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Essays in Health Economics

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ABSTRACT

ESSAYS IN HEALTH ECONOMICS

BY

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This dissertation consists of three chapters, each of which examines a different topic within the sphere the health economics.

In the first chapter, I use unique, proprietary medical practice data from 2019 to investigate the relationship between physicians, various categories of non-physician clinical staff, and other non-labor inputs in the production of patient office visits. Preliminary results suggest that, for some inputs, their marginal productivity has fallen over time. Cross-input elasticities generally match in terms of their historical classification as either compliments or substitutes, although the magnitudes of the elasticities have also fallen over time. One possible interpretation of these results is that medical practices have already adapted to changes in the economic, regulatory, and technological environment in which they practice and have achieved the easy efficiency gains that were once readily available to them.

In the second chapter, I use 17 years of hospital cost report data and a difference-in-differences identification strategy to examine the financial performance and utilization of safety-net hospitals in Massachusetts following the state's 2006 reform. The results suggest the largest safety-net hospitals experienced a decline in patient revenue because of the reform and may have responded by transferring operations from inpatient facilities to outpatient centers as a cost-cutting maneuver. Other safety-net hospitals, however, did not experience the same decline in

patient revenue. Should states need to reduce their supplemental payments to safety-net hospitals as part of national health care reform, these results suggest they should target their remaining funds to their most financially vulnerable safety-net hospitals.

The final chapter, co-authored with James Marton and Benjamin Ukert, evaluates the impact of the Affordable Care Act Medicaid expansion on health insurance coverage, access to care, and self-reported health for individuals with and without chronic conditions. Using five years of post-reform data (2014–2018) from the Behavioral Risk Factor Surveillance System and a difference-in-differences identification strategy, we find that the reform led to improvements in access to care and self-reported health for both groups. Although these improvements are mostly larger in magnitude for individuals with chronic conditions, the differences in magnitude are not statistically significant.

ESSAYS IN HEALTH ECONOMICS

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of the Requirements for the Degree
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ACCEPTANCE

This dissertation was prepared under the direction of Derek Ryan Hoodin's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

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Introduction

The health care industry is in a constant state of transition. It is always adapting to new legislation, new health care technology, and to changes in the underlying health of the population it serves. The three chapters in this dissertation focus on how just some of these changes have transformed health care, for both providers and patients. The first chapter explores the organization and staffing of doctor offices. The second chapter examines the short- and long-term effects of the 2006 Massachusetts health care reform on the financial performance and utilization of the state's safety-net hospitals. Lastly, the third chapter investigates how the 2010 Affordable Care Act impacted access to care and self-reported health outcomes for individuals with chronic conditions.

The Affordable Care Act conferred insurance to millions of Americans just as the baby boomer generation began to retire. As a result, many health policy analysts have raised the alarm that the US health care system is unprepared to meet growing patient demand. One potential avenue to meet this growing demand is to revisit the organization and staffing of doctor offices. Because of a lack of available US data, the most recent studies on this topic use data from the 1980s, and physicians today practice medicine in a very different economic, regulatory, and technological environment than they did 40 years ago. The first chapter in this dissertation therefore uses unique, proprietary medical practice data from 2019 to explore the relationships between physicians, various categories of non-physician clinical staff, and other non-labor inputs in the production process for patient office visits.

Preliminary results suggest that, for some inputs, their marginal productivity has fallen over time. Similarly, while many of the cross-input elasticities match in terms of their historical classification as either complements or substitutes, their magnitude has also fallen over time.

One possible interpretation of these results is that medical practices have already adjusted the size and skill mix of their clinical workforce, in addition to making other organizational improvements, to achieve the easy efficiency gains that were once readily available to them. Policymakers will want to use this information as they consider different methods to increase health care capacity, such as strengthening government support for graduate medical education or adjusting the scope-of-practice laws that limit the autonomy of non-physician clinical staff.

The second chapter revisits the 2006 Massachusetts health care reform in order to gain some insight into the likely consequences of one of the provisions of the Affordable Care Act that will soon be enacted. Specifically, the federal government is scheduled in 2021 to cut $4 billion in supplemental payments to safety-net hospitals, which are those that have traditionally cared for many low-income and uninsured patients. Further reductions of $8 billion are set to occur every year from 2022 through 2025. These payments are being withdrawn to help pay for the Medicaid expansions and the subsidies on the insurance exchanges. However, not every state has decided to expand Medicaid, and this leaves their so-called safety-net hospitals financially vulnerable.

Previous studies that examined safety-net hospitals in Massachusetts following the state's reform limited to their analysis to a select handful of safety-net hospitals and only had one or two years of post-reform data at their disposal. By contrast, I examine the impact of the reform using multiple sets of criteria for identifying safety-net hospitals and employ a much longer time frame. The results suggest the largest providers of uncompensated care did suffer a financial setback because of the reform and, in an attempt to restore their financial health, may have responded by transferring operations to outpatient settings. Other safety-net hospitals, however, do not appear to have suffered financial losses to the same degree. When the federal reductions occur, states may wish to target their remaining funds to their largest safety-net hospitals. Similarly, non-expansion states may wish to reconsider adopting the Medicaid expansion.

The third chapter, co-authored with James Marton and Benjamin Ukert, evaluates the impact of the Affordable Care Act Medicaid expansion on health insurance coverage, access to care, and self-reported health outcomes for vulnerable and chronically ill individuals using data from the Behavioral Risk Factor Surveillance System. We focus on these individuals because their chronic conditions may have prevented them from obtaining health insurance and accessing health care before the reform. Using five years of post-reform data between 2014–2018 and a difference-in-differences identification strategy, we find that the expansions led to improvements in access to care among both those with and without chronic conditions. While the magnitude of these improvements are mostly larger for those with a chronic condition, the differences in magnitude are not statistically significant. We also find statistically significant improvements in self-assessed health for those without chronic conditions. Finally, we find larger improvements in access to care among those with chronic conditions in states with higher-than-average pre-reform uninsured rates, though these gains in access did not translate to improvements in self-assessed health for this group.

Economists have long documented many unique features of the health care industry which make it distinct from other sectors of the economy. Some examples include the asymmetric information between patients and insurers and between patients and providers, as well as the positive and negative externalities of good and bad health, among other market failures. Policymakers often introduce new pieces of legislation with the aim of minimizing the costs associated with these market failures. Providers then adjust their operations in response to the new policies or other changing dynamics within the health care landscape. When health policy professionals speak of the Triple Aim, they are referring to improving the patient experience, improving the health of populations, and reducing the per capita costs of health care. As the industry evolves, new research will always be needed to ensure any changes deliver on the Triple Aim, and the three chapters in this dissertation are written firmly in that spirit.

Chapter 1

Medical Practice Staffing and the Production of Office Visits

I. Introduction

For over a decade, there has been widespread concern that the American health care system is unprepared to meet growing patient demand. In 2008, the Bureau of Health Professions released a report arguing that the overall demand for medical services would grow faster than the supply (HRSA 2008). A 2013 report reached the same conclusion, and numerous health policy experts and industry professionals have made similar predictions (Hofer, Abraham, and Moscovice 2011; Kirch, Henderson, and Dill 2012; Petterson et al. 2012; 2015; Salsberg 2013; Zhang et al. 2020; AAMC 2019). For example, Petterson et al. (2015) expect there will be a shortage of more than 33,000 primary care physicians by 2035. In addition, other studies have suggested a nursing shortage of 918,232 by 2030 (Juraschek et al. 2019).

These predictions are based on concurrent changes occurring in the supply and demand for health care.[1] On the demand side, the 2010 Affordable Care Act conferred insurance to 60 million Americans just as the Baby Boomer generation began to retire (Obama 2016; Vespa, Medina, and Armstrong 2020). On the supply side, the physicians and nurses are also older and, due to increasing administrative tasks, many are suffering from burnout and working fewer hours than they did in the past (Aiken, Cheung, and Olds 2009; Juraschek et al. 2019; Rao et al. 2017; Staiger 2010). The nursing profession, which is predominately female, also suffers from high turnover, attrition related to career and family choices, and a faculty shortage in nursing schools (Aiken, Cheung, and Olds 2009).

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[1] For an overview of the methods used to make these projections, see Lopes, Almeida, and Almada-Lobo (2015).

Perhaps the clearest way to increase health care capacity is to increase the supply of doctors, as they are often seen as the main input in the production process for health care. Indeed, the 115th Congress introduced several pieces of legislation designed to increase federal support for graduate medical education (see H.R. 2267, S. 1301, and H.R. 6056). Producing more doctors, however, is a long and expensive process: a medical student must complete four years of undergraduate education, then four years of medical school, and then three to five more years of residency training, depending on the specialty (MEDPAC 2009).

As an alternative to producing more physicians, others have suggested that non-physician labor can be used more effectively. Alleviating the shortage, they say, may require greater reliance on nurse practitioners and physician assistants—staff sometimes referred to as advanced practice providers (APPs) (Auerbach et al. 2013; Sarzynski and Barry 2019). For example, Bodenheimer and Smith (2013) write that "Primary care practices could greatly increase their capacity to meet patient demand if they reallocate clinical responsibilities… to nonphysician team members and to patients themselves." Surveys of patient preferences indicate that patients may be willing to receive more care from other clinical staff (Dill et al. 2013).

Compared to nurses, APPs are more highly trained clinical staff. They carry out many tasks similar to doctors while working with them but not independently from them (Sibbald, Laurant, and Scott 2006). Numerous studies indicate APPs can provide the same quality of care as doctors when handling first-time patient encounters and treating more common illnesses (Horrocks 2002; Kurtzman and Barnow 2017; Lovink et al. 2017; Yang et al. 2018). It is worth noting that the studies described above focus exclusively on labor inputs, rather than considering both labor and non-labor inputs, such as capital or technology. Very little research has directly examined the extent to which the labor inputs of a modern medical practice (including physicians, APPs, registered nurses (RNs), licensed practical nurses (LPNs), certified nurse assistants (CNAs), etc.) as well as non-labor inputs (capital, technology) may serve as compliments or substitutes in the production of office visits.

This study estimates a production function for patient office visits in order to examine the complementarity and substitutability of various labor and non-labor inputs in the production process. Uwe Reinhardt was the first researcher to apply production function theory to patient office visits in the US in this way (Reinhardt 1972). In his seminal paper, he models office visits as a function of physician time, other labor inputs, and office characteristics. His results show that offices could, at the time of his study, profitably increase their patient volume by substituting doctor time with more nurse time.

Thurston and Libby (2002) revisit Reinhardt's study using data from the 1980s and a new production function. Their chosen function, the generalized linear production function developed by Diewert (1971), is written as a series of cross-products wherein each input is separately multiplied by itself and all other inputs. These cross-product terms allow Thurston and Libby (2002) to examine complementarity in the production process. For example, of the ten input pairs they consider, they find six are complements and four are substitutes. More recent studies tend to rely on the empirical framework developed by Reinhardt (1972) and Thurston and Libby (2002), but use data from outside the United States (Olsen et al. 2013; Sarma, Devlin, and Hogg 2009).[2]

My study estimates the same production function as Thurston and Libby (2002) but uses unique, proprietary data on today's medical practices provided by the American Medical Group Association (AMGA). These data are more granular than the data used in previous studies, and the more detailed information is leveraged on both sides of the production function. First, output

[2] Older studies that use Reinhardt's production function include Brown (1988), Hurdle and Pope (1989), Gaynor and Pauly (1990), Headen (1991), DeFelice and Bradford (1997), and Conrad et al. (2002).

is measured as both patient office visits and work relative value units (RVUs, a productivity measure used by Medicare to calculate physician fees). Second, three separate models of increasing specificity are estimated for each output measure. In the first model, APPs and nurses are combined into one category in the production function. This is done so that the estimates can be easily compared to the earlier estimates from Thurston and Libby (2002). In the second model, APPs and nurses are separated into their own categories. Finally, in the third model, nurses are subdivided even further into three categories (RNs, LPNs, and CNAs).

Preliminary results using data from 2019 show that regression-based estimates of marginal productivity have declined for some inputs over time. For example, in the 1980s one additional hour of physician time was associated with one and one-third additional patient office visits per week (Thurston and Libby 2002). Now, one additional hour of physician time is associated with less than a tenth of a visit. Similarly, when nurses and APPs are combined into one category, the hiring of an additional nurse is associated with slightly more than four office visits per week. Before, the hiring of an additional nurse was associated with over seven and a half more visits (Thurston and Libby 2002). When work RVUs are used as the output measure, the results are generally similar but larger in magnitude.

The 2019 results also indicate the productive relationships between the inputs may be changing over time. If two inputs are q-complements, then the presence of one raises the productivity of the other. Alternatively, if two inputs are q-substitutes, then the presence of one lowers the productivity of the other. Input pairs which may be q-complements based on the preliminary results of this study include physician time and technicians, physician time and nurses, and APPs and LPNs. However, depending on the model, the degree of complementarity or substitutability has declined over time relative to the findings reported in Thurston and Libby

(2002). Together, the results suggest that over the past 40 years medical practices have realized the efficiency gains that were once readily available.

This study provides insight into the complex relationship between various inputs in the production of office visits at a time of increased stress on the health care system. My findings should be of particular interest to medical practices as they continue to search for strategies to produce to both produce more visits and reduce costs. In addition, policymakers will want to use this information when they weigh the costs and benefits of expanding the supply of medical personnel or adjusting the laws and regulations that proscribe what work can be performed by whom.

II. Data

The data for this study come from AMGA's 2019 Medical Group Operations and Finance Survey. AMGA is a trade association representing large, multispecialty medical groups. The operations and finance survey is a practice-level survey containing information on patient access, office operations, revenue and expenses, and staffing. The 2019 edition of the survey is the most recent and detailed edition available. These data will be supplemented with data from the 2020 edition upon its release.

The operations and finance survey is weighted towards the large, multispecialty medical groups that AMGA serves. Unlike previous studies which focus primarily on independently owned and operated single physician practices, the unit of analysis in this study is a practice, typically employing several physicians, located within a medical group.[3] In most cases, there are multiple practices per medical group and, as mentioned, many physicians working within each

[3] For example, only 12% of the physicians surveyed in the 1988 data that Thurston and Libby use are employed by a group practice.

practice. Although the operations and finance survey does not generalize to the universe of medical practices, there is a growing trend of medical practices consolidating into larger groups (Kirchhoff 2013).

Physician survey data of this kind are extremely rare (Berk 2016). In the mid-1990s, the Healthcare Financing Administration (HCFA) discontinued its survey of physician offices. Since then, neither the Centers for Medicare and Medicaid Services (CMS, the successor to HCFA) nor any other government agency has collected similar data. This is perhaps one reason why more recent studies rely on non-US data. It also explains why the study by Thurston and Libby was published in 2002 using data from the mid-1980s. AMGA is graciously providing access to their current data under a special agreement specifically written for this study.

II.A Output Measures

Two measures of medical practice output are used: total weekly patient office visits and total weekly work RVUs. The RVUs are a productivity measure used to calculate Medicare payments. In the mid-1980s, HCFA became concerned about growing Medicare costs and low reimbursement rates for primary care physicians. To help alleviate these concerns, the agency tasked Harvard economist William Hsiao to develop a new payment scheme. In response, Hsiao and his colleagues created the Resource-Based Relative Value System (RBRVS).

The system is designed such that each medical procedure is given a code (called a Current Procedure Terminology or CPT code). Each code corresponds to a certain number of RVUs of a specific type. The three types are for work effort, practice expense, and liability insurance. The Medicare payment equals the total RVU multiplied by a dollar conversion factor, which is adjusted for geographic variations in costs. The RVUs are designed to reflect the resource usage of any one service relative to the resource usage of other services. Congress did not authorize RBRVS until 1989 and the system was not fully phased in until 1992. Today, most private payers also rely on the system, and many physicians use the work RVUs to measure their productivity (Clemens and Gottlieb 2017; Smith 2015).[4]

II.B Input Measures

All three models estimated in this paper include physician time, technicians, office aids, and capital as inputs in the production process of office visits. Physician time is defined as the sum of all the hours worked per week by every physician within a given practice. This differs slightly from the earlier studies (Reinhardt 1972; Thurston and Libby 2002) wherein physician time was defined as the number of hours worked per week by the physician owner being surveyed. Even so, this previous definition still captured the total hours worked per week in practices where the physician owner was the only doctor (i.e. the vast majority of practices at the time). Moreover, for both definitions, a one-unit change in this input is interpreted as one additional hour of physician time.

Technicians refers to radiology/imaging staff and laboratory staff, and office aids includes referral coordinators, medical receptionists, and office call center staff. These labor categories were defined this way to be as similar as possible to the earlier studies. This way, any changes in the results should reflect the underlying changes in the productive relationships. All labor inputs (including APPs and nurses) are measured as the number of full time equivalent (FTE) employees normalized by the number of FTE physicians working at the office. One FTE is equivalent to 2,080 hours of paid time per year, inclusive of vacation and holiday time. Capital

[4] For more information on RBRVS, see Laugesen (2014) and Smith (2015).

refers to the yearly rental and depreciation costs of office space and equipment. All models also include as controls the number of physicians per 1,000 population and an indicator for specialty.[5]

The three models are differentiated by how APPs and nurses enter the production function. APPs refers to nurse practitioners and physician assistants. These are the most highly trained non-physician clinical staff. Generally, their autonomy varies according to state scope-of-practice laws, but in most states, they have full authority to prescribe prescription drugs. Registered nurses (RNs) are the next most highly trained staff, followed by licensed practical nurses (LPNs), and then nurses aids (hereafter referred to as certified nursing assistants or CNAs).

III. Methods

The goal of this study is to examine the extent to which the labor inputs of a modern medical practice as well as non-labor inputs may serve as compliments or substitutes in the production of office visits. To do so, this study follows the previous literature by estimating a production function for office visits. This function expresses one output as a function of one or more inputs. This relationship can be written without a specific functional form as:

$$Q = F(\boldsymbol{X}) = f[H(z), X_2, X_3, \dots, X_K] \quad (1)$$

where, in this context, Q denotes the number of office visits produced by a medical practice, $H(z)$ is the rate of input of doctor time in the practice, and the elements of the vector $\boldsymbol{X} = (X_1, \dots, X_K)$ are the quantities of other labor and non-labor inputs used at the practice.

[5] The number of physicians per 1,000 population is calculated using data from the Association of American Medical College's (AAMC's) 2019 State Physician Workforce Data Report. See https://www.aamc.org/data-reports/workforce/report/state-physician-workforce-data-report.

The next step is to choose, from a menu of production functions, one that, for theoretical reasons, best describes the production process. Most of the commonly used production functions satisfy the following four properties:

1. *Nonnegativity:* $F(\boldsymbol{X})$ is a finite, non-negative, and real-valued number.
2. *Strong Essentiality:* $F(\boldsymbol{X})$ requires some of every input in the production process.
3. *Monotonicity:* If $\boldsymbol{X}^1 \geq \boldsymbol{X}^0$ then $F(\boldsymbol{X^1}) \geq F(\boldsymbol{X^0})$. In other words, additional units of an input will increase output.
4. *Concavity:* For vectors $\boldsymbol{X}^1$ and $\boldsymbol{X}^0$, and $0 \leq \theta \leq 1$, $F(\theta\boldsymbol{X}^0 + (1-\theta)\boldsymbol{X}^1) \geq \theta F(\boldsymbol{X}^0) + (1-\theta)F(\boldsymbol{X}^1)$. In other words, a variety of inputs will produce no less output than many of one type of input.

These properties are neither exhaustive nor universal. For example, property (2) is often replaced with *weak essentiality*, meaning $F(\boldsymbol{X})$ only requires at least one input. Property (3) is also often relaxed to *nondecreasing*, whereby additional units of an input will not decrease output.

To conduct my empirical analysis I use the generalized linear production function proposed by Diewert (1971), which is the same function selected by Thurston and Libby (2002). For a vector of K inputs $\boldsymbol{X} = (X_1, \ldots, X_K)$, with $X_0 = 1$, this function is defined as follows:

$$Y = F(X) = F(X_0, \ldots, X_K) = \sum_{i=0}^{K}\sum_{j=0}^{K} \alpha_{ij}\sqrt{X_i}\sqrt{X_j} \qquad (2)$$

where $\alpha_{ij} = \alpha_{ji}$. If $\alpha_{ij} \geq 0$ for $i, j = 1, \ldots, K$ then this function is everywhere nondecreasing and satisfies every condition for a legitimate production function. Even if some $\alpha_{ij} < 0$, it may still be a valid production function so long as the negative coefficients are not too large. Most importantly, this function satisfies *weak essentiality*. This requirement was originally set by Reinhardt, as not every practice will employ every type of labor.

Since the multiplication is commutative, Equation (2) can be simplified slightly:

$$Y = \sum_{i=0}^{K} \sum_{j=i}^{K} \beta_{ij} \sqrt{X_i} \sqrt{X_j} \tag{3}$$

where $\alpha_{ij} = \beta_{ij}$ for $i = j$ and $\alpha_{ij} = \beta_{ij}/2$ otherwise. I estimate three versions of the production function given by Equation (3), which are differentiated by how the non-physician clinical staff are categorized. Formally, the three versions are:

- Model 1: $K = 5$, $\boldsymbol{X} =$\{physician hours, nurses, technicians, office aids, and capital\}.
- Model 2: $K = 6$, $\boldsymbol{X} =$\{physician hours, APPs, nurses, technicians, office aids, and capital\}.
- Model 3: $K = 8$, $\boldsymbol{X} =$\{physician hours, APPs, RNs, LPNs, CNAs, technicians, office aids, and capital\}.

In the first model [Model 1], APPs, RNs, LPNs, and CNAs are combined into one "nurse" category to allow for easy comparison with the previous literature (Reinhardt 1972; Thurston and Libby 2002). In the second model [Model 2], APPs are included in the production function by themselves, while RNs, LPNs, and CNAs are left combined as the "nurse" category. Finally, in the last model [Model 3], APPs, RNs, LPNs, and CNAs are included in the production function separately.

To examine q-complementarity, the Hick's elasticity of complementarity, η^H, is calculated for each input pair following Thurston and Libby (2002). For any two inputs, i, j $(i \neq j)$, the elasticity is defined as:

$$\eta_{ij}^{H} = \frac{Y \times F_{ij}}{F_i \times F_j} \; \forall i \neq j \tag{4}$$

where Y is the output, F_{ij} is the cross-partial for inputs i and j, and F_i and F_j are the marginal products for inputs i and j, respectively.[6] So long as the marginal products are positive ($F_i > 0$), the sign of the elasticity is determined by the sign of β_{ij}. A positive elasticity indicates the two inputs are complements; a negative elasticity indicates the two inputs are substitutes.

IV. Results

Table 1.1 shows a comparison of means for the inputs. The leftmost 1965–67 column shows the values from Reinhardt (1972), while the middle two 1985 and 1988 columns show the values from Thurston and Libby (2002). The rightmost column shows the values for the 2019 AMGA data used in my analysis. Similarly, the top half of the table displays the means for the inputs that are common across all three studies, while the bottom half displays the means for the inputs unique to my analysis.

Table 1.1. Comparison of Means

Variable	1965–67	1985	1988	2019
Common Inputs				
Physician Time	34.12	25.28	25.28	40.00[a]
Capital	\$464	\$35,433	\$55,304	\$455,000[b]
RNs	0.46	0.33	0.35	0.55
Technicians	0.26	0.29	0.49	0.51[c]
Office Aids	1.24	1.53	1.74	0.87[d]
New Inputs				
APPs				1.03
LPNs				0.82
CNAs				1.13
All Nursing Staff including APPs				2.40
All Nursing Staff excluding APPs				1.88

Note: [a] Physician time is defined as the total number of hours worked per week by every physician at the office. It is calculated by multiplying the number of FTE physicians working at an office by 40. [b] Capital values for 1965–67, 1985, and 1988 are adjusted for inflation to 2019 dollars. [c] Technicians does not include pharmacy staff, dietician/nutritionists, behavioral health/social work staff, and other direct patient care support staff. [d] Office aids does not include quality assurance personnel.

