higher uninsured rate for non-Medicare eligibles.

2.5 Discussion

This study is the first national analysis of Direct Primary Care pricing segmented by age, geographic, and service factors, and the first to study factors related to DPC practice presence in a given region. Our research adds to a limited body of prior work studying pricing for DPC practices. We also provide the first analysis of where DPC practices choose to locate.

An often-cited benefit of the DPC practice model is access, defined broadly as inclusive of affordability, ability to quickly see a physician, and appointment time with physician (Restrepo, 2017; Landon, Weinreb, and Bitton, 2022). Our results indicate that DPC practices tend to locate in wealthier and more educated census tracts, and areas with more black patients also have less practices. DPC access disparities also are correlated to disparities in access to other types of care. However, areas with more uninsured patients have more DPC practices and areas with a larger portion of households earning over $100,000 or with higher rent have less DPC practices. Disparities in access may result from these practice location patterns, and more research on patient panels is needed to understand how DPC may impact access for different groups. There is an extensive literature documenting worse health outcomes among poorer patients, and understanding whether DPC is accessible those who stand to benefit most is paramount (Chetty et al., 2016). Legislation introduced in the current session of Congress would allow employers to offer patients tax-advantaged high-deductible plans without cost-sharing for DPC, which could also have implications for access (Cassidy, 2023).

Our price findings are consistent with previous studies on DPC pricing, suggesting that DPC fees have remained relatively consistent over the last five years as practice growth continues. After controlling for other factors, urban status is not associated with higher prices as compared to rural areas. However, pricing exhibits a wide distribution within urban and rural geographies. Future research could examine price differences by patient panel size or age distribution, as physician and practice patient panels may vary widely.

Practices working with employers charge lower direct-to consumer prices, while flat fees across age are associated with higher prices. Pricing practices suggest potential differences in pricing sophistication across practices. Practices working with employers may be more aware of the price sensitivity of demand. Charging a flat fee may also be indicative of insensitivity to demand, as clinician treatment costs and patient demand may vary across age. Additionally, only seeing adult patients is associated with higher prices. Not seeing children may be associated with different medical training that might have different outside earning options as compared to family medicine. Additionally, practices may charge more because they face a smaller potential patient panel.

Offering more physician services is only associated with higher prices if those services are explicitly financial in nature, such as radiology discounts and medication dispensing. While offering more services is not associated with higher prices, medication dispensing offers an alternative source of practice revenue, so a practice's ability to compete for patients via lower membership pricing is intuitive. Additionally, practices that explicitly offer financial services may be more sophisticated in their pricing. However, the lack of relationship between services offered and pricing is surprising. One reason for this may be that the service set offered by most practices is similar enough that the benefit of offering certain additional services is insufficient to justify higher pricing. We are interested in studying whether offering common high-cost services within DPC practices, such as forms of advanced imaging (i.e. MRI & CT scans), might shift consumer willingness to pay and thus practice pricing.

Further research should investigate the additional services DPC practices provide for wholesale, cash pay prices. Practices do not seem to provide a "standard" service menu for their periodic fee, so understanding what is included in the fee may provide further insight into how a practice chooses to price. Research is also needed to understand how DPC impacts the quality of care that patients receive.

In summary, we perform the first robust look at Direct Primary Care practices nationally and their pricing levels across ages. We find that median income and not accepting children as patients are associated with higher prices and medication dispensing is associated with lower prices. Meanwhile, a lower poverty rate, higher education status, and passing a state law related to DPC is associated with higher likelihood of physical location. This study may inform DPC practice owners and managers on ranges for market rate pricing by age cohort and geography and act as a foundation for further research related to disparities in access to DPC and other types of care.

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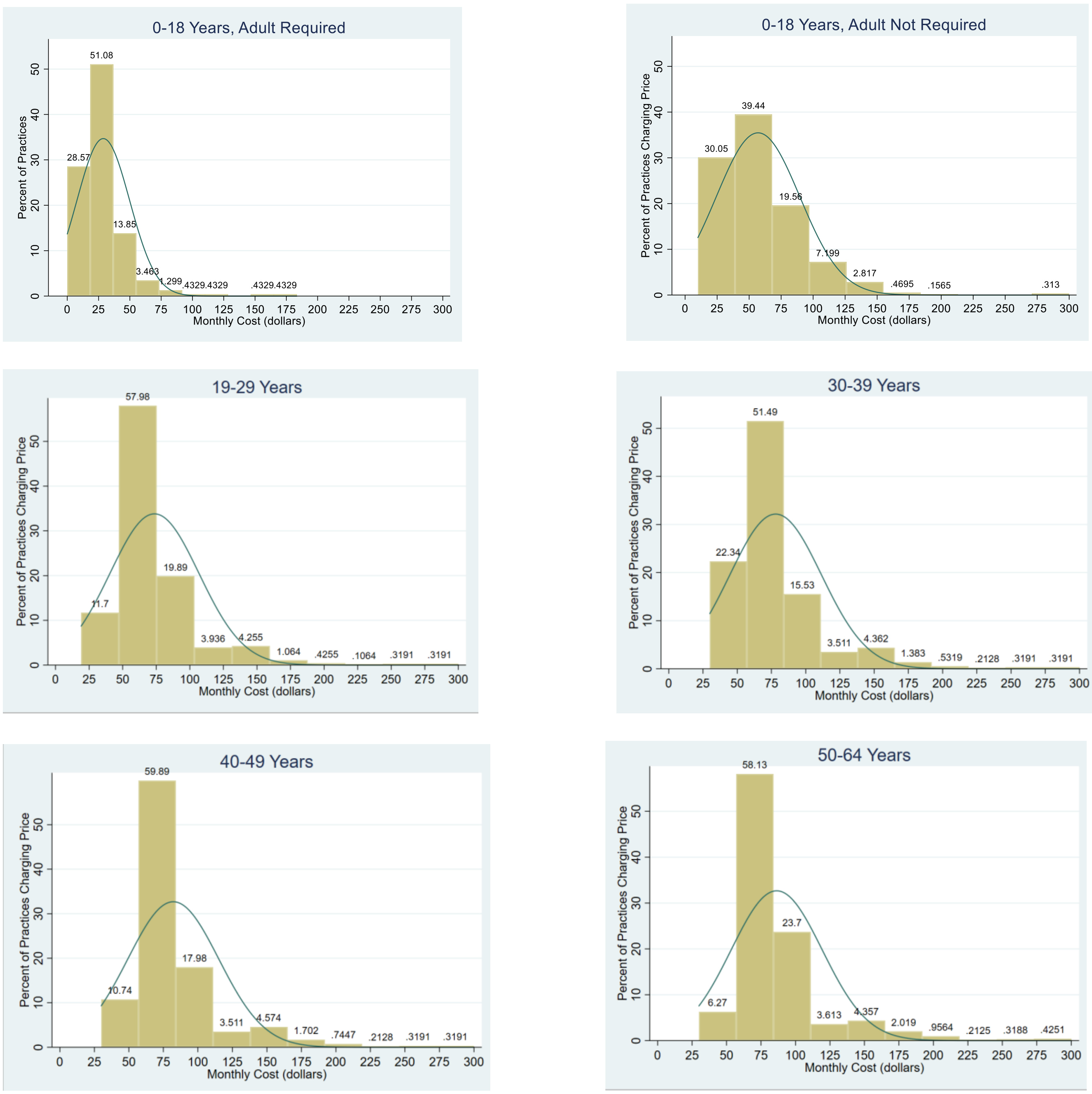
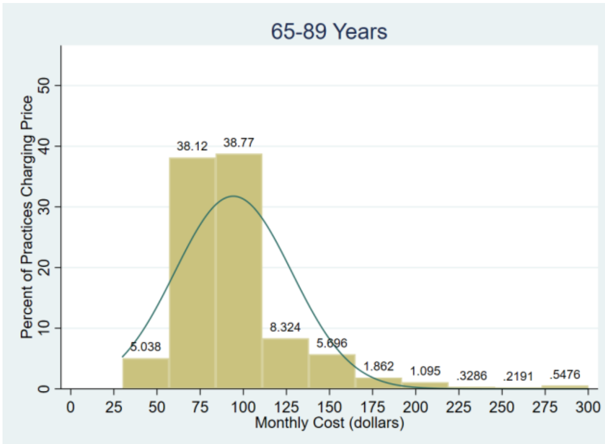
A Appendix

Figure 2.A.1: Monthly Price Distribution of DPC Practices by Percent Charging a Given Monthly Fee, by Age Bracket





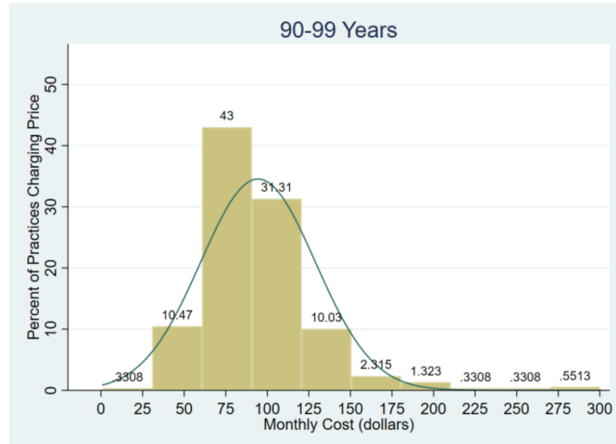


Figure 2.A.2: Geographic Dispersion of DPC Practices – Indianapolis & Cincinnati

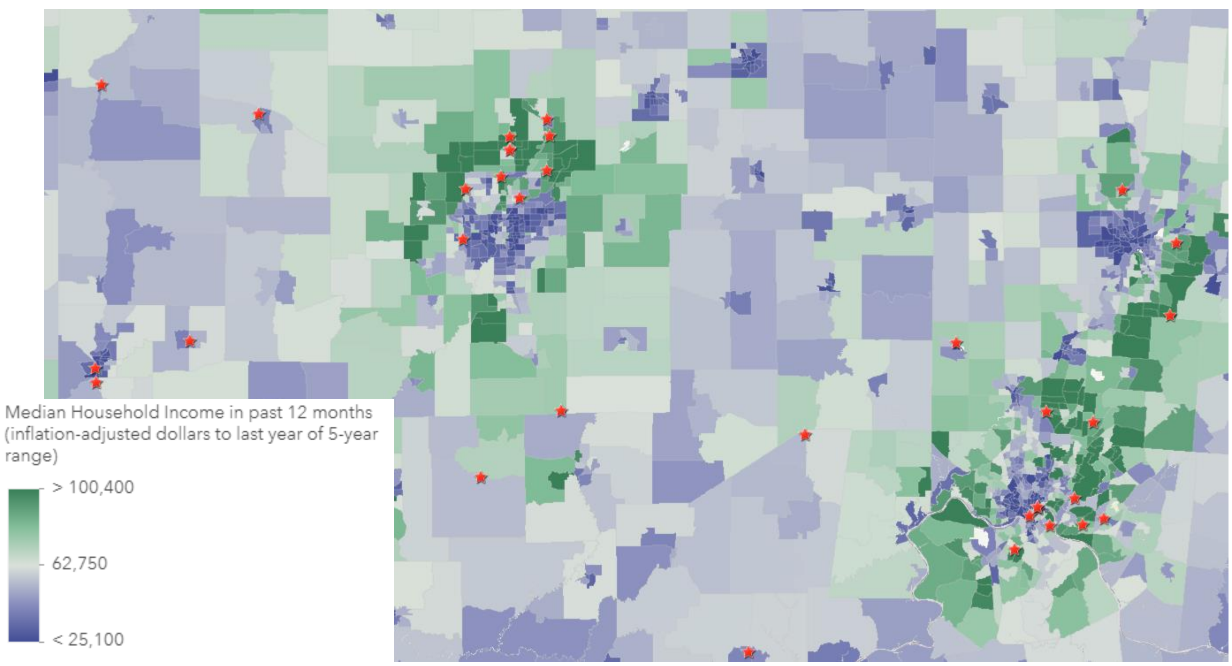


Figure 2.A.3: Geographic Dispersion of DPC Practices - Denver

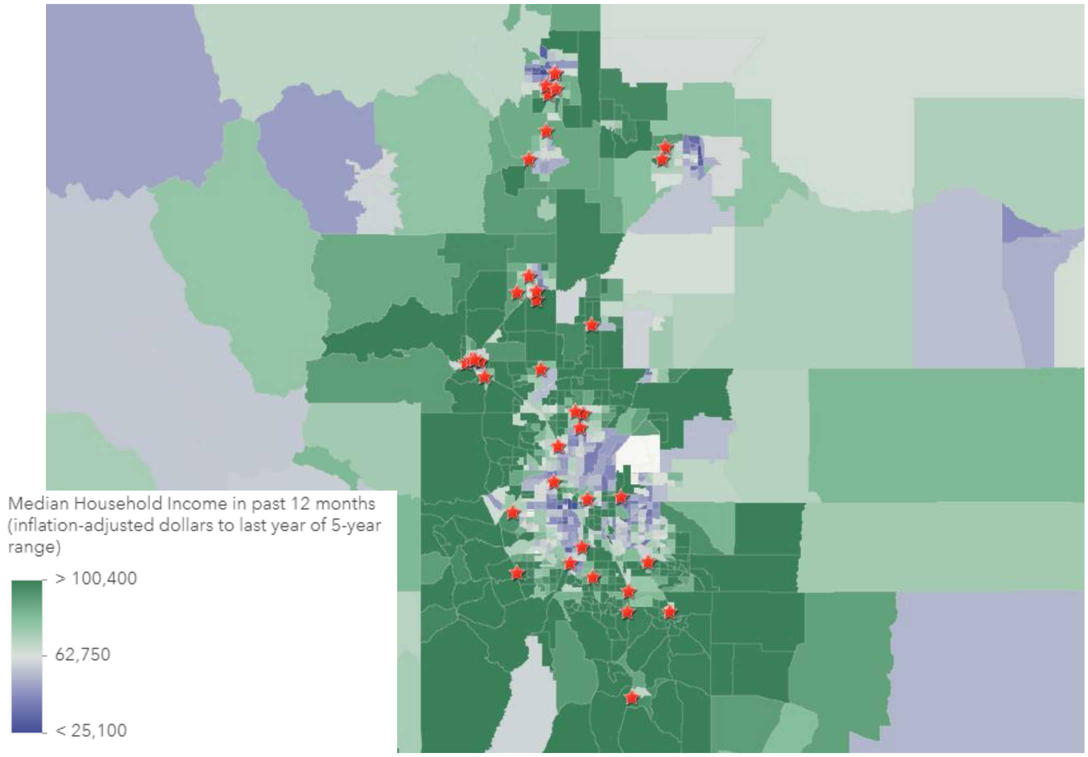


Figure 2.A.4: Geographic Dispersion of DPC Practices - Chicago

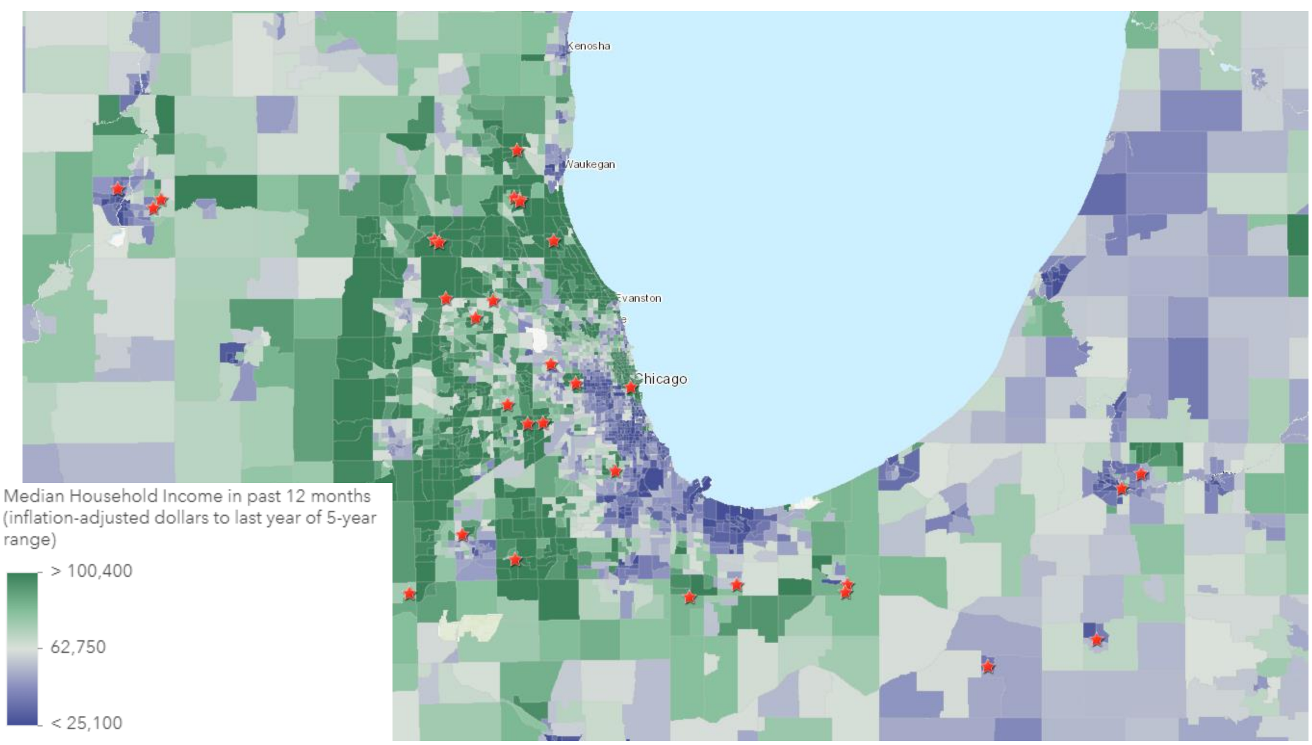


Figure 2.A.5: Geographic Dispersion of DPC Practices - Seattle

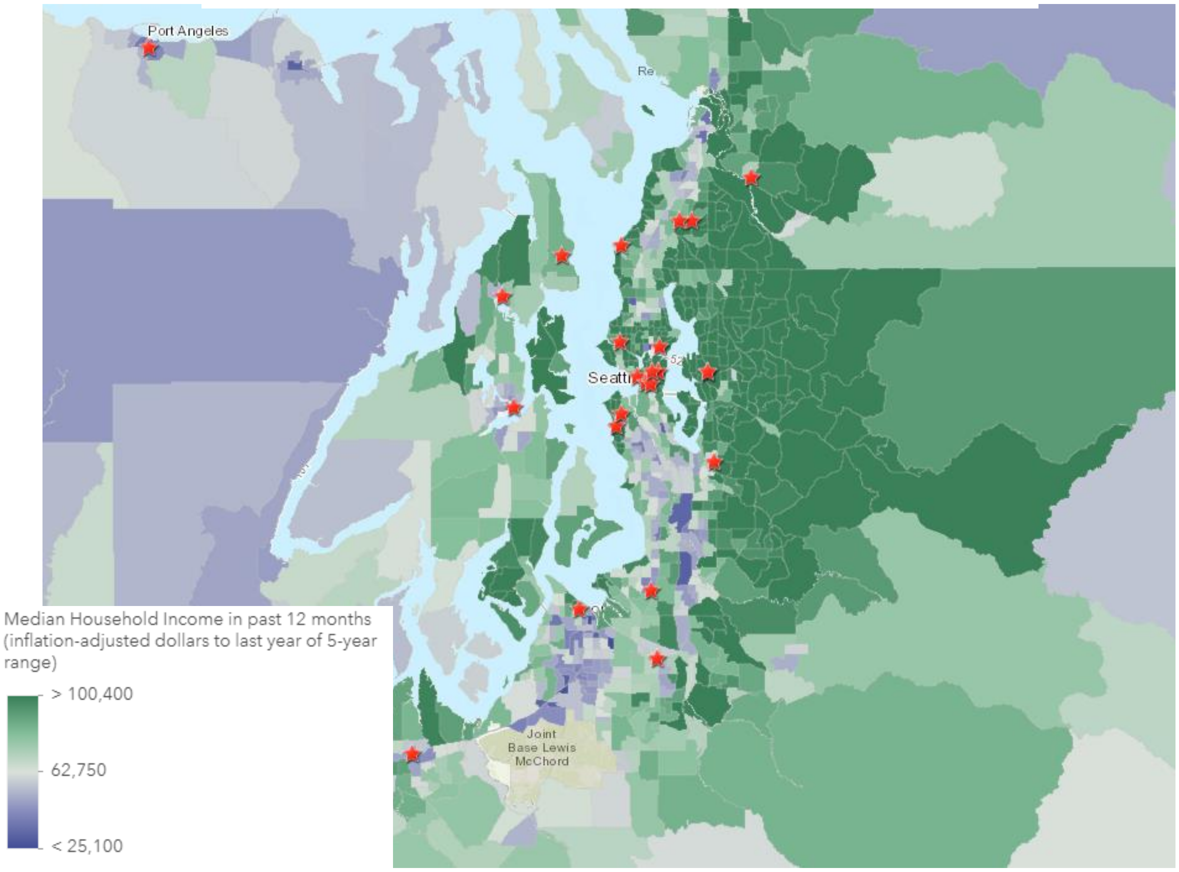


Figure 2.A.6: State Level Average DPC Monthly Prices as Compared to Median Nationally

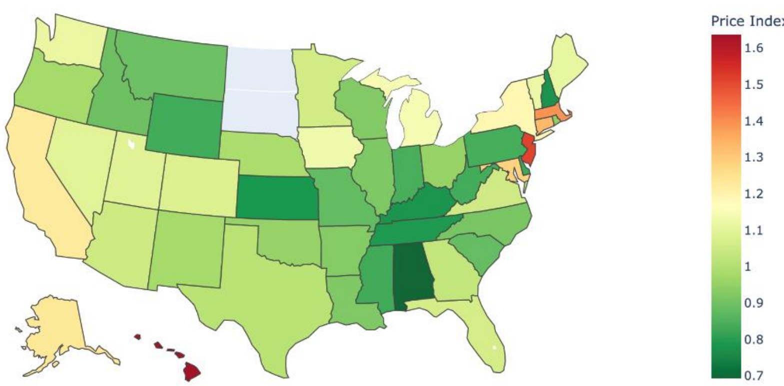


Table 2.A.1: Association between average price and area characteristics

Average adult price	Coef.	St.Err.	t-value	p-value	[95% Conf	Interval]	Sig
DPCs per 10000	-5.142	6.765	-0.76	.447	-18.418	8.135	
Median income (000s)	.145	.084	1.72	.086	-.021	.31	*
Population (000s)	-.001	.001	-0.50	.619	-.002	.001	
Rural zip code	-1.233	3.528	-0.35	.727	-8.157	5.691	
Located in Northeast	-1.132	4.664	-0.24	.808	-10.285	8.021	
Located in Midwest	2.855	4.287	0.67	.506	-5.558	11.268	
Located in South	1.166	3.434	0.34	.734	-5.573	7.905	
Employer price listed	-5.595	2.943	-1.90	.058	-11.371	.181	*
DPC chain	1.421	3.135	0.45	.651	-4.732	7.574	
DPC law	1.998	2.718	0.73	.463	-3.338	7.333	
Flat fee structure	1.526	2.396	0.64	.524	-3.176	6.228	
Enrollment fee noted	-6.154	2.014	-3.06	.002	-10.107	-2.201	***
No children in practice	19.502	2.718	7.18	0	14.168	24.836	***
Median gross rent	.01	.005	1.91	.057	0	.021	*
% public transit	.287	.194	1.48	.139	-.094	.669	
Uninsured %	.357	.306	1.16	.245	-.245	.958	
Proportion with income over $100,000	.026	.357	0.07	.942	-.674	.726	
% with children	.314	.308	1.02	.307	-.29	.919	
% bachelors or more	-.251	.287	-0.88	.382	-.815	.312	
Poverty rate	.08	.249	0.32	.747	-.409	.57	
% employed in administration	.11	.643	0.17	.864	-1.151	1.371	
% in labor force	-.336	.689	-0.49	.625	-1.688	1.015	
% employed in manufacturing	-.033	.238	-0.14	.889	-.501	.434	
% employed in government	-.5	.4	-1.25	.212	-1.286	.285	
% employed in education, health, and social services	.694	.289	2.40	.016	.127	1.26	**
Household size	-4.275	7.057	-0.61	.545	-18.126	9.576	
% black	.05	.122	0.41	.682	-.189	.289	
% Medicaid	-.027	.256	-0.10	.917	-.53	.476	
% above 65	.566	.259	2.18	.029	.057	1.076	**
% No internet access	-.381	.318	-1.20	.231	-1.005	.243	
Gini inequality index	.694	.307	2.26	.024	.092	1.297	**
% adults getting a checkup in the last 12 months	-.908	.452	-2.01	.045	-1.794	-.022	**
PCPs per 10000	.533	.486	1.10	.273	-.421	1.488	

Mean dependent var	86.138	SD dependent var	32.018
R-squared	0.203	Number of obs	935.000
F-test	6.737	Prob > F	0.000
Akaike crit. (AIC)	8992.409	Bayesian crit. (BIC)	9161.828

**** p<.01, ** p<.05, * p<.1*

Chapter 3: Value-Based Payment Models and Healthcare Investments: Evidence from Global Budgets*

3.1 Introduction

How does compensation structure impact decision-making and investment? Various payment structures have emerged across industries, including hourly rates, membership fees, pay for performance, and combinations thereof. With different payment models come distinct incentives that shape strategic investments and types of services provided (Calvano and Polo, 2020). Despite their significance, little is known about how compensation models impact investment behavior, as compensation models are endogenous strategic decisions that depend on numerous factors.

Hospitals provide an ideal environment to study compensation structure, as payment models can arise both endogenously and from government payment policies. Changes in reimbursement shift the marginal revenue of hospital investments and may change which types of investments yield the most financial benefit. Hospitals are also empirically important, accounting for 5.6% of gross domestic product and 31% of total health care spending (Centers for Medicare and Medicaid Services, 2021). Hospital decisions are crucial for mortality and quality of life outcomes, making it essential to understand how payment models affect their incentives. Most hospitals are non-profits, where organizational considerations may impact the reallocation of resources in response to changes in payment structure. Furthermore, optimal regulation and design of payment models are considered primary policy levers in the face of rising medical spending. Mechanisms by which payment

*I am grateful to Barton Hamilton, Stephen Ryan, Brent Hickman, Mark Fendrick, Timothy McBride, Cecilia Diaz-Campo, and John Dooley for their helpful comments.

models can improve outcomes are of particular interest, as a better understanding of the relevant mechanisms could inform the design of optimal payment models in the future.

I study the impact of payment incentives in the context of the Maryland global budget program, where all-payer hospital revenue targets are set by regulators. Hospitals have the flexibility to adjust hospital volume and prices to meet revenue targets, but face penalties for underperforming on quality measures or missing revenue targets. Global budgets change overall financial incentives with the aim of changing hospital decisions to produce improved outcomes without explicitly incentivizing specific decisions. Previous evaluations have shown promise in decreasing costs without declining quality (Haber et al., 2019; Shammas et al., 2022). However, the mechanisms by which these improved outcomes were achieved has not been studied. I examine how health system decisions change after payment incentives are modified by the Maryland global hospital budget program. Specifically, I investigate how the global budget program impacts hospital volume, hiring, quality, investment, and care provision decisions.

Health systems could respond to global budgets in several ways. They could forego volume-generating or higher-cost investments because of a lack of incentive. Additionally, since global budgets create an increased incentive to reduce costs, hospitals could respond by reducing staffing levels or substituting between high-skilled and lower-skilled workers. Hospitals could make productivity-increasing investments to treat patients in less resource-intensive ways, or shift to treating healthier, less cost-intensive patients (Aliu et al., 2021). Finally, there could be concerns that the new constraint could reduce investment or harm quality.

To measure hospital responses to the global budget program, I use detailed financial filing data and information about hospital staffing, investment, and service line offerings from Hospital Cost Report filings and the American Hospital Association Hospital Survey to study hospital decisions over time. My data spans from 2011 to 2018, including three years before the implementation of the global budget model and five years after. Since the Maryland Hospital global budget program was adopted in response to health care market conditions specific to Maryland, there are concerns about selection bias. I address these concerns in several ways. First, I use a covariate balanced propensity-score weighted difference-in-difference estimator to increase the balance between Maryland hospitals and control hospitals, controlling for state-specific trends. Second, I produce estimates that relax the parallel trends assumption, examining the sensitivity of the results to pre-trends. I use the method of Rachmadan and Roth (2022) to make assumptions about trends in the post-treatment period based on the trends in the pre-treatment period.

I find that hospital volume decreases and staffing declines for all types of employees. On a per-unit basis, staffing shifts to lower-skilled, cheaper workers, with decreased physicians per adjusted admission and an increase in non-clinical staffing per adjusted admission after several years. Financial outcomes are largely unchanged after global budgets are introduced, however, the level of fixed assets and the share of administrative costs increases. Quality effects are mixed but concerning, with a decrease in 30-day risk-adjusted heart failure readmissions and an increase in 30-day risk-adjusted pneumonia mortality. I find no evidence that global budgets impact hospital technology adoption or the profitability of service lines offered.

My findings on reduced staffing are similar to findings by Andreyeva et al. (2022) and Duggan et al. (2023) that hospital acquisitions lead to decreases in labor inputs and by Bruch et al. (2023) on substitutions to lower-skilled staffing, indicating that shocks to hospital operations may incentivize hospitals to reduce staffing. Richards and Whaley (2023) find that private equity acquisition increases volume and shifts patient surgical composition. In some contexts, substitution from higher-skilled to lower-skilled clinician staffing can have adverse health outcomes (Chan and Chen, 2022; Okeke, 2023), but this could vary based on the clinical context (Carillo and Feres; 2019). However, the substitution I observe is between physician and non-clinical staffing, which may involve a substitution of functions, suggesting that global budgets may shift the type of care hospitals deliver. Other research has shown that care coordination and patient education, which could be job functions of some non-clinical employees, can improve healthcare outcomes (Nyqvist, Guariso, Svensson, and Yanagizawa-Drott, 2019; Evans et al., 2021). In contrast to previous literature, my work emphasizes the intersection between payment structure and operating structure by studying how incentive structures motivate different healthcare investments. My findings add evidence to the central question about what kinds of payment structures can improve the trade-off between cost and quality.

Unlike earlier work, the shock I study is a shift in the payment model rather than a shock to financial status. Aghamolla, Karaca-Mandic, Li, and Thakor (2022) find that hospitals increase volume while quality decreases when facing credit shocks. Dranove, Garthwaite, and Ody (2017) find that hospitals facing negative financial shocks do not cut back on staffing but do provide lower levels of unprofitable services and capital expenditures on medical records, in contrast to my findings about staffing reallocation from physicians to non-clinicians. Payment changes could provoke different responses than financial shocks because they represent long-term changes in marginal revenue from hospital decisions rather than short-term changes in response to shocks, which may change the benefit of reallocating resources. Whaley and Richards (2023) find that changes in hospital behavior after acquisition persist after private equity divestiture in another context where hospitals re-optimize with longer-term goals in mind. Quality effects may also differ based on the differential responses. However, I do find potentially concerning mortality effects, similar to the findings from short-term negative financial or credit shocks.

The remainder of the paper proceeds as follows. Section 3.2 provides additional background about hospital spending, payment models, and global budgets and a theoretical discussion about the effects of global budgets. I describe the study data and the empirical methodology in Section 3.3. Section 3.4 presents the estimated effects of global budgets. Section 3.5 concludes.

3.2 Background on Maryland and Hospital Global Budgets

3.2.1 Hospital Spending and Investments

The hospital industry is economically significant in the United States, accounting for $1.3 trillion in spending in 2021, representing 5.6% of gross domestic product (GDP) and 31% of total health care spending (Centers for Medicare and Medicaid Services, 2021). Hospital spending increases have exceeded inflation and risen as a share of GDP over the last two decades. In light of this spending, a variety of reimbursement models have attempted to shift payment incentives from a fee for service to a "value-based" basis. A major shift in hospital reimbursement occurred with the adoption of the Medicare Prospective Payment System (PPS) in 1984, which shifted hospital Medicare reimbursement from being based on patient treatment expenditures to being fixed by patient diagnosis-related group (DRG) regardless of treatment cost or patient length of stay. The change in payment led both to effects on care delivery and outcomes (Cutler, 1995) and on coding intensity (Dafny, 2005). However, spending increases have continued to outpace inflation.

A more recent model designed to incentive quality investments by hospitals is the Hospital Readmissions Reduction Program (HRRP) included in the Affordable Care Act. The HRRP penalizes hospitals for readmission rates exceeding risk-adjusted national averages (Gupta, 2021). Hospitals targeted by the HRRP improve care, leading to better patient outcomes (Gupta, 2021), with one potential mechanism being improved care coordination across organizations (Gupta, David, and Kim, 2022). Beyond these national policies, there have been several attempts to change payment structure in the hospital setting. The Center for Medicare and Medicaid Innovation (CMMI) has implemented a variety of payment models in recent years to change hospital payment incentives in varying treatment settings, including the Bundled Payment for Care Improvement Advanced model for lower extremity joint replacement and the Kidney Care Choices model for chronic kidney disease. Many of these models have focused on specific clinical contexts rather than over-arching changes to incentives on the hospital level. While other government reimbursement models, such as accountable care organizations, change hospital incentives more broadly to tie reimbursement to healthcare outcomes and spending, there is voluntary selection into these models, making causal estimates of the impact of payment incentives challenging (Gupta, Navathe, and Martinez, 2022; Einav, Finkelstein, and Mahoney, 2022).

3.2.2 Maryland Global Budget Model

To abstract away from the issue of selection into payment models, I study the Maryland global hospital model, where hospitals change payment models as a result of a state-level waiver from the Center for Medicare and Medicaid Innovation. Unlike other states, Maryland has set rates for hospital inpatient and on-site outpatient care for all payers since 1971 with a waiver from the Centers for Medicare and Medicaid Services (CMS). The purpose of the waiver was to limit per admission spending. Under the rate-setting system, Maryland has had above-average Medicare prices and below-average commercial prices. While these rates capped price increases for hospital services, they continued to incentivize growth in inpatient utilization, which threatened Maryland's fulfillment of the waiver conditions that allowed it to set all-payer rates. Volume growth made concerns about fee-for-service reimbursement described in the previous section more acute in Maryland's payer rate setting. (Patterson, 2019)

In response, a new payment model demonstration with CMS began in 2014, an all-payer global hospital budget model. In this model, hospitals are given a revenue constraint that caps per capita total hospital cost growth at 3.58%. If hospitals fail to meet the target, they are penalized with a lowered future revenue constraint, with the pass-through depending on how far they are from the target. Hospitals still charge patients on a fee-for service basis; however, prices are adjusted upwards and downwards as needed throughout the year to ensure that the hospital is on pace to meet the revenue target. Importantly, this necessitates that hospitals retain their previous payment infrastructure while adding additional administrative requirements that may require additional infrastructure. There were two hospital budget models implemented in Maryland: the Total Patient Revenue (TPR) payment model for rural hospitals, implemented in 2010, and the Global Budget Revenue (GBR) model for the remaining urban hospitals, which is the focus of this paper. All 36 hospitals eligible for the GBR program negotiated global budgets by July 2014, and budgets were retroactive to January 2014.

Global budget volumes are anchored to a 2013 baseline. While hospital revenue is based on the prospective budget, payers continue to face fee-for-service rates when patients utilize care. Hospitals can adjust rates within 5% of the set rate to meet budget targets (10% with regulatory approval). Physician revenue is excluded from global hospital budgets, so physician incentives are not shifted as directly as hospital incentives, especially for clinicians practicing outside of the hospital setting. Hospitals can be penalized for missing revenue targets regardless of whether revenue exceeds the target or does not meet target revenue. To encourage hospitals to maintain care quality in the face of global budgets, hospitals are also compensated based on quality-based reimbursement and potentially avoidable utilization. Below is the generalized target formula from Haber et al. (2016):

$$Rev_{ht} = Rev_{h,2013}[(1 + Infl_t + Adj_t)(1 + V_{olht})] \tag{1}$$

where hospital h revenue at time t is based on 2013 revenue adjusted for inflation $Infl_t$, a year-specific adjustment Adj_t, and allowed growth in hospital volume V_{olht} (changes to market share, service area demographic shifts, and rates of potentially avoidable utilization).

Several exceptions were made to partially exclude revenue from global budgets for some hospitals. The University of Maryland Medical Center negotiated non-resident revenue exclusions from global hospital budgets until June 2015, and three Johns Hopkins Health System hospitals (the Johns Hopkins Hospital, Suburban Hospital, and Johns Hopkins Bayview Medical Center) had non-resident revenue exclusions through June 2017. Furthermore, burn case revenues were excluded from Johns Hopkins Hospitals and Johns Hopkins Bayview Medical Center (Health Services Cost Review Commission, 2014a), and the University of Maryland Charles Medical Center negotiated global budget exclusion for Medicare Part B (Health Services Cost Review Commission, 2014b), The University of Maryland Medical Center and the Johns Hopkins Hospital were considered statewide resources for tertiary and quaternary care for several specialized categories of care that were excluded from global budgets, such as inpatient hematologic malignancies, inpatient solid organ transplants, inpatient transfers from acute care hospitals, and inpatient and outpatient blood marrow transplants. (Health Services Cost Review Commission, 2014a; Health Services Cost Review Commission, 2014b; Patterson, 2019).[1]

Global budgets can be appealing from the perspective of limiting cost growth - since spending levels are set by regulators, the regulators can set the rate of cost growth. Concerns come from how

[1] Since incentives are shifted towards global budgets for all hospitals, I do not distinguish between hospitals with partial exemptions from global budgets and hospitals without exemptions. I plan to explore how exemptions may impact investment with a synthetic control approach in future work.

hospitals will respond to these limits and whether these decisions may harm quality or innovation. In Maryland, regulators include measures to penalize hospitals for poor quality outcomes in specific disease categories, however, global budgets may still change incentives for quality-enhancing hospital investments.

3.2.3 Theory

Rather than targeting a specific procedure type or care setting, global budgets change financial incentives at the hospital level by imposing a new constraint. Below, I discuss how hospital incentives may change on several testable dimensions.

Volume – A global budget may create more explicit incentives to reduce treatment costs than a fee-for-service model. A global budget model reduces the marginal revenue of an additional service as compared to fee-for-service, therefore hospitals may respond by decreasing volume. Reductions in volume could vary among specific dimensions, such as the expected cost of care or the value of care, as different types of volume may have different implications for expected cost. However, prior research does not detect reductions in low-value care (Oakes, Sen, and Segal, 2020).

Hospital staffing – Moving to a global budget model introduces a revenue budget constraint, which may change hospital staffing incentives. Health systems could respond by reallocating clinical staff and changing staffing levels, as different hires have different levels of clinical expertise and expense. Hospital decisions could change both the absolute level of hospital staffing and the allocation of labor, modifying the mix of physicians and nurses and the direct clinical to non-clinical staffing ratio. With a global budget, there could be an increased incentive to treat patients in a less resource-intensive manner, which may incentivize substitution from more expensive and highly trained physicians to less expensive nurses. In Maryland, hospital reimbursement changes while physician compensation does not, introducing principal-agent problems that may impact hospital staffing decisions. Additionally, a changing reimbursement model could increase the demand for non-clinical employees with expertise in implementing cost-saving practices or meeting administrative compliance needs.

Changes in Financial Outcomes - Hospitals could be more willing to shift procedures to sites not subject to global hospital budgets, such as ambulatory surgery centers, to reduce hospital resource utilization. Hospitals may strategically select different types of patients or payers to reduce treatment costs. Furthermore, when payment changes are regulatory, mandating a new program with additional requirements without any offsetting simplification of existing requirements may result in additional administrative burden.

Care offerings and technology adoption – Hospitals offer both services that are generally profitable, such as cardiac care and oncology, alongside services typically unprofitable, such as psychiatric emergency room care and trauma centers. Hospitals may offer unprofitable service lines for reasons of public benefit, as not for-profit hospitals are more likely to provide unprofitable service lines than for-profit hospitals (Horwitz and Nichols, 2022). Shifting to a global budget model may change the expected profit of traditionally profitable or unprofitable service lines.