[6] $F_i = \sum_{j=0}^{K} \alpha_{ij} \sqrt{\frac{X_j}{X_i}} = \beta_{ii} + \frac{1}{2}\left(\sum_{j=0}^{K} \beta_{ij} \sqrt{\frac{X_j}{X_i}}\right)$ and $F_{ij} = \frac{1}{4}\beta_{ij}\left(\frac{1}{\sqrt{X_i}\sqrt{X_j}}\right)$

Starting at the top of Table 1.1 and reading from left to right, the average number of hours worked per week by a single physician appears to have declined from the mid-1960s to the mid-1980s. Other research has shown this trend continuing into the present (Rao et al. 2017; Staiger 2010). However, in this study, the average number of hours worked by a single physician is 40 by definition, since this is based on the number of FTE physicians reported at a given practice. To be consistent with how the previous literature (Reinhardt 1972; Thurston and Libby 2002) measure physician time, I multiply the number of reported FTE physicians by 40 hours. Moving down, Table 1.1 also shows that physician offices have become more capital intensive over time. Likewise, if APPs, RNs, LPNs, and CNAs are added together into one category, then physician offices have also become more nurse intensive. The number of technicians appears to have remained constant, while the number of office aids increased through the 1980s before falling again.

Tables 1.2, 1.3, and 1.4 report the regression results for Models 1, 2, and 3 described above for both office visits and work RVUs. It is difficult to interpret the regression coefficients directly because of the square root transformation and cross-products. As a result, the remainder of this section focuses on the marginal product and elasticity estimates based on these regression coefficients.

Table 1.5 presents a comparison of the marginal products and Hick’s Elasticity estimates based on the regression coefficients from Model 1. The top panel reports estimates from Thurston and Libby (2002). The middle and bottom panels report estimates using the 2019 AMGA data. The middle panel shows the results when the outcome is office visits while the bottom panel shows the results when the outcome is work RVUs. Standard errors are calculated using the “delta method.” If two inputs are complements, then the elasticity will be greater than zero. Alternatively, if two inputs are substitutes, then the elasticity will be less than zero.

Table 1.2. Regression Results from Model 1

	Total Visits		Total wRVUs	
	Coefficient	Std. Error	Coefficient	Std. Error
Constant	8.283	(9.840)	0.558	(16.678)
$1 \times \sqrt{H}$	0.685	(1.746)	2.372	(3.552)
$1 \times \sqrt{L_1}$	-4.146	(8.560)	-4.262	(15.366)
$1 \times \sqrt{L_2}$	-1.124	(6.491)	-18.111	(17.949)
$1 \times \sqrt{L_3}$	4.285	(4.674)	32.887***	(11.209)
$1 \times \sqrt{K}$	-1.637	(1.202)	-3.358	(2.757)
$\sqrt{H} \times \sqrt{H}$	0.051***	(0.009)	0.093***	(0.026)
$\sqrt{H} \times \sqrt{L_1}$	0.192	(0.722)	0.083	(1.364)
$\sqrt{H} \times \sqrt{L_2}$	0.872	(0.638)	2.313	(1.427)
$\sqrt{H} \times \sqrt{L_3}$	-1.295*	(0.663)	-3.191**	(1.453)
$\sqrt{H} \times \sqrt{K}$	-0.001	(0.012)	-0.002	(0.032)
$\sqrt{L_1} \times \sqrt{L_1}$	-0.287	(1.242)	-0.680	(2.524)
$\sqrt{L_1} \times \sqrt{L_2}$	-0.858	(1.234)	0.254	(4.202)
$\sqrt{L_1} \times \sqrt{L_3}$	-2.561	(2.118)	-5.264	(4.276)
$\sqrt{L_1} \times \sqrt{K}$	0.825*	(0.465)	1.466	(1.003)
$\sqrt{L_2} \times \sqrt{L_2}$	2.334	(1.652)	4.609	(5.055)
$\sqrt{L_2} \times \sqrt{L_3}$	-1.708	(2.086)	-5.264	(6.789)
$\sqrt{L_2} \times \sqrt{K}$	-0.448	(0.292)	-0.200	(0.548)
$\sqrt{L_3} \times \sqrt{L_3}$	1.728	(1.599)	-0.962	(3.515)
$\sqrt{L_3} \times \sqrt{K}$	0.669	(0.399)	0.453	(0.836)
$\sqrt{K} \times \sqrt{K}$	-0.005*	(0.003)	-0.001	(0.006)
N	870		853	

Note: Inputs are defined as H: physician time; L_1: nurses; L_2: technicians; L_3: office aids; and K: capital. Nurses includes APPs, RNs, LPNs, and CNAs. Standard errors are heteroskedastic robust and clustered at the office level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.3. Regression Results from Model 2

	Total Visits		Total wRVUs	
	Coefficient	Std. Error	Coefficient	Std. Error
Constant	19.951**	(8.234)	28.186*	(14.191)
$1 \times \sqrt{H}$	0.842	(1.569)	2.573	(3.273)
$1 \times \sqrt{L_1}$	-16.504**	(7.493)	-42.540**	(18.041)
$1 \times \sqrt{L_2}$	-0.505	(7.790)	8.782	(14.800)
$1 \times \sqrt{L_3}$	-2.097	(7.802)	-22.678	(19.870)
$1 \times \sqrt{L_4}$	5.165	(6.173)	25.381**	(11.119)
$1 \times \sqrt{K}$	-2.289	(1.386)	-4.735	(3.131)
$\sqrt{H} \times \sqrt{H}$	0.042***	(0.012)	0.070**	(0.028)
$\sqrt{H} \times \sqrt{L_1}$	1.684**	(0.787)	3.558**	(1.625)
$\sqrt{H} \times \sqrt{L_2}$	-0.685	(1.055)	-1.849	(1.892)
$\sqrt{H} \times \sqrt{L_3}$	0.813	(0.613)	2.353	(1.491)
$\sqrt{H} \times \sqrt{L_4}$	-1.085**	(0.484)	-2.459**	(1.085)
$\sqrt{H} \times \sqrt{K}$	0.010	(0.014)	0.025	(0.032)
$\sqrt{L_1} \times \sqrt{L_1}$	4.226*	(2.232)	9.433*	(5.166)
$\sqrt{L_1} \times \sqrt{L_2}$	1.785	(2.455)	5.392	(5.017)
$\sqrt{L_1} \times \sqrt{L_3}$	1.570	(1.657)	2.651	(3.857)
$\sqrt{L_1} \times \sqrt{L_4}$	0.073	(1.645)	-1.277	(3.461)
$\sqrt{L_1} \times \sqrt{K}$	-0.292	(0.386)	0.007	(0.835)
$\sqrt{L_2} \times \sqrt{L_2}$	-3.395	(2.342)	-9.032	(6.113)
$\sqrt{L_2} \times \sqrt{L_3}$	-1.232	(1.750)	0.475	(4.748)
$\sqrt{L_2} \times \sqrt{L_4}$	-2.564	(3.194)	-3.093	(3.968)
$\sqrt{L_2} \times \sqrt{K}$	1.523*	(0.839)	2.581	(1.784)
$\sqrt{L_3} \times \sqrt{L_3}$	4.675*	(2.300)	11.580	(7.645)
$\sqrt{L_3} \times \sqrt{L_4}$	-1.994	(2.333)	-6.173	(7.517)
$\sqrt{L_3} \times \sqrt{K}$	-0.617*	(0.336)	-0.680	(0.693)
$\sqrt{L_4} \times \sqrt{L_4}$	0.161	(1.701)	-0.790	(3.429)
$\sqrt{L_4} \times \sqrt{K}$	0.609*	(0.317)	0.240	(0.677)
$\sqrt{K} \times \sqrt{K}$	-0.007**	(0.003)	-0.005	(0.007)
N	808		791	

Note: Inputs are defined as H: physician time; L_1: APPs; L_2: nurses; L_3: technicians; L_4: office aids; and K: capital. Nurses includes RNs, LPNs, and CNAs. Standard errors are heteroskedastic robust and clustered at the office level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.4. Regression Results from Model 3

	Total Visits		Total wRVUs	
	Coefficient	Std. Error	Coefficient	Std. Error
Constant	14.453*	(7.169)	11.112	(15.206)
$1 \times \sqrt{H}$	0.994	(1.423)	2.779	(2.903)
$1 \times \sqrt{L_1}$	-17.862**	(6.750)	-43.342***	(15.092)
$1 \times \sqrt{L_2}$	3.183	(3.162)	2.425	(10.408)
$1 \times \sqrt{L_3}$	1.681	(6.354)	16.874	(12.590)
$1 \times \sqrt{L_4}$	-5.128	(4.930)	-1.282	(11.023)
$1 \times \sqrt{L_5}$	-2.745	(6.952)	-22.433	(19.324)
$1 \times \sqrt{L_6}$	4.124	(5.156)	17.133*	(8.826)
$1 \times \sqrt{K}$	-2.285*	(1.232)	-4.818*	(2.701)
$\sqrt{H} \times \sqrt{H}$	0.041***	(0.010)	0.069***	(0.023)
$\sqrt{H} \times \sqrt{L_1}$	1.689**	(0.769)	3.658**	(1.543)
$\sqrt{H} \times \sqrt{L_2}$	-0.082	(0.422)	1.014	(1.279)
$\sqrt{H} \times \sqrt{L_3}$	-0.090	(0.640)	-1.719	(1.491)
$\sqrt{H} \times \sqrt{L_4}$	-1.420	(1.193)	-3.889	(3.049)
$\sqrt{H} \times \sqrt{L_5}$	0.517	(0.558)	1.571	(1.513)
$\sqrt{H} \times \sqrt{L_6}$	-0.725*	(0.402)	-0.928	(1.058)
$\sqrt{H} \times \sqrt{K}$	0.014	(0.013)	0.030	(0.026)
$\sqrt{L_1} \times \sqrt{L_1}$	3.996*	(2.293)	8.924*	(4.748)
$\sqrt{L_1} \times \sqrt{L_2}$	2.866	(2.416)	6.956*	(3.526)
$\sqrt{L_1} \times \sqrt{L_3}$	3.043*	(1.517)	4.594	(3.701)
$\sqrt{L_1} \times \sqrt{L_4}$	-1.439	(1.732)	-2.759	(3.941)
$\sqrt{L_1} \times \sqrt{L_5}$	0.812	(1.545)	0.440	(3.050)
$\sqrt{L_1} \times \sqrt{L_6}$	-0.511	(1.334)	-1.835	(2.867)
$\sqrt{L_1} \times \sqrt{K}$	-0.155	(0.370)	0.184	(0.788)
$\sqrt{L_2} \times \sqrt{L_2}$	-0.579	(1.725)	-0.242	(4.804)
$\sqrt{L_2} \times \sqrt{L_3}$	-2.566**	(1.063)	-4.714	(2.965)
$\sqrt{L_2} \times \sqrt{L_4}$	-1.407	(2.270)	-4.158	(5.187)
$\sqrt{L_2} \times \sqrt{L_5}$	-0.023	(1.476)	6.444	(6.207)
$\sqrt{L_2} \times \sqrt{L_6}$	-1.695	(2.019)	-2.513	(2.960)
$\sqrt{L_2} \times \sqrt{K}$	0.089	(0.243)	-0.747	(0.641)
$\sqrt{L_3} \times \sqrt{L_3}$	-2.474	(1.730)	-9.073**	(4.196)
$\sqrt{L_3} \times \sqrt{L_4}$	-4.121	(2.518)	-7.362	(7.152)
$\sqrt{L_3} \times \sqrt{L_5}$	0.036	(1.908)	-1.769	(5.440)
$\sqrt{L_3} \times \sqrt{L_6}$	-2.158	(2.157)	-3.912	(3.302)
$\sqrt{L_3} \times \sqrt{K}$	0.696	(0.473)	1.941	(1.317)
$\sqrt{L_4} \times \sqrt{L_4}$	-0.530	(1.588)	-2.581	(3.625)

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	Total Visits		Total wRVUs	
	Coefficient	Std. Error	Coefficient	Std. Error
$\sqrt{L_4} \times \sqrt{L_5}$	-3.428*	(1.962)	-7.811	(7.390)
$\sqrt{L_4} \times \sqrt{L_6}$	-1.052	(2.343)	1.020	(4.820)
$\sqrt{L_4} \times \sqrt{K}$	2.051**	(0.952)	4.137	(2.481)
$\sqrt{L_5} \times \sqrt{L_5}$	5.451***	(1.834)	13.274*	(7.282)
$\sqrt{L_5} \times \sqrt{L_6}$	2.676	(2.646)	5.417	(3.392)
$\sqrt{L_5} \times \sqrt{K}$	-0.588*	(0.334)	-0.462	(0.663)
$\sqrt{L_6} \times \sqrt{L_6}$	1.021	(1.499)	2.199	(3.417)
$\sqrt{L_6} \times \sqrt{K}$	0.322	(0.271)	-0.813	(0.633)
$\sqrt{K} \times \sqrt{K}$	-0.010**	(0.004)	-0.009	(0.006)
N	808		791	

Note: Inputs are defined as H: physician time; L_1: APPs; L_2: RNs; L_3: LPNs; L_4: CNAs; L_5: technicians; L_6: office aids; and K: capital. Standard errors are heteroskedastic robust and clustered at the office level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

In the 1980s, one additional hour of physician time was associated with more than one additional office visit per week. Now, a physician hour is associated with less than a tenth of a visit. While it may be the case that physicians have become less productive over time, the smaller marginal product for physician time may also be a mechanical result of large offices employing multiple physicians. That is, suppose that an office employs ten physicians, meaning a one unit increase in physician time would be measured as a change from 400 to 401 hours. Perhaps this change is simply less meaningful than a change from 40 to 41 hours, which would be the more common case in the older data when offices were more likely to be owned and operated by a single physician.

Table 1.5. Elasticity Estimates for Model 1

		1985, 1988 Samples—Total Visits				
Marginal		Physician Time	Nurses	Technicians	Office Aids	Capital
Productivity F_i		1.34	7.42	10.75	6.17	0.17
Hicks Elasticity η^H	Nurses	1.97***				
		(0.76)				
	Technicians	1.42	-7.87			
		(1.21)	(6.25)			
	Office Aids	0.28	3.17*	-2.19		
		(0.55)	(2.82)	(2.65)		
	Capital	-2.94***	-0.43	5.11***	0.08	
		(0.06)	(12.91)	(0.07)	(1.69)	
		2019 Sample—Total Visits				
Marginal		Physician Time	Nurses	Technicians	Office Aids	Capital
Productivity F_i		0.06	4.15	4.75	-3.97	-0.005
Hicks Elasticity η^H	Nurses	0.12				
		(0.28)				
	Technicians	1.55**	-0.25			
		(0.76)	(0.45)			
	Office Aids	1.58	0.51	0.94		
		(1.33)	(0.41)	(1.54)		
	Capital	0.04	-4.96	7.47	-6.39	
		(0.43)	(9.34)	(14.78)	(16.10)	
		2019 Sample—Total wRVUs				
Marginal		Physician Time	Nurses	Technicians	Office Aids	Capital
Productivity F_i		0.12	6.69	18.11	-15.51	-0.02
Hicks Elasticity η^H	Nurses	0.03				
		(0.45)				
	Technicians	0.93***	0.02			
		(0.35)	(0.34)			
	Office Aids	0.93	0.32	0.34		
		(0.87)	(0.27)	(0.37)		
	Capital	0.02	-2.55	0.38	-0.54	
		(0.27)	(4.43)	(0.87)	(0.93)	

Note: Hick's elasticities of complementarity, η^H, are calculated at the means of the data. Standard errors are calculated by the "delta method" and are in parentheses. For $\eta^H > 0$, the inputs are complements, and, for $\eta^H < 0$, the inputs are substitutes. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A comparison of the top and middle panels in Table 1.5 shows that the marginal products for nurses and technicians have also declined, although at 4.15 and 4.75, respectively, these estimates are still reasonable. However, hiring one more office aid is now associated with a loss of four visits per week. If we look down at the elasticity estimates, we can see that the estimate for office aids and capital is large and negative (η^H = -6.39). It could be that the negative marginal product for office aids and the large negative elasticity between them and capital reflect an increasing administrative burden. For example, increased use of health IT systems which may not communicate well with each other and which may be cumbersome to use could be reducing the contribution of office aids towards total physician output. Another possibility is that office aids are expending large amounts of time and energy obtaining prior authorization from insurance companies and, should reimbursement be denied after a treatment has been performed, pursuing bad denials.

For most other input pairs, the sign of the elasticity has remained unchanged, meaning complements remain complements and substitutes remain substitutes for the most part. For example, Thurston and Libby (2002) find strong q-complementarity between physician time and nurses (η^H = 1.97, p<0.01). In the 2019 data for office visits, this elasticity is much smaller and not statistically significant (η^H = 0.12). The one exception to this trend of smaller elasticities is for physician time and technicians, which is large and significant (η^H = 1.55, p<0.05), implying the two inputs are strong complements. This could be due to more tests being available today than in the 1980s or physicians practicing medicine more defensively to forestall medical malpractice lawsuits. Looking at the results that use the 2019 data for work RVUs (the bottom panel), the marginal products and elasticities exhibit the same pattern as the results produced using office visits.

One interpretation of these results is that since the mid-1980s, medical practices may have already adjusted the size and skill mix of their clinical workforce and made other changes to achieve the efficiency gains that were once readily available to them. The marginal products are therefore lower and the elasticities between some inputs are smaller. These changes may have been driven by the rise of managed care and its use of capitated payments. Note that many of the 2019 elasticity estimates are not statistically significant. Incorporating more data from

AMGA in the near future should improve the precision of these estimates and produce more statistically significant results.

Table 1.6 presents the results for Model 2 where APPs and nurses enter the production function as separate categories. The top panel of Table 1.6 presents the results for when output is measured as total visits and the bottom panel presents the results when output is measured as total work RVUs. Here, the results show nurses provide practices with more marginal visits than APPs (15.02 vs. 7.52) but contribute fewer work RVUs than APPs (22.45 vs. 27.8). This may reflect the fact that APPs have their own fee schedule under the RBRVS and directly contribute to their office's total RVU count, whereas nurses operate in a purely supporting role.

Interestingly, the marginal products for technicians and office aids have changed sign in the results for total visits. Employing one more technician is now associated with 11 fewer patient visits per week but hiring an additional office aid is associated with an increase of four visits per week. These changes in sign are likely a reflection of the nature of APP work. Depending on state scope-of-practice laws, APPs may be performing a great deal of work in the office but are unable to order tests and scans—only the physician can do that. Similarly, the tasks carried out by APPs may be less subject to change than treatments rendered by physicians, and so office aids may not need to spend so much time acquiring prior authorization and pursuing bad denials.

Table 1.6. Elasticity Estimates for Model 2

		Total Visits					
	Marginal	Physician Time	APPs	Nurses	Technicians	Office Aids	Capital
	Productivity F_i	0.07	7.52	15.02	-11.06	4.40	0.005
Hicks Elasticity η^H	APPs	0.97***					
		(0.26)					
	Nurses	-0.11	0.07				
		(0.39)	(0.10)				
	Technicians	-0.50	-0.22	0.05			
		(0.48)	(0.22)	(0.07)			
	Office Aids	-1.05	0.02	-0.16	0.46		
		(1.47)	(0.35)	(0.28)	(0.55)		
	Capital	0.35	-2.22	3.35	5.05	7.78	
		(0.83)	(5.86)	(2.55)	(10.11)	(12.46)	
		Total wRVUs					
	Marginal	Physician Time	APPs	Nurses	Technicians	Office Aids	Capital
	Productivity F_i	0.16	27.80	22.45	0.68	-10.73	-0.02
Hicks Elasticity η^H	APPs	0.49***					
		(0.14)					
	Nurses	-0.18	0.07				
		(0.57)	(0.07)				
	Technicians	19.78	2.81	0.36			
		(228.37)	(32.45)	(4.43)			
	Office Aids	0.81	0.05	0.09	15.63		
		(0.75)	(0.15)	(0.14)	(186.58)		
	Capital	-0.17	-0.01	-1.57	35.06	0.49	
		(0.27)	(0.71)	(3.38)	(415.01)	(0.91)	

Note: Hick's elasticities of complementarity, η^H, are calculated at the means of the data. Standard errors are calculated by the "delta method" and are in parentheses. For $\eta^H > 0$, the inputs are complements, and, for $\eta^H < 0$, the inputs are substitutes. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.7. Elasticity Estimates for Model 3

		Physician Time	APPs	RNs	LPNs	CNAs	Technicians	Office Aids	Capital
Total Visits									
Marginal Productivity F_i		0.07	8.92	0.34	9.93	19.81	-8.36	2.35	0.05
Hicks Elasticity η^H	APPs	0.67^{***}							
		(0.24)							
	RNs	-1.15	5.65						
		(12.32)	(29.06)						
	LPNs	-0.04	0.17^{**}	-5.08					
		(0.28)	(0.08)	(28.22)					
	CNAs	-0.24	-0.03	-1.19	-0.10				
		(0.50)	(0.04)	(6.34)	(0.07)				
	Technicians	-0.31	-0.07	0.07	-0.003	0.12			
		(0.37)	(0.13)	(4.54)	(0.16)	(0.08)			
	Office Aids	-1.18	-0.12	-13.81	-0.50	-0.10	-0.92		
		(1.59)	(0.32)	(88.00)	(0.63)	(0.24)	(1.06)		
	Capital	0.05	-0.07	1.45	0.32^{***}	0.40	0.41	0.61	
		(0.04)	(10.39)	(10.39)	(0.11)	(0.32)	(0.47)	(0.53)	
Total wRVUs		Physician Time	APPs	RNs	LPNs	CNAs	Technicians	Office Aids	Capital
Marginal Productivity F_i		0.15	29.40	-7.48	29.38	42.22	-1.53	-16.24	-0.06
Hicks Elasticity η^H	APPs	0.50^{***}							
		(0.13)							
	RNs	-0.63	-0.49						
		(1.19)	(0.62)						
	LPNs	-0.27	0.08	0.39					
		(0.47)	(0.31)	(0.59)					
	CNAs	-0.31	-0.02	0.17	-0.08				
		(0.78)	(0.03)	(0.31)	(0.08)				
	Technicians	-6.14	-0.20	12.92	0.91	1.98			
		(38.84)	(1.94)	(80.82)	(5.45)	(11.35)			
	Office Aids	0.21	0.05	-0.29	0.12	-0.02	4.01		
		(0.27)	(0.07)	(0.45)	(0.11)	(0.07)	(24.81)		
	Capital	0.08	0.06	1.02	0.68^{***}	0.72	3.98	0.41	
		(0.07)	(0.22)	(1.83)	(0.12)	(0.75)	(26.44)	(0.51)	

Note: Hick's elasticities of complementarity, η^H, are calculated at the means of the data. Standard errors are calculated by the "delta method" and are in parentheses. For $\eta^H > 0$, the inputs are complements, and, for $\eta^H < 0$, the inputs are substitutes. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

The elasticity estimates in Table 1.6 are generally small and insignificant, which is consistent with the estimates shown in the previous table. Once again, it should be noted that the addition of more data could adjust these estimates and tighten the standard errors, thereby producing more statistically significant results. Currently, though, the elasticity for physician time and APPs is positive and significant (η^{H}= 0.97, $p<0.01$), which suggests these inputs are strong complements and work well together. This is also likely caused by state scope-of-practice laws, as physicians must be in a supervisory role with their APPs, although the degree of supervision can vary by state.