One concern about attempts to limit medical cost growth is the effects on quality and medical innovation. With a global budget, there may be increased incentive for cost-reducing innovations but a decreased incentive for adopting other types of innovation, as technology has been a leading contributor to increased health care spending (Fuchs, 1996; Cutler, 2007). Hospitals could make investments in increasing productivity in order to treat patients in less resource-intensive ways. Additionally, hospitals could forego revenue-generating investments because of the revenue budget constraint, and some of these investments could enhance quality. Health systems may also increase investments in preventing unnecessary procedures and low-value care because of increased financial incentives to avoid care with low clinical value; however, existing research does not detect such effects (Oakes, Sen, and Segal, 2020).

There may be fixed costs to reallocating resources that blunt responses to exogenous payment shocks. The non-profit status of hospitals may have additional constraints to reallocation, although previous work shows that non-profit hospitals change capital investment in response to financial shocks in ways similar to for-profit firms in other sectors (Adelino, Lewellen, and Sundaram, 2015). Furthermore, responses may differ between short-term shocks and long-term shocks, as the decision to not reallocate in response to a policy shock that imposes a long-term revenue constraint may be more costly than not reallocating after a temporary shock.

3.3 Study Data and Methods

I use data on acute care hospitals from the Healthcare Cost Report Information System (HCRIS), using a version compiled by the RAND Corporation (Santa Monica, CA). This data is submitted to the Centers for Medicare and Medicaid Services. Hospitals that accept federal funding from CMS, including any that see Medicare patients, must submit an annual report detailing financial information and basic hospital characteristics, such as the number of beds and the number of intensive care units. Additionally, I use survey data from the American Hospital Association (AHA) Hospital Survey from 2011 to 2018 for data on hospital hiring, investment, and service line offerings. These data sources have been used previously to study hospital financial outcomes and strategic investments. For outcomes derived from HCRIS, I winsorize data at the 5% and 95% levels to reduce measurement error, following other studies that use this source (Coomer, Ingber, Coots, and Morley, 2017; Dranove, Garthwaite, and Ody, 2017; Gaynor et al., 2021; Aghamolla, Karaca-Mandic, Li, and Thakor, 2022).

I use patient satisfaction data from the Hospital Consumer Assessment of Healthcare Providers (HCAHPS), a survey administered to a random sample of adult patients asking about patient hospital experiences. The survey asks for overall hospital ratings, nurse and physician communication, cleanliness, and discharge information, among other patient experience questions. Additionally, I obtain risk-adjusted readmissions and mortality data for health failure, pneumonia, and acute myocardial infarction (AMI) from CMS Hospital Compare to examine the impact on quality from 2011 to 2016. For county-level demographic information about the communities where hospitals are located, I use the Area Health Resources Files (AHRF) from the Health Resources and Services Administration.

Since the Maryland Global Hospital program was expanded to urban areas in 2014 and rural hospital programs began before my sample, I restrict to urban hospitals. All but one hospital in

Maryland is non-profit; therefore, I restrict my sample to non-profit hospitals. My sample includes 911 hospitals from 23 states, with 36 treated hospitals in Maryland and 875 control hospitals in other states. I classify health system employees into three types using AHA Hospital Survey Data: physicians, nurses, and other employees. I assume that other employees are non-clinical; however, a limitation of my data is that I cannot decompose this category further. Some employees in the other category may be less-trained clinical staff, and I cannot decompose between employees in business functions and employees that represent a direct investment in clinical care, such as case managers.

A potential threat to my analysis is that the global budget program was implemented at the same time as Medicaid Expansion took effect. To mitigate these concerns, I only include states that expanded Medicaid at the same time as Maryland in 2014. Additionally, I remove hospitals in states participating in the State Innovations Model (SIM) program, a concurrent state-level CMMI model. Hospitals receiving other treatments cannot serve as adequate controls for the counterfactual state of Maryland without the Global Hospital Budget model.

Pre-treatment summary statistics for included hospitals are presented in Table 3.1. Before the introduction of global budgets, Maryland had a similar number of hospital beds per hospital, but a notably larger amount of full-time physicians, a higher occupancy rate, and a lower operating margin. It is unknown whether increased staffing reflects hospital inefficiency or higher levels of patient care. Maryland also has a lower level of hospitals that are part of a major hospital system while having more large hospitals (200 or more beds) as compared to control states, in addition to higher levels of total fixed assets and median household income. Maryland also has a higher percentage of black patients.

Table 3.1: Pre-Global Budget Summary Statistics

Variable	**N**	**All Hospitals**	**Never Global Budgets**, N = 2,415	**Future Global Budgets**, N = 99
Population density	2,514	2,945.86 (8,625.25)	2,930.52 (8,778.21)	3,319.98 (3,071.16)
Black percentage	2,514	11.53 (11.65)	10.40 (9.36)	38.97 (23.07)
Total beds	2,500	281.96 (210.52)	281.64 (211.50)	289.73 (185.99)
Population estimate	2,514	1,412,983 (2,267,229)	1,445,525 (2,306,691)	619,178 (289,968)
Poverty rate	2,514	15.06 (4.99)	15.13 (4.85)	13.56 (7.47)
Unemployment rate	2,514	8.54 (2.29)	8.57 (2.29)	7.70 (2.06)
Median HH income	2,514	57,296 (13,973)	56,947 (13,444)	65,803 (21,886)
Major teaching hospital	2,514	396 (16%)	380 (16%)	16 (16%)
Major hospital system	2,514	604 (24%)	598 (25%)	6 (6.1%)
Operating margin	2,427	0.03 (0.11)	0.03 (0.11)	0.02 (0.04)
Physicians	2,500	53.31 (156.66)	52.34 (155.78)	76.73 (176.00)
Nurses	2,500	544.62 (531.70)	542.16 (532.83)	604.19 (502.30)
Other FTE	2,500	1,242.82 (1,403.39)	1,232.37 (1,406.90)	1,496.28 (1,296.33)
Admin cost share	2,512	0.19 (0.06)	0.19 (0.06)	0.17 (0.03)
Occupancy	2,509	0.61 (0.15)	0.61 (0.15)	0.73 (0.07)
Fixed assets (millions)	2,514	146.921 (154.874)	146.160 (154.782)	165.161 (157.106)
Patient revenue (millions)	2,452	321.321 (356.604)	320.147 (357.378)	349.063 (338.333)
Large hospital	2,514	1,317 (52%)	1,251 (52%)	66 (67%)

Notes: Total fixed assets, administrative cost share, net patient revenue, occupancy, and commercial revenue outcomes are winsorized at the 5% and 95% levels. A large hospital is defined as a hospital with at least 200 beds.

I construct measures of technology adoption as in Garthwaite, Starc, and Ody (2022). From the AHA Hospital Survey, I calculate hospital adoption of birth, cardiac, diagnostic imaging, radiation therapy, and transplantation technologies by averaging the adoption of specific technologies in each category. I calculate overall technological adoption by averaging across these categories. To construct measures of profitable and unprofitable service line offerings, I follow Cercullo et al. (2021) measures of profitable and unprofitable service lines. I calculate offerings on the hospital level by averaging offerings within the profitable and unprofitable categories.

3.3.1 Empirical Strategy

To recover the causal effect of the Maryland Global Budget model on health system investments and hiring, I use a difference-in-difference strategy to compare hospitals subject to global budgets to similar hospitals that are not subject to global budgets. I restrict the sample to hospitals in states that did not expand Medicaid in 2014 and were not treated with the SIM model, mitigating concerns about unobserved differences between hospitals and the conflation of treatment effects.

I employ the following event study specification:

$$Y_{ist} = \alpha_i + \lambda_t + \sum_{t=2011}^{2018} \beta_t \mathbb{1}\{E_i = D\} + X_{it} + \tau_{st} + \varepsilon_{ist} \tag{2}$$

where Y_{ist} is the dependent outcome variable for hospital i in state s in year t, $\{E_i = D\}$ is an indicator for treatment status in a given year, X_{it} represents hospital level controls, α_i represents hospital fixed effects, λ_t represents year fixed effects, and τ_{st} represents state-specific time trends, while ε_{ist} represents the normally distributed error term. I cluster standard errors on the state-year level, since the state is the unit of treatment. I use a dynamic difference-in-difference linear probability model for binary outcomes, including service line offerings. I utilize a dynamic difference model because hospital responses to Global Hospital Budgets may change over time. Some investments may represent up-front costs of the payment model change and others may represent ongoing costs. Additionally, hospitals may learn from their responses and adjust investment.

Even with the inclusion of hospital fixed effects, which necessitate that estimates are driven by within-hospital variation, there may be concerns regarding endogenous treatment. Since the Maryland global budget model was adopted in response to market conditions specific to Maryland, there may be concern about differences in trends between Maryland and other states. For this reason, I include state-specific pre-trends in the difference-in-difference model. To further mitigate selection issues, I use an inverse propensity score weighting approach to model selection in the Global Hospital Budget program. I utilize a covariate balancing propensity score (CBPS) for the likelihood of treatment to weigh the observations so that my model is more robust to misspecified propensity scores. CBPS uses a generalized method of moments (GMM) framework, which combines score conditions and covariate balance moment conditions to balance the conditional probability of treatment assignment with balanced covariates between treatment groups. (Imai and Ratkovic, 2014). To generate propensity scores, I use county characteristics for population density, poverty rate, unemployment, dual-eligible percentage, uninsured percentage below the Medicaid expansion poverty rate of 138%, hospital Herfindahl–Hirschman Index (HHI), major teaching hospital status, and large hospital status.[2]

The underlying assumption for the strategy is that changes in outcomes would have evolved similarly between hospitals in Maryland and hospitals in control states in the absence of the Global Hospital Budget program, conditional on covariates and year and hospital fixed effects. This assumption is inherently untestable, especially given Medicaid expansion; however, I provide support for this assumption in several ways. First, I incorporate the uninsured percentage of the patient population under 138% of the Federal Poverty Line as a covariate to weigh observations in order to make treatment and control groups more equivalent, mitigating concerns about unequal effects of Medicaid expansion between the treatment and the control groups. Second, I include state-specific time trends in the model. Additionally, to adjust for the potential presence of pre-trends, I use tools from Rambachan and Roth (2022) to derive valid inferences in a setting where parallel pre-trend assumptions may be violated, making assumptions about post-treatment trends based on the pre-treatment trends.

I use the following equation to estimate pre-trends:

$$\delta_{-1} = \mathbb{E}(\hat{\beta_{-1}}) = \mathbb{E}[Y_{i,0}(0) - Y_{i,1}(0)|D=1] - \mathbb{E}[Y_{i,0}(0) - Y_{i,1}(0)|D=0] \tag{3}$$

Here, δ_{-1} represents the pre-trend in the final pre-treatment period, which is informative about trends in the post-treatment period. For inference in the presence of pre-trends, I make an assumption

[2] Results are similar when observations are unweighted, with the exception of volume outcomes, which are presented in the Appendix.

about the relationship between the maximum of $\delta_{-1}, \delta_{-2}, \delta_{-3}$ and δ_1 of the form

$$|\delta_1| \leq \bar{M} max_{r<0} |\delta_r|.$$

Here, $\bar{M}$ scales the relationship between pre-trends and post-trends based on assumptions and δ_r represents trends in pre-treatment periods. Using this pre-trend approach allows me to relax to parallel pre-trends assumption while still obtaining credible causal estimates under weaker assumptions. To estimate the sensitivity of the results to post-trends, I weigh each post-treatment period equally to create a single estimate of the average post-treatment effect and plot how differing values of $\bar{M}$ impact my findings.

3.4 Results

3.4.1 Effect on Volume

In Figure 3.1, I plot raw trends in hospital volume. Plot (a) shows there are fewer beds in Maryland hospitals than in control hospitals, and there is a decline in Maryland hospital beds after global budgets are implemented while the number of beds in control states increases. Plot (b) shows the volume of adjusted admissions is higher in Maryland relative to control states before the implementation of global budgets. The volume of adjusted admissions decreases for Maryland hospitals after global budgets, while the volume increases for control state hospitals.[3]

[3] Adjusted admissions are calculated by scaling the number of beds by the outpatient to inpatient revenue ratio.

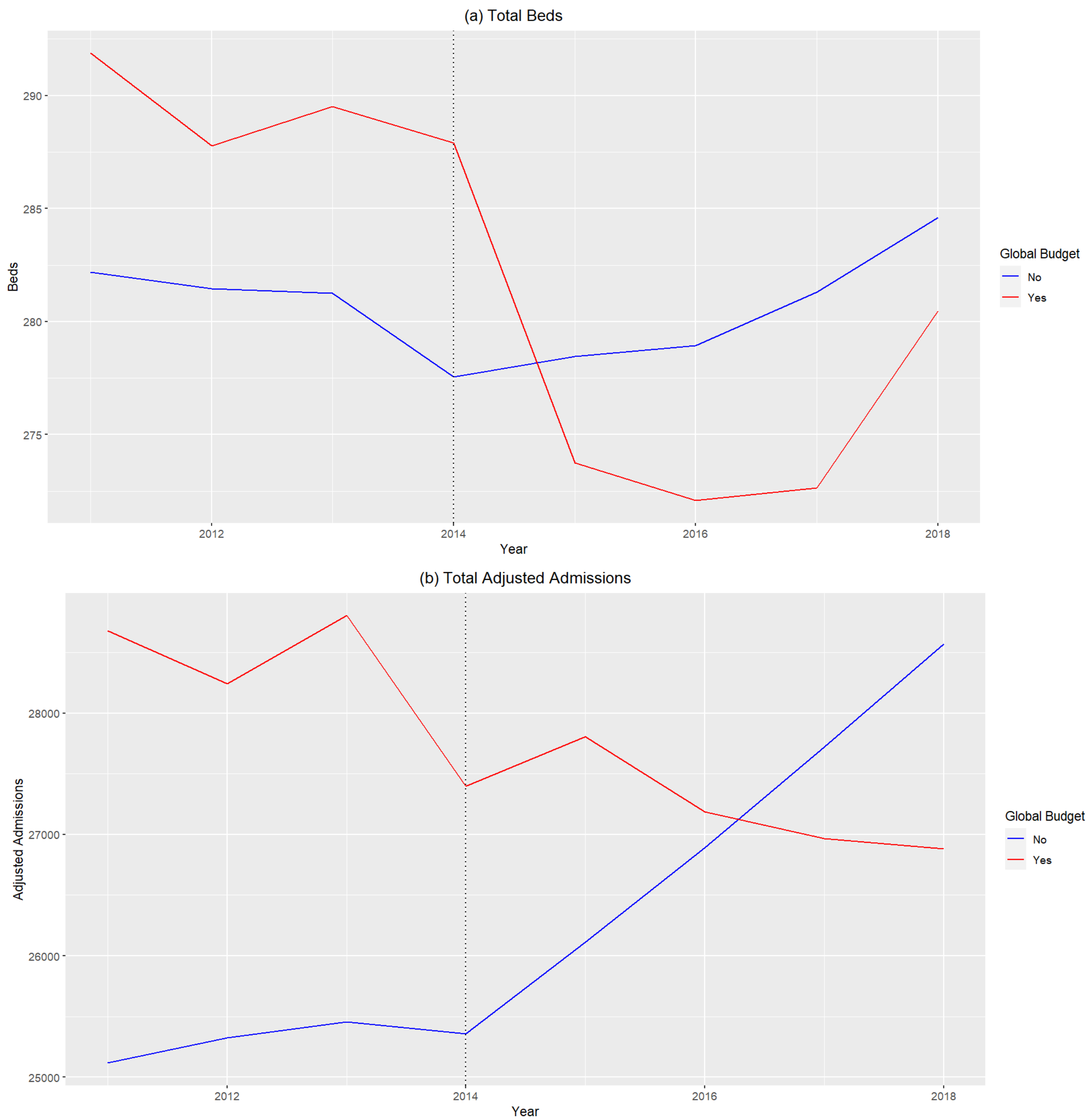


Figure 3.1: Changes in bed volume and adjusted admissions volume over time. 2014 is the first year of treatment.

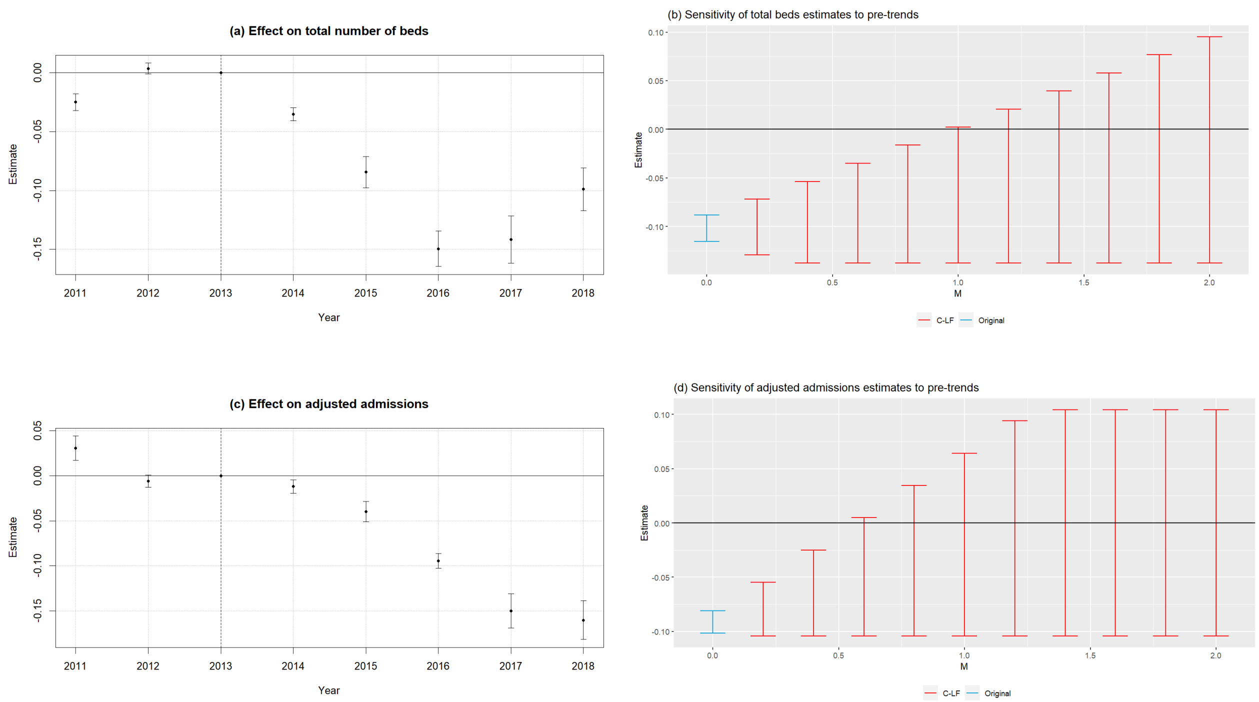


Figure 3.2: Estimates for equation (2). 95% confidence intervals are shown. Plots (b) and (d) report a sensitivity analysis using Rambachan and Roth (2022) HonestDiD package. The confidence interval on the left-hand side of each subfigure is the estimate assuming no pre-trends. The other confidence intervals impose assumptions on the relationship between the largest pre-trend in the pre-treatment period and the trends in the post-treatment period. The x-axis represents the scaling of the largest pre-treatment trend to the assumed post-treatment trend. The dependent variables are the natural logarithm of beds (a and b) and the natural logarithm of adjusted admissions (c and d) where adjusted admissions are hospital admissions scaled by the outpatient to inpatient revenue ratio. I construct conditional least-favorable hybrid confidence sets (C-LF). Each post-treatment year estimate is weighted equally in the sensitivity analysis.

In Figure 2, I present difference-in-difference estimates for the causal effects of global budgets on volume. I use natural logarithm values to mitigate the influence of outlier variables on the outcomes. Plot (a) shows a reduction in bed volume after global budgets are implemented. Plot (c) shows there is a reduction in adjusted admission volume after the introduction of global budgets. In plot (b), I show that the estimated decline in bed volume is robust to a differential trend of 0.8 times the size of the largest pre-treatment trend. Meanwhile, as shown in plot (d), the estimated decline in adjusted admissions is robust to a differential trend of 0.4 times the size of the largest pre-treatment trend.[4]

[4]Figure 3.A.6 in Section A5 shows that the estimated decline in bed volume and adjusted admissions is robust to a post-treatment trend of 2.0 times and 1.0 times the largest pre-period trend, respectively.

Overall, the results provide some evidence of a decline in volume after global budgets are implemented, with the effect being most apparent for inpatient volume. The change in volume outcomes are intuitive; volume was an outcome targeted by global budgets, as regulatory price-setting leaves volume as the main lever for increasing hospital revenue. Hospitals can more directly impact inpatient beds as compared to outpatient volume, and the decrease in beds is more evident than changes in adjusted admissions. When the relationship between volume and hospital revenue is changed, as under global budgets, hospitals may move resources from increasing volume to other ways of maximizing profits.

3.4.2 Effect on Staffing

Figure 3.3 shows the evolution of staffing trends over time for hospitals in Maryland and for control states, before and after the implementation of the global hospital budget policy. Plots (a), (c), and (e) show a decrease in staffing in Maryland relative to control states in physician, nurse, and other full-time employee (FTE) staffing, respectively. The absolute number of physicians is 44% higher in Maryland before the implementation of global budgets than in control states, with similar trends for both treatment and control groups. However, the average number of physicians becomes lower in Maryland than in control states by the end of the sample. Nurse staffing is also higher in Maryland than in control states before the introduction of global budgets and at similar levels after global budgets are introduced. There are 20% more other FTEs in Maryland than in control states before the introduction of global budgets, while after global budgets are introduced, the levels of other FTEs are similar between Maryland and control states.

However, as shown in (b), (d), and (f), the decline in Maryland staffing levels is in part driven by a decrease in volume in Maryland relative to control states. The intensity of clinician staffing per adjusted admission varies based on the level of training for the clinician staffing. Plot (b) shows physician staffing per adjusted admission decreases in Maryland relative to control states after treatment, similar to the decline in hospital-level physician staffing. Plot (d) shows that nurse staffing per admission increases at a faster rate in Maryland compared to control states both before

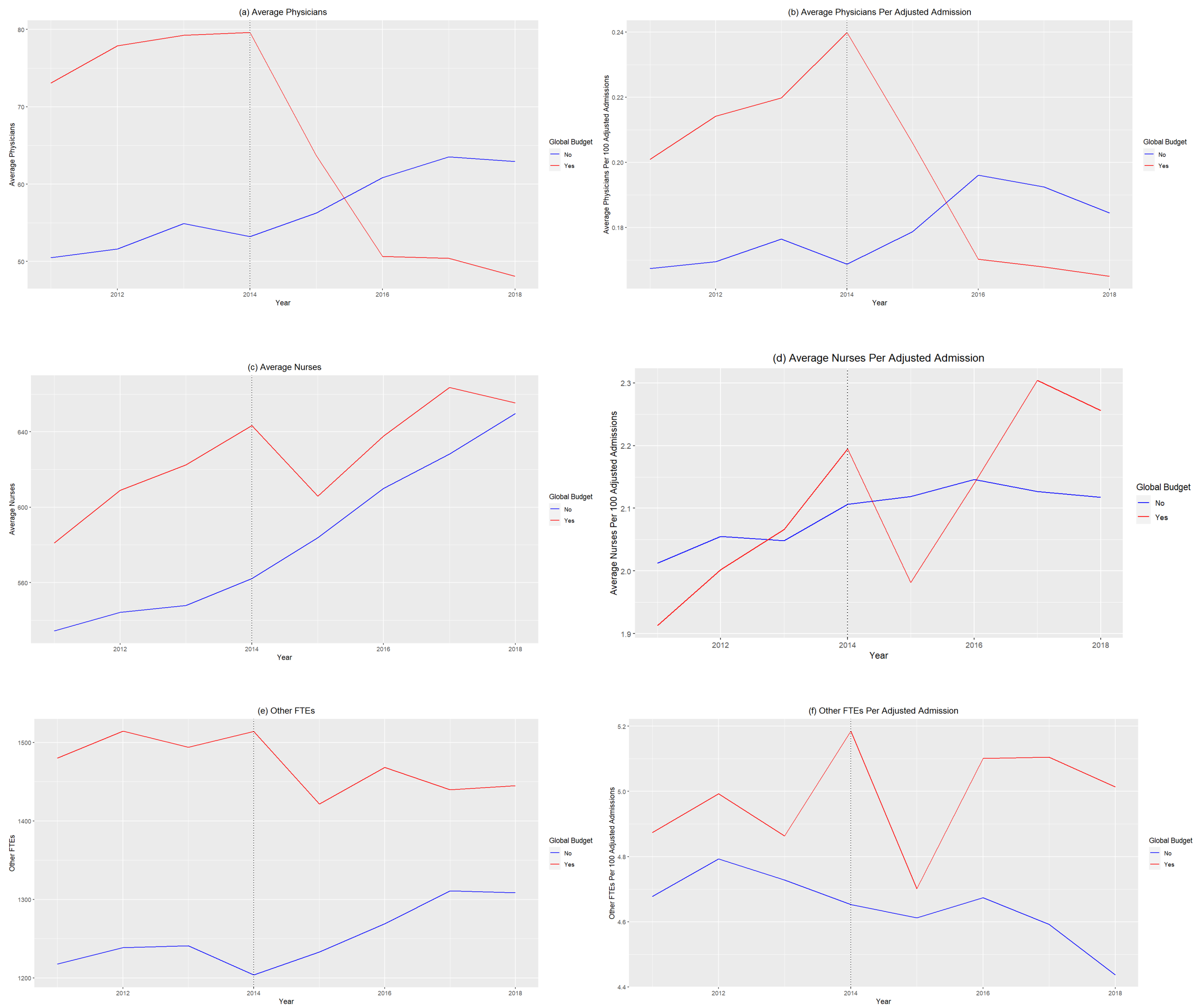


Figure 3.3: Changes in aggregate and per-unit hospital staffing over time. 2014 is the first year of treatment.

and after the implementation of global budgets. Meanwhile, plot (f) shows other FTE staffing per adjusted admission appears to increase in Maryland relative to control states after global budgets are adopted, showing that effects may differ based on the level of skilled labor.

Figure 3.4 presents the difference-in-difference results for different hiring outcome variables. The change in physician staffing is presented in plot (a), and the change in physicians per adjusted admission is presented in plot (b). Nurse staffing outcomes are presented in plot (c), and nurses per adjusted admission outcomes are presented in plot (d). Non-clinical staffing outcomes are presented in plot (e), and non-clinical staffing per admission outcomes are presented in plot (f). Payroll expenses are presented in plot (g), and payroll expenses per adjusted admission are presented in plot (h).

As in Figure 3.3, hospital hiring decreases in all categories relative to control states on an aggregate basis, as shown in plots (a), (c), and (e). Effects differ by year, with evidence of an initial increase in physicians and non-clinical FTEs in Maryland relative to control states in the year global budgets are first implemented and a decrease compared to other states in future years. There may be an initial increase in labor needs as the model first takes effect, followed by a decrease once the up-front investment has occurred.

On a per adjusted-admission basis, the allocation of resources differs based on the type of employee, with less physician staffing, which represents the most expensive highest-trained clinical hiring category, and more hiring of non-clinical FTEs. Plot (b) shows an initial increase in the physician per adjusted admission ratio in 2014, followed by statistically significant reductions in physicians per adjusted admission from 2015 to 2018. Plot (d) shows an increase in nurses per adjusted admissions; however, there is also a trend towards increased nurses per adjusted admission relative to control states prior to treatment, although pre-trends are present. Plot (f) also shows an increase in non-clinical employees per adjusted admission in later years, despite some pre-trends.

To look at overall hiring trends and intensity, I examine changes to total payroll and payroll per adjusted admission in plots (g) and (h). There is a statistically significant decrease in payroll after global budgets are implemented, as shown in (g). However, plot (h) shows an initial decrease in payroll per adjusted admission after global budgets are implemented, followed by an increase in later years. In Figure 3.A.4, I find that aggregate staffing levels also decline after global budgets are implemented, but staffing levels increase on a per-unit adjusted admission basis.

These results indicate that similar staffing resources are being expended on a per-unit basis after global budgets are implemented, with overall staffing and volume simultaneously declining. There is a shift from high-skilled to lower-skilled labor input and the number of full-time equivalent employees per adjusted admission increases, while overall labor resource intensity is unchanged on a per-unit basis.

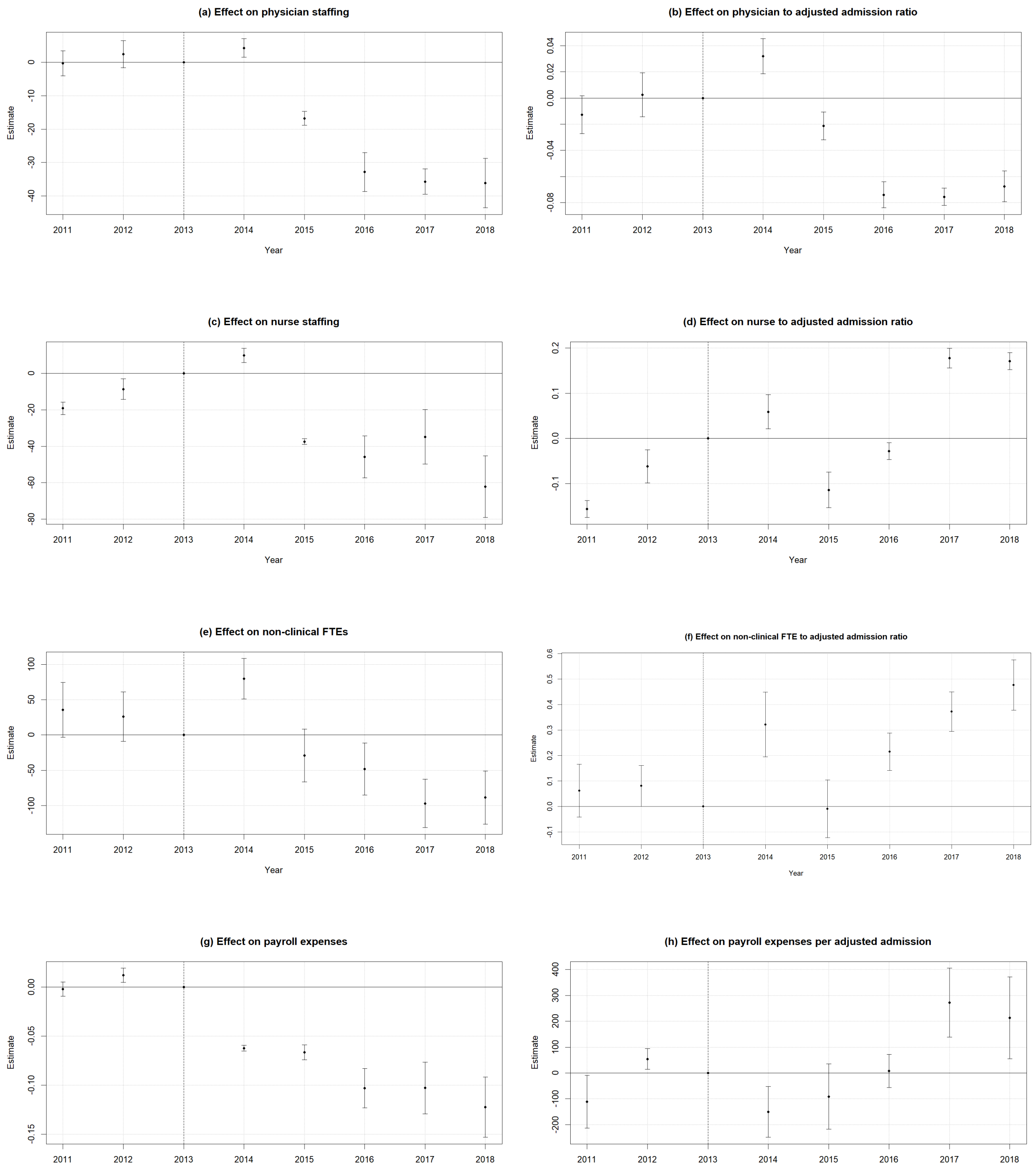


Figure 3.4: Estimates for equation (2). 95% confidence intervals are shown. Plots report difference-in-difference estimates for staffing on an aggregate and a per-adjusted admission basis. The dependent variables are the number of physicians (a), 100 times physicians per adjusted admission (b), the number of nurses (c), 100 times nurses per adjusted admission (d), the number of other FTEs (e), 100 times other FTEs per adjusted admission (f), the natural log of payroll expenses (g), and payroll expense per adjusted admission (h).

I analyze the sensitivity of other FTE per adjusted admission, nurse staffing, and aggregate payroll outcomes to pre-trend assumptions in Figure 3.5.[5] I find in plot (a) that the decline in nurse staffing is robust to a post-trend of 0.6 times the largest pre-period trend, while plot (b) shows that the increase in nurse to adjusted admission ratio is not robust to any pre-trend. Plot (c) shows that an increase in other FTEs per adjusted admission is robust to a post-period trend of up to 0.4 times the largest pre-period trend. Plot (d) shows that the decrease in payroll expenses is robust to a post-trend of up to 1.6 times the largest pre-period trend. Plot (e) shows that there is no change in payroll expenses per adjusted admission robust to the presence of any pre-trend.

[5] Sensitivity of other hiring outcomes to pre-trends are included in the appendix.

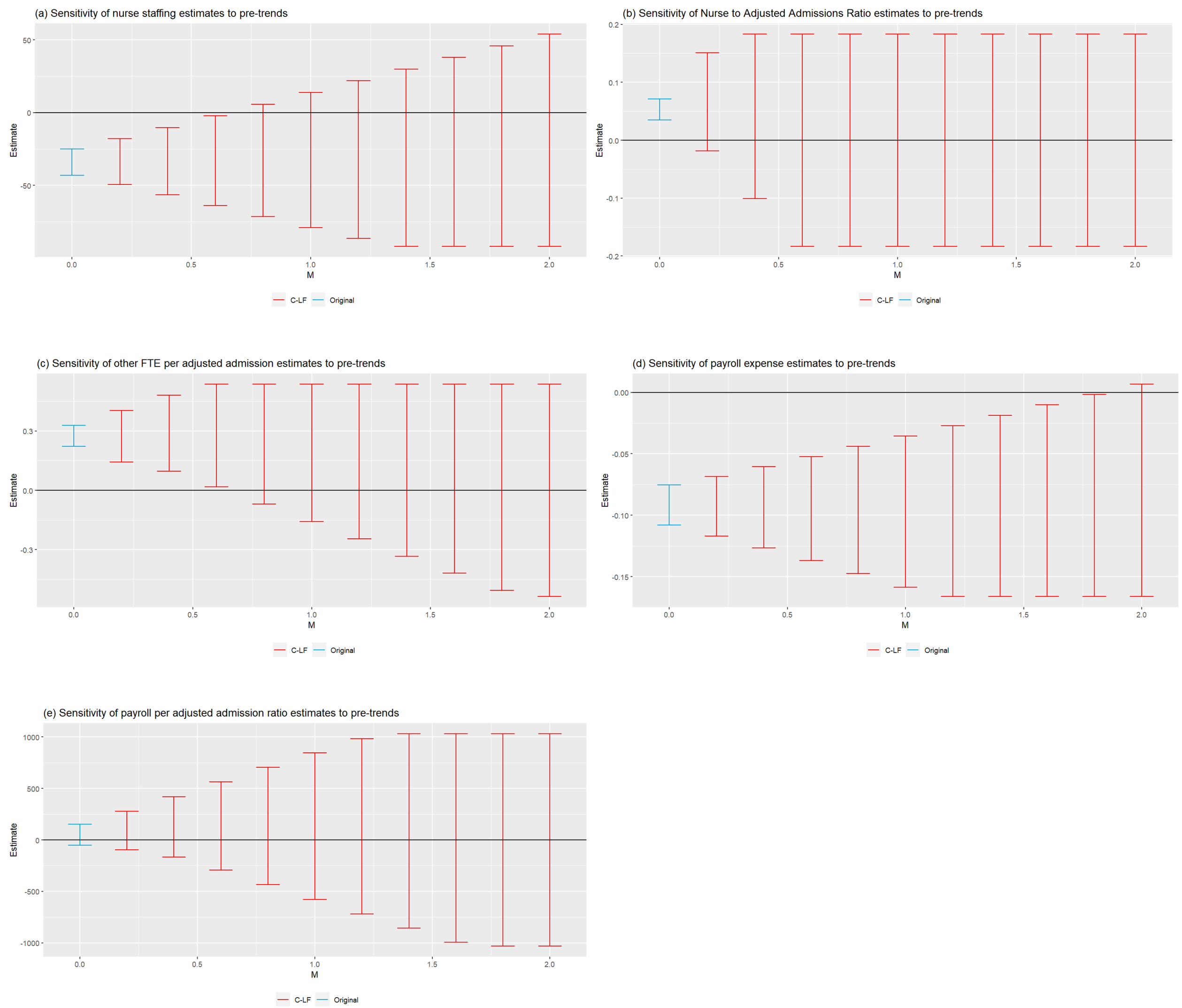


Figure 3.5: Estimates for equation (2). 95% confidence intervals are shown. Plots (a), (b), (c), (d), and (e) report a sensitivity analysis using Rambachan and Roth (2022) HonestDiD package. The confidence interval on the left-hand side of each subfigure is the estimate assuming no pre-trends. The other confidence intervals impose assumptions on the relationship between the largest pre-trend in the pre-treatment period and the trends in the post-treatment period. The x-axis represents the scaling of the largest pre-treatment trend to the assumed post-treatment trend. The dependent variables are nurse staffing (a), nurse staffing per adjusted admission (b), other FTE per adjusted admission (c), logged payroll expenses (d), and payroll expenses per adjusted admission (e). I construct conditional least-favorable hybrid confidence sets (C-LF).

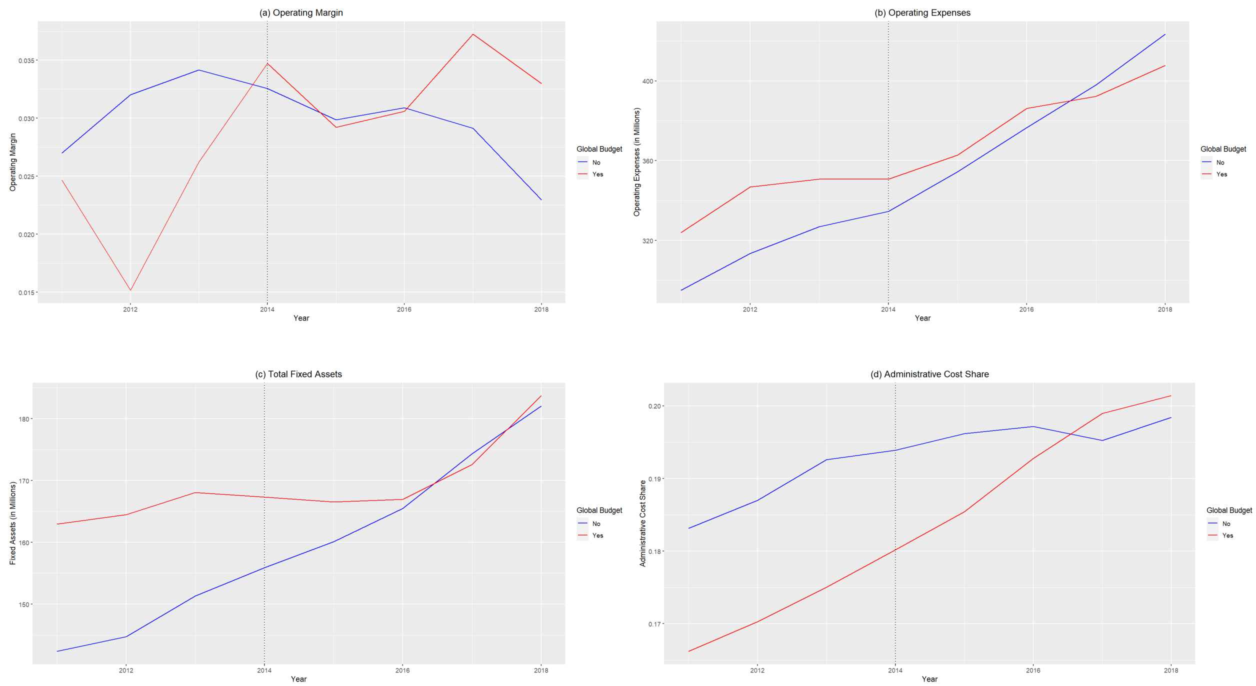


Figure 3.6: Changes in operating margin, operating expenses, fixed assetes, and administrative cost share over time. 2014 is the first year of treatment. Operating margin outcomes are winsorized at the 5% and 95% level.