Table 1.7 presents the results for Model 3 where nurses are further subdivided into RNs, LPNs, and CNAs. The panels in this table are organized the same way as in Table 1.6. Here, APPs and LPNs exhibit roughly equal marginal products (8.92 and 9.93, respectively, for total visits), while RNs show the smallest marginal product (0.34 for total visits) and CNAs show the largest (19.81 for total visits). At first glance, these results may seem surprising, as CNAs are the least skilled nurses. However, CNAs are the nurses who will take a patient's temperature, weight, and blood pressure upon entering the office. The marginal product for CNAs is likely higher than the rest because they will do this for every patient, regardless of any patient's particular illness. As for the other nursing occupations, one possibility is that the tasks that have traditionally fallen to RNs have shifted to APPs and LPNs.

The elasticity estimates are consistent with the interpretation that APPs and LPNs may be taking on more work previously done by RNs. If we look at the results for total visits, we can see that the elasticity for physician time and APPs is still positive and significant (η^{H} = 0.67, $p<0.01$). However, the elasticity for APPs and LPNs is also positive and significant (η^{H} = 0.17, $p<0.05$), which suggests the two inputs are strong complements that work well together and are productivity enhancing. As is the case in the earlier tables, the marginal products and elasticities for work RVUs show the same pattern as the results for total visits.

V. Discussion

This paper investigates the productive relationships between different labor and non-labor inputs in the production process for office visits using unique, proprietary data on medical practices from 2019. Given the increased stress placed on the health care system by the influx of patients insured under the Affordable Care Act and the aging population, it is important to understand how each input contributes to the production of office visits and whether opportunities exist to better optimize across inputs.

In the model where more and less highly trained nurse staff enter the production function as one group, my preliminary results suggest the contribution of each input may have diminished to some degree since the mid-1980s. For example, one additional hour of physician time, one additional nurse, and one additional technician are all associated with fewer additional office visits than they were 40 years ago. Similarly, more office aids are associated with fewer office visits. Models that have each nurse type enter the production function separately show a wide range of nurse contributions to the production process, whether output is measured by office visits or work RVUs. In these models, more technicians are associated fewer visits but more work RVUs, while more office aids are associated with more visits but a fewer number of RVUs.

The elasticity estimates also hint at changing workplace dynamics. Here, the preliminary results suggest complementarity between physician time and technicians, between physician time and APPs, and between APPs and LPNs. In many cases, however, the degree of complementarity or substitutability between inputs appears to have lessened over time as many of the 2019 elasticity estimates are close to zero. One possible explanation for these results is that, in response to the rise of managed care and capitated payments, medical practices have grown more efficient over the past four decades.

The results presented here have an immediate, practical application in the discussions surrounding health care capacity and workforce shortages. Namely, we can imagine a hypothetical medical practice that must meet its obligation to patients, but which operates in an environment where there are worker shortages. If the practice were to lose a worker of some kind, we could trace out different alternative input combinations that would maintain the same level of output as before the worker left. The following scenario is an illustrative example based on the preliminary results.

Suppose the hypothetical practice has quantities of inputs that correspond to the sample averages. Since there are many health policy professionals forecasting a shortage of physicians, let's imagine the number of physician hours at the practice is cut in half from 333 to 166. To maintain production of the same number of office visits, the practice could increase the number of APPs from about one to about three or increase the number of CNAs from around one to around six. If the practice were to substitute with capital, annual spending on capital would need to increase by 34 percent (from about $450K to about $600K). If the number of physician hours were to fall by 40 (representing the loss of one FTE physician), the practice could hold output constant by increasing the number of CNAs from one to four.

The exercise above illustrates how the results of this sort of analysis can be applied. While this analysis is based on my preliminary results, the final version of this study will likely differ in at least three important ways. First, the inclusion of more data should roughly double the existing sample and may modify the current estimates and standard errors, improving precision.

Second, the AMGA data also contain worker salaries. Incorporating this information will allow for a full-blown cost-benefit analysis, which will also show which input combinations can maintain the same patient volume and be profitable to employ. Finally, the AMGA data contain counts of other FTE employees that will be added to the analysis. Specifically, the technician category might be expanded into a broader “clinical support staff” category that also consists of pharmacy staff, nutritionists, and behavioral health staff. Likewise, the office aids category might be expanded to include quality assurance personnel. Finally, counts of FTE administrative staff will also be added to the analysis to investigate the productive capacity of administrators.

VI. Conclusion

Using unique, proprietary data from 2019, this study estimates a production function for office visits in order to examine the complementarity and substitutability of various labor and non-labor inputs in the production process. The inputs considered include physician time, the number of FTE workers of different types, and capital, which is measured as the rental and depreciation costs of office space and equipment. Production function regression coefficients are used to calculate Hick’s Elasticities for different pairs of inputs, which show which combinations are q-complements (and therefore efficiency enhancing) and which are q-substitutes (and therefore not efficiency enhancing).

Preliminary results suggest that the marginal productivity of some inputs is smaller than their marginal productivity from several decades ago (Reinhardt 1972; Thurston and Libby 2002). While many elasticity estimates match in terms of their historical classification as compliments or substitutes, their magnitudes have also fallen over time. One possible interpretation of these results is that medical practices have already adapted to changes in the economic, regulatory, and technological environment in which they practice and have achieved the easy efficiency gains that were once available to them.

Today, the health care system must respond to the sharp increase in the demand for medical care brought on by the influx of newly insured patients under the Affordable Care Act and the aging population. As policymakers contemplate how to increase health care capacity, they should take a holistic view and consider options that encompass the entire production process for medical care. This way, they can help make sure the health care sector is maximizing the amount of care produced while at the same time minimizing cost.

Chapter 2

Cutting the Safety Net: The Long-Term Effects of the 2006 Massachusetts Reform on Safety-Net Hospitals

I. Introduction

The 2010 Affordable Care Act (ACA) was designed to enact universal insurance coverage by reforming the non-group insurance market, mandating that all individuals purchase insurance, and helping low-income individuals comply with the mandate by expanding Medicaid and providing subsidies. To help cover the cost of the Medicaid expansion and the subsidies, the law also included a provision whereby payments to safety-net hospitals—those that traditionally served many low-income and uninsured individuals—would be gradually phased out (MACPAC 2016).

These reductions were originally set to begin in fiscal year (FY) 2014. Several pieces of legislation since 2010, however, have delayed the payment cuts. As of this writing, the first federal reduction, totaling $4 billion, will occur in FY 2021 with further reductions of $8 billion occurring every year from 2022 through 2025 (MACPAC 2020a; MACPAC 2020b). In 2017, the Centers for Medicare and Medicaid Services (CMS) began soliciting feedback on its proposed method for implementing the reductions, and on September 25, 2019, the agency issued its final ruling (CMS 2019).

In theory, these payments would no longer be necessary because every patient would have insurance. In 2012, however, the Supreme Court ruled that the Medicaid expansion would be optional, and since then not every state has decided to move forward with the expansion (Musumeci 2012; KFF 2019). As a result, some hospitals may find themselves in a situation whereby they lose their supplemental payments without a corresponding increase in their number of insured patients. Even hospitals in expansion states may find themselves on uncertain financial footing if the reductions in supplemental payments exceed the revenue they receive from newly insured patients.

To provide some insight into the future of these safety-net hospitals, this paper revisits the 2006 Massachusetts reform, which had many similar features to the ACA. Studies which examined safety-net hospitals in the immediate aftermath of the reform found that those hospitals suffered financially compared to their non-safety-net counterparts (Mohan et al. 2013; Bazzoli and Clement 2014). Those safety-net hospitals, however, still exist today.

This study therefore uses hospital cost report data to investigate the long-term impact of the reform on the financial health of the safety-net hospitals. It also tests the sensitivity of previously established results to different safety-net definitions. The results presented here are generally larger in magnitude and more highly significant than previous results in the literature which are based on a shorter time frame. In addition, if less stringent federal definitions of safety-net hospitals are used, then the results are positive, indicating the reform benefited those hospitals. If a stricter definition is used, however, then the results turn negative. This implies hospitals that provided the most care to the uninsured were indeed hurt by the reform.

II. Background

The 2006 Massachusetts reform sought to achieve universal insurance coverage in the state using many of the same mechanisms as the ACA. In fact, the Massachusetts reform and the ACA share the same chief architect—MIT economist Jonathan Gruber—and the state reform served as a model for the national law. In particular, the state reform expanded MassHealth, the state's Medicaid and Children's Health Insurance Program, to cover families with incomes up to

300 percent of the federal poverty line. In 2011, this amounted to a subsidy of $32,670 for an individual and $67,050 for a family of four (KFF 2012). The state law also established the Commonwealth Health Insurance Connector as the health insurance exchange website wherein residents could purchase both subsidized and non-subsidized private health insurance. After the first year of the reform, the state uninsured rate fell by almost half, dropping from 10.9 percent to 5.5 percent according to data from the Current Population Survey (KFF 2012).

The state law also restructured the way safety-net hospitals received supplemental payments. Prior to the reform, the Massachusetts Division of Healthcare Finance and Policy (DHCFP) issued block grants to safety-net hospitals based on each hospital's reported charges from the previous year. These block grants were sourced from the state's Uncompensated Care Pool (UCP), which itself was funded by general tax revenues and other sources from the state. After the reform, the DHCFP switched to making payments based on hospital claims that were adjudicated under Medicare reimbursement principles. In addition, funding switched to an assessment on acute hospitals' private sector charges and a surcharge on payments made to hospitals by various payers, among other sources (Office of Medicaid 2012). These changes resulted in a decrease in the total funds available for supplemental payments from $620 million in 2007 under the UCP to $373 million in 2008 under the HSN (DHCFP 2007; 2008). By design, these funds were diverted to help finance the MassHealth expansion and the subsidies on the Connector (Sullivan 2009).

Several studies examined the impact of the Massachusetts reform on the financial wellbeing of safety-net hospitals in the immediate aftermath of the reform. The first of these studies, Ku et al. (2011), define safety-net hospitals as those that received 20 percent or more of their net patient service revenue from Medicaid, Commonwealth Care, or HSN in 2009. These criteria identify 17 hospitals as safety net. The authors examine changes in inpatient discharges and ambulatory care visits. They find that between 2006 and 2009, inpatient discharges at both safety-net and non-safety-net hospitals grew by roughly 2 percent, but that nonemergency ambulatory care visits from outpatient departments rose by 9.2 percent at safety-net hospitals compared to 4.1 percent at non-safety-net hospitals.

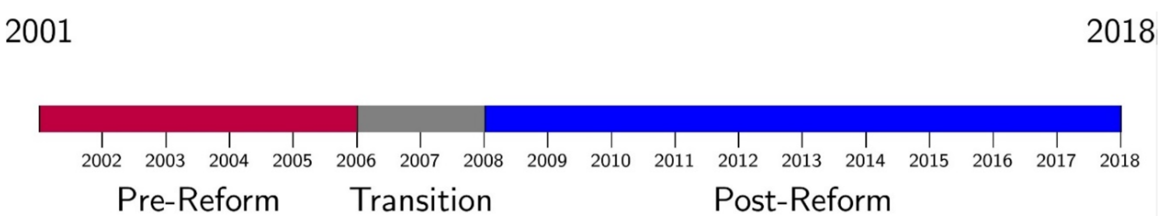
Mohan et al. (2013) use a difference-in-differences study design to compare changes in different financial performance measures at safety-net hospitals before and after the reform to changes in the same measures at non-safety-net hospitals before and after the reform. The result is a causal estimate of how the reform impacted the financial health of safety-net hospitals. Here, the authors use very restrictive criteria which classify just seven hospitals as safety net. Mohan et al. (2013) examine changes in the number of inpatient discharges, number of outpatient visits, net inpatient and outpatient service revenue, net inpatient service revenue per discharge, net outpatient service revenue per outpatient visit, and operating margin. They find that between 2006 and 2009, safety-net hospitals continued to serve many disadvantaged patients but saw their financial situation decline. In fact, the authors show that the reform resulted in a statistically significant decrease in net inpatient service revenue and net outpatient service revenue, and that by the end of their study period safety-net hospitals had a negative operating margin.

Lastly, Bazzoli and Clement (2014) conduct a descriptive analysis on a sample of hospitals which spans 2004 to 2010. The authors use the same safety-net definition as the state health agency. Namely, hospitals qualify as safety-net if the sum of their patient charges for Medicare, Medicaid, other government payers, and free care meets or exceeds 63 percent of total charges. This definition classifies 16 or 17 hospitals as safety-net in each year of their sample. Bazzoli and Clement (2014) show that the operating margin of the state's two largest safety-net hospitals fell after the reform. When these two hospitals are removed from the sample, however, operating margins improved for both hospital types.

III. Data

This study uses data from two sets of hospital cost reports, one submitted to the state of Massachusetts and the other to CMS. The state data come from the 403-Hospital Cost Reports, which were originally filed with the DHCFP. In 2012, Massachusetts passed legislation that dissolved the DHCFP, created the Center for Health Information and Analysis in its place, and transferred administration of the HSN to the Office of Medicaid within the Executive Office of Health and Human Services (Office of Medicaid 2012). These changes in administration aside, the 403 reports were collected for FY 2001 through FY 2018. This 17-year long panel dataset contains five years of data before the transition period (July 2006 through January 2008) and ten years of data after the transition period. This ensures that pre-reform trends can be analyzed, and that the long-term effects of the reform can be traced out. Figure 2.1 shows a timeline of the reform as it pertains to the collected data.

Figure 2.1. Timeline of the 2006 Massachusetts Reform



The state cost reports contain information on basic hospital characteristics, gross patient service revenue by payer, net inpatient and net outpatient service revenue, number of inpatient discharges by payer, number of outpatient visits by payer, number of emergency room visits, and total costs. Hospitals were not required to report revenue or utilization rates from UCP until

2008, at which point HSN replaced the UCP as the source of funds for supplemental payments. Similarly, hospitals did not report revenue or utilization rates from Commonwealth Care until its creation in 2007. The data collected from the state 403 reports are supplemented with additional data from the Hospital Financial Statements, which contain state-audited profit information. The financial statements were collected for FY 2003 through FY 2017. They contain each hospital's operating margin.

The national data come from the Medicare Cost Reports (MCRs), which are annual cost reports submitted to CMS by every Medicare-certified institutional provider. As a result, every hospital that filed a 403 report with Massachusetts also filed an MCR with CMS. The MCRs are maintained in the Healthcare Cost Reporting Information System and contain similar information to the 403 reports, notwithstanding with a few key differences. Operating margin, which was reported directly in the financial statements, is calculated as ((total operating revenue – total operating costs)/total operating revenue) × 100. In the MCRs, net patient revenue is the sum of net inpatient and net outpatient service revenue. Finally, unlike in the 403 reports, total costs includes capital expense, and total inpatient discharges includes deaths.[7] National MCR data were collected for FY 2001 through FY 2018.

IV. Methods

To examine the impact of the 2006 Massachusetts reform on safety-net hospitals, this study begins with a difference-in-differences model, specified as follows:

$$y_{it} = \beta_0 + \beta_1(SNH_i \times Transition_t) + \beta_2(SNH_i \times Post_t) + \tau_t + \theta_i + \varepsilon_{it} \qquad (1)$$

[7] Net outpatient service revenue, outpatient visits, and emergency room visits are not separately reported in the MCR data. Outpatient visits and emergency room visits may be calculated by combining MCR data with data from the Hospital Outpatient Prospective Payment System (OPPS). However, OPPS data are restricted, and the public use files are only available from 2011–2018.

where y_{it} is an outcome measure for hospital i at time t; SNH_i is an indicator equal to one if hospital i is classified as safety net; $Transition_t$ is an indicator equal to one if period t is in the transition period of 2006 to 2008; $Post_t$ is an indicator equal to one if period t is in the post-reform year of 2009 or later; $(SNH_i \times Transition_t)$ is an interaction between SNH_i and $Transition_t$; $(SNH_i \times Post_t)$ is an interaction between SNH_i and $Post_t$; τ_t is a time fixed effect; θ_i is a hospital fixed effect; and ε_{it} is the error term. The terms $Transition_t$ and $Post_t$ are not separately included because they would be collinear with the time fixed effects, while SNH_i is not separately included since it would be collinear with the hospital fixed effects. In this specification, β_1 captures the effect of the transition period and β_2 captures the effect of the post-reform period.

The validity of the difference-in-differences model rests on the assumption that the trends in utilization and financial performance at safety-net hospitals would have been the same as those at non-safety-net hospitals in the absence of the reform. Although this assumption can never be directly tested because the true counterfactual is never known, its likelihood can be tested by including a set of interaction terms between safety-net status and each year. Therefore, the model is amended to take the form:

$$y_{it} = \sum\nolimits_j D_{tj} + \gamma_t + \gamma_i + \varepsilon_{it} \qquad (2)$$

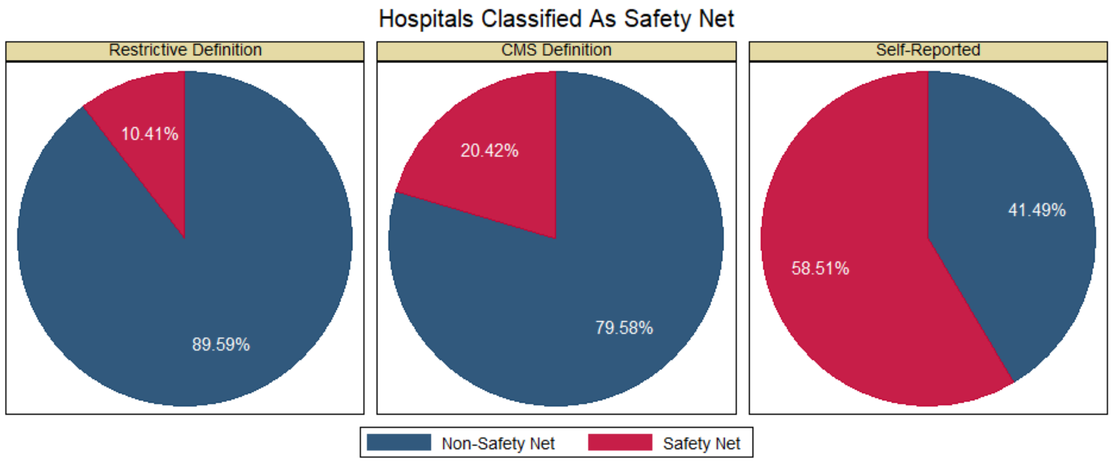
where D_{tj} is an indicator variable for whether treatment gets switched on in year t; γ_t are year fixed effects; and γ_i are hospital fixed effects. Here, FY 2005 is excluded to serve as reference. If the trends in utilization and financial performance in the safety-net and non-safety-net hospitals are the same in the pre-reform period, then the interaction terms for those years should be statistically insignificant. An added benefit of this event study model is that the interactions in the post-reform period show whether the treatment effect dissipates, stays constant, or even increases over time.

States have broad flexibility in determining which hospitals qualify as safety net and in calculating the supplemental payments they receive. However, payments must be made to hospitals that meet the minimum criteria stipulated in Section 1923(b) of the Social Security Act. If states wish to broaden these criteria to include more hospitals, they may, and as a result, some states make supplemental payments to a relatively small number of hospitals—those that meet the minimum requirements—while others make payments to a much higher percentage of hospitals. Because of the wide variation in which hospitals are counted as safety-net, this study uses three different definitions. Formally, the three definitions are:

- Definition 1 (Restrictive): Hospitals which have a high rate of Medicaid utilization (> one standard deviation above the mean) and a low rate of commercial insurance utilization (> one standard deviation below the mean). This is the same definition used by Mohan et al. (2013), which classifies just seven hospitals as safety net.
- Definition 2 (CMS): Hospitals must either have (1) a Medicaid utilization rate that is at least one standard deviation above the mean for hospitals in the state that receive Medicaid payments or (2) have a low-income utilization inpatient utilization rate that is greater than 25 percent. These hospitals are required to receive supplemental payments.
- Definition 3 (Self-Reporting): These are the hospitals which self-report in the MCRs receiving supplemental payments.

Figure 2.2 gives a breakdown of the proportion of safety-net hospitals under each definition. As Figure 2.2 shows, when the criteria for identifying safety-net hospitals are broadened, this proportion increases. The first two definitions are applied to data from the state 403 cost reports in 2006.

Figure 2.2. Proportion of Hospitals Classified as Safety Net



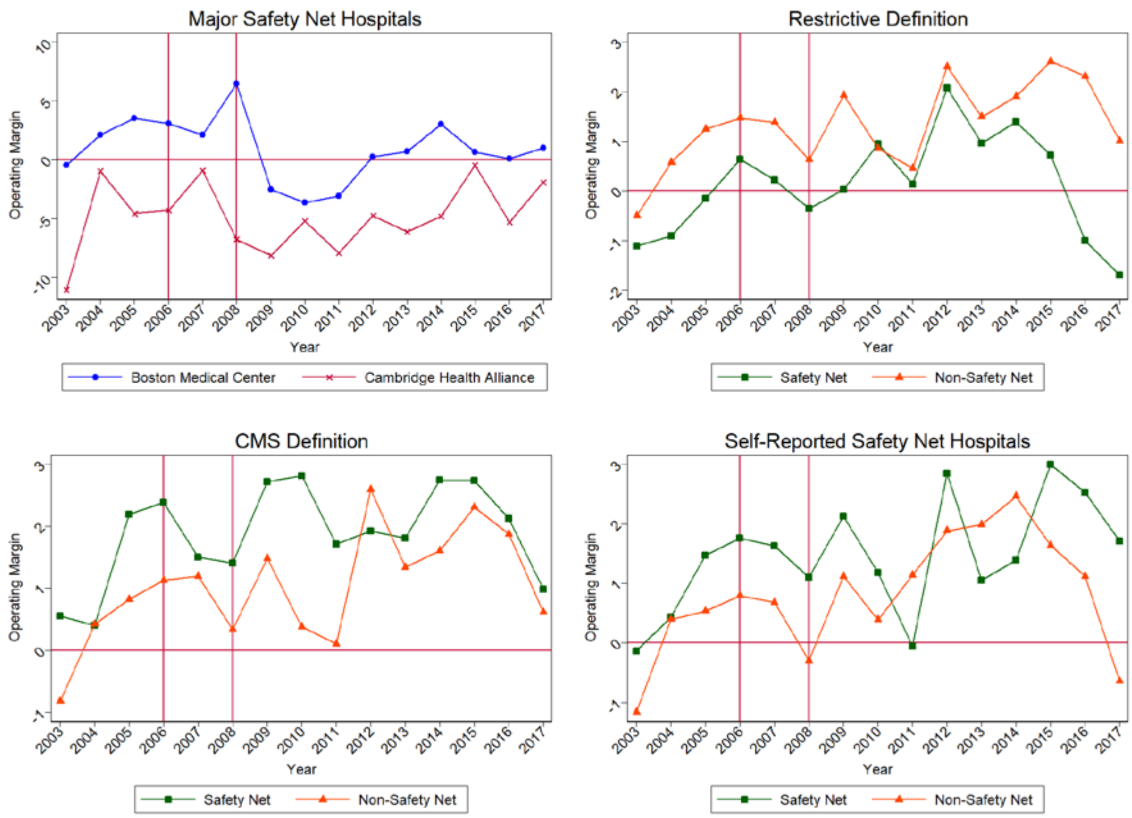
V. Results

Figure 2.3 displays trends in hospital operating margin. The graph in the top left corner shows the trends in operating margin for the state's largest two safety-net hospitals, Boston Medical Center and Cambridge Health Alliance. The graph in the top right corner shows the trends in operating margin for safety-net and non-safety-net hospitals using the restrictive definition. The remaining two graphs in the bottom left and right corners show operating margin trends for CMS-defined and self-reporting safety-net hospitals, respectively.

Boston Medical Center and Cambridge Health Alliance show a declining operating margin around 2008, which marks the end of the reform transition period. Although both hospitals slowly recovered, Cambridge Health Alliance operated at a loss in every year of the data. Regardless of the safety-net definition used, trends in operating margin appear to decline for both safety-net and non-safety-net hospitals at the end of the reform transition period.

Similarly, hospitals that fall under the restrictive definition had a consistently lower operating margin than their non-safety-net peers. However, CMS-defined and self-reporting safety-net hospitals had a higher average operating margin in most years of the data.

Figure 2.3. Operating Margin Trend Graphs

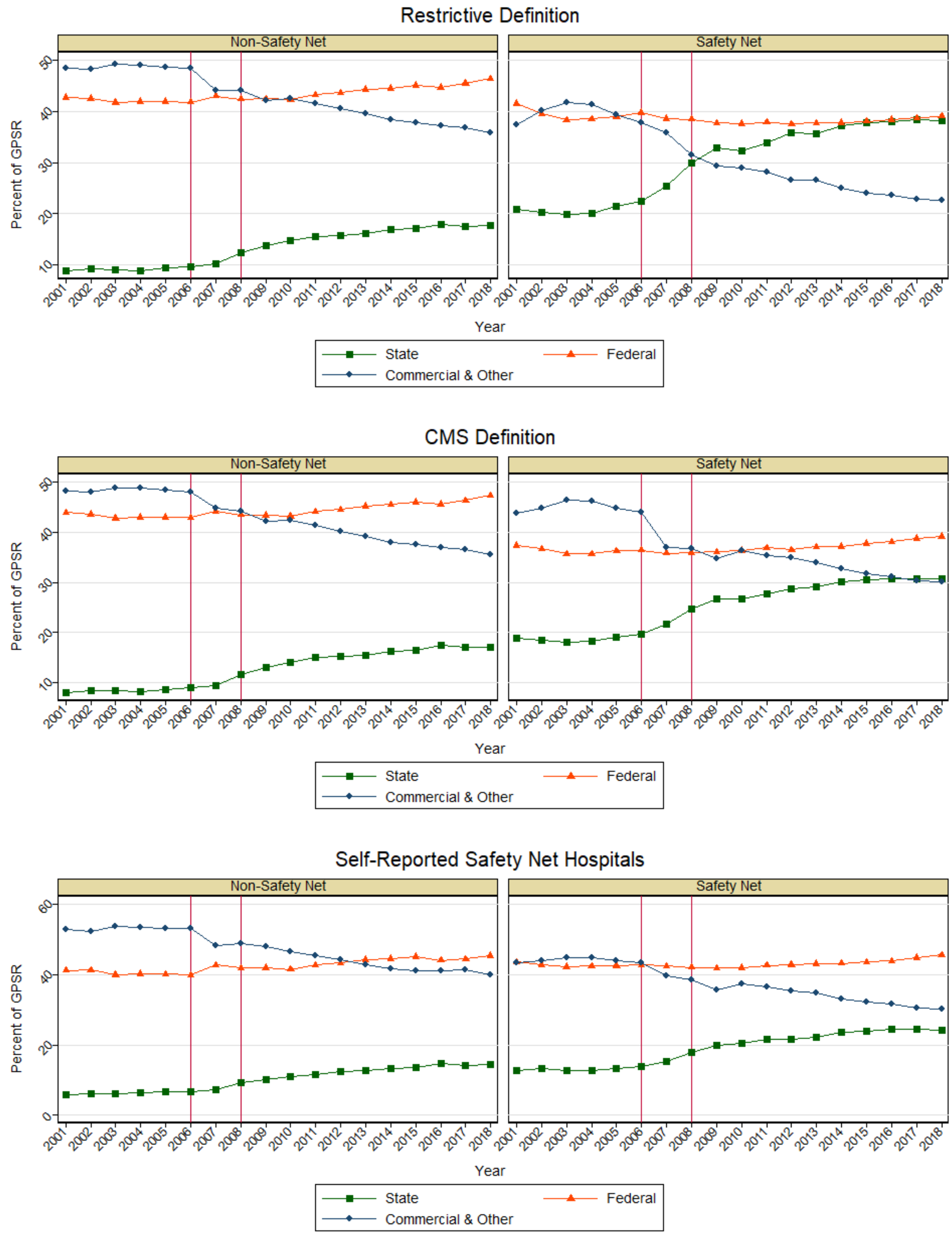


Note: Vertical lines denote reform transition period.

Figure 2.4 shows trends in the percentage of gross patient service revenue (GPSR) by payer. The individual payer types are grouped together as either state, federal, or commercial and other. Specifically, state = Medicaid + Commonwealth Care + HSN; federal = Medicare + other government; and commercial and other = managed care + non-managed care + self-pay + other. The top graphs show tends for the restrictive definition, the middle graphs show trends for the CMS definition, and the bottom graphs show trends for the hospitals that self-report receiving supplemental payments. Figures 2.5 and 2.6 show trends in the percentage of inpatient discharges and outpatient visits by payer, respectively, and are read in the same way as Figure 2.4.

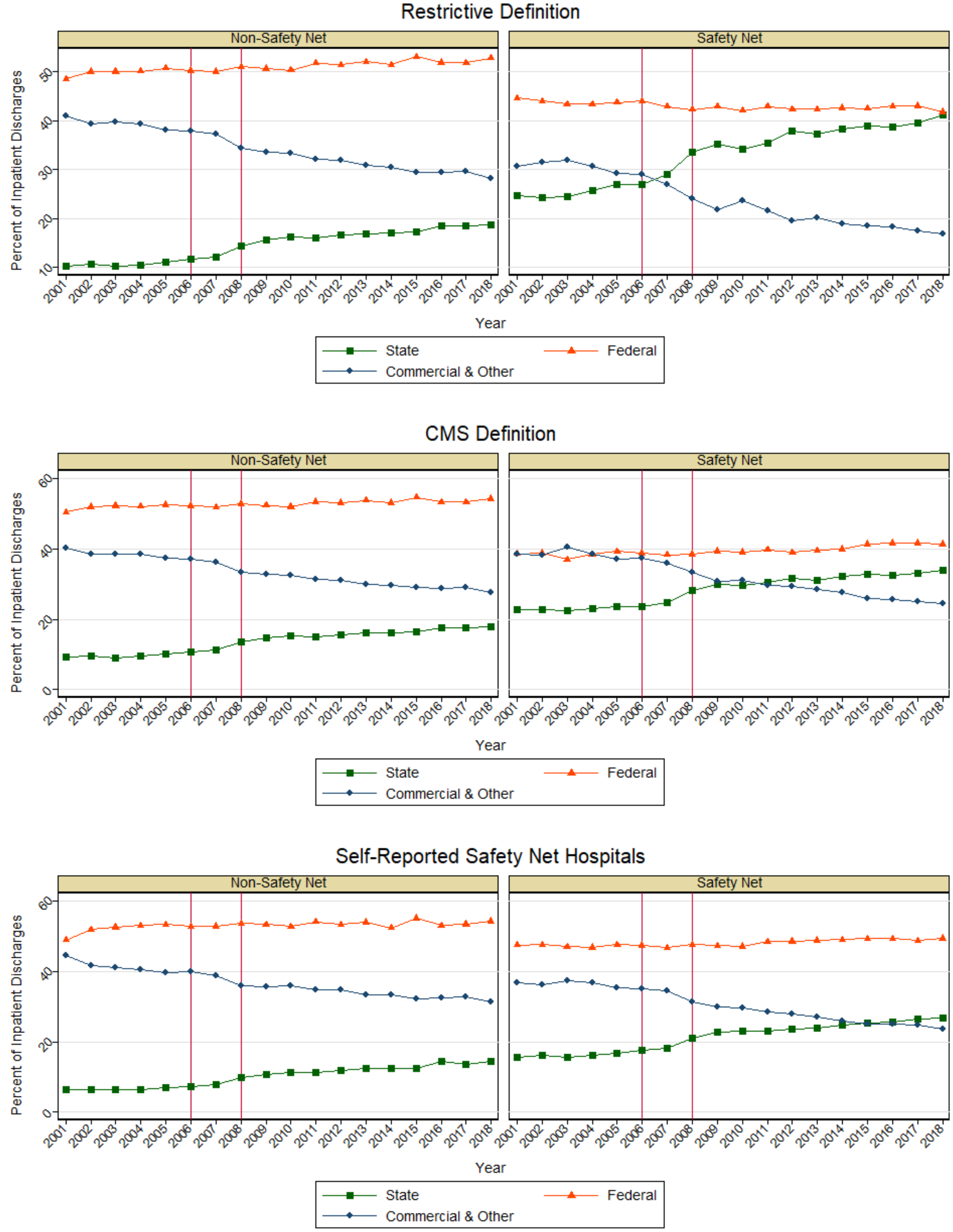
Hospitals classified as safety net under the restrictive and CMS definitions experienced, on average, a steep decline in GPSR from commercial payers during the reform transition period. At the same time, these hospitals also saw a sharp increase in revenue from state sources. The average changes in GPSR for self-reporting hospitals follow the same pattern, although the adjustments are more gradual. Similarly, this pattern is also present in the utilization trend graphs. The one notable difference is the high proportion of inpatient discharges from federal payers. However, this likely reflects hospitalizations among the elderly who are insured by Medicare.

Figure 2.4. Percent of GPSR by Payer Trend Graphs



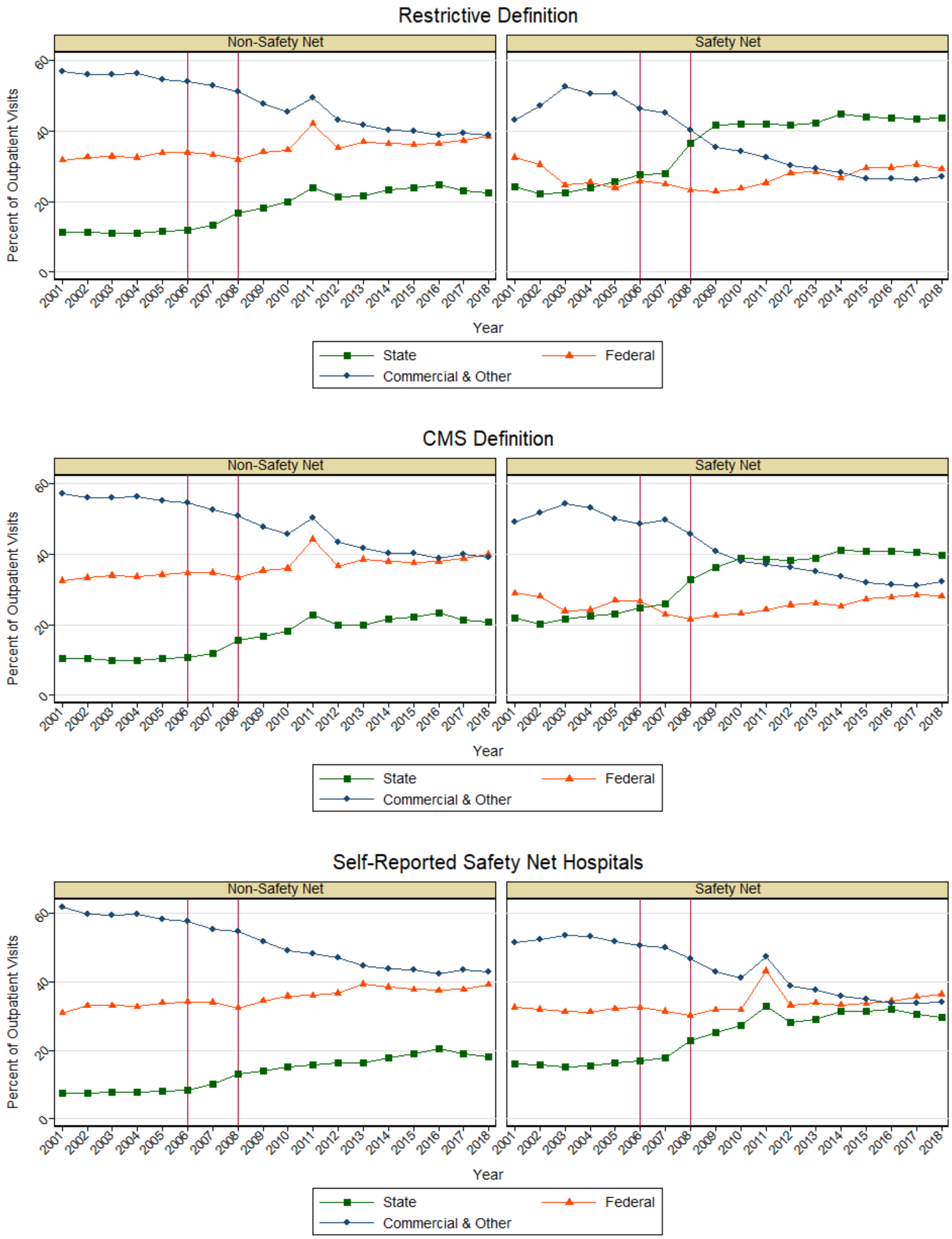
Note: Vertical lines denote reform transition period.

Figure 2.5. Percent of Inpatient Discharges by Payer Trend Graphs



Note: Vertical lines denote reform transition period.

Figure 2.6. Percent of Outpatient Visits by Payer Trend Graphs



Note: Vertical lines denote reform transition period.

V.A Difference-in-Differences Results

Table 2.1 displays the results from estimating Equation (1) on the financial performance measures. Reading from left to right, column (1) shows the results for operating margin, columns (2) through (4) show the results for GPSR, net inpatient service revenue, and net outpatient service revenue, and column (5) shows the results for total costs. All three revenue measures and total costs are adjusted for inflation and expressed in dollars per million. Panels A, B, and C separate the results by the safety-net definition used, wherein Panel A shows the results for the restrictive definition, Panel B shows the results for the CMS definition, and Panel C shows the results for the self-reporting hospitals.

Starting with Panel A, the reform transition period is not associated with any statistically significant change in financial performance. However, the post-reform period is associated with statistically significant declines in GPSR (coef: -125.1, $p<0.01$), net inpatient service revenue (coef: -51.15, $p<0.01$), and net outpatient service revenue (coef: -23.88, $p<0.05$). The estimate for operating margin, although negative, is not statistically significant. By contrast, many of the estimates in Panel B are positive and significant. For example, the post-reform period is associated with a statistically significant increase in GPSR (coef: 378.9, $p<0.01$), net inpatient service revenue (coef: 40.37, $p<0.01$), and net outpatient service revenue (coef: 56.38, $p<0.01$). However, total costs also increase (coef: 98.93, $p<0.01$), and there is no significant change in operating margin. In Panel C, the estimates generally have the same sign and significance level as in Panel B but are smaller in magnitude.

Table 2.1. Financial Performance Difference-in-Differences Results

	(1) Operating Margin	(2) GPSR ($ million)	(3) Net Inpatient Service Rev ($ million)	(4) Net Outpatient Service Rev ($ million)	(5) Total Costs ($ million)
	Panel A: Restrictive Definition				
Transition	0.167	-19.13	-1.838	8.333	8.581
	(1.195)	(45.84)	(8.969)	(12.63)	(15.80)
Post-Reform	-0.112	-125.1***	-51.15***	-23.88**	-20.07
	(1.084)	(47.53)	(7.112)	(10.44)	(15.82)
	Panel B: CMS Definition				
Transition	-0.0557	149.6*	39.71***	30.71**	60.64***
	(0.795)	(78.61)	(11.79)	(14.58)	(17.22)
Post-Reform	-0.00365	378.9***	40.37***	56.28***	98.93***
	(0.719)	(75.52)	(11.17)	(11.82)	(16.89)
	Panel C: Self-Reporting				
Transition	0.492	71.46	12.87**	10.27	17.63
	(0.687)	(46.22)	(6.448)	(9.386)	(11.30)
Post-Reform	-0.430	196.1***	19.13***	13.96	37.28***
	(0.663)	(45.86)	(6.290)	(9.026)	(11.56)
Observations	956	1164	1164	1164	1162

Note: Standard error in parentheses. All regressions include hospital and year fixed effects. Pre-reform reference period includes fiscal years 2001–2005 (2003–2005 for operating margin). Reform transition period includes fiscal years 2006–2008. Post-reform period includes fiscal years 2009–2018 (2009–2017 for operating margin). Gross patient service revenue, net inpatient service revenue, net outpatient service revenue, and total costs are expressed as dollars million. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.2 displays the results from estimating Equation (1) on the utilization measures. Column (1) shows the results for total inpatient discharges, column (2) shows the results for total outpatient visits, and column (5) shows the results for emergency room visits. Columns (3) and (4) present the results for inpatient service revenue per discharge and outpatient service revenue per visit, both expressed in dollars. Table 2.2 is otherwise read in the same manner as Table 2.1.

Table 2.2. Utilization Difference-in-Differences Results

	(1) Total Inpatient Discharges	(2) Total Outpatient Visits	(3) Inpatient Service Rev Per Discharge	(4) Outpatient Service Rev Per Visit	(5) ER Visits
	Panel A: Restrictive Definition				
Transition	129.2	-966.6	88.48	-69.36	4347.3**
	(356.5)	(28123.9)	(365.4)	(56.63)	(2119.2)
Post-Reform	-1683.9***	59256.3**	-1942.4***	-259.4***	9368.3***
	(305.2)	(27344.7)	(316.2)	(51.72)	(2110.2)
	Panel B: CMS Definition				
Transition	570.5*	3709.0	805.7*	-1.388	2438.4
	(310.7)	(19120.3)	(443.9)	(51.64)	(1746.2)
Post-Reform	-385.3	29181.0	256.9	-34.29	7956.0***
	(288.1)	(18273.7)	(433.4)	(50.08)	(1507.3)
	Panel C: Self-Reporting				
Transition	379.4	-2654.5	108.0	70.25	758.4
	(251.5)	(10515.9)	(287.4)	(47.39)	(1756.8)
Post-Reform	81.66	17557.2*	-1117.1***	7.032	6161.3***
	(232.9)	(9647.1)	(255.1)	(45.86)	(1270.2)
Observations	1176	1176	1163	1144	1162

Note: Standard error in parentheses. All regressions include hospital and year fixed effects. Pre-reform reference period includes fiscal years 2001–2005. Reform transition period includes fiscal years 2006–2008. Post-reform period includes fiscal years 2009–2018. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.2 Panel A shows that safety-net hospitals in the post-reform period experienced a statistically significant decline in inpatient discharges relative to non-safety-net hospitals (coef: -1683.9, p<0.01), but also saw a statistically significant increase in outpatient visits (coef: 59256.3, p<0.05) and ER visits (coef: 9368.3, p<0.01). The estimates for inpatient service revenue per discharge and outpatient service revenue per visit are also negative and significant (coef: -1942.4, p<0.01 and coef: -259.4, p<0.01), which are consistent with the unadjusted financial performance results observed in the previous table. Moving on to Panels B and C, many of the estimates again flip sign from negative to positive, although most fail to achieve statistical significance. The one exception is ER visits which stays large, positive, and significant regardless of which definition is used to identify hospitals as safety-net.

Table 2.3. Difference-in-Differences Results—MCR Data

	(1) Operating Margin	(2) GPSR ($ million)	(3) Net Patient Revenue ($ million)	(4) Total Costs ($ million)	(5) Total Inpatient Discharges
			Panel A: Restrictive Definition		
Transition	15.15	-102.4	-4.427	7.266	232.2
	(9.710)	(63.08)	(16.72)	(19.06)	(295.5)
Post-Reform	15.64	-124.6***	-49.66***	-44.37***	-957.3***
	(11.53)	(46.90)	(16.26)	(17.15)	(244.3)
			Panel B: CMS Definition		
Transition	8.346	30.84	30.85	45.78*	199.0
	(6.251)	(89.17)	(23.58)	(26.77)	(396.2)
Post-Reform	11.58	255.0***	75.31***	79.27***	-471.8
	(7.386)	(81.36)	(26.22)	(27.00)	(333.3)
			Panel C: Self-Reporting		
Transition	2.579	57.76	28.68**	34.07**	442.1*
	(2.231)	(48.56)	(14.25)	(15.50)	(241.5)
Post-Reform	2.315	209.0***	50.00***	47.67***	15.22
	(2.570)	(47.24)	(14.88)	(16.59)	(215.4)
Observations	1179	1183	1179	1183	1183

Note: Standard error in parentheses. All regressions include hospital and year fixed effects. Pre-reform reference period includes fiscal years 2001–2005. Reform transition period includes fiscal years 2006–2008. Post-reform period includes fiscal years 2009–2018. Gross patient service revenue, net inpatient service revenue, net outpatient service revenue, and total costs are expressed as dollars million. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.3 presents the results from estimating Equation (1) on the financial performance and utilization measures available in the MCR data. Although the measures are not exact one-to-one matches across both datasets, the estimates presented here should be roughly comparable to the estimates shown in Tables 2.1 and 2.2. In fact, the results are very similar. Looking at Panel A, the reform is associated with statistically significant declines in GPSR (coef: -124.6, p<0.01), net patient revenue (coef: -49.66, p< 0.01), and total costs (coef: -44.37, p<0.01) for safety-net hospitals with no significant change in operating margin. Panel B shows that the reform is associated with statistically significant increases in GPSR (coef: 255.0, p<0.01), net patient revenue (coef: 75.31, p<0.01), and total costs (coef: 79.27, p<0.01) for safety-net hospitals with no significant change in operating margin. Panel C shows the same pattern as Panel B, although the estimates are again not quite as large in magnitude.