3.4.3 Effect on Financial Decisions

In this section, I examine how the global hospital budget model changes financial outcomes and hospital performance by presenting descriptive changes and difference-in-difference estimates. For all spending variables, I use natural logarithm values to mitigate the influence of outlier variables on outcomes. Coefficients can be interpreted as estimating percentage changes in the outcome variable.

Figure 3.6 shows the evolution over time for financial measures in Maryland and control states. Plot (a) shows that operating margin improves in Maryland relative to control states both before and after global budgets are introduced. Plot (b) shows that operating expenses increase more slowly in Maryland relative to control states. Plot (c) shows that total fixed assets, which I use to quantify hospital capital investment, start at higher levels in Maryland as compared to control states, but are at lower levels in Maryland than in control states after global budgets are implemented. Plot (d) shows that administrative cost share increases more rapidly in Maryland than in control states both before and after global budgets are implemented.

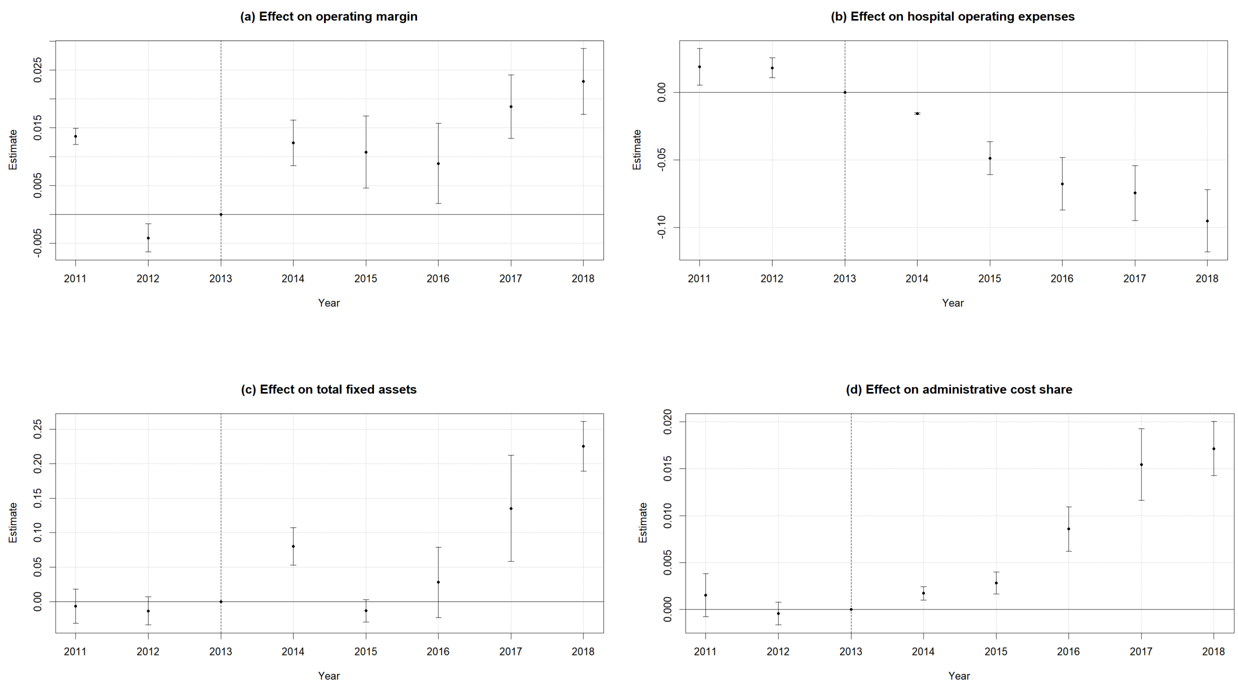


Figure 3.7: Estimates for equation (2). 95% confidence intervals are shown. Plots report difference-in-difference estimates for technology adoption and the profitability of service lines. The dependent variables are operating margin (a), logged operating expenses (b), logged fixed assets (c), and the share of expenses attributable to administrative costs (d).

In Figure 3.7, I show the difference-in-difference results for financial outcomes over time. In plot (a), there is a significant increase in operating margin following the implementation of global budgets; however, the presence of pre-trends makes interpretation unclear. Plot (b) shows a decrease in operating expenses, however, pre-trends are present. In plot (c), fixed assets increase after global budgets are implemented. In plot (d), I examine the impact of treatment on hospital administrative expenses as a share of revenue. I find evidence of a statistically significant increase in administrative cost share in response to global budgets.

In Figure 3.8, I check the sensitivity of the difference-in-difference results presented in Figure 7 to the presence of pre-trends. Plot (a) shows that the operating margin increase is robust to a post-trend of 0.2 times the size of the largest pre-trend. Meanwhile, the decline in operating expenses is robust to a post-trend of 0.8 times the size of the largest pre-trend. The increase in fixed assets in plot (c) is robust to a post-trend of up to 1.2 times the size of the largest pre-trend. In plot

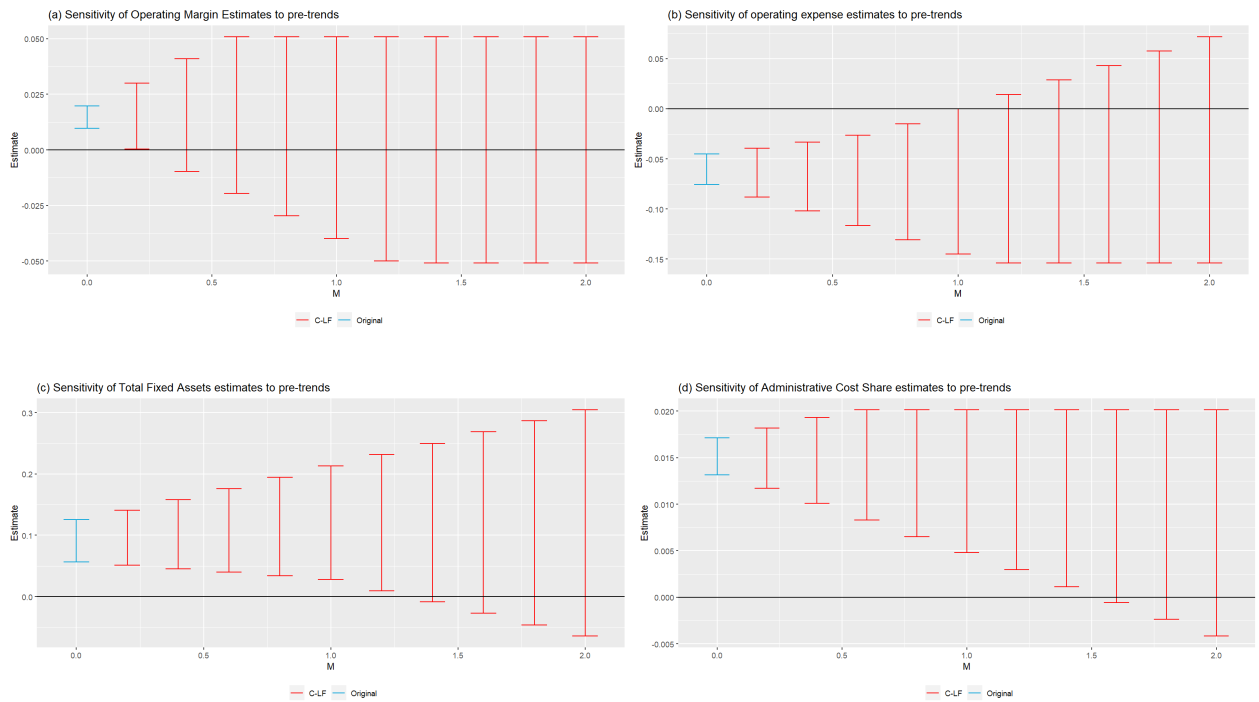


Figure 3.8: Plots report a sensitivity analysis using Rambachan and Roth (2022) HonestDiD package. The confidence interval on the left-hand side of each subfigure is the estimate assuming no pre-trends. The other confidence intervals impose assumptions on the relationship between the largest pre-trend in the pre-treatment period and the trends in the post-treatment period. The x-axis represents the scaling of the largest pre-treatment trend to the assumed post-treatment trend. The dependent variables are operating margin (a), the natural logarithm of operating expenses (b), the natural logarithm of total fixed assets (c), and the share of total expenses that are administrative (d). I construct conditional least-favorable hybrid confidence sets (C-LF). Each post-treatment year estimate is weighted equally in the sensitivity analysis.

(d), I find evidence that the increase in administrative cost share is robust to a post-trend of up to 1.4 times the size of the largest pre-period trend.

Overall, I do not find evidence of statistically significant changes to operating margin or expenses. However, I do find evidence of additional administrative burden. To the extent that new models may drive improved outcomes, there may be a trade-off between these outcomes and increased compliance costs (Shi, 2022; League, 2023). Fixed assets increase after global budgets are implemented. There is no evidence of a decline in capital investment from global budgets, and investment levels may even increase, suggesting that the revenue constraint does not disincentivize investment.

3.4.4 Effect on Quality

Given the above results about decreases in staffing levels and composition, one concern is about potential trade-offs between decreasing hospital spending and patient outcomes. In this section, I examine how quality may have changed in response to the global budget program. I use hospital readmission and mortality outcomes, using data from 2011 to 2016, excluding 2015.

Rules for the HRRP were first announced in August 2011 for both Maryland and non-Maryland hospitals. However, since the implementation of global budgets in Maryland in 2014, Maryland participates in the Maryland-specific Readmissions Reduction Incentive Program (RRIP) rather than HRRP. While the HRRP is limited to Medicare and a subset of medical conditions, RRIP, as a component of the global budget program, functions on an all-payer basis and includes all causes.[6]

Figure 3.9 presents difference-in-difference estimates for hospital quality. Plot (a) shows a decline in heart failure readmissions, however, pre-trends are present. Plot (b) does not show any evident change in pneumonia readmissions from global budgets. Plot (c) shows some evidence of declining AMI readmissions after global budgets, however, pre-trends are present. Plot (d) shows no apparent effect of global budgets on heart failure mortality. Plot (e) shows an increase in pneumonia mortality but also the presence of pre-trends. Plot (f) does not show a clear change in the AMI mortality rate after implementation of global budgets.

[6]Because I conservatively use the largest pre-period trend to interpret the sensitivity of post-treatment outcomes to trends, I do not remove 2011 from my analysis, as mechanically, this could only decrease the largest pre-period trend and make the results appear less sensitive to trends.

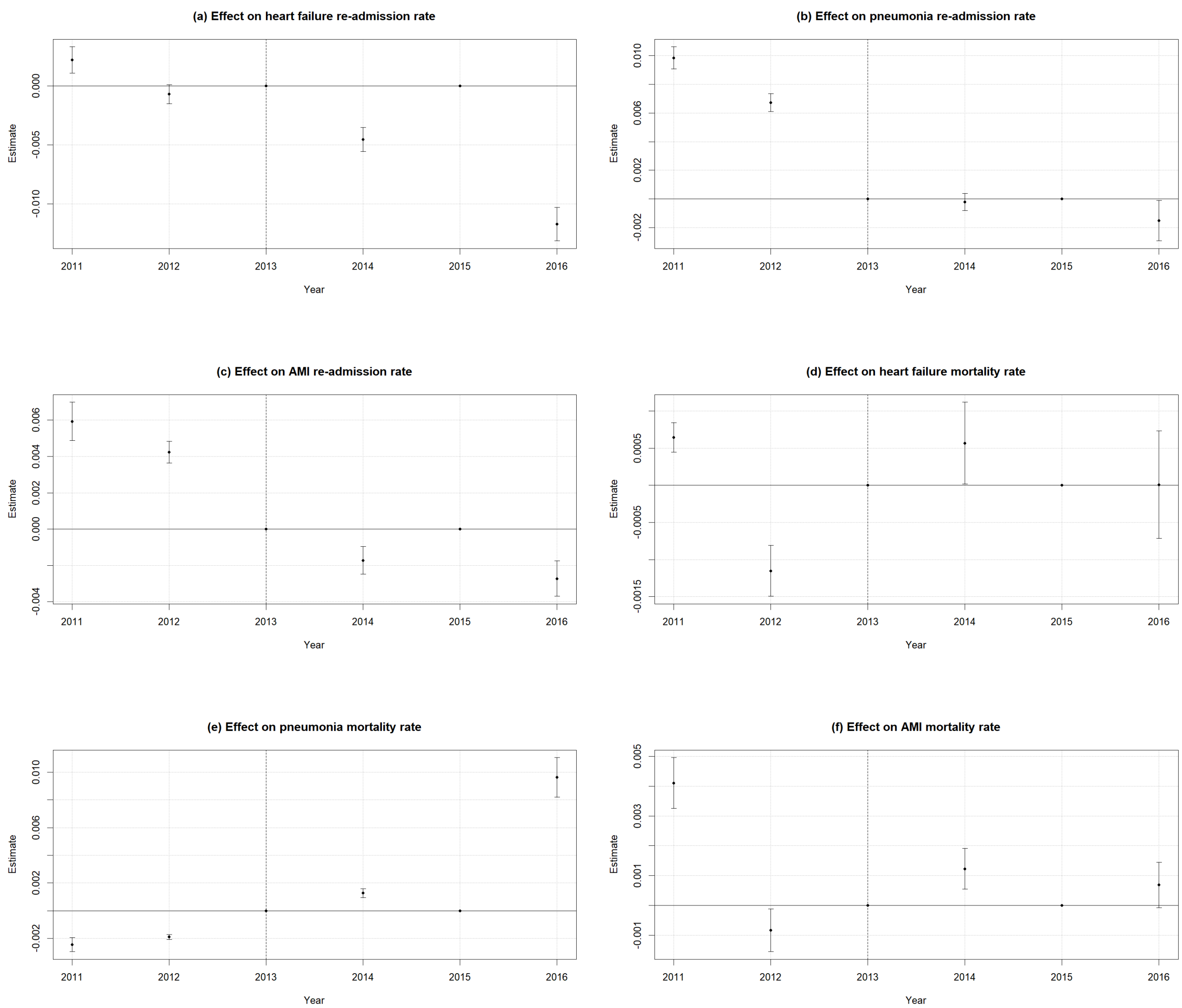


Figure 3.9: Estimates for equation (2). 95% confidence intervals are shown. Plots report difference-in-difference estimates for technology adoption and the profitability of service lines. The dependent variables are heart failure readmission rate (a), pneumonia readmission rate (b), AMI readmission rate (c), heart failure mortality rate (d), pneumonia mortality rate (e), and AMI mortality rate (f).

I test the sensitivity of the results to pre-trends in Figure 3.10. Assuming a post-period trend equal to the largest pre-period trend, heart failure readmission rates decrease by 0.7%, representing a 3.1% decrease from baseline. Pneumonia mortality rates increase by 0.5%, representing a 4.6% increase over baseline. Plot (a) shows that the decrease in heart failure readmissions is robust to a post-trend of up to 1.4 times the largest pre-period trend. Plot (b) shows that any decline in pneumonia readmission is not robust to any non-zero pre-trend. Plot (c) shows that a decline in AMI readmissions is robust to a post-trend of 0.8 times the largest pre-period trend. Plot (d) shows no change in heart failure mortality regardless of pre-trends. Plot (e) shows that an increase in pneumonia mortality is robust to a post-period trend of up to 1.8 times the pre-period trend. Plot (f) shows an increase in AMI mortality that is not robust to any non-zero pre-trend.

I find mixed evidence of quality changes from global budget programs. There is no impact on pneumonia and AMI readmissions and a decline in 30-day heart failure readmissions. However, 30-day pneumonia mortality increases. While these results do not take into account how hospital behavior may change the composition of admissions and how they may impact outcomes, they are risk-adjusted, mitigating selection concerns. Furthermore, Doyle, Graves, and Gruber (2019) show that 30-day mortality and readmission outcomes are correlated to measures of hospital quality that account for selection. The magnitude of the mortality increase is comparable to decreases from the HRRP (Gupta, 2021).

3.4.5 Effect on Technology Adoption and Service Line offerings

In addition to short-term quality outcomes, there may be concern that hospitals decrease technological investment in the same way they decrease staffing. Quality investments have been central to improved mortality outcomes in the US overall the last several decades (Garthwaite, Starc, and Ody, 2022; Chandra, Kakani, and Sacarny, 2022), and some technological investment decreases could prevent future improvements in outcomes. Figure 3.11 presents descriptive plots about changes in hospital service provision in response to the global budget program. Plot (a) shows trends in technology adoption appear similar over time in treatment and control states, without much

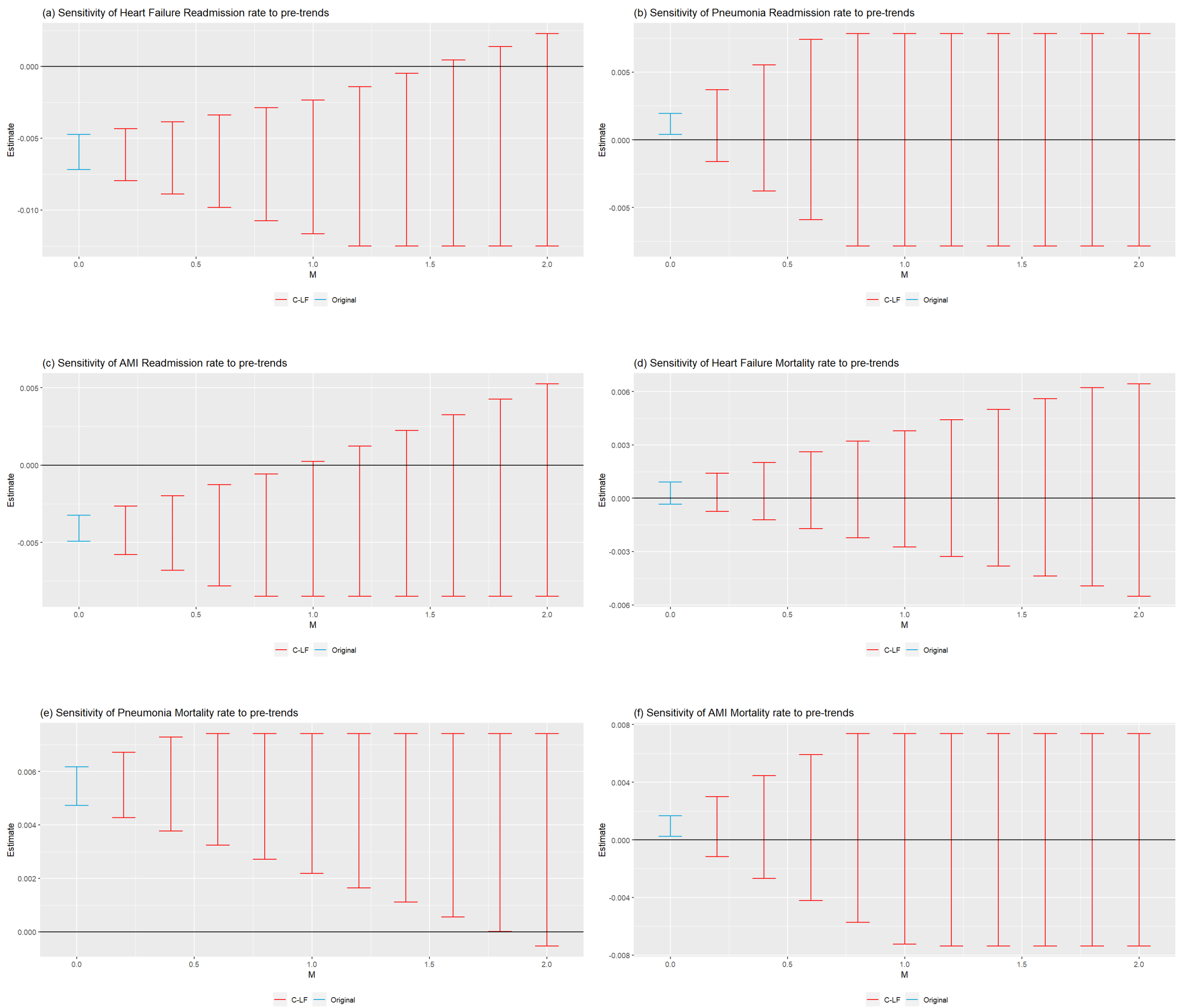


Figure 3.10: Plots report a sensitivity analysis using Rambachan and Roth (2022) HonestDiD package. The confidence interval on the left-hand side of each subfigure is the estimate assuming no pre-trends. The other confidence intervals impose assumptions on the relationship between the largest pre-trend in the pre-treatment period and the trends in the post-treatment period. The x-axis represents the scaling of the largest pre-treatment trend to the assumed post-treatment trend. The dependent variables are heart failure readmission rate (a), pneumonia readmission rate (b), AMI readmission rate (c), heart failure mortality rate (d), pneumonia mortality rate (e), and AMI mortality rate (f). I construct conditional least-favorable hybrid confidence sets (C-LF). Each post-treatment year estimate is weighted equally in the sensitivity analysis.

difference in overall trend after the implementation in global budgets. Plot (b) shows profitable service line offerings also are similar between groups, with evidence suggesting decreased trends of profitable service line provision in Maryland as compared to control states. Meanwhile, plot (c) presents evidence that generally unprofitable service lines may have higher uptake in Maryland than in control states after global budgets are implemented.

In Figure 3.12, I present difference-in-difference estimates for technology adoption and the provision of service lines. I also perform a sensitivity analysis of my estimates in the presence of pre-trends. In (a), I find a significant decrease in technology adoption in response to global budget adoption. However, in plot (b), I show that this decrease is not robust to any non-zero pre-trend. Plots (c) and (d) show no apparent effect on profitable service-line offerings. Plot (e) shows a significant increase in unprofitable service-line adoption, and plot (f) shows that this outcome is robust to a post-trend of up to 0.6 times the largest pre-period trend.

Overall, I cannot rule out zero changes to care provision.[7] I do not detect statistically significant changes in either technological adoption or the profitability of the type of care hospitals deliver. As technological adoption may represent up-front costs for the hospital, the lack of evidence for reduced technological adoption combined with previous results presented in Section 3.4.3 suggest that the change in profit structure from global budgets does not preclude investments with a longer time horizon return.

[7] In the appendix, I present difference-in-difference and pre-trend sensitivity estimates for birth, cardiac, diagnostic imaging, and radiation therapy adoption. I find no impact for birth, cardiac, and diagnostic imaging adoption, but radiation therapy adoption increases, robust to a post-trend of 1.6 times the largest pre-period trend.

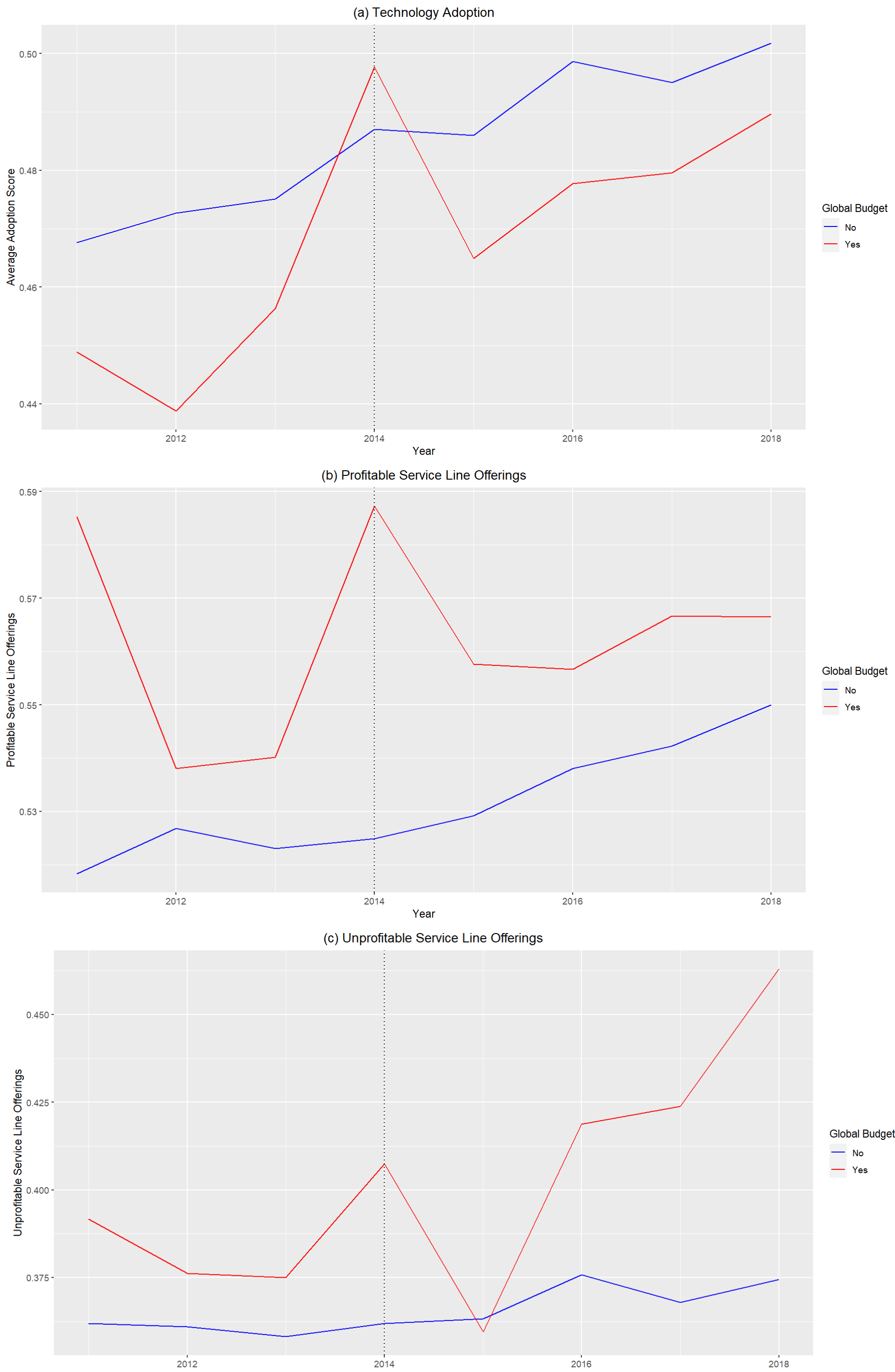


Figure 3.11: Changes in technology adoption, profitable service line offerings, and unprofitable service line offerings over time. 2014 is the first year of treatment.

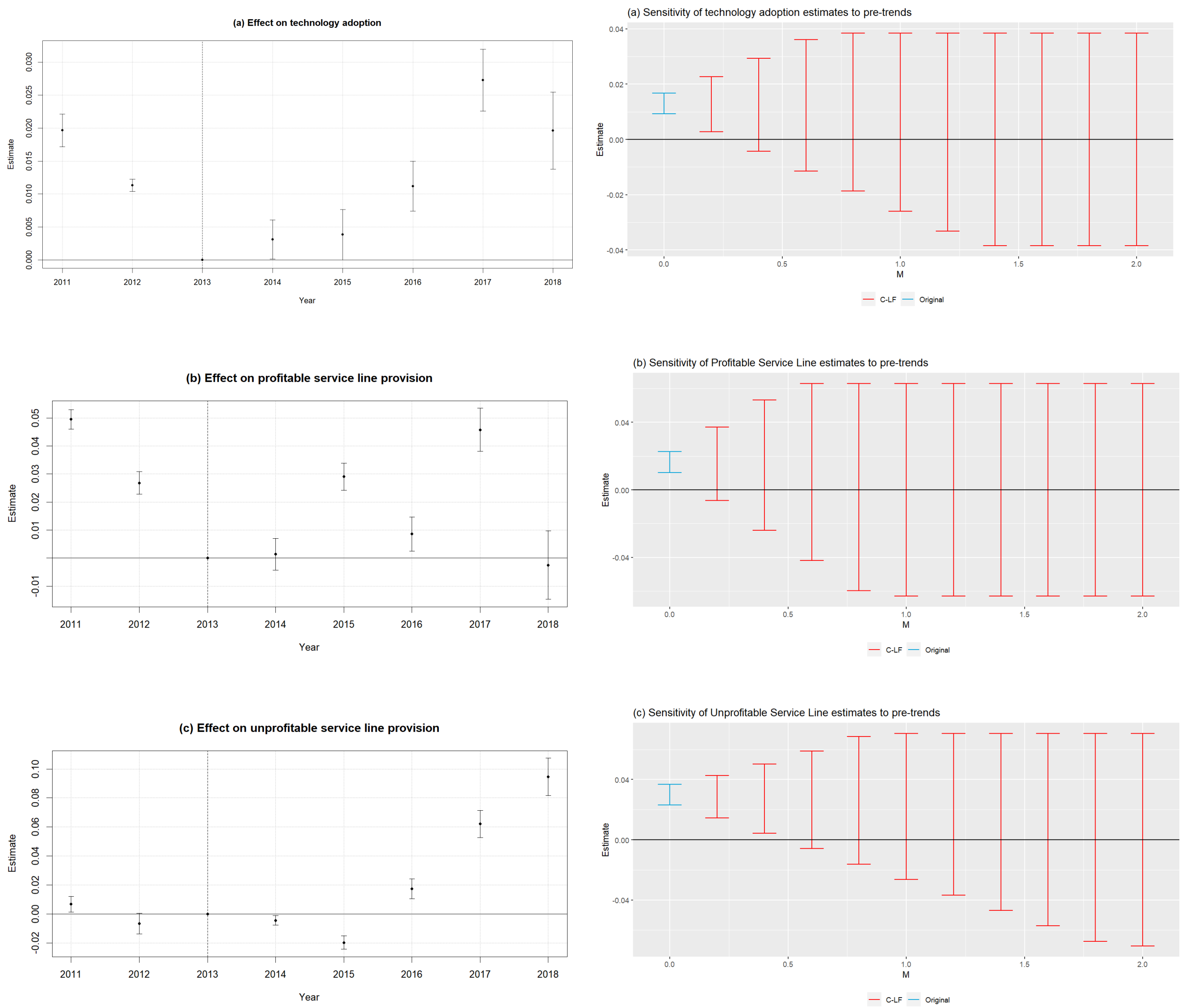


Figure 3.12: Estimates for equation (2). 95% confidence intervals are shown. Plots (a), (c), and (e) show difference-in-difference results. Plots (b), (d), and (f) report a sensitivity analysis using Rambachan and Roth (2022) HonestDiD package. The confidence interval on the left-hand side of each subfigure is the estimate assuming no pre-trends. The other confidence intervals impose assumptions on the relationship between the largest pre-trend in the pre-treatment period and the trends in the post-treatment period. The x-axis represents the scaling of the largest pre-treatment trend to the assumed post-treatment trend. The dependent variables are the average technologies adopted on a scale of 0 to 1 (a and b), the average amount of profitable services offered (c and d), and the average amount of unprofitable services offered (e and f). I construct conditional least-favorable hybrid confidence sets (C-LF). Each post-treatment year estimate is weighted equally in the sensitivity analysis.

3.5 Conclusion

Understanding how health systems may change health care supply in response to payment incentives is of great interest to policymakers and payers. I study this in the context of the Maryland Global Hospital Budget model, finding that hospitals cut back on volume and hiring while shifting per-unit service from physician to non-clinical staffing. Hospital financial outcomes are unchanged while the administrative cost share increases. With cost-containment measures, there may be concern about quality or investment responses. Heart failure readmissions fall, but pneumonia mortality increases. I do not detect significant effects of global budgets on technology adoption or service line provision, mitigating concerns about decreased investment because of decreasing incentives to generate revenue. The fact that hospital changes in response to global budgets are more focused on staffing rather than technology adoption is notable, as technological change is considered a leading reason for increased medical spending (Fuchs et al, 1996; Cutler, 2007). Smith, Newhouse, and Cuckler (2022) find a reduced contribution of technological change to medical spending in recent years, which may help explain the lack of hospital response in my setting.

Payment model design is particularly tied to how different incentives affect hospital behavior. By systematically analyzing health system responses to a variety of payment models and connecting those responses to outcomes, policymakers can better understand the relevant policy levers to target when designing new payment models. In my context, payment model changes cause a reallocation of staffing towards lower-skilled workers. The increase in 30-day mortality rates for pneumonia is concerning, suggesting that policymakers should better understand the marginal benefits of specialized, costly workers as opposed to cheaper less-specialized workers in different contexts. Additionally, there may be trade-offs between administrative costs and achieving desired outcomes. The increases in administrative spending I find could be the mechanism by which program outcomes are achieved (Brot-Goldberg et al.) or could represent increased compliance costs.

Because of the lack of for-profit hospitals in Maryland, I study the response of non-profits to payment changes. Given incentive differences across firm structures, future work could explore how differences in for-profit status and organizational structure impact responses to payment models.

Also, further research could identify how changing payment incentives lead to differential responses by the value of care. Other work shows no evidence of declines in low-value care in response to global budgets (Oakes et al. 2020), in addition to shifts in patient composition towards healthier patients (Aliu et al. 2021). Future work could better de-compose changes related to changes in patient care and strategic changes in patient composition.

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A Appendix

A.1 Sample construction

The sample consists of all urban hospitals in states that expanded Medicaid in 2014, did not participate in a prior Medicaid expansion, and did not participate in a concurrent Center for Medicare Medicaid Innovation model. The study includes hospitals in the following states:

Treatment: Maryland

Control: Arizona, Colorado, New York, Minnesota, Ohio, New Jersey, Washington, Rhode Island, New Mexico, Arkansas, Connecticut, Delaware, Illinois, Iowa, Kentucky, Michigan, Nevada, New Hampshire, North Dakota, Oregon, Vermont, West Virginia

A.2 Difference-in-difference results

In addition to the dynamic difference-in-difference model, I also estimate the following equation:

$$Y_{ist} = \alpha_i + \lambda_t + \beta_t \mathbb{1}\{E_i = D\} + X_{it} + \tau_{st} + \varepsilon_{ist} \tag{4}$$

Here, β_t represents the regressor of interest, the causal impact of the Maryland Global Hospital budget program on outcomes in year t, with $\{E_i = D\}$ being an indicator for treatment status in a given year. X_{it} represents relevant covariates for a given outcome variable, α_i represents hospital fixed effects, λ_t represents year fixed effects, and τ_{st} represents state-specific time trends, while ε_{ist} represents the normally distributed error term. I cluster standard errors on the state-year level, since the state is the unit of treatment. I use a difference-in-difference linear probability model for binary outcomes, including service line offerings.

In Table 3.A.1, I find staffing decreases for all categories of employees and an overall decrease in payroll. Table 3.A.2 shows a decrease in physicians per adjusted admission and an increase in nurses and other FTEs on a per-unit basis, with no change in payroll. In Table 3.A.3, I find worse patient scores along with improvements in hospital 30-day readmission rates for all three conditions.

Table 3.A.4 shows an increase in pneumonia mortality rate and no change in heart failure or AMI mortality rate. Table 3.A.5 shows an increase in operating margin, a decrease in operating expenses, an increase in administrative cost share, and no change in fixed assets. In Table 3.A.6, I find no effects of global budgets on technology adoption, a decrease in profitable service line offerings, and an increase in unprofitable service line offerings.

Table 3.A.1: Difference-in-Difference results for staffing

	(1) Physicians	(2) Nurses	(3) Other FTE	(4) Payroll
Treatment	-24.113***	-24.803**	-56.920***	-13525966.784***
	(2.351)	(6.644)	(9.304)	(3398087.028)
Observations	6644	6644	6644	6644
R^2	0.819	0.969	0.961	0.970

Notes: * p < 0.05, ** p < 0.01, *** p < 0.001. Estimates for Equation (A1). Year and hospital fixed effects are used. The dependent variables are the number of physicians (a), the number of nurses (b), the number of other FTEs (c), and the natural log of payroll expenses (d). Standard errors are clustered on the state-year level.

Table 3.A.2: Difference-in-Difference results for staffing per adjusted admission

	(1) Physicians per adjusted admission	(2) Nurses per adjusted admission	(3) Other FTE per adjusted admission	(4) Payroll per adjusted admission
Treatment	-0.038***	0.126***	0.229***	68.239
	(0.006)	(0.013)	(0.037)	(77.844)
Observations	6644	6644	6644	6644
R^2	0.854	0.806	0.900	0.912

Notes: * p < 0.05, ** p < 0.01, *** p < 0.001. Estimates for Equation (A1). Year and hospital fixed effects are used. The dependent variables are 100 times physicians per adjusted admission (a), 100 times nurses per adjusted admission (b), 100 times other FTEs per adjusted admission (c), and payroll expense per adjusted admission (d). Standard errors are clustered on the state-year level.

Table 3.A.3: Difference-in-Difference results for patient score and readmissions

	(1)	(2)	(3)	(4)
	Patient overall score	Heart Failure Readmission	Pneumonia Readmission	AMI Readmission
Treatment	-0.009***	-0.009***	-0.005***	-0.006***
	(0.002)	(0.001)	(0.001)	(0.001)
Observations	4938	4679	4711	4069
R^2	0.914	0.801	0.750	0.827

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Estimates for Equation (A1). Year and hospital fixed effects are used. The dependent variables are patient overall score (a), heart failure readmission (b),pneumonia readmission (c), and acute myocardial infection readmission (d).

Table 3.A.4: Difference-in-Difference results for hospital 30-day mortality rates

	(1)	(2)	(3)
	Heart Failure Mortality Rate	Pneumonia Mortality Rate	AMI Mortality Rate
Treatment	0.000	0.007***	0.000
	(0.000)	(0.001)	(0.001)
Observations	4662	4704	4255
R^2	0.769	0.872	0.741

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Estimates for Equation (A1). Year and hospital fixed effects are used. The dependent variables are heart failure re-mortality (a), pneumonia mortality (b), and acute myocardial infection mortality (c). Standard errors are clustered on the state-year level.

Table 3.A.5: Difference-in-Difference results for financial outcomes

	(1)	(2)	(3)	(4)
	Operating Margin	Operating Expenses	Administrative Cost Share	Fixed Assets
Treatment	0.012**	-0.073***	0.009***	0.099**
	(0.003)	(0.012)	(0.001)	(0.028)
Observations	6433	6679	6679	6682
R^2	0.687	0.988	0.842	0.922

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Estimates for Equation (A1). Year and hospital fixed effects are used. The dependent variables are operating margin (a), the natural logarithm of operating expenses (b), the share of total expenses that are administrative (c), and the natural logarithm of total fixed assets (d). Standard errors are clustered on the state-year level.

Table 3.A.6: Difference-in-Difference results for care provision

	(1) Technology adoption	(2) Profitable service lines	(3) Unprofitable service lines
Treatment	-0.004	-0.009*	0.013*
	(0.003)	(0.004)	(0.005)
Observations	5841	5839	5840
R^2	0.954	0.844	0.890

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Estimates for Equation (A1). Year and hospital fixed effects are used. The dependent variables are the average technologies adopted on a scale of 0 to 1 (a), the average amount of profitable services offered (b), and the average amount of unprofitable services offered (c). Standard errors are clustered on the state-year level.

A.3 Sensitivity to pre-period trends

To test the robustness of the results to imposing the assumption of pre-period trends, I employ the test from Rambachan and Roth (2022) for additional staffing outcomes in Figure 3.A.1. A decrease in physicians is robust to a post-trend of up to 0.6 times the size of the largest pre-period trends. A decrease in physician per adjusted admissions staffing is robust to a post-trend up to 0.8 times the size of the largest pre-period trend.