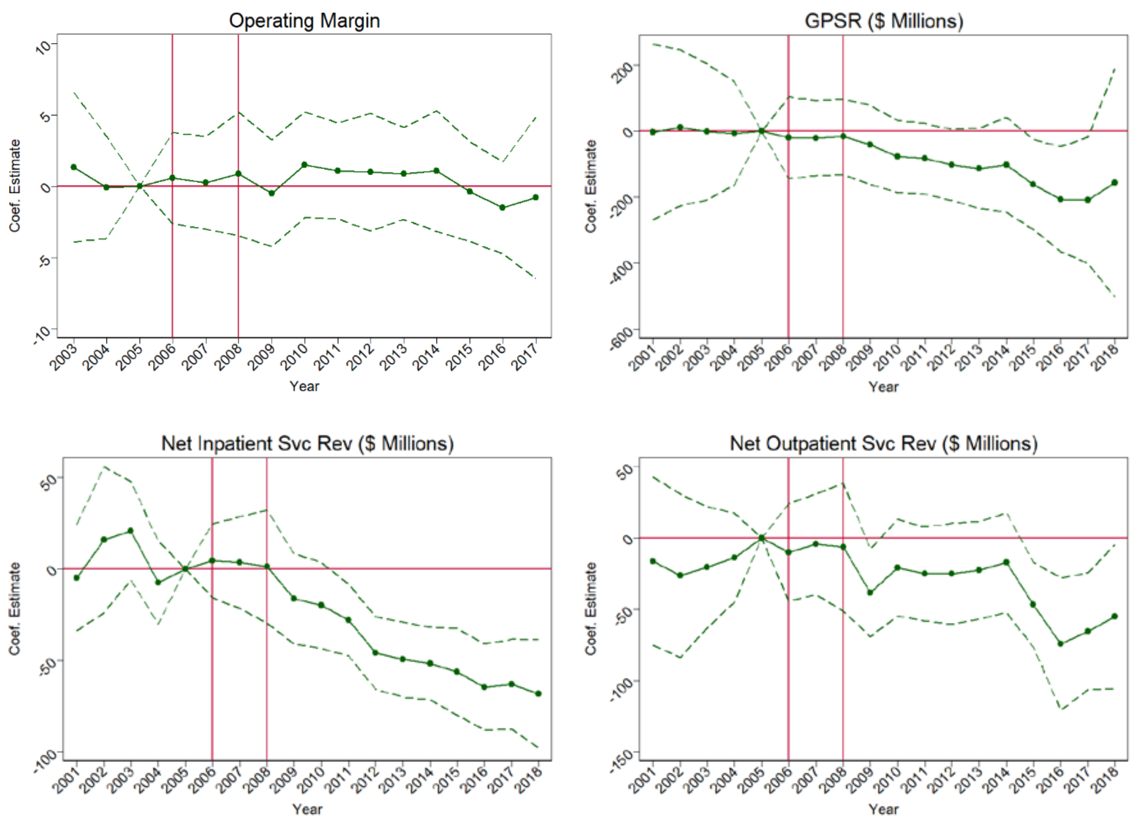
V.B Event Study Results

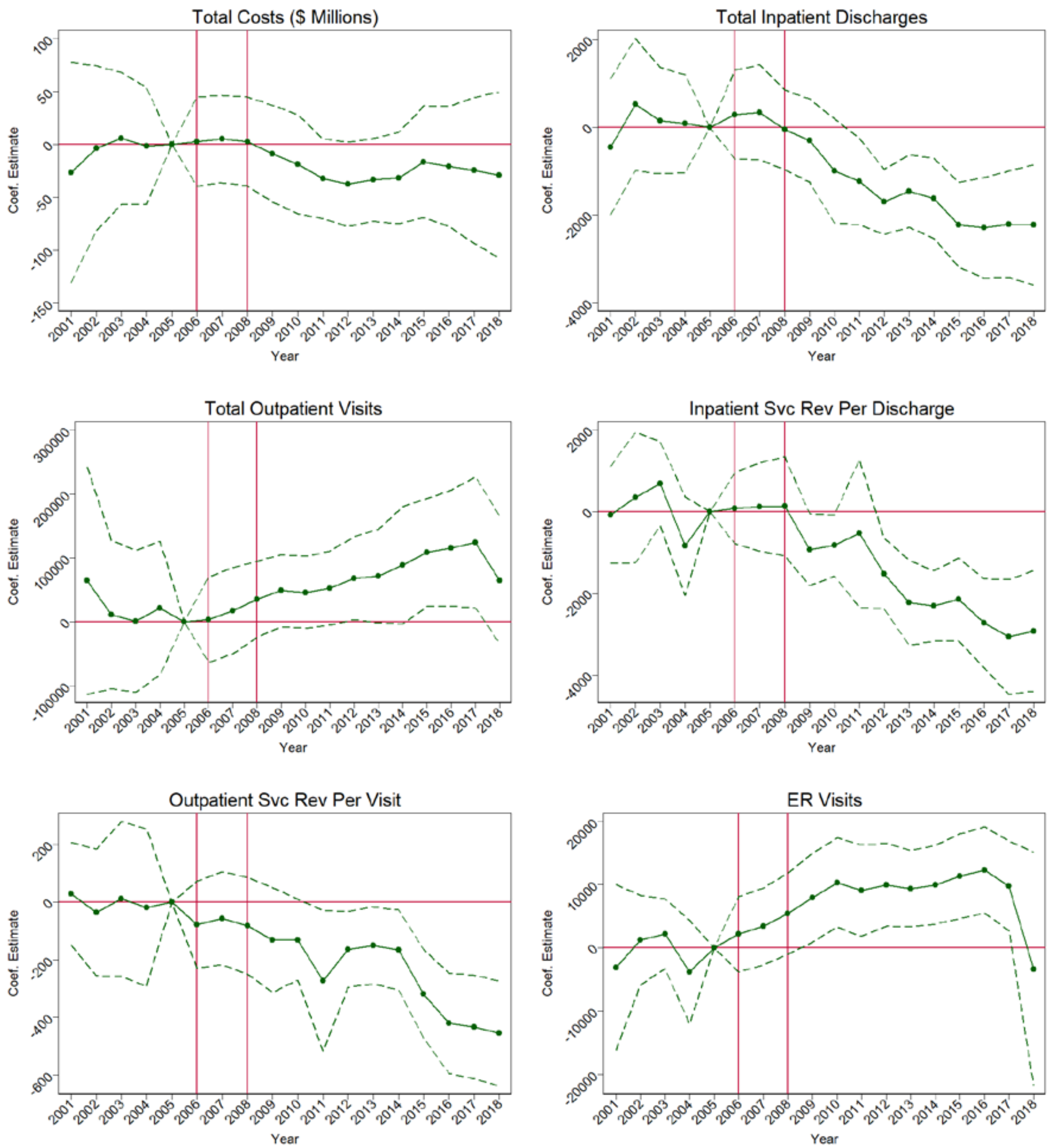
Figures 2.7 and 2.8 plot the event study coefficients and confidence intervals over time for hospitals classified as safety-net under the restrictive and CMS definitions, respectively. The event study graphs for hospitals that self-report receiving supplemental payments are shown in Figure 2.9. Appendix tables A through F report the coefficients and standard errors associated with these graphs. Across all three definitions, the event study graphs do not show statistically significant coefficients for any outcome measure prior to the reform. This provides some evidence that the parallel trends assumption is satisfied.

The same general patterns observed in the difference-in-differences results can be seen in the event study graphs. Among the seven hospitals classified as safety net under the restrictive definition, both GPSR and total costs declined gradually after the reform. Net inpatient service revenue dropped sharply after 2008 while net outpatient service revenue dropped once in 2009 just after the reform, held steady for a few years, and then fell again. When the safety-net definition is relaxed to include more hospitals, the event study graphs show a gradual rise in GPSR and total costs after the reform. Net inpatient and net outpatient revenue grow during the transition period and remain above their pre-reform levels, but do not show any further changes in the post-reform period.

In terms of utilization, the initial seven safety-net hospitals underwent an immediate drop in total inpatient discharges while total outpatient visits grew steadily. Both inpatient service revenue per discharge and outpatient service revenue per visit show a sustained decline. Once again, though, when more hospitals are counted as safety net, these changes in utilization become more muted. No change in total inpatient discharges occurs until 2011 (for CMS-defined safety-net hospitals) and no change in total outpatient visits occurs until 2015 (for hospitals that self-report receiving supplemental payments). Similarly, only inpatient service revenue per discharge shows a gradual and uneven decline in the post-reform period for self-reporting hospitals. ER visits is the one outcome measure that shows a consistent increase, regardless of how safety-net hospitals are identified.

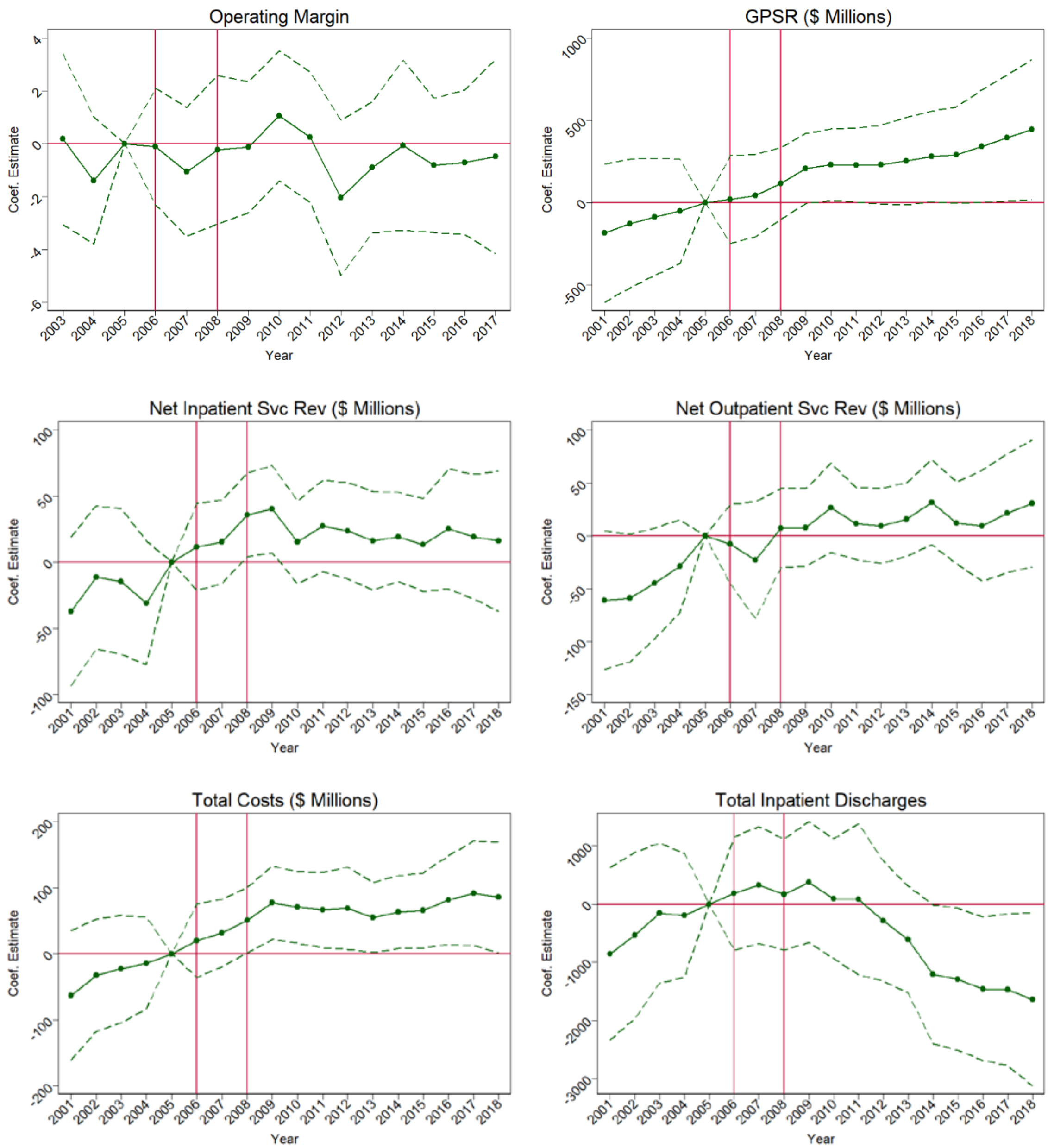
Figure 2.7. Event Study Graphs for Hospitals Classified as Safety Net under the Restrictive Definition





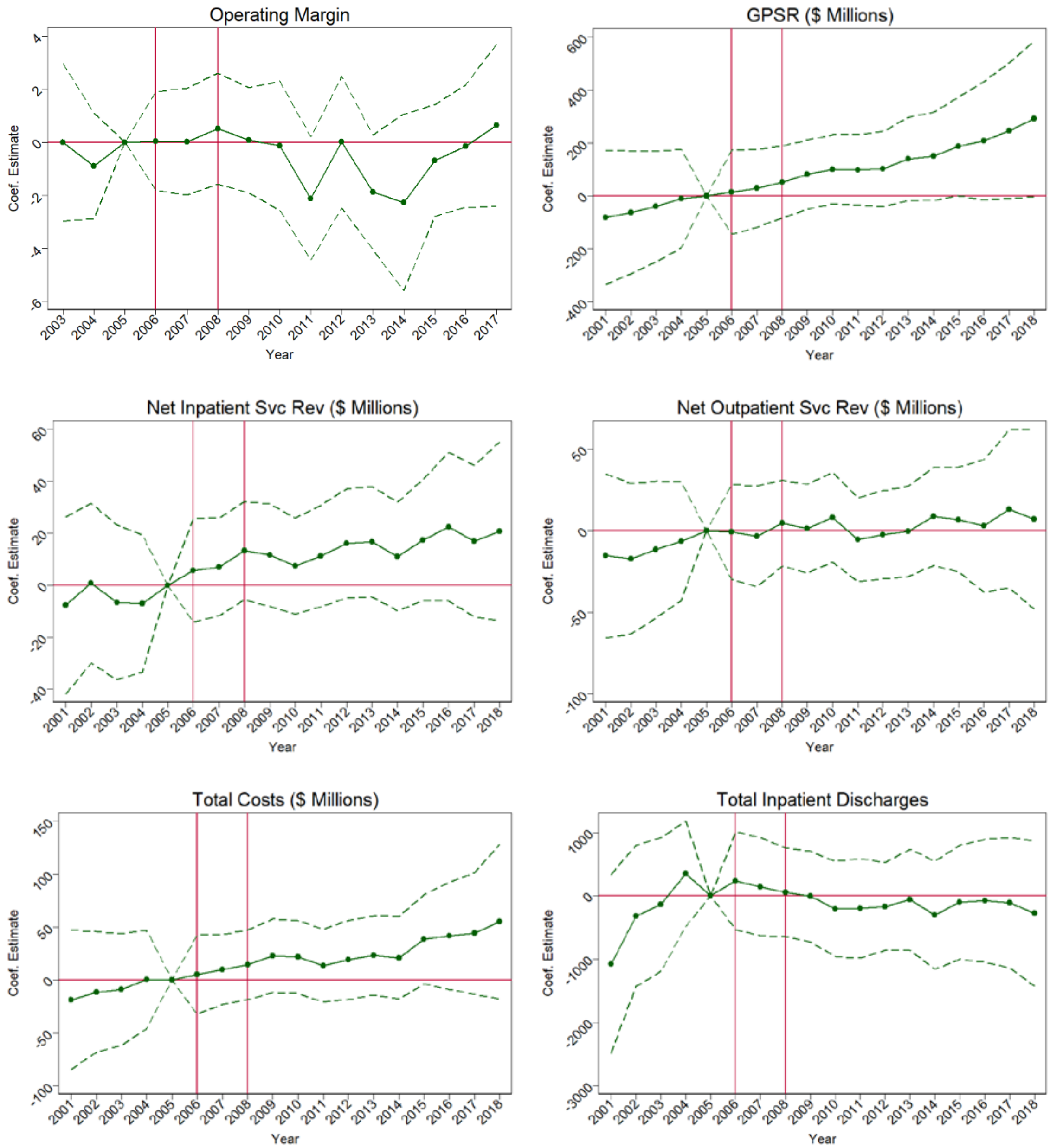
Note: Vertical lines denote reform transition period. Fiscal year 2005 is omitted to serve as reference year.

Figure 2.8. Event Study Graphs for Hospitals Classified as Safety Net under the CMS Definition



Note: Vertical lines denote reform transition period. Fiscal year 2005 is omitted to serve as reference year.

Figure 2.9. Event Study Graphs for Hospitals which Self-Reported Receiving Supplemental Payments



Note: Vertical lines denote reform transition period. Fiscal year 2005 is omitted to serve as reference year.

VI. Discussion

When Massachusetts enacted health care reform in 2006, the state set a precedent for the national ACA law that passed four years later. Both pieces of legislation sought to confer universal health insurance by reforming the non-group insurance market, mandating that all individuals purchase insurance, and expanding Medicaid and providing subsidies to help individuals comply the mandate. To help cover the costs of the subsidies and Medicaid expansion, both laws also contain provisions which divert funds previously set aside for safety-net hospitals—those that traditionally served a disproportionate number of low-income and uninsured patients. Since the federal ACA reductions are scheduled to begin in 2021 and continue for the next five years, this study examines the financial health of safety-net hospitals in Massachusetts following the state's 2006 reform.

This study uses three different definitions to identify safety-net hospitals. The first is a very restrictive definition previously established in the literature and which identifies only the hospitals that serve the greatest number of disadvantaged patients. The second is the definition used by CMS to identify the hospitals to which states must send supplemental payments. Lastly, this study also examines hospitals which self-report receiving supplemental payments. By doing so, this study also captures the additional hospitals, beyond the handful identified by CMS, that Massachusetts considers to be safety net.

The results suggest that the largest safety-net hospitals—the ones that cared for the greatest number of disadvantaged patients—experienced a decline in revenue because of the reform. These hospitals also saw a decline in total costs, a decline in inpatient utilization, and an increase in outpatient utilization. When the definition used to identify safety-net hospitals is relaxed to include hospitals that serve fewer low-income patients, the results change and, in many ways, become the mirror opposite of what they were before. The reform instead becomes associated with an increase in revenue and total costs. The number of inpatient discharges and outpatient visits also do not adjust to the same extent.

One possible interpretation of these results is that the hospitals which served many low-income patients relied heavily on the supplemental payments as a source of revenue. The revenue from the newly insured patients was not enough to compensate for the loss in income caused by the withdrawn payments. These hospitals thus responded by aggressively transferring operations from inpatient facilities to outpatient centers as a cost-cutting maneuver. Generally, outpatient centers are less expensive to run because they do not suffer from the same overhead costs as inpatient facilities. For example, inpatient facilities operate 24/7, are required by law to have a certain proportion of registered nurses to patients, and will take on far more medically complex cases.

By contrast, the hospitals that served fewer low-income patients but are still considered safety net by either CMS or the state of Massachusetts may have received significantly less in terms of supplemental payments. These hospitals may have covered the expense of treating their low-income and uninsured patients using revenue from other more profitable services. The reform therefore led to an increase in revenue, although, as has been the case across the nation, costs rose as well. One interesting finding is that, for both groups of safety-net hospitals, revenue and costs appear to have moved together in ways that offset each other to produce no statistically significant change in operating margin. This can be seen most easily by comparing the revenue and cost estimates from the MCR data.

All safety-net hospitals, regardless of how they were defined, experienced a sustained increase in ER visits. Medical care delivered in emergency room settings is expensive. In addition, if patients enter the ER with conditions that can be treated elsewhere, then those visits consume resources that could be spent on patients who truly need emergency care. One of the hoped-for consequences of expanding insurance in Massachusetts and at the national level was a reduction in ER visits as individuals learn to seek care in more appropriate settings. However, the ER finding in the event study analysis suggests the opposite occurs in both the short and long run. This is consistent with previous literature on insurance expansions, perhaps most notably the Oregon Health Insurance Experiment (Finkelstein et al. 2012). This may be because Medicaid reimbursement is so low that few providers are willing to accept new Medicaid patients, and so the newly insured have no choice but to seek care at the ER.

The federal funds set aside for supplemental payments are first allotted to states and then distributed to safety-net hospitals. The first federal allotment reduction, totaling $4 billion, is scheduled to take effect in 2021. Further reductions of $8 billion each are scheduled to occur every year from 2022 through 2025. On September 25, 2019, CMS finalized its proposed method for implementing the reductions. The agency will apply smaller reductions to states with historically low federal funding allotments and larger reductions to states with lower uninsured rates. Larger reductions will also be levied against states that do not target their payments to hospitals with high Medicaid volumes (MACPAC 2020a).

Each year, the Medicaid and CHIP Payment and Access Commission (MACPAC) issues a report to Congress that, among other Medicaid-related topics, covers the state of the supplemental payments. In its most recent report, the commission notes the wide variation in how states distribute the supplemental payments. In 2015, about 14 percent of hospitals met the CMS criteria for safety-net status. This is roughly comparable to the 20 percent identified as safety net in the state of Massachusetts under the CMS criteria for this study. Three states made payments to fewer than 10 percent of the hospitals in their state (Arkansas, Iowa, and Maine), while three states made payments to more than 90 percent of hospitals in their state (New York, Oregon, and Rhode Island). Similarly, while some states targeted the largest share of their supplemental payments to CMS-defined hospitals, others more evenly dispersed their payments. Nationally, the CMS-defined safety-net hospitals represented 30 percent of all safety-net hospitals but received nearly 66 percent of all supplemental payments (MACPAC 2020a).

The challenge in identifying safety-net hospitals and making payments to them has appeared in other contexts as well. Chatterjee et al. (2020) document how the absence of a clear definition for safety-net status made it difficult for the Department of Health and Human

Services to target pandemic-related resources. The authors recommend reimagining safety-net status as a place along a sliding scale rather than a binary indicator that switches on after crossing some threshold. This would be in keeping with the results presented here, which suggest the largest safety-net hospitals in Massachusetts took a financial hit because of the state's 2006 reform, but that others fared much better and may have even benefitted.

If the Massachusetts experience is indicative of what will happen nationally, and if hospitals in other states are able to respond in the same way as the Massachusetts hospitals, then safety-net hospitals may not be as financially vulnerable as one might assume. Even so, states may wish to respond by making selective cuts to the hospitals that have traditionally received payments but which serve comparatively fewer low-income and uninsured patients, rather than applying the cuts proportionally to all hospitals that have received payments. Certainly, the safety-net hospitals in states that did not expand Medicaid are the most at risk, and so these states may also want to reconsider expanding Medicaid. The American Rescue Plan Act of 2021 provides an additional incentive for these states to expand Medicaid by increasing the states' regular matching rate for the current Medicaid enrollees by five percentage points for two years. This would be in addition to the 90 percent match rate for the new enrollees. The Kaiser Family Foundation estimates this incentive would provide a net fiscal benefit to the non-expansion states (Corallo, Rudowitz, and Garfield 2021). In any case, when the federal reductions do begin, the financial health of safety-net hospitals across the nation should be closely monitored.

VII. Conclusion

This paper examines the financial performance and utilization of safety-net hospitals in Massachusetts following the state's 2006 reform, which served as the foundation for the 2010

Affordable Care Act. As part of the reform, the state withdrew funds previously set aside for hospitals that cared for high percentages of low-income and uninsured patients. In contrast to earlier studies, this paper analyzes three sets of safety-net hospitals wherein each set consists of hospitals identified as safety net according to different criteria. Finally, this paper also uses a 17-year long panel of data to test pre-trends and tease out the long-term effects of the reform. Earlier studies limited their scope to a select handful of hospitals and used only one or two years of post-reform data.

The results indicate that the reform may have threatened the financial solvency of the state's largest providers of uncompensated care, as these hospitals experienced a sharp and immediate decline in patient revenue. The results also point to these hospitals seeing a decrease in inpatient discharges and an increase in outpatient visits, which suggests they may have deliberately transferred operations from their inpatient facilities to their outpatient centers as a cost-cutting maneuver. Total costs fell for this group, and operating margin did not change in any statistically significant way. When the criteria used to identify hospitals as safety net are broadened to include more hospitals, the results change. Although there is still no change in operating margin, the reform becomes associated with an increase in patient revenue and total costs. In addition, the reform is no longer associated with shifts in patient volume from inpatient facilities to outpatient centers.

The federal government sits on the cusp of reducing its financial support for states to make supplemental payments to their safety-net hospitals. Since the results presented here show that the Massachusetts reform impacted safety-net hospitals differently based on how much care they provide to low-income and uninsured patients, states may wish to respond to the reductions by targeting their remaining funds to their most financially vulnerable hospitals. In addition, non-

Medicaid expanding states may wish to reconsider adopting the expansion, as their safety-net hospitals will likely see their supplemental payments withdrawn without a corresponding increase in the number of insured patients they treat.

Chapter 3

Do Those with Chronic Health Conditions Benefit from the ACA?

I. Introduction

The Affordable Care Act (ACA) represents one of the largest insurance expansions in recent history (Courtemanche, Marton, and Yelowitz 2016; Obama 2016; Gruber and Sommers 2019). The primary components of the ACA were implemented in 2014, including the individual mandate, subsidized Marketplace coverage, and state Medicaid expansions. A growing literature has evaluated how state-specific insurance coverage, access to care, and self-assessed health changed following the law's implementation. Studies focusing on the causal effects of the ACA tend to find that the law increased insurance coverage and access to care, but these improvements in coverage and access did not translate into clear measurable improvements in self-assessed health.[8]

One reason for the lack of health improvements may be that coverage gains were concentrated among a relatively healthy group of individuals or among relatively sick individuals where health investments require time to translate into better health. While the literature (Wehby and Lyu 2018; Yue et al. 2018; Courtemanche et al. 2019b; Courtemanche et al. 2020a) has produced heterogeneous subgroup effects among different demographic groups (e.g., by income,

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[8] Coverage studies include Sommers et al. (2014), Sommers et al. (2015), Golberstein et al. (2015), McMorrow et al. (2016), Wherry and Miller (2016), Benitez et al. (2016), Buchmueller et al. (2016), Courtemanche et al. (2017) , Frean et al. (2017), Hinde (2017), Kaestner et al. (2017), Heim et al. (2018), Collins et al. (2018), Berchick et al. (2019), Courtemanche, Marton, and Yelowitz (2020a) , and Courtemanche et al. (2020b). Access to care studies include Sommers et al. (2016), Shartzer et al. (2016), and Kirby and Vistnes (2016), in addition to the previously mentioned papers. Studies focusing on health and health behaviors include Sommers et al. (2012), Simon et al. (2017), Miller and Wherry (2017), Sommers et al. (2017), Courtemanche et al. (2018a) , Courtemanche et al. (2018b) , Borgschulte and Vogler (2019), Courtemanche et al. (2019a) , Graves et al. (2020), and Soni et al. (2020). Other outcomes considered in the literature include employment (Kaestner et al., 2017), political participation (Courtemanche, Marton, and Yelowitz 2020b), and ambulance response times (Courtemanche et al. 2019c).

gender, race etc.) few studies have focused on the causal effect of the ACA on those with chronic conditions, that may have rendered them "uninsurable" prior to the ACA, compared to those who are considered in good health.