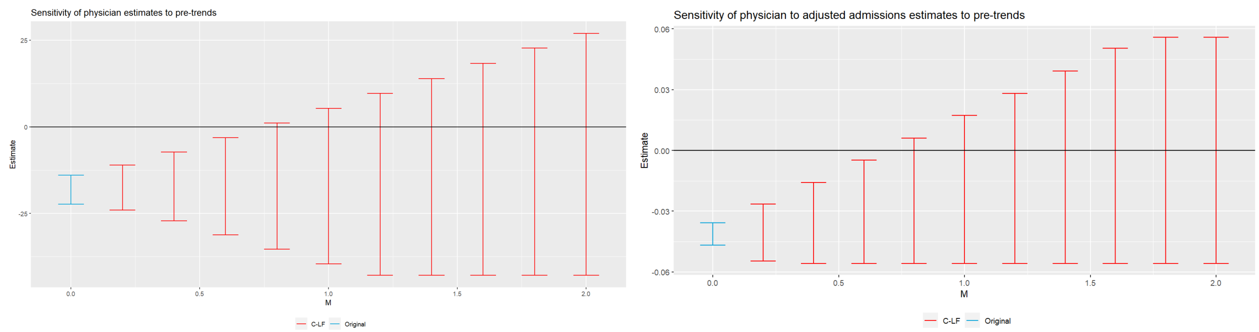

Figure 3.A.1: Estimates for equation (2). 95% confidence intervals are shown. Plots (a) reports a sensitivity analysis using Rambachan and Roth (2022) HonestDiD package. The confidence interval on the left-hand side of each subfigure is the estimate assuming no pre-trends. The other confidence intervals impose assumptions on the relationship between the largest pre-trend in the pre-treatment period and the trends in the post-treatment period. The x-axis represents the scaling of the largest pre-treatment trend to the assumed post-treatment trend. The dependent variables are the number of physicians (a), 100 physicians per adjusted admission (b). I construct conditional least-favorable hybrid confidence sets (C-LF). Each post-treatment year estimate is weighted equally in the sensitivity analysis.

A.4 Additional Outcome Variables

In this section, I provide additional outcomes from the adoption of global budgets. I decompose effects on administrative spending and technology adoption to further explore mechanisms. I examine the impact of global budgets on general administrative expenses, nursing administrative expenses, and electronic medical record expenses. I look at how global budgets change staffing levels to better understand staffing changes. Additionally, I examine the effect of global budgets on patient hospital scores.

I present administrative spending difference-in-difference estimates and sensitivity of the results to the presence of pre-trends in Figure 3.A.2. I find an increase in general administrative expenses robust to a post-period trend of up to 0.2 times the largest pre-period trend. I find a decrease in nursing administrative expenses that is not robust to the presence of any pre-trend. Finally, I find a decrease of EMR administrative expenses robust to a post-period trend up to 0.4 times the largest pre-period trend.

For technology adoption, I examine the impact of global budgets for birth technology adoption, cardiac technology adoption, diagnostic imaging technology adoption, and radiation therapy technology adoption. I calculate adoption scores as in Garthwaite, Starc, and Ody (2022). I present technology adoption difference-in-difference estimates and sensitivity of the results to the presence of pre-trends in Figure 3.A.3. I find no significant impacts of technology adoption for births, cardiac technology, and diagnostic imaging. For radiation therapy adoption, I find an increase after global budgets are implemented that is robust to post-trends up to 1.6 times the largest pre-period trend.

I present full-time equivalent staffing in Figure 3.A.4. In plot (a), I show that aggregate staffing decreases after global budgets are implemented. In plot (b), I show that the staffing per adjusted admission increases in later years. In plot (c), I show that the increase in overall FTE per adjusted admission ratio is robust to a post-trend up to 0.6 times the largest pre-period trend.

Next, I present patient score estimates of hospital quality in Figure 3.A.5. I do not find any significant changes in overall patient score, nurse communication score, or physician communication score that are robust to the presence of pre-trends.

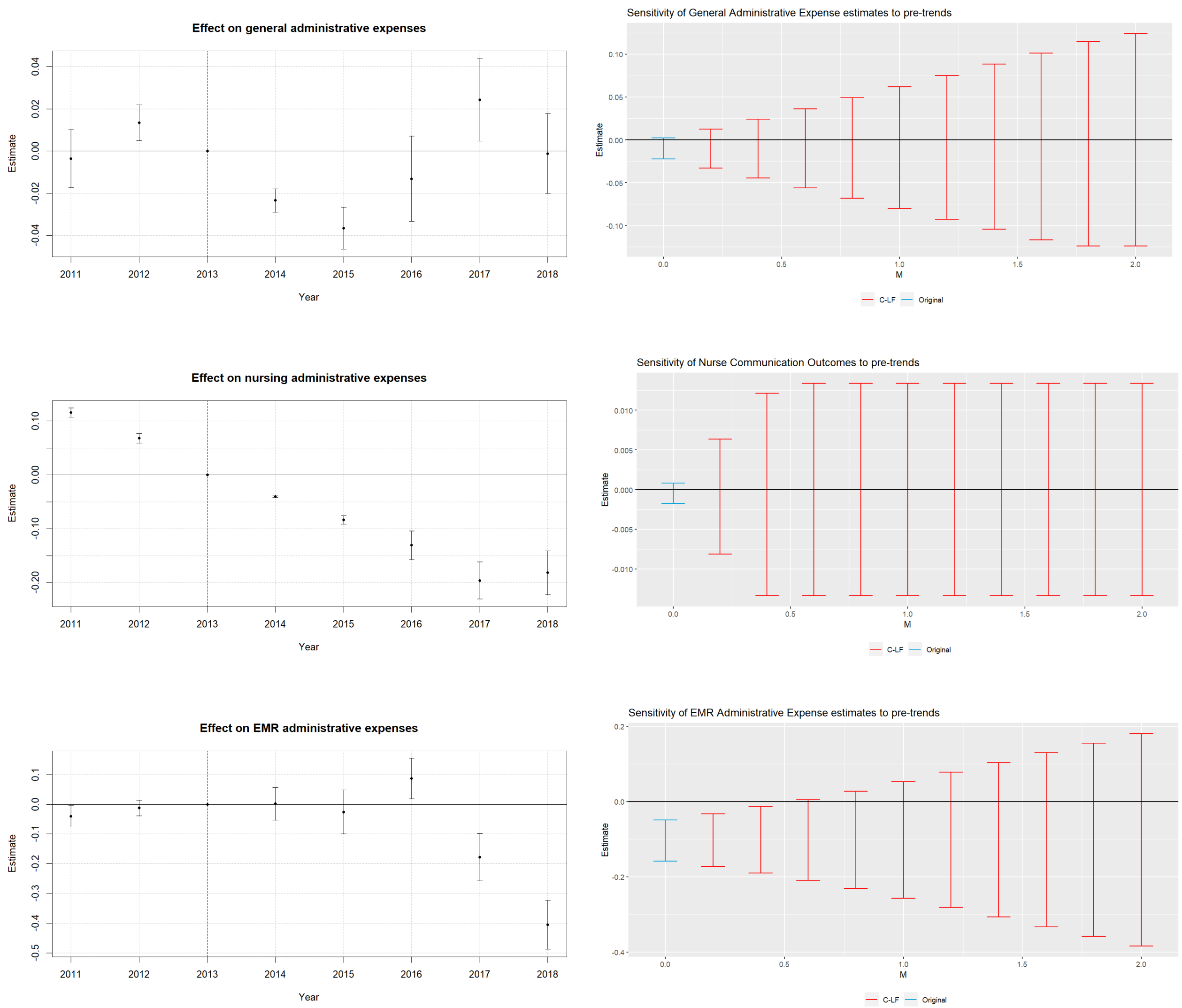

Figure 3.A.2: Estimates for equation (2). 95% confidence intervals are shown. Plots (b), (d), and (f) report a sensitivity analysis using Rambachan and Roth (2022) HonestDiD package. The confidence interval on the left-hand side of each subfigure is the estimate assuming no pre-trends. The other confidence intervals impose assumptions on the relationship between the largest pre-trend in the pre-treatment period and the trends in the post-treatment period. The x-axis represents the scaling of the largest pre-treatment trend to the assumed post-treatment trend. The dependent variables are the natural logarithm of general and administrative expenses (a and b), the natural logarithm of nursing administration expenses (c and d), and the natural logarithm of electronic medical record administrative expenses (e and f). I construct conditional least-favorable hybrid confidence sets (C-LF).

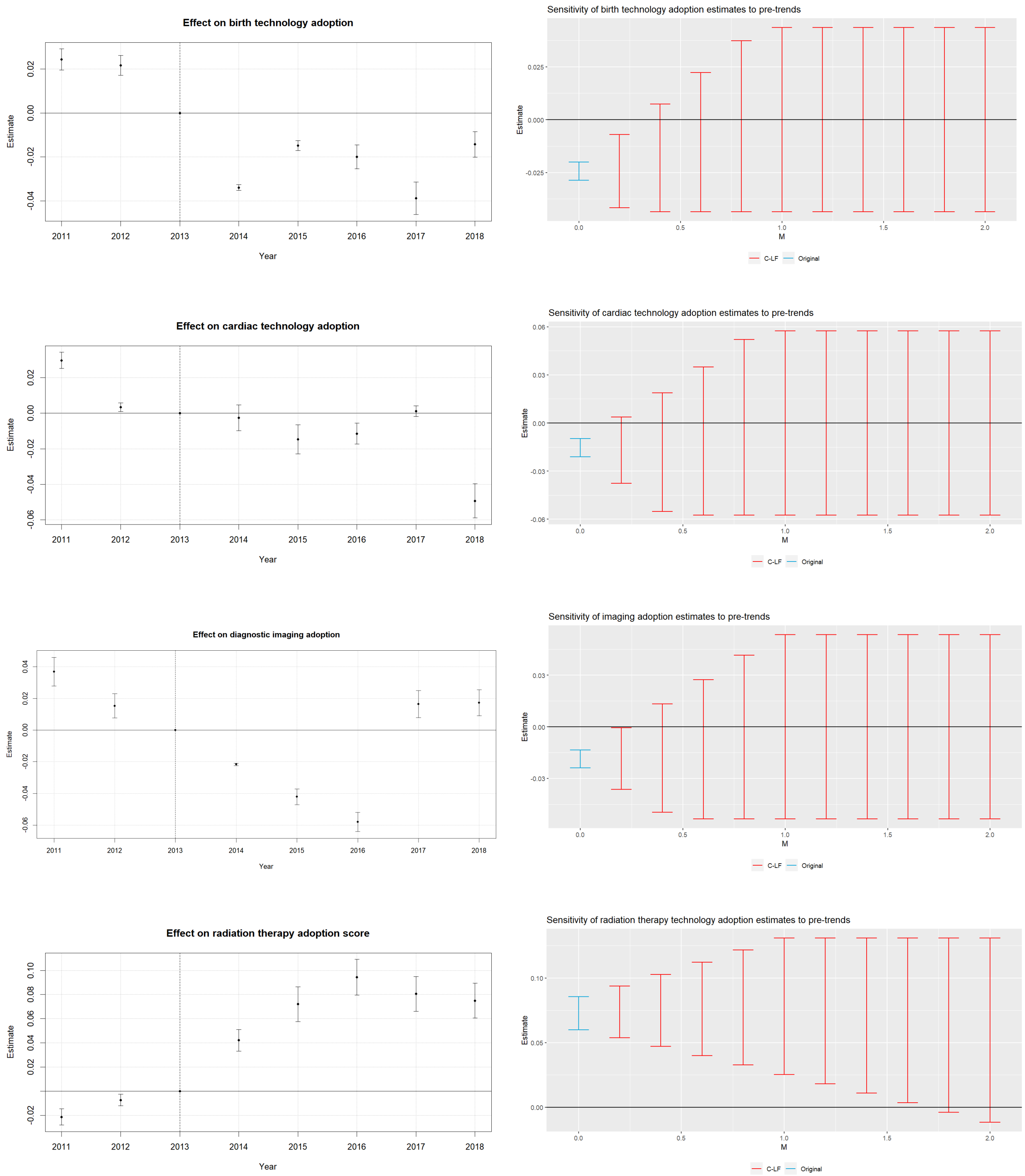

Figure 3.A.3: Estimates for equation (2). 95% confidence intervals are shown. Plots (b), (d), (f), and (h) report a sensitivity analysis using Rambachan and Roth (2022) HonestDiD package, as described in other figures. The dependent variables are birth technology adoption (a and b), cardiac technology adoption (c and d), diagnostic imaging adoption (e and f), and radiation therapy adoption (g and h). I construct conditional least-favorable hybrid confidence sets (C-LF).

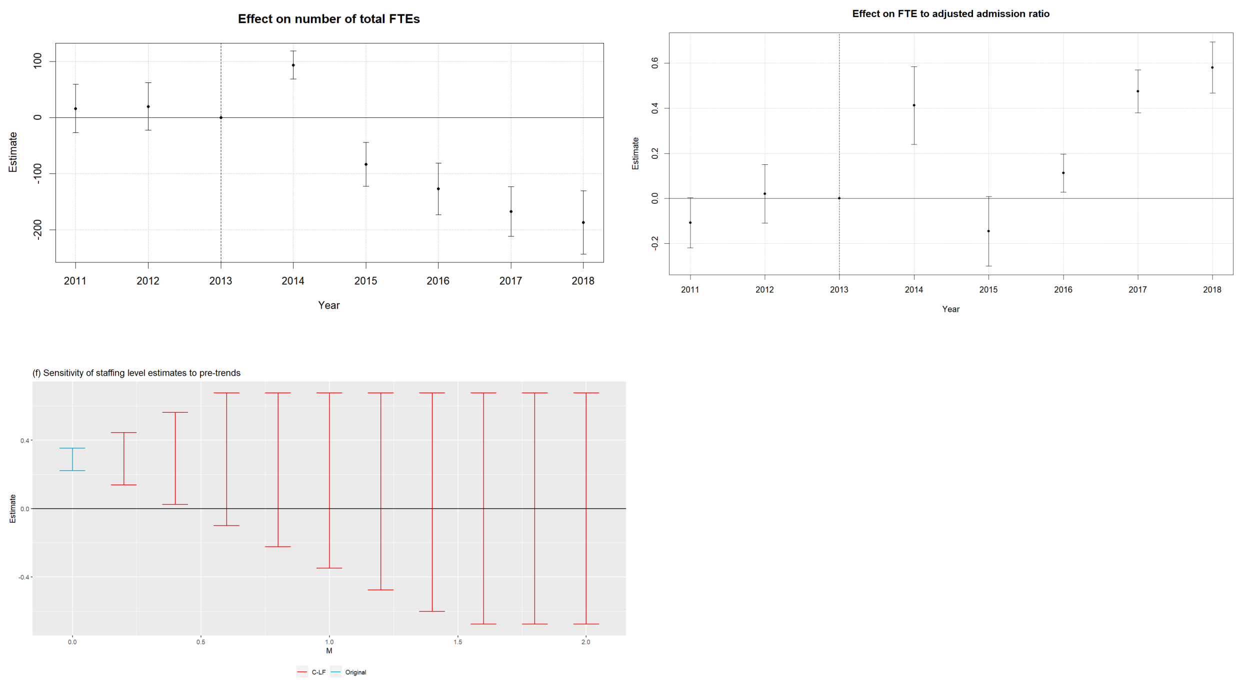

Figure 3.A.4: Estimates for equation (2). 95% confidence intervals are shown. Plot (c) reports a sensitivity analysis using Rambachan and Roth (2022) HonestDiD package. The confidence interval on the left-hand side of figure (c) is the estimate assuming no pre-trends. The other confidence intervals impose assumptions on the relationship between the largest pre-trend in the pre-treatment period and the trends in the post-treatment period. The x-axis represents the scaling of the largest pre-treatment trend to the assumed post-treatment trend. The dependent variables are the number of total FTEs (a and c), and the ratio of total FTEs to adusted admissions (b). I construct conditional least-favorable hybrid confidence sets (C-LF).

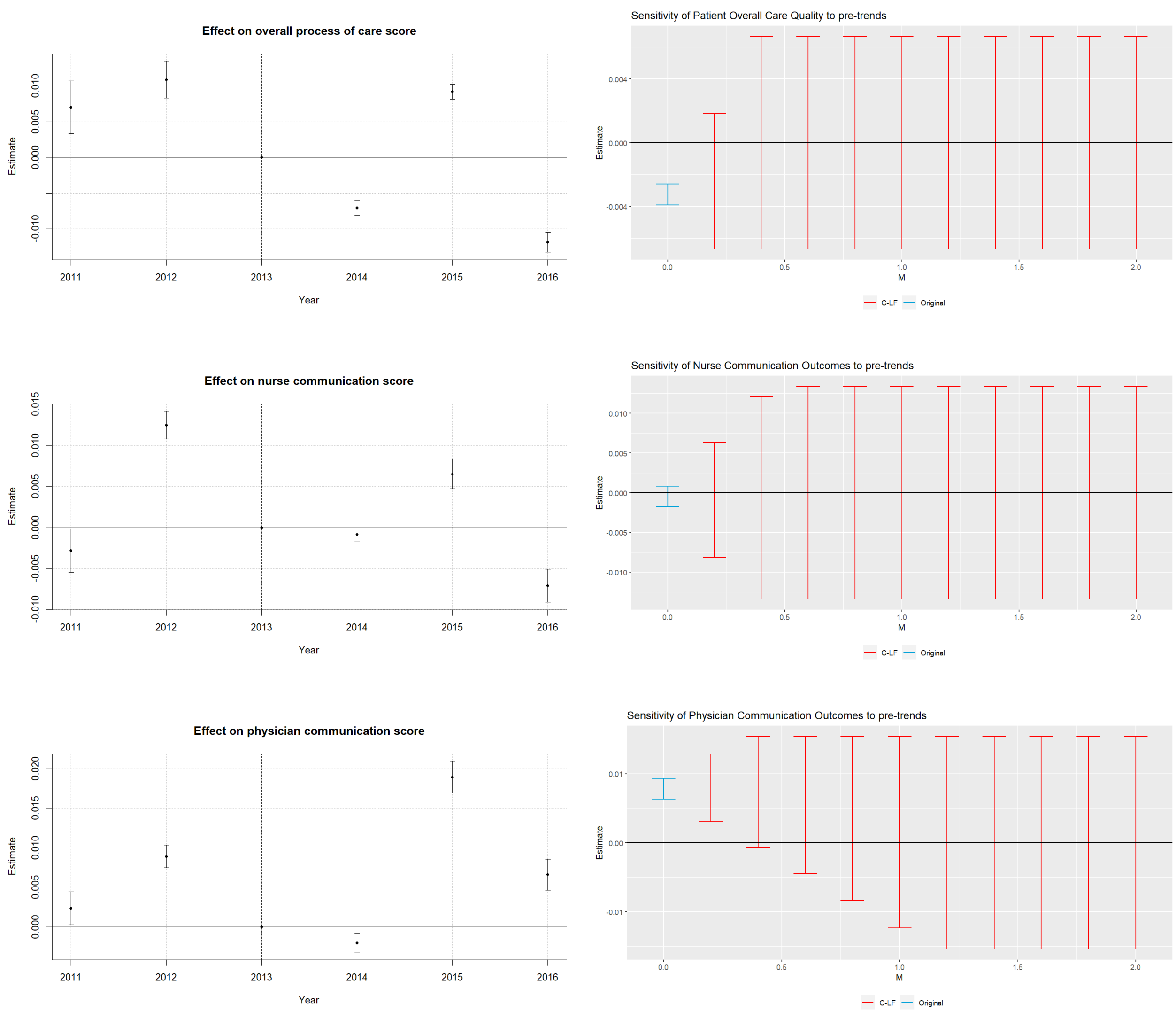

Figure 3.A.5: Estimates for equation (2). 95% confidence intervals are shown. Plots (b), (d), and (f) report a sensitivity analysis using Rambachan and Roth (2022) HonestDiD package. The confidence interval on the left-hand side of each subfigure is the estimate assuming no pre-trends. The other confidence intervals impose assumptions on the relationship between the largest pre-trend in the pre-treatment period and the trends in the post-treatment period. The x-axis represents the scaling of the largest pre-treatment trend to the assumed post-treatment trend. The dependent variables are the 3-point overall care score (a and b), the 3-point nursing communication score (c and d), and the 3-point physician communication score (e and f). I construct conditional least-favorable hybrid confidence sets (C-LF).

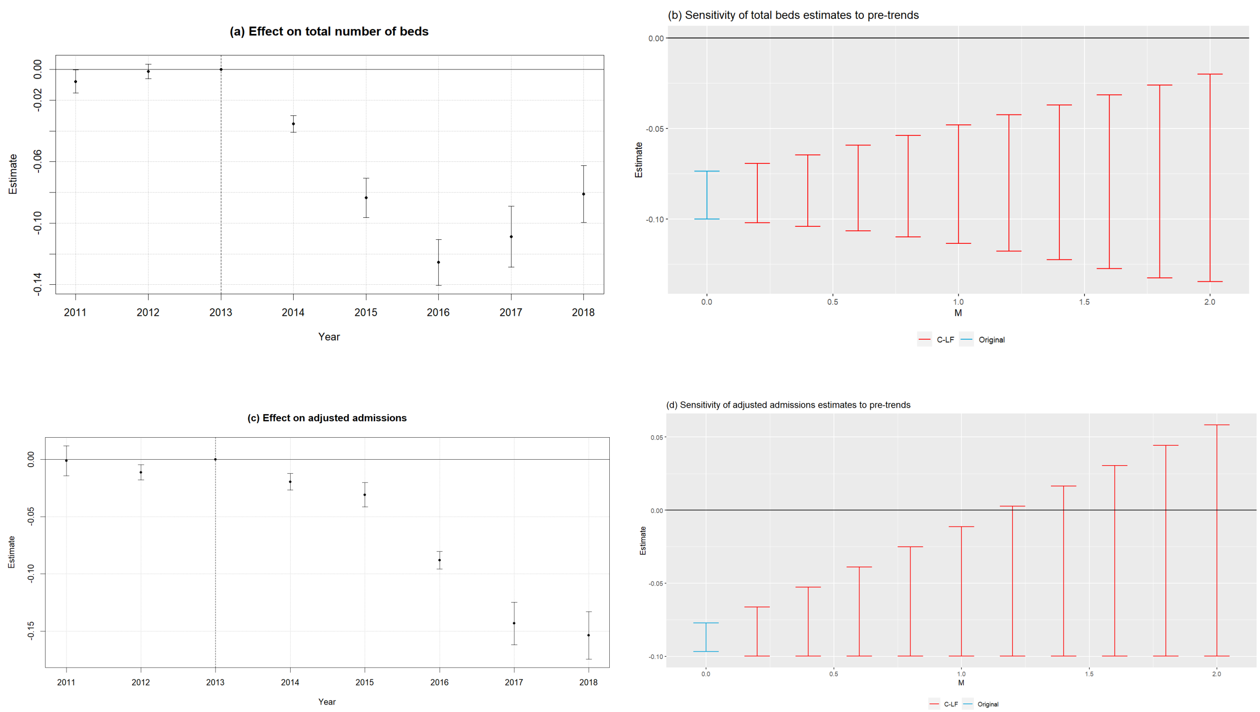

Figure 3.A.6: Estimates for equation (2). 95% confidence intervals are shown. Plots (b) and (d) report a sensitivity analysis using Rambachan and Roth (2022) HonestDiD package. The confidence interval on the left-hand side of each subfigure is the estimate assuming no pre-trends. The other confidence intervals impose assumptions on the relationship between the largest pre-trend in the pre-treatment period and the trends in the post-treatment period. The x-axis represents the scaling of the largest pre-treatment trend to the assumed post-treatment trend. The dependent variables are the natural logarithm of hospital-level beds (a and b) and the natural logarithm of hospital-level adjusted admissions (c and d) where adjusted admissions are hospital admissions scaled by the outpatient to inpatient revenue ratio. Observations are un-weighted. I construct conditional least-favorable hybrid confidence sets (C-LF).

A.4.1 Unweighted volume estimates

In Figure 3.A.6, I present difference-in-difference estimates for unweighted hospital-level effects of global budgets on volume. In plot (a), there is a reduction in total beds after global budgets are implemented. In plot (c), there is a reduction in adjusted admission volume after treatment. Due to the presence of pre-trends, I test the sensitivity of the results to the presence of pre-trends.

In plot (b), I show that the estimated decline in bed volume is robust to a differential trend of 2.0 times the size of the largest pre-treatment trend. Meanwhile, as shown in plot (d), the estimated decline in adjusted admissions is robust to a differential trend of 1.0 times the size of the largest pre-treatment trend. Results are directionally similar but are more robust to pre-trends as compared to the inverse-propensity score weighted outcomes in my preferred specification in Section 4.1.

In Figure 3.A.7, I present difference-in-difference estimates for the causal effects of global budgets on volume on the hospital service area level, as in (Duggan et al., 2023). In plot (a), there is a reduction in bed volume after global budgets are implemented. In plot (c), there is a reduction in adjusted admission volume after treatment. Due to the presence of pre-trends, I test the sensitivity of the results to the presence of pre-trends.

In plot (b), I show that the estimated decline in bed volume is robust to a differential trend of 1.4 times the size of the largest pre-treatment trend. Meanwhile, as shown in plot (d), the estimated decline in adjusted admissions is robust to a differential trend of 0.2 times the size of the largest pre-treatment trend. Therefore, I cannot rule out a zero effect of global budgets on adjusted admissions. These results are similar to the hospital-level results for changes in volume.

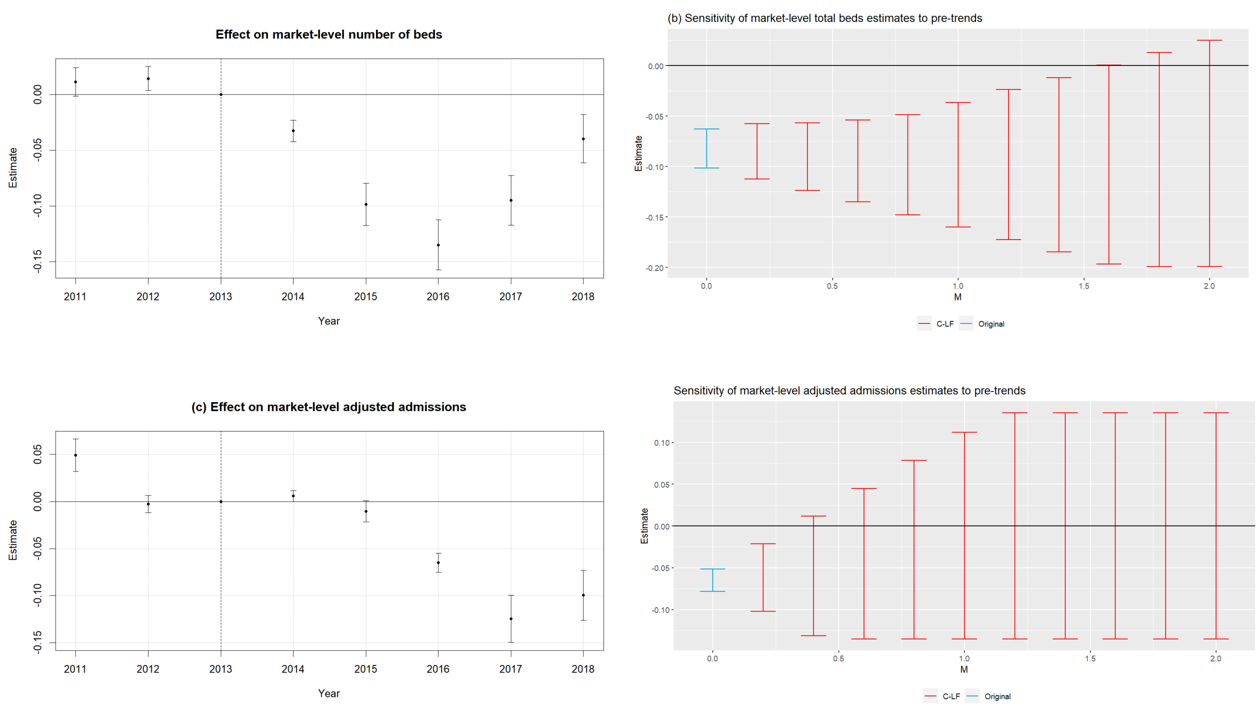

Figure 3.A.7: Estimates for equation (2). 95% confidence intervals are shown. Plots (b) and (d) report a sensitivity analysis using Rambachan and Roth (2022) HonestDiD package. The confidence interval on the left-hand side of each subfigure is the estimate assuming no pre-trends. The other confidence intervals impose assumptions on the relationship between the largest pre-trend in the pre-treatment period and the trends in the post-treatment period. The x-axis represents the scaling of the largest pre-treatment trend to the assumed post-treatment trend. The dependent variables are the natural logarithm of market-level beds (a and b) and the natural logarithm of market-level adjusted admissions (c and d) where adjusted admissions are hospital admissions scaled by the outpatient to inpatient revenue ratio. I construct conditional least-favorable hybrid confidence sets (C-LF).

In Figure 3.A.8, I present difference-in-difference estimates for unweighted hospital-level effects of global budgets on mortality outcomes. In plot (a), there is an increase in heart failure mortality, although pre-trends are present. In plot (c), there is an increase in pneumonia mortality with the presence of pre-trends. In plot (e), there is no apparent change in AMI mortality after global budgets are implemented. Due to the presence of pre-trends, I test the sensitivity of the results to the presence of pre-trends.

In plot (b), I show that the estimated increase in heart failure mortality is robust to a differential trend of 1.4 times the size of the largest pre-treatment trend. As shown in plot (d), the estimated increase in pneumonia mortality is robust to a differential trend of 1.6 times the size of the largest pre-treatment trend. With the exception of 30-day heart failure mortality, mortality results are similar as compared to the inverse-propensity score weighted outcomes in my preferred specification in Section 4.4. Heart failure mortality significantly increases with an unweighted differences-in-differences model, while in my preferred weighted specification, there is no significant change to heart failure mortality from global budgets.

Aside from volume and mortality outcomes, results for other outcomes are similar between weighted and unweighted results. These results are available upon request.

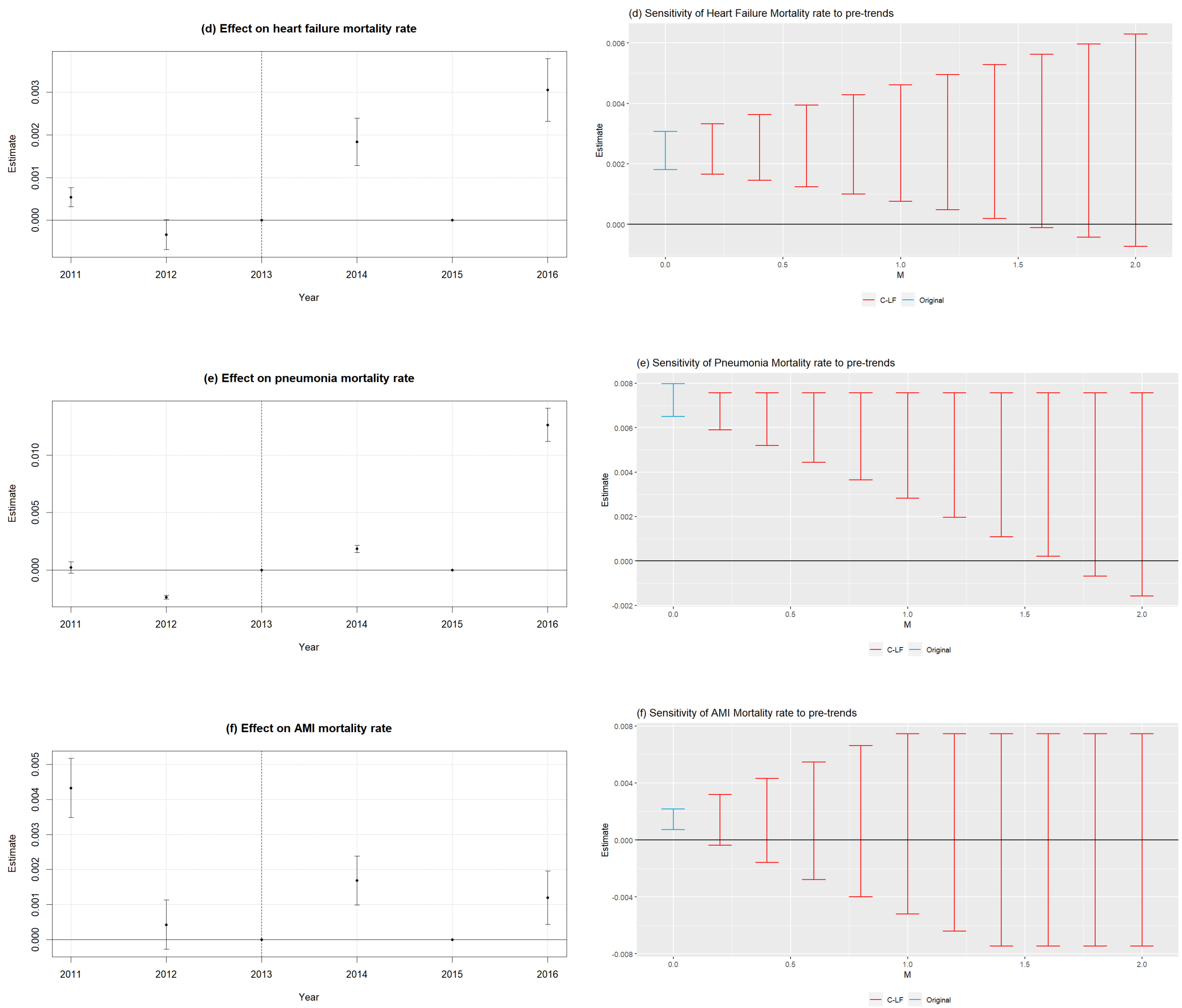

Figure 3.A.8: Estimates for equation (2). 95% confidence intervals are shown. Plots (b), (d), and (f) reports a sensitivity analysis using Rambachan and Roth (2022) HonestDiD package. The confidence interval on the left-hand side of each subfigure is the estimate assuming no pre-trends. The other confidence intervals impose assumptions on the relationship between the largest pre-trend in the pre-treatment period and the trends in the post-treatment period. The x-axis represents the scaling of the largest pre-treatment trend to the assumed post-treatment trend. The dependent variables are the 30 risk-adjusted heart failure mortality rate (a and b), the 30 risk-adjusted pneumonia mortality rate (c and d), and the 30 risk-adjusted AMI mortality rate (e and f). I construct conditional least-favorable hybrid confidence sets (C-LF).

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Bachelor of Science, Economics and Applied Mathematics

RESEARCH INTERESTS

Healthcare Innovation, Patient and Clinician Payment Models, Health Policy, Public Economics, Industrial Organization

PUBLICATIONS

Long-haul COVID: Healthcare Utilization and Medical Expenditures 6 Months Post-diagnosis (*BMC Health Services Research*, 2022) Antonios Koumpias, **David Schwartzman**, Owen Fleming

Local Supply of Post-Discharge Care Options Predicts Hospital Readmission Rates (*Health Affairs*, 2022) Kevin Griffith, **David Schwartzman**, Stephen Pizer, Jacob Bor, Vijaya Kolachalama, Brian Jack, Melissa Garrido

Refining the Recipe for Alternative Payment Models for Surgical Care - Importance of Patient Mix and Venue Match (*JAMA Network Open*, 2021) **David Schwartzman**, Kyle Sheetz, A Mark Fendrick

Public policy and economic dynamics of COVID-19 spread: a mathematical modeling study (*PLOS One,* 2020) Uri Goldsztejn, **David Schwartzman**, Arye Nehorai

WORKING PAPERS

The Interaction of Patient and Clinician Incentives: Evidence from Direct Primary Care
Abstract: *Little is known about the optimal design of healthcare payment models in the presence of market frictions to targeting benefits. I leverage a natural experiment where an employer introduces Direct Primary Care, a form of care delivery where clinicians are paid a capitated monthly fee for high-touch access to a bundle of primary care services. This model modifies moral and behavioral hazard incentives for both patients and clinicians while changing the trade-off between preventive investments and downstream care. I document selection into Direct Primary Care by younger and less costly employees, who have lower primary care spending and shorter job tenures, suggesting the importance of switching costs. Using an instrumental variables approach that leverages plan inertia and a difference-in-difference strategy, I examine the impact on costs of care and demand for preventive care and low-value cardiac imaging. Patient out-of-pocket costs increase for those choosing the Direct Care Plan, and total costs increase for lower spenders but not for more expensive patients. I also find a decrease in potentially low-value imaging and an increase in high-value mammography screening, suggesting that quality improves. My findings suggest the importance of considering market frictions to targeting models when designing payment incentives for patients and clinicians.*

David Schwartzman

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Value-based Payment Models and Health System Investments: Evidence from Maryland

Abstract: *Optimal design of payment models to reduce health care costs and improve health care quality demands increasing attention in the face of rising medical spending. Mechanisms by which payment models can improve outcomes are of particular interest. I focus on health system responses to changes in payment incentives, studying the Maryland global hospital budget model, which previous evaluations have shown promise in reducing costs. However, the mechanisms by which improved outcomes were achieved have not been studied. Using a dynamic difference-in-difference model and testing the sensitivity of results to the presence of pre-trends, I examine changes in health system investments following the adoption of the global hospital budget model. I find a decrease in volume, a shift from clinical to non-clinical employees in Maryland relative to other states, and an increase in the administrative share of spending, suggesting a shift to lower-skilled workers and a change in care functions. Heart failure readmissions fall and pneumonia mortality increases, while technology adoption is unchanged. My findings highlight the value of linking program evaluation to mechanisms in health care to improve the design of future payment models.*

Direct Primary Care: Practice Distribution and Cost across the United States (with Ross Klosterman and Namrata Ramakrishna)

Abstract: *Little is known about the growing clinician movement known as Direct Primary Care (DPC). We use national data from several online directories and the websites of DPC practices to identify Direct Primary Care practices and to assess the factors associated with service price and with physical location. We find that the average adult price charged by a DPC practice is $81.33 per month. Median income and not accepting children as patients are associated with higher prices. Offering more services is not associated with higher prices, and medication dispensing is associated with lower prices. Lower poverty rate, lower percentages of black residents, higher education status, and passing a state law related to DPC is associated with more physical locations. Our results constitute the first examination of the factors associated with the price and location of DPC practices, which has broader implications for access to care and organizational care delivery structure in health care.*

WORKS IN PROGRESS

Care and Distributional Implications of Value-Based Employer Health Insurance Programs: Evidence from a Bundled Payment Program (with Barton Hamilton, Cecilia Diaz-Campo, and A. Mark Fendrick)

Firm-Specific Human Capital Investment in Health and Retention: Evidence from On-site Clinics

Characteristics of Assistant Physicians and Impact on Health Professional Supply: Evidence from Missouri (with Timothy McBride)

Medicare Opt-Outs by Specialty Since the Affordable Care Act

REVIEWING

JAMA Network Open, Health Economics

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PROFESSIONAL MEMBERSHIPS

American Society of Health Economists
National Association of Business Economists
Center for the Study of Race, Ethnicity & Equity - Graduate student affiliate

PRESENTATIONS

2023: University of Pennsylvania Payment Insights Team, Abt Associates, RTI International
2022: American Society of Health Economists Conference (poster presentation), Cato Junior Academics Health Policy Symposium, Washington University Collaborative on Administrative Data, Health Services, and Policy Research Meeting
2021: Michigan Medicine VBC/ACO Working Group
2020: Saint Luke's Hospital Grand Rounds

RESEARCH EXPERIENCE

Research Assistant to Barton Hamilton, Olin Business School June 2019 - December 2020

TEACHING EXPERIENCE

Teaching Assistant, Olin Business School, Washington University in Saint Louis
Research in Healthcare Management (Spring 2021, Spring 2022, Spring 2023) - Undergraduate Capstone Course

Teaching Assistant, Olin Business School, Washington University in Saint Louis
Competitive Industry and Strategy Development (Fall 2021) - MBA Core Course