The purpose of this paper is to evaluate the impact of the ACA Medicaid expansion on health insurance coverage, access to care, and self-reported health, separately for the newly eligible with and without chronic conditions using data from the Behavioral Risk Factor Surveillance System (BRFSS) between 2011 and 2018. The BRFSS is a commonly used data source in the ACA literature because it includes a number of questions related to the health status of individuals, health care access, and self-assessed health. The BRFSS it is large enough to precisely estimate the effects of state policy interventions and includes a number of questions that identify chronic conditions, making it especially suitable to evaluate the impact on the population that would potentially benefit most from improved access to care. To the best of our knowledge, ours is the first paper to examine the causal impact the ACA Medicaid expansion on a variety of measures of access to care and self-assessed health separately for those with and without chronic conditions using a national sample and five years of post-reform data.[9]

We contribute to the literature by estimating difference-in-differences (DD) models in order to isolate the impact of the ACA Medicaid expansion among the newly eligible by comparing changes over time for individuals in states that expanded Medicaid to individuals in states that did not expand Medicaid. These models are estimated for the full newly eligible sample, as well as separately for those meeting the Centers for Disease Control and Prevention

[9] Myerson and Crawford (2020) examine the impact of the ACA on one measure of access to care (insurance coverage) using five years of post-reform data. They do not appear to restrict the sample to those with low incomes who most likely would qualify for Medicaid in their Medicaid expansion analysis, leading them to find smaller than anticipated coverage impacts. Their chronic condition disease set is also smaller and includes a different set of conditions compared to our definition (which is based on the CDC guidelines).

(CDC) definition of having a chronic health condition and those that do not. We also estimate event study models in order to both look for evidence of parallel trends in the pre-period and examine year-by-year changes in our outcomes of interest in the post-period. Finally, we estimate a large number of robustness checks that evaluate the validity of our results.

Our results suggest that the ACA Medicaid expansion led to improvements in access to care among those with and without chronic health conditions. While the magnitude of these improvements are mostly larger for those with a chronic health condition, the differences in magnitude are not statistically significant. For example, the ACA Medicaid expansion led to a 10.1 percentage point increase in the likelihood of having any insurance coverage among the chronic health condition group as compared to a 7.7 percentage point increase among those without a chronic health condition. We also find statistically significant improvements in self-assessed health for those without chronic health conditions. In particular, the ACA Medicaid expansion led to a 3 percentage point increase in the likelihood of reporting good or better health, a 3 percentage point increase in the likelihood of reporting very good or excellent health, and a 2 percentage point increase in the likelihood of reporting excellent health among those without chronic health conditions.

In addition, we find larger improvements in access to care among those with chronic health conditions in states with higher-than-average 2013 uninsured rates for those with chronic conditions. The overall 10.1 percentage point increase in coverage among those with a chronic health condition mentioned above reflects a 14.2 percentage point increase in states with a higher-than-average pre-ACA uninsured rate for the chronic condition group, and a 6.0 percentage point increase in states with a below average pre-ACA uninsured rate for the chronic condition group. These gains in access in states with a higher-than-average pre-ACA chronic condition uninsured rate did not translate into improvements in self-assessed health among those with chronic conditions.

These results shed new light on the differential impact of a coverage expansion on those with versus those without a chronic health condition. This is important as additional states are considering expanding Medicaid, yet little is known about the capacity of these state health care systems and whether or not they are resilient enough to accommodate the diverse care needs of the newly eligible patient population without affecting overall access to care at the system level.

II. Data

Our analysis uses data from the BRFSS, an annual representative telephone survey of health and health behaviors of the US population conducted across all 50 states and the District of Columbia. The BRFSS collects information on more than 300,000 adults per year providing a large sample size that is critical to obtaining meaningful precision because the ACA Medicaid expansion affected health insurance coverage and subsequently health for only a fraction of the population. The BRFSS is a commonly used data source in the ACA literature on health care access and self-assessed health because it continuously surveys individuals on many health outcomes and behaviors while also providing state identifiers to assign individuals to Medicaid expansion and non-expansion states (Simon et al., 2017; Courtemanche et al., 2018a; Courtemanche et al., 2018b; and Courtemanche et al., 2019a).

Our main analysis uses BRFSS data from 2011 to 2018. We begin our sample in 2011 to have a sufficient number of pre-Medicaid expansion years over which we can compare trends between expansion and non-expansion states. In addition, 2011 is the first year in which the

BRFSS included cell phones in its sampling frame. We use data until the last available wave, which is 2018, to follow the long-term effects of the ACA Medicaid expansions.

We limit our sample to low-income adults aged 19–64 years old who were interviewed between 2011 and 2018, as they were the individuals most likely to gain eligibility under the Medicaid expansion. We classify individuals as low-income if their reported household income was below or equal to 100 percent of the Federal Poverty Limit (FPL). The BRFSS provides household income information in brackets, so we use the mean of the household income bracket and the household size to assign a FPL level to each respondent.

We also stratify our sample into those diagnosed with a chronic disease in the past and those who have not. Our primary chronic health condition indicator is based on the CDC chronic disease conditions list (heart disease, cancer, chronic lung disease, stroke, diabetes) using affirmative responses to BRFSS questions about whether the respondent has ever been diagnosed with a heart attack, angina or coronary heart disease, stroke, asthma, skin cancer, any other type of cancer, chronic obstructive pulmonary disease (COPD), emphysema or chronic bronchitis, arthritis, or diabetes.[10] In addition, we experiment with a broader definition that also includes any individuals who responded that they have been told that they either had a depressive disorder, borderline or pre-diabetes, or blindness. We also consider a narrower classification that only includes individuals who reported having a heart attack, heart disease, stroke, skin cancer, other cancer, or COPD. This narrower classification does not include those reporting arthritis, asthma, or diabetes.

Our outcomes focus on insurance coverage, access to medical care, preventive care, and self-reported health status. We measure insurance coverage with a binary variable equal to one if

[10] For further information, see: https://www.cdc.gov/chronicdisease/about/index.htm

the respondent answered “yes” to having any form of health insurance, and zero otherwise. Our access to medical care outcomes are binary variables equal to one if the respondent has a primary care doctor, and having any care needed but foregone because of cost in the past twelve months, and zero otherwise. Our preventative care outcome is a binary variable equal to one if the respondent had a regular physician check-up in the past twelve months, and zero otherwise. Self-reported health status is based on categorical variables of overall health rated as poor, fair, good, very good, or excellent. We create dummy variables from the categorical measure for whether overall health is good or better (i.e., good, very good, or excellent), very good or excellent, and excellent. Three other continuous measures of self-reported health status that we observe include the number of days of the last 30 not in good mental health, not in good physical health, and with health-related functional limitations. Subjective self-assessed health variables have been shown to be correlated with objective measures of health, such as mortality (Ider and Benyamini, 1997; DeSalvo et al., 2006; Phillips et al., 2010).

We create our binary independent variable of interest, whether a state expanded Medicaid, based on information collected by the Kaiser Family Foundation (KFF 2020). By the end of 2018, 31 states and the District of Columbia expanded Medicaid and all expanded Medicaid before 2016 while no state expanded in 2017 or 2018. Most states expanded Medicaid in January 2014, while Michigan expanded in April 2014 and New Hampshire in August 2014. Three states (Pennsylvania, Indiana and Alaska) expanded in January, February, and September of 2015, respectively. Montana and Louisiana expanded in January and July of 2016, respectively. States are classified as part of the Medicaid expansion treatment group beginning the month-year of their expansion. Other state-level variables include indicators for whether states set up their own insurance exchanges, whether these exchanges experienced glitches in 2014 (KFF 2020; Kowalski 2014), and the seasonally adjusted monthly state unemployment rate from the Bureau of Labor Statistics.

Our analysis is done at the individual-year-state level. To better account for regional trends in access to care and health over time we compute each respondent's local area of residence within a state. The publicly available BRFSS does not include geographic identifiers narrower than the state, but does tell us whether the respondent resides in the center city of an MSA, outside the center city of an MSA but inside the county containing the center city, inside a suburban county of an MSA, or not in an MSA. We use this variable to construct four subgroups within each state: those living within a central city, suburbs, non-MSA, and within-state location unavailable (this is the case for respondents interviewed on their cell phone). This follows previous ACA work using the BRFSS (Courtemanche et al. (2018a); Courtemanche et al. (2018b); and Courtemanche et al. (2019a)).

We construct individual-level control variables for age using dummy variables for five-year increments (from 25–29 to 60–64, with 19–24 as the reference group), gender (female), race/ethnicity (non-Hispanic black, Hispanic, and other, with non-Hispanic white as the reference group), marital status (married), education (high school degree, some college, and college graduate, with less than a high school degree as the reference group), household income ($10,000–$15,000, $15,000–$20,000, $20,000–$25,000, $25,000–$35,000, $35,000–$50,000, $50,000–$75,000, and >$75,000, with <$10,000 as the reference group), indicator variables for the number of children under the age of 18 in the household (with no children as the reference group), whether the respondent reports a primary occupation of student, and whether the respondent is unemployed.

Table 3.1 provides pretreatment means and standard deviations of our ten dependent variables of interest for the period of 2011 to 2013 and Appendix Table B.1 reports the means and standard deviations for the control variables. We stratified our full low-income sample into four groups based on whether the respondent's state expanded Medicaid and whether the respondent has a chronic health condition. According to Table 3.1, 79 percent of the full low-income sample had some form of health insurance prior to 2014. Individuals in Medicaid expansion states (columns 2 and 3) were more likely to have insurance prior to 2014 than those in non-expansion states (columns 4 and 5). In both expansion and non-expansion states, individuals with a chronic health condition were more likely to have coverage, a primary care doctor, and a recent check-up as compared to those without such a condition prior to the expansion. Similarly, individuals with a chronic health condition reported worse self-assessed health and more days with health problems as compared to those without a chronic condition in both expansion and non-expansion states.

Table 3.1. Means and Standard Deviations of Dependent Variables by State Medicaid Expansion Status and Chronic Condition Status

		Medicaid Expansion		Non-Expansion	
	Full Sample	Chronic Group	Non-Chronic Group	Chronic Group	Non-Chronic Group
Insurance Coverage	0.788	0.836	0.797	0.776	0.731
	(0.408)	(0.370)	(0.402)	(0.417)	(0.443)
Primary Care Doctor	0.741	0.838	0.708	0.803	0.654
	(0.438)	(0.369)	(0.455)	(0.398)	(0.476)
Check-Up	0.627	0.693	0.582	0.697	0.588
	(0.484)	(0.461)	(0.493)	(0.460)	(0.492)
Cost Barrier	0.192	0.219	0.149	0.276	0.183
	(0.394)	(0.413)	(0.356)	(0.447)	(0.387)
Good or Better Health	0.840	0.723	0.921	0.690	0.923
	(0.366)	(0.448)	(0.269)	(0.463)	(0.266)
Very Good or Excellent	0.536	0.393	0.641	0.354	0.628
Health	(0.499)	(0.488)	(0.480)	(0.478)	(0.483)
Excellent Health	0.204	0.112	0.266	0.100	0.261
	(0.403)	(0.316)	(0.442)	(0.300)	(0.439)
Days Not in Good	3.648	6.294	1.930	6.728	1.820
Physical Health	(7.945)	(10.057)	(5.416)	(10.464)	(5.407)
Days Not in Good	4.108	5.803	3.038	6.077	2.881
Mental Health	(8.194)	(9.607)	(6.790)	(10.009)	(6.803)
Days with Health-	2.508	4.400	1.270	4.781	1.166
Related Limitations	(6.779)	(8.789)	(4.518)	(9.263)	(4.413)

Note: Standard deviations in parentheses.

III. Methods

Our goal is to estimate the effect of the Medicaid expansion among the newly eligible population by comparing outcomes in expansion states to non-expansion states before versus after the expansions. To do so, we follow the literature and estimate difference-in-differences (DD) models. Formally, the model is as follows:

$$y_{iast} = \gamma_0 + \gamma_1 MEDICAID_s + \gamma_2 POST_{st} + \gamma_3 (MEDICAID_s \times POST_{st}) + \gamma_4 X_{iast} + \theta_{at} + \alpha_{as} + \varepsilon_{iast} \quad (1)$$

where y_{iast} represents one of our outcome variables for individual i in area type (central city, rest of MSA, non-MSA, cell phone) *a* in state *s* in month-year *t*. $MEDICAID_s$ equals one if state *s* participated in the ACA's Medicaid expansion and $POST_{st}$ is an indicator equal to one in state *s* beginning at the date of the Medicaid expansion. X_{iast} is a vector of control variables described above, θ_{at} denotes fixed effects for each time-by-area-type combination (e.g., central city in April 2011), and α_{as} denotes fixed effects for each area (e.g., non-MSA in Kentucky). Note that $POST_{st}$ is absorbed by the time fixed effects (θ_{at}) and $MEDICAID_s$ is absorbed by the area fixed effects (α_{as}) so both are not estimated in Equation (1). We use sampling weights to account for the complex survey design.

The DD model requires identification through the parallel trends assumption. To indirectly test the parallel trends assumption and to investigate how the Medicaid expansion effects varied over time, we estimate event-study models where we replace $POST_{st}$ with a set of year dummies:

$$y_{iast} = \varphi + \sum_{t=2011}^{T} \theta_t(MEDICAID_s \times Y_t) + \delta X_{iast} + \alpha_{as} + \varepsilon_{iast} \tag{2}$$

where Y_t, is an indicator for whether year *t* is 2011, 2012, …, 2018, with 2013 being the omitted reference year and the other terms being as described in Equation (1). The effects of the Medicaid expansion during 2014, 2015, …, 2018 are given by $(\theta_{2014} \times MEDICAID_s)$, $(\theta_{2015} \times MEDICAID_s)$, …, $(\theta_{2018} \times MEDICAID_s)$ respectively.

The parallel trends assumption suggests that the trends in outcomes for Medicaid expansion and non-expansion states would have been the same in the absence of the expansion. We can test this indirectly during the pre-expansion period by examining interactions between the Medicaid expansion indicator and the pre-expansion years of 2011 and 2012 relative to the excluded year of 2013. If the coefficients on these interactions (θ_{2011}, θ_{2012}) are insignificant then this is suggestive evidence that the parallel trends assumption holds.

IV. Results

The top panel of Tables 3.2 and 3.3 presents the regression results from Equation (1) that measure the effect of the ACA Medicaid expansion on access to care and self-reported health among low-income individuals, respectively. In the first row we display the full sample effect of the ACA Medicaid expansion on our outcomes of interest. In the second and third row we stratify the sample into chronic and non-chronic subsamples. The bottom panel of Tables 3.2 and 3.3 present the results of the event study analysis based on Equation (2).

VI.A Effects on Access to Care

The first column suggests that the ACA Medicaid expansion increased the probability of having insurance by 8.8 percentage points. Individuals with a chronic health condition are somewhat more likely to gain coverage (10.1 percentage points) as compared to those without a chronic health condition (7.7 percentage points), though the estimates are not statistically significant different from each other. Thus, we find that the Medicaid expansion had large effects on insurance coverage among both groups of individuals. The fact that we do not observe a larger differential increase among those with a chronic condition could reflect both demand-side conditions, as those with a chronic condition were already more likely to seek coverage prior to the ACA, and supply-side conditions, as states may have already been targeting those with chronic health conditions for coverage outreach and initiatives prior to the ACA.

Table 3.2. Effects of the ACA Medicaid Expansion on Health Care Access

	Insurance Coverage	Primary Care Doctor	Check-Up	Cost Barrier
Full Low-Income Group				
Medicaid Expansion 2014-2018	0.088^{***}	0.019^{**}	0.029^{**}	-0.050^{***}
	(0.016)	(0.009)	(0.011)	(0.011)
Sample Size	225,159	225,084	225,931	225,300
Chronic Group				
Medicaid Expansion 2014-2018	0.101^{***}	0.028^{**}	0.019	-0.050^{***}
	(0.022)	(0.013)	(0.017)	(0.014)
Sample Size	119,705	119,622	119,979	119,681
Non-Chronic Group				
Medicaid Expansion 2014-2018	0.077^{***}	0.014	0.036^{**}	-0.048^{***}
	(0.018)	(0.012)	(0.011)	(0.014)
Sample Size	105,454	105,462	105,952	105,619
Event-Study Model Full Income Group				
Medicaid Expansion in 2011	0.001	$-0.019^{***\#\#\#}$	$0.016^{\#\#\#}$	$-0.006^{\#\#\#}$
	(0.016)	(0.009)	(0.019)	(0.010)
Medicaid Expansion in 2012	-0.010	$0.012^{\#}$	$0.023^{\#\#\#}$	0.016
	(0.018)	(0.013)	(0.020)	(0.010)
Medicaid Expansion in 2014	0.048^{**}	-0.003	0.031^{***}	-0.024
	(0.020)	(0.017)	(0.011)	(0.017)
Medicaid Expansion in 2015	0.073^{***}	0.016	$0.038^{***\#}$	-0.033^{**}
	(0.015)	(0.017)	(0.015)	(0.015)
Medicaid Expansion in 2016	0.076^{***}	$0.008^{\#\#\#}$	0.049^{***}	$-0.058^{***\wedge\wedge\#}$
	(0.016)	(0.013)	(0.013)	(0.015)
Medicaid Expansion in 2017	0.079^{***}	0.002	0.023	$-0.062^{***\wedge\wedge\#}$
	(0.018)	(0.016)	(0.015)	(0.023)
Medicaid Expansion in 2018	$0.085^{***\wedge}$	0.0015	$0.047^{***\#\#}$	$-0.050^{***\#\#\#}$
	(0.019)	(0.016)	(0.013)	(0.014)

Note: BRFSS sampling weights are used. All regressions include state×location type and year×location type fixed effects as well as the controls. Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. *** indicates statistically significant at 1% level; ** 5% level; * 10% level. In addition, we denote statistically significantly different effects in a post expansion year relative to 2014 by $^{\wedge\wedge\wedge}$ at 1% level; $^{\wedge\wedge}$ at 5% level; $^{\wedge}$ at 10% level. Finally, we denote statistically significantly different effects for the chronic vs. non-chronic group by $^{\#\#\#}$ at the 1% level; $^{\#\#}$ at the 5% level; $^{\#}$ at the 10% level.

Table 3.3. Effects of ACA Medicaid Expansion on Self-Assessed Health

	Good or Better Health	Very Good or Excellent Health	Excellent Health	Days Not in Good Physical Health	Days Not in Good Mental Health	Days with Health-Related Limitations
Full Low-Income Group						
Medicaid Expansion 2014–2018	0.016	0.020*	0.015**	0.064	-0.006	0.183
	(0.009)	(0.011)	(0.007)	(0.244)	(0.242)	(0.198)
Sample Size	225,182	225,182	225,182	220,504	221,288	222,267
Chronic Group						
Medicaid Expansion 2014–2018	0.005	0.013	0.009	-0.102	-0.025	-0.065
	(0.019)	(0.011)	(0.006)	(0.298)	(0.311)	(0.295)
Sample Size	119,540	119,540	119,540	116,547	117,112	117,333
Non-Chronic Group						
Medicaid Expansion 2014–2018	0.030***	0.029**	0.021*	0.118	0.021	0.346
	(0.009)	(0.011)	(0.011)	(0.215)	(0.336)	(0.220)
Sample Size	105,642	105,642	105,642	103,957	104,176	104,934
Event-Study Model Full Low-Income Group						
Medicaid Expansion in 2011	0.002	-0.008	-0.004	0.239	0.068	0.329
	(0.014)	(0.020)	(0.016)	(0.337)	(0.255)	(0.304)
Medicaid Expansion in 2012	-0.003$^{\#}$	0.001	-0.002	0.250	-0.270	0.162
	(0.014)	(0.020)	(0.009)	(0.291)	(0.239)	(0.240)
Medicaid Expansion in 2014	0.009	0.013	0.006	0.570$^{**\#\#}$	-0.165	0.354
	(0.014)	(0.019)	(0.013)	(0.283)	(0.353)	(0.248)
Medicaid Expansion in 2015	0.011	0.025	0.016	0.119$^{\wedge}$	0.132	0.347
	(0.014)	(0.021)	(0.015)	(0.327)	(0.322)	(0.311)
Medicaid Expansion in 2016	0.017	0.010	0.006	0.009$^{\wedge}$	-0.147	0.340
	(0.012)	(0.015)	(0.011)	(0.307)	(0.314)	(0.274)
Medicaid Expansion in 2017	0.017	0.005	0.013	0.368	-0.227	0.556
	(0.019)	(0.019)	(0.016)	(0.399)	(0.354)	(0.361)
Medicaid Expansion in 2018	0.023	0.015	0.015	0.343	-0.213$^{\#}$	0.366
	(0.016)	(0.024)	(0.016)	(0.427)	(0.352)	(0.275)

Note: BRFSS sampling weights are used. All regressions include state×location type and year×location type fixed effects as well as the controls. Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. *** indicates statistically significant at 1% level; ** 5% level; * 10% level. In addition, we denote statistically significantly different effects in a post expansion year relative to 2014 by $^{\wedge\wedge\wedge}$ at 1% level; $^{\wedge\wedge}$ at 5% level; $^{\wedge}$ at 10% level. Finally, we denote statistically significantly different effects for the chronic vs. non-chronic group by $^{\#\#\#}$ at the 1% level; $^{\#\#}$ at the 5% level; $^{\#}$ at the 10% level.

We also observe that the ACA Medicaid expansion led to increases in the probability of having a primary care doctor (1.9 percentage points) within the full low-income sample. This effect seems to be driven by those with a chronic condition (2.8 percentage points) as we observe no statistically significant effect among the healthy. In terms of routine check-up visits, in the full low-income sample the ACA Medicaid expansion increased the likelihood of reporting a recent check up by 2.9 percentage points, and unsurprisingly this effect is driven by the healthy (3.6 percentage points) rather than the sick. In other words, we might expect those with a chronic health condition to be more diligent about getting a routine check-up than those without a chronic condition prior to gaining coverage. Reporting cost being a barrier to seeking care was reduced after the ACA Medicaid expansion by 5.0 percentage points for the full low-income sample with similar reductions for both those with a chronic condition (5.0 percentage points) and the healthy (4.8 percentage points).

The bottom panel of Table 3.2 reports the results of our event study analysis. An indirect test of the parallel trends assumption necessary to causally interpret our results involves examining the interactions of the ACA Medicaid expansion indicator with the pre-reform years of 2011 and 2012. If expansion states and non-expansion states are comparable, we would not expect these pre-ACA interactions to be statistically significant. Among the eight pre-ACA interactions in Table 3.2, only one is statistically significant (and none out of twelve in Table 3.3, as will be discussed below). This gives us confidence in giving our results a causal interpretation.

The interactions of the ACA Medicaid expansion indicator with the post-reform years of 2014 through 2018 allow us to unpack the combined post-period impact estimates in the top panel into year-by-year estimates. With respect to insurance coverage, we see year-by-year growth in the impact of the ACA Medicaid expansion on the full low-income sample, with a 4.8 percentage point increase in the likelihood of being covered in 2014 growing to an 8.5 percentage point increase in 2018 that is statistically different from the 2014 effect. We also see a stronger impact over time on reporting cost being a barrier to care, with a 2.4 percentage point decrease in 2014 and a 5.0 percentage point decrease in 2018, and an increase over time in respondents receiving a recent check-up, with a 3.1 percentage point increase in 2014 growing to a 4.7 percentage point increase in 2018. However, these coefficients are not statistically different in 2018 relative to 2014.[11] Finally, we see no consistent pattern with respect to reporting having a primary care physician, with each single year interaction being statistically insignificant.

VI.B Effects on Health

The top panel of Table 3.3 reports some evidence that the ACA Medicaid expansion improved health for the full low-income sample after five years, with marginal improvements in individuals reporting “very good or excellent” health (2 percentage points) and “excellent” health (1.5 percentage points). Stratifying our sample shows that these results are driven by those without a chronic health condition. Among this group, Table 3.3 reports improvements in reporting “good or better” health (3.0 percentage points), “very good or excellent” health (2.9 percentage points), and “excellent” health (2.1 percentage points). These findings may be less surprising given that the chronically ill may require significantly more health investments to improve their overall well-being. On the other hand, the healthy may only require small levels of health services to close gaps in care that can improve their overall well-being.

As in Table 3.2, the bottom panel of Table 3.3 reports results from the event study analysis of self-assessed health. None of the twelve pre-ACA interactions with the ACA Medicaid expansion indicator are statistically significant, suggesting that the parallel trends assumption holds, and we can interpret our results causally. The post-ACA year-by-year interactions show essentially no statistically significant impact on self-assessed health among the full low-income sample, despite the statistically significant increases in reporting “very good or

[11] The reduction in reporting cost being a barrier to care was statistically significantly larger in 2016 (5.8 percentage points) and 2017 (6.2 percentage points) relative to 2014 (2.4 percentage points).

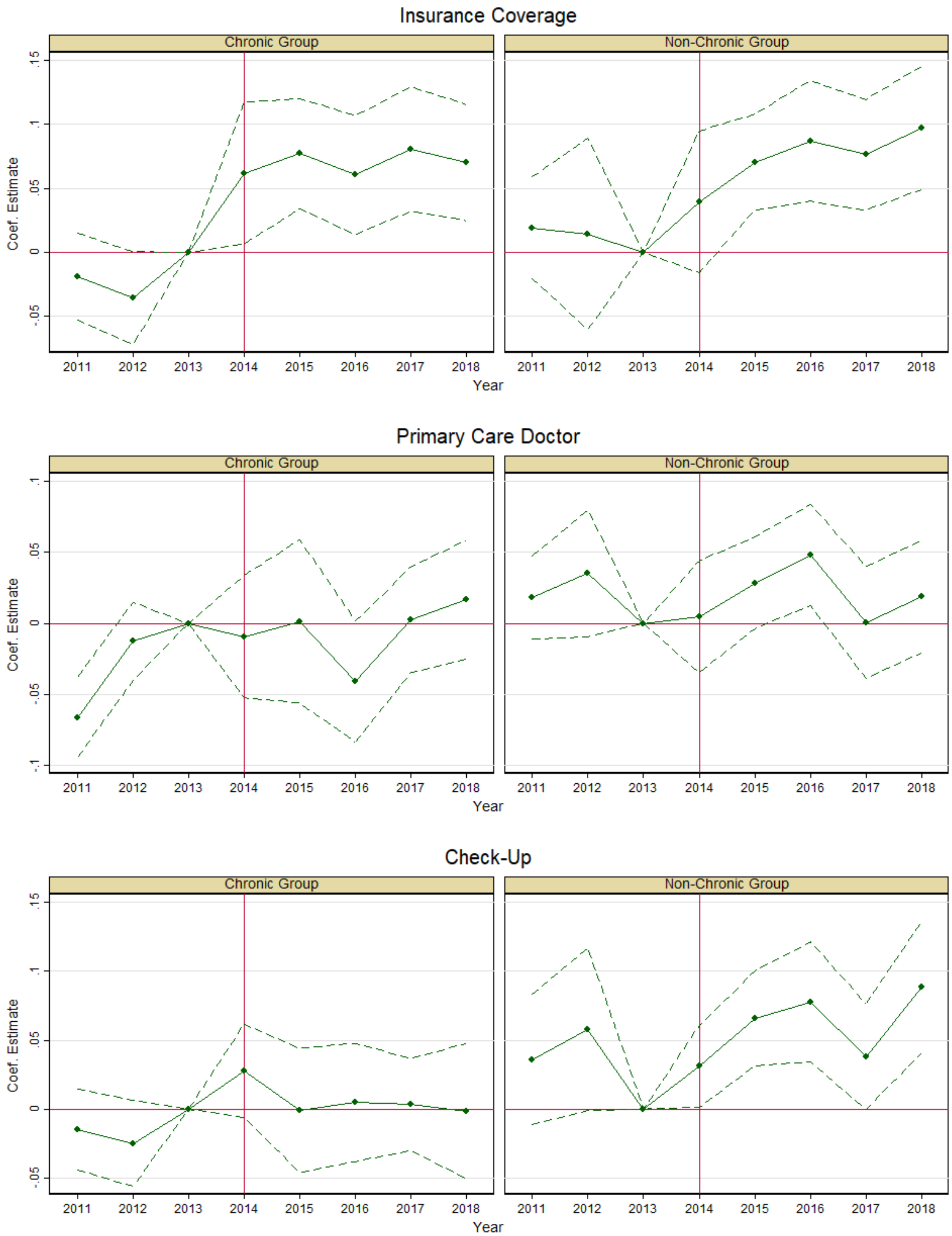
excellent" health and "excellent" health in the combined post-period results.[12] This is likely due in part to less statistical power to identify each individual post-period year interaction as compared to estimating one combined post-period effect.

VI.C Event Study Results by Chronic Condition Status

Figures 3.1 and 3.2 plot the event study coefficients and confidence intervals over time on the access to care and self-reported health outcomes by chronic condition status, while Appendix Tables I and J reports the coefficients and standard errors associated with these event study graphs.[13] Our event study results suggest that gains in insurance coverage materialized immediately after 2014 with little growth in coverage over time for those with a chronic health condition and those without such a condition. Growth in having a primary care doctor occurred predominately in the early years (2014–2016) for those without a chronic health condition, with little effect in later years. The effect of the ACA Medicaid expansion on routine doctor checkups was also concentrated among those without a chronic health condition with a gradual emergence, highlighting that maneuvering through the health care system may take time.

[12] We do see a statistically significant increase in the number of days not in good physical health in 2014.
[13] Appendix Figure 1 provides similar plots as in figures 1 and 2 for each outcome for the full low-income sample.

Figure 3.1. Health Care Access Event Study Results by Chronic Condition Status



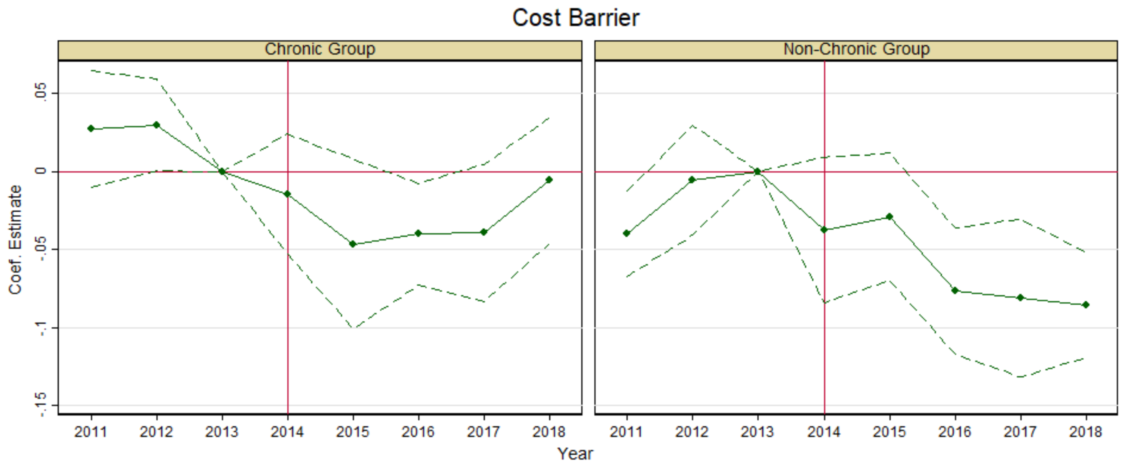
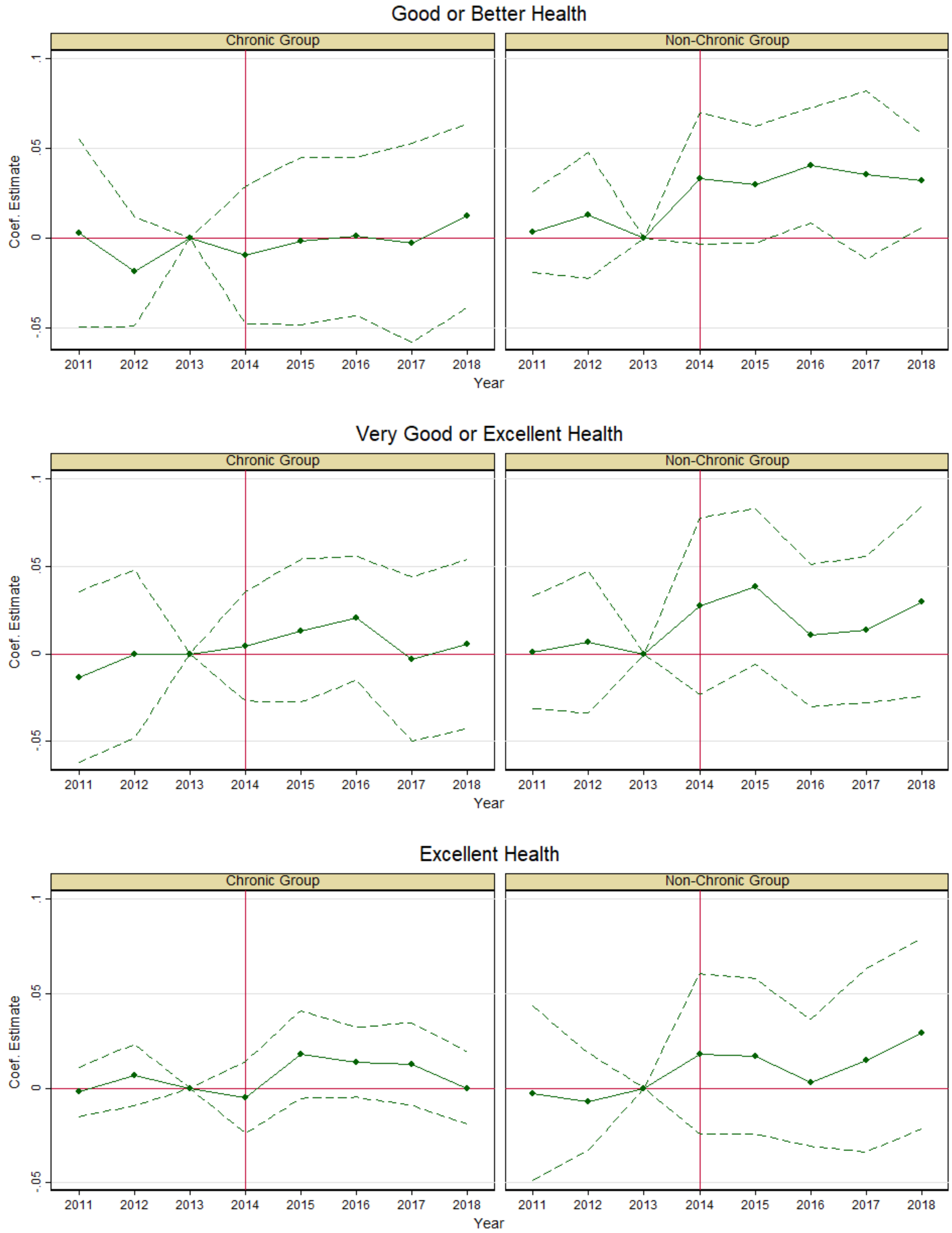
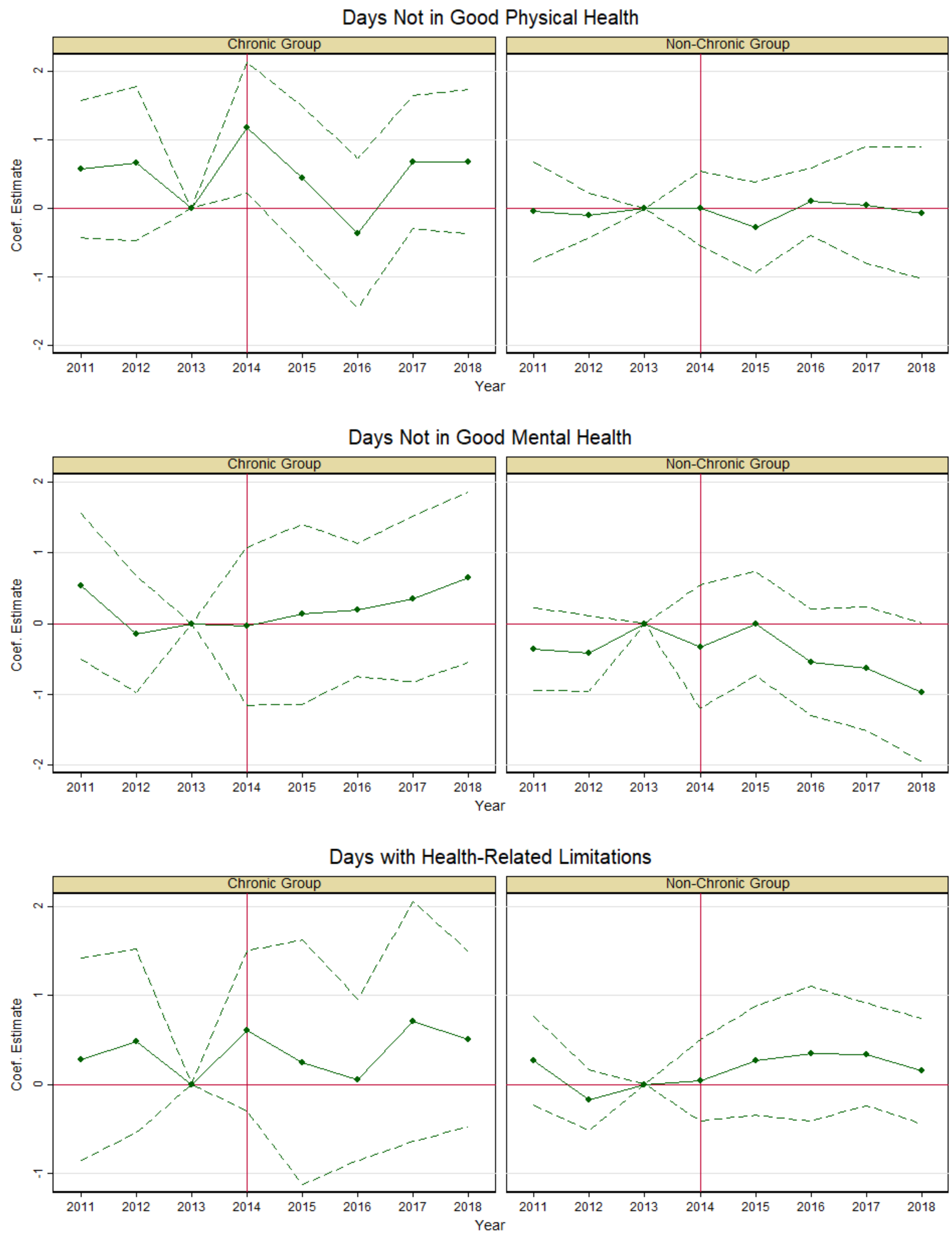

Cost Barrier
Chronic Group
Non-Chronic Group
Coef. Estimate
.05
0
-.05
-.1
-.15
2011
2012
2013
2014
2015
2016
2017
2018
Year

Figure 3.2. Self-Assessed Health Event Study Results by Chronic Condition Status




Days Not in Good Physical Health
Chronic Group
Non-Chronic Group
Coef. Estimate
2011 2012 2013 2014 2015 2016 2017 2018
Year
Days Not in Good Mental Health
Chronic Group
Non-Chronic Group
Coef. Estimate
Year
Days with Health-Related Limitations
Chronic Group
Non-Chronic Group
Coef. Estimate
Year

We also observe different patterns of reductions in cost being a barrier to care for those with and without a chronic health condition. Those with a chronic health condition saw a consistent drop in cost being a barrier of care that disappears by 2018, while those without a chronic health condition experienced a growing impact in each year. By 2018 the effect for those without a chronic health condition was almost 15 times as large as compared to those with a chronic health condition (an 8.6 percentage point reduction compared to 0.6 percentage point reduction, respectively). Regarding self-assessed health in Figure 3.2, the improvements in good or better health among those without a chronic health condition emerged in 2014 and persist throughout 2018, while the improvements in very good or excellent health fluctuate by year. We observe no clear pattern of improvements in self-reported health for those with chronic health conditions.

VI.D Varying the Definition of the Chronic Health Condition Group

As mentioned, our primary chronic health condition indicator is based on the CDC chronic disease conditions list including those reporting having a heart attack, angina or coronary heart disease, stroke, asthma, skin cancer, any other type of cancer, chronic obstructive pulmonary disease (COPD), emphysema or chronic bronchitis, arthritis, or diabetes. We report results here of both a broader definition that also includes any individuals who responded that they have been told that they either had a depressive disorder, borderline or pre-diabetes, or blindness and a narrower classification that only includes individuals who reported having a heart attack, heart disease, stroke, skin cancer, other cancer, or COPD. Table 3.4 reports our health care access results for our full low-income sample, our baseline chronic vs. non-chronic stratification (all repeated from Table 3.2), as well as our broader chronic vs. non-chronic stratification and our narrower chronic vs. non-chronic stratification. Table 3.5 does the same thing for our self-assessed health results.

Table 3.4. Effects on Health Care Access—Broadening vs. Narrowing the Chronic Classification

	Insurance Coverage	Primary Care Doctor	Check-Up	Cost Barrier
Full Low-Income Group				
Medicaid Expansion 2014–2018	0.088***	0.019**	0.029**	-0.050***
	(0.016)	(0.009)	(0.011)	(0.011)
Sample Size	225,159	225,084	225,931	225,300
Chronic Group				
Medicaid Expansion 2014–2018	0.101***	0.028**	0.019	-0.050***
	(0.022)	(0.013)	(0.017)	(0.014)
Sample Size	119,705	119,622	119,979	119,681
Non-Chronic Group				
Medicaid Expansion 2014–2018	0.077***	0.014	0.036***	-0.048***
	(0.018)	(0.012)	(0.011)	(0.014)
Sample Size	105,454	105,462	105,952	105,619
*Chronic **Broad** Group*				
Medicaid Expansion 2014–2018	0.100***	0.033**	0.029*	-0.053***
	(0.021)	(0.016)	(0.015)	(0.013)
Sample Size	138,710	138,616	139,073	138,698
*Non-Chronic **Broad** Group*				
Medicaid Expansion 2014–2018	0.074***	0.011	0.027*	-0.034*
	(0.018)	(0.013)	(0.015)	(0.017)
Sample Size	86,449	86,468	86,858	86,602
*Chronic **Narrow** Group*				
Medicaid Expansion 2014–2018	0.119***	0.046***##	0.030**	-0.058***
	(0.022)	(0.011)	(0.015)	(0.016)
Sample Size	58,090	58,079	58,221	58,089
*Non-Chronic **Narrow** Group*				
Medicaid Expansion 2014–2018	0.082***	0.014	0.027**	-0.046***
	(0.018)	(0.011)	(0.012)	(0.012)
Sample Size	167,069	167,005	167,710	167,211

Note: BRFSS sampling weights are used. All regressions include state×location type and year×location type fixed effects as well as the controls. Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. *** indicates statistically significant at 1% level; ** 5% level; * 10% level. We denote statistically significantly different effects for the chronic vs. non-chronic group ### at the 1% level; ## at the 5% level; # at the 10% level.

Table 3.5. Effects on Self-Assessed Health—Broadening vs. Narrowing the Chronic Classification

	Good or Better Health	Very Good or Excellent Health	Excellent Health	Days Not in Good Physical Health	Days Not in Good Mental Health	Days with Health-Related Limitations
Full Low-Income Group						
Medicaid Expansion 2014–2018	0.016 (0.009)	0.020* (0.011)	0.015** (0.007)	0.064 (0.244)	-0.006 (0.242)	0.183 (0.198)
Sample Size	225,182	225,182	225,182	220,504	221,288	222,267
Chronic Group						
Medicaid Expansion 2014–2018	0.005 (0.019)	0.013 (0.011)	0.009 (0.006)	-0.102 (0.298)	-0.025 (0.311)	-0.065 (0.295)
Sample Size	119,540	119,540	119,540	116,547	117,112	117,333
Non-Chronic Group						
Medicaid Expansion 2014–2018	0.030** (0.009)	0.029** (0.011)	0.021* (0.011)	0.118 (0.215)	0.021 (0.336)	0.346 (0.220)
Sample Size	105,642	105,642	105,642	103,957	104,176	104,934
*Chronic **Broad** Group*						
Medicaid Expansion 2014–2018	0.004 (0.015)	0.017 (0.011)	0.010* (0.005)	-0.026 (0.254)	0.124 (0.306)	0.048 (0.256)
Sample Size	138,545	138,545	138,545	135,111	135,731	136,058
*Non-Chronic **Broad** Group*						
Medicaid Expansion 2014–2018	0.032*** (0.010)	0.029*** (0.009)	0.023** (0.010)	0.039 (0.217)	-0.188 (0.277)	0.255 (0.196)
Sample Size	86,637	86,637	86,637	85,393	85,557	86,209
*Chronic **Narrow** Group*						
Medicaid Expansion 2014–2018	-0.008 (0.022)	0.000## (0.014)	0.003# (0.006)	0.046 (0.405)	0.174 (0.521)	0.215 (0.367)
Sample Size	57,978	57,978	57,978	56,533	56,694	56,751
*Non-Chronic **Narrow** Group*						
Medicaid Expansion 2014–2018	0.022** (0.010)	0.025** (0.009)	0.018** (0.008)	0.056 (0.246)	-0.050 (0.258)	0.193 (0.197)
Sample Size	167,204	167,204	167,204	163,971	164,594	165,516

Note: All regressions include state×location type and year×location type fixed effects as well as the controls. Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. *** indicates statistically significant at 1% level; ** 5% level; * 10% level. BRFSS sampling weights are used. We denote statistically significantly different effects for the chronic vs. non-chronic group ### at the 1% level; ## at the 5% level; # at the 10% level.

Table 3.4 suggests that the baseline findings of somewhat larger improvements in insurance coverage, having a primary care doctor, and reporting cost being a barrier to receiving care among the chronic health condition group persist whether we broaden or narrow inclusion in the group. The difference between the chronic (4.6 percentage point increase) vs. non chronic (1.4 percentage point increase) groups becomes statistically significant for the primary care doctor outcome under the narrow definition of having a chronic health condition. Our baseline results suggest a larger improvement in the likelihood of receiving a check-up among the non-chronic group, while there is almost no difference for either the broader (2.9 percentage point increase vs. 2.7 percentage point increase) or narrower (3.0 percentage point increase vs. 2.7 percentage point increase) chronic health condition classification. None of these differences are statistically significantly different. That being said, the baseline coefficient associated with receiving a check-up (1.9 percentage point increase) for the chronic group becomes statistically significant when we either broaden (2.9 percentage point increase) or narrow (3.0 percentage point increase) the inclusion criteria.

Table 3.5 suggests that the larger relative increases we observed in our baseline non-chronic condition group in reporting "good or better" health, "very good or excellent" health, or "excellent" health remain whether we broaden or narrow the definition of a chronic health condition. The difference between the narrow chronic and the narrow non-chronic groups is statistically significant for the "very good or excellent" health outcome and the "excellent" health outcome. The coefficients associated with the three "days of poor health" measures are almost never statistically significant across all three chronic health condition classifications. Thus, Tables 3.4 and 3.5 suggest that our baseline results are not sensitive to changes in the classification of chronic health conditions.

VI.E Other Specification Checks

Appendix Tables K and L present results from our specification checks for the full low-income sample. The first set of results evaluates whether our main results hold when we change the composition of the sample. Specifically, we drop those who were interviewed via cell phone, those who were young enough (19–25 years old) to have been potentially impacted by the ACA dependent coverage expansion, states that expanded Medicaid "early" prior to 2014, and states that expanded Medicaid after 2014.[14] We also evaluate whether dropping the sample weights materially affects our outcomes. In our last check we allow those with higher incomes to remain in the sample by raising the income limit to anyone below 200 percent of the FPL (rather than 100 percent of the FPL).

According to Appendix Table B.4, our health care access results are remarkably robust to the changing sample. Dropping 19–25 year-olds reduces the effect on having a primary care doctor, and results from the sample including everyone making less than 200 percent of the FPL expectedly attenuates our findings across all outcomes to at least some degree. Appendix Table B.5 suggest that coefficients on our self-assessed health outcomes display a marginally larger amount of variability as we change the sample. Specifically, we observe that our baseline statistically significant results on "very good or excellent" health (2.0 percentage points) and "excellent" health (1.5 percentage points) from Table 3.3 both become statistically insignificant in two specifications (drop 19–25 year-olds and increase the sample income limit) and one of the

[14] Early expansion states are California (11/1/2010) Connecticut (4/1/2010), D.C. (7/1/2010), Minnesota (3/1/2010), New Jersey (4/14/2011), and Washington (1/3/2011). Late expansion states are Alaska (9/1/2015), Indiana (2/1/2015), Louisiana (7/1/2016), Michigan (4/1/2014), Montana (1/1/2016), New Hampshire (8/15/2014), and Pennsylvania (1/1/2015), Details can be reviewed at the Kaiser Family foundation websites https://www.kff.org/health-reform/issue-brief/states-getting-a-jump-start-on-health/ and https://www.kff.org/health-reform/state-indicator/state-activity-around-expanding-medicaid-under-the-affordable-care-act/?currentTimeframe=0&sortModel=%7B%22colId%22:%22Location%22,%22sort%22:%22asc%22%7D.

two become statistically insignificant in two other specifications (drop cell phone uses and drop early expansion states) due to larger standard errors and an attenuation of the estimated coefficients. Overall, our specification checks suggest that our main results are largely unchanged by restricting and expanding the sample, and therefore provide additional support for the validity of these results.

VI.F Exploring Heterogeneous Effects

There are several potential explanations as to why we do not observe differential increases in access among those with a chronic condition compared to those without a chronic condition. For example, state eligibility requirements to qualify for Medicaid in the pre-ACA period were typically more generous for those with chronic health conditions, ceteris paribus. Thus, although one might expect the Medicaid expansion to increase demand for access to care for those with chronic health conditions, because such individuals are more likely to enroll given the higher value they place on coverage, a smaller share of those individuals were uncovered prior to the ACA. This is illustrated in Figures 3.3 and 3.4 which display 2013 uninsured rates stratified by chronic condition status and by state, with Figure 3.3 focusing on expansion states and Figure 3.4 on non-expansion states, respectively.[15] Another reason may be that states with higher percentages of uninsured individuals with a chronic health condition did not expand their Medicaid programs, as suggested by Figures 3.3 and 3.4, which could attenuate the overall increase in demand for access that we observe in our main results in Table 3.2.[16]

[15] Among expansion states, Washington D.C had the lowest uninsured rate for those with chronic health conditions at 9.8 percent and Arkansas had the highest rate at 48.4 percent. Among non-expansions states, Maine had the lowest rate at 13.7 percent and Texas had the highest rate at 53.3 percent. However, individuals with chronic conditions generally had lower uninsured rates in each state compared to those without a chronic condition.
[16] The average uninsured rate for those with chronic health conditions in 2013 was 29.38 percent in expansion states and 43.76 percent in non-expansion states.

An additional consideration is whether access to care changed pre- to post-ACA due to Medicaid policy changes that impacted the supply side of the health care market. For example, primary care providers initially received higher reimbursement rates for accepting new Medicaid patients after the ACA (Snyder et al., 2014). Thus, even among individuals with a chronic health condition that had coverage pre-ACA, access to care may have improved due to such supply-side factors. Ultimately our baseline results reflect both these supply-side factors and the demand-side factors mentioned above.

In an attempt to isolate the demand-side response associated with gaining coverage, we conduct additional analysis where we add controls for medical care market supply-side factors. Specifically, we control for the state specific changes after the ACA in Medicaid primary care reimbursement fees and the total number of primary care providers.[17] Because these supply-side controls are only available through 2016, we compare them to an updated version of our baseline analysis that only includes data through 2016.

[17] County Health Rankings and Roadmaps, a Robert Wood Johnson Foundation program, publishes county- and state-level data that include the average Medicare reimbursement per enrollee (see https://www.countyhealthrankings.org/app/). We use these data and the Medicaid-to-Medicare Fee Indices published by the Kaiser Family Foundation to obtain the average Medicaid reimbursement per enrollee (see Zuckerman and Goin (2012), Zuckerman, Skopec, and McCormack (2014), and Zuckerman, Skopec, and Epstein (2017)). The total number of primary care providers are calculated using data from the National Plan and Provider Enumeration System (NPPES) National Provider Identifier (NPI) Registry (see https://www.resdac.org/articles/overview-nppesnpi-downloadable-file).

Figure 3.3. Uninsured Rate for Chronic and Non-Chronic Group in 2013 for Medicaid Expansion States

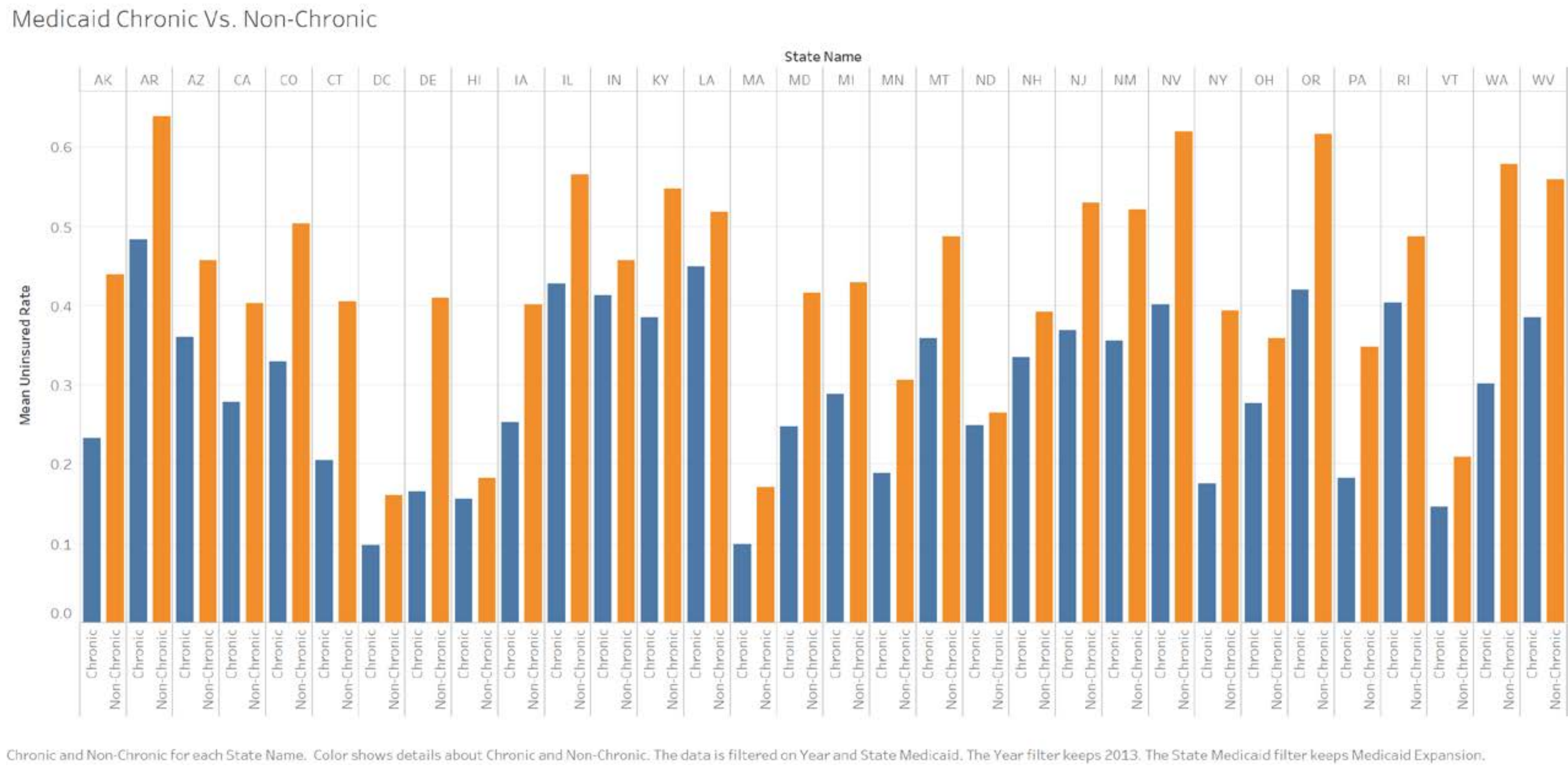
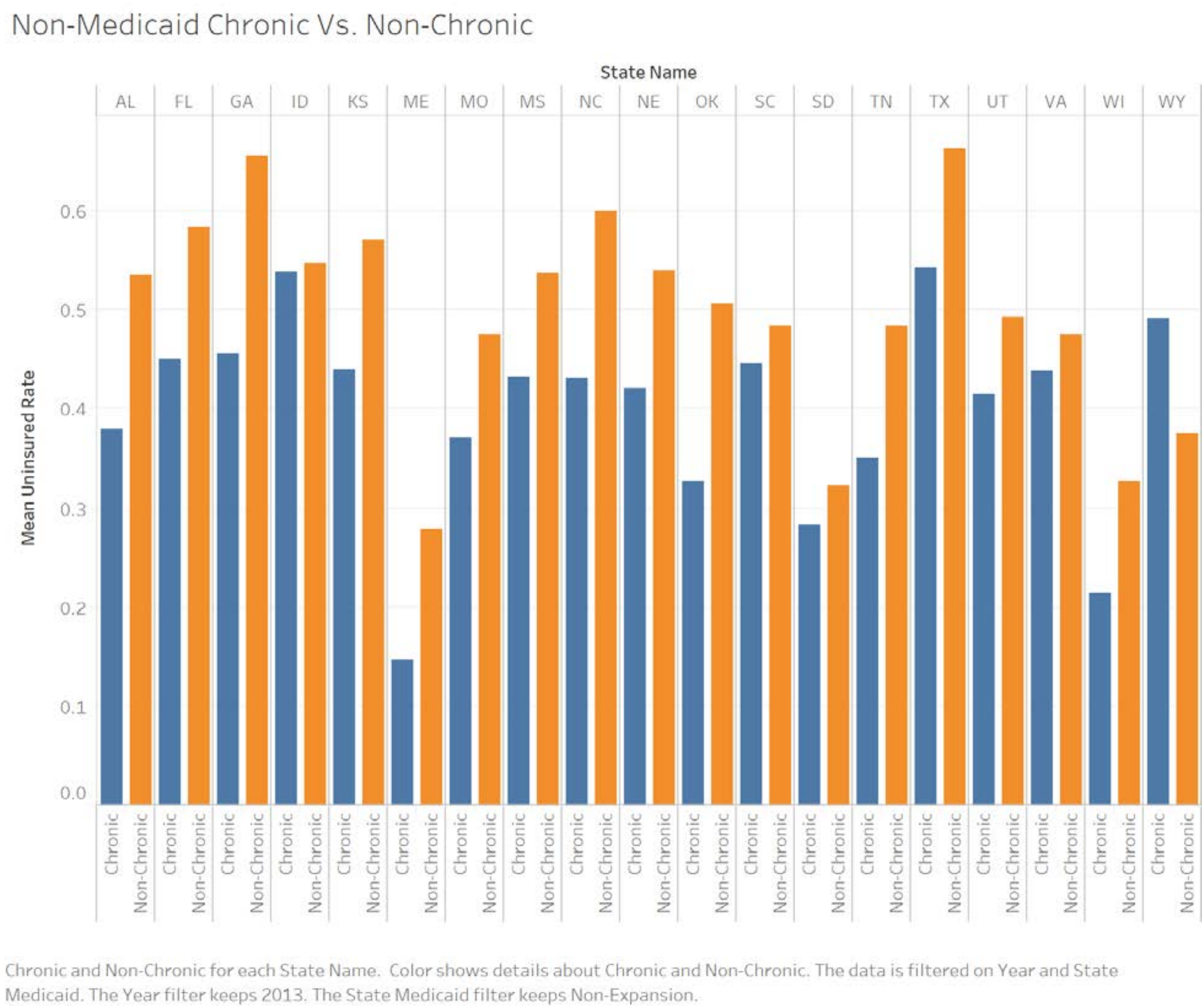


Figure 3.4. Uninsured Rate for Chronic and Non-Chronic Group in 2013 for Non-Medicaid Expansion States



Tables 3.6 and 3.7 report the results of this analysis. We find that including supply control variables somewhat increases the coefficient estimates for both the chronic and non-chronic sub-samples, though most of them are not statistically significantly different compared to our updated baseline results. However, the new coefficients from the regressions with the supply-side controls are economically meaningful. For example, among the chronic sub-sample we observe in Table 3.7 a statistically significant increase of 1.5 percentage points in reporting “excellent" health, and a statistically significant reduction in days not in good physical health of 0.568 days. Among the non-chronic sub-sample, we observe a statistically significant 3.7 percentage point reduction in cost being a barrier to care in Table 3.6 and a statistically significant 2.9 percentage point increase in reporting “excellent” health in Table 3.7.

In other additional analysis we estimate our chronic vs. non-chronic regressions separately for states with above and below median pre-ACA chronic group uninsured rates. We do this to account for other unobservable supply-side differences between states with “high” versus “low” uninsured rates. A second reason for doing this is that we would expect a stronger demand-side impact of the ACA among individuals with a chronic health condition in states with high chronic group uninsured rates as compared to individuals with a chronic health condition in states with low chronic group uninsured rates.

Table 3.6. Effects on Health Care Access—Controlling for Medical Care Market Supply-Side Factors

	Insurance Coverage	Primary Care Doctor	Check-Up	Cost Barrier
Full Low-Income Group				
Medicaid Expansion 2014–2016	0.076***	0.006	0.022*	-0.040***
	(0.014)	(0.009)	(0.011)	(0.013)
Sample Size	143,737	143,730	144,217	143,845
Chronic Group				
Medicaid Expansion 2014–2016	0.092***	-0.001	0.012	-0.040**
	(0.023)	(0.015)	(0.018)	(0.016)
Sample Size	76,335	76,307	76,497	76,327
Non-Chronic Group				
Medicaid Expansion 2014–2016	0.063***	0.014	0.029**	-0.043
	(0.016)	(0.016)	(0.013)	(0.017)
Sample Size	67,402	67,423	67,720	67,518
Chronic Group – Supply Controls				
Medicaid Expansion 2014–2016	0.091***	0.009^	0.016	-0.043**
	(0.025)	(0.015)	(0.018)	(0.016)
Sample Size	75,445	75,418	75,607	75,439
Non-Chronic Group – Supply Controls				
Medicaid Expansion 2014–2016	0.072***	0.025	0.037***	-0.037**
	(0.017)	(0.017)	(0.013)	(0.017)
Sample Size	66,903	66,925	67,221	67,019

Note: BRFSS sampling weights are used. All regressions include state×location type and year×location type fixed effects as well as the controls. Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. *** indicates statistically significant at 1% level; ** 5% level; * 10% level. We denote statistically significantly different effects for the chronic vs. non-chronic group ### at the 1% level; ## at the 5% level; # at the 10% level. We also denote statistically significant different effects for the chronic (non-chronic) supply control groups compared to the chronic (non-chronic) baseline results with ^^^ at the 1% level, ^^ at the 5% level, and ^ at the 10% level.

Table 3.7. Effects on Self-Assessed Health—Controlling for Medical Care Market Supply-Side Factors

	Good or Better Health	Very Good or Excellent Health	Excellent Health	Days Not in Good Physical Health	Days Not in Good Mental Health	Days with Health-Related Limitations
Full Low-Income Group						
Medicaid Expansion 2014–2016	0.019*	0.021*	0.016	-0.150	0.009	-0.055
	(0.009)	(0.012)	(0.011)	(0.221)	(0.286)	(0.246)
Sample Size	143,743	143,743	143,743	140,820	141,300	141,945
Chronic Group						
Medicaid Expansion 2014–2016	0.012	0.017	0.009	-0.421	-0.282	-0.565
	(0.020)	(0.014)	(0.008)	(0.334)	(0.364)	(0.453)
Sample Size	76,224	76,224	76,224	74,344	74,695	74,830
Non-Chronic Group						
Medicaid Expansion 2014–2016	0.033***	0.029**	0.025	-0.042	0.178	0.262
	(0.009)	(0.013)	(0.015)	(0.176)	(0.437)	(0.243)
Sample Size	67,519	67,519	76,224	66,476	66,605	67,115
Chronic Group – Supply Controls						
Medicaid Expansion 2014–2016	0.025^	0.019	0.015**^^	-0.568*	-0.377	-0.538
	(0.018)	(0.015)	(0.007)	(0.303)	(0.368)	(0.405)
Sample Size	75,335	75,335	75,335	73,492	73,830	73,971
Non-Chronic Group – Supply Controls						
Medicaid Expansion 2014–2016	0.036***	0.042***^	0.029*	-0.231^	-0.026	0.095
	(0.011)	(0.015)	(0.017)	(0.157)	(0.437)	(0.216)
Sample Size	67,019	67,019	67,019	65,988	66,118	66,618

Note: All regressions include state×location type and year×location type fixed effects as well as the controls. Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. *** indicates statistically significant at 1% level; ** 5% level; * 10% level. BRFSS sampling weights are used. We denote statistically significantly different effects for the chronic vs. non-chronic group ### at the 1% level; ## at the 5% level; # at the 10% level. We also denote statistically significant different effects for the chronic (non-chronic) supply control groups compared to the chronic (non-chronic) baseline results with ^^^ at the 1% level, ^^ at the 5% level, and ^ at the 10% level.

Tables 3.8 and 3.9 report the results of this analysis. We find that the effects on access to care are generally much stronger for the chronic group in states with high pre-ACA chronic uninsured rates (14.2 percentage point increase in coverage and a 5.2 percentage point increase in the probability of having a primary care doctor) compared to the full sample chronic group results (10.1 percentage point increase in coverage and a 2.8 percentage point increase in the probability of having a primary care doctor), while we observe little difference in access to care for the non-chronic group when making the same comparison. In low pre-ACA chronic uninsured rate states, we observe statistically significant increases in insurance coverage and reduced cost being a barrier to care for the chronic group that are attenuated compared to the full sample chronic group results. On the other hand, we find similar strong increases in insurance take-up and regular check-ups for the non-chronic group in both low and high pre-ACA chronic uninsured rate states. These findings suggest that access to care was generally better for those with chronic health conditions in low chronic uninsured rate states compared to high chronic uninsured rate states prior to the ACA, leading to larger gains in access among the chronic group in high chronic uninsured rate states after the ACA Medicaid expansion. Conversely, the non-chronic group broadly benefited from the ACA Medicaid expansion across both high and low pre-ACA chronic uninsured rate states.

In terms of self-reported health, we do not observe any changes in self-reported health for the chronic group in high or low pre-ACA chronic uninsured rate states, but similarly strong improvements in self-reported health in high and low pre-ACA chronic uninsured rate states for the non-chronic group. Overall, the findings in Tables 3.8 and 3.9 highlight that the ACA Medicaid expansion greatly improved access to care for those with chronic conditions in states that were relatively less generous in coverage before the ACA for single adults, and that non-chronic individuals received significant improvements in access to care that immediately translated into measurable improvements in self-reported health.

Table 3.8. Effects on Health Care Access—High vs. Low Pre-ACA Chronic Condition Uninsured Rates

	Insurance Coverage	Primary Care Doctor	Check-Up	Cost Barrier
Full Low-Income Group				
Medicaid Expansion 2014–2018	0.088^{***}	0.019^{**}	0.029^{**}	-0.050^{***}
	(0.016)	(0.009)	(0.011)	(0.011)
Sample Size	225,159	225,084	225,931	225,300
Chronic Group				
Medicaid Expansion 2014–2018	0.101^{***}	0.028^{**}	0.019	-0.050^{***}
	(0.022)	(0.013)	(0.017)	(0.014)
Sample Size	119,705	119,622	119,979	119,681
Non-Chronic Group				
Medicaid Expansion 2014–2018	0.077^{***}	0.014	0.036^{***}	-0.048^{***}
	(0.018)	(0.012)	(0.011)	(0.014)
Sample Size	105,454	105,462	105,952	105,619
Chronic Group – High Pre-ACA Chronic Condition Uninsured Rate States				
Medicaid Expansion 2014–2018	$0.142^{***\#\#\#\wedge\wedge\wedge}$	$0.052^{***\#}$	0.034^{**}	-0.061^{***}
	(0.028)	(0.013)	(0.016)	(0.019)
Sample Size	65,094	65,041	65,250	65,062
Non-Chronic Group – High Pre-ACA Chronic Condition Uninsured Rate States				
Medicaid Expansion 2014–2018	0.077^{***}	0.021	0.044^{***}	$-0.066^{***\wedge}$
	(0.027)	(0.014)	(0.015)	(0.017)
Sample Size	55,299	55,337	55,556	55,365
Chronic Group – Low Pre-ACA Chronic Condition Uninsured Rate States				
Medicaid Expansion 2014–2018	0.060^{***}	-0.000	0.014	-0.035^{**}
	(0.014)	(0.017)	(0.025)	(0.013)
Sample Size	54,611	54,581	54,729	54,619
Non-Chronic Group – Low Pre-ACA Chronic Condition Uninsured Rate States				
Medicaid Expansion 2014–2018	0.085^{***}	0.022	0.046^{**}	-0.019
	(0.019)	(0.018)	(0.019)	(0.021)
Sample Size	50,155	50,125	50,396	50,254

Note: BRFSS sampling weights are used. All regressions include state×location type and year×location type fixed effects as well as the controls. Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. *** indicates statistically significant at 1% level; ** 5% level; * 10% level. We denote statistically significantly different effects for the chronic vs. non-chronic group $^{\#\#\#}$ at the 1% level; $^{\#\#}$ at the 5% level; $^{\#}$ at the 10% level. We also denote statistically significant different effects for the chronic (non-chronic) group in states with a high pre-ACA uninsured rate compared to the chronic (non-chronic) group in states with a low pre-ACA uninsured rate with $^{\wedge\wedge\wedge}$ at the 1% level, $^{\wedge\wedge}$ at the 5% level, and $^{\wedge}$ at the 10% level.

Table 3.9. Effects on Self-Assessed Health—High vs. Low Pre-ACA Chronic Condition Uninsured Rates

	Good or Better Health	Very Good or Excellent Health	Excellent Health	Days Not in Good Physical Health	Days Not in Good Mental Health	Days with Health-Related Limitations
Full Low-Income Group						
Medicaid Expansion	0.016	0.020*	0.015**	0.064	-0.006	0.183
2014–2018	(0.009)	(0.011)	(0.007)	(0.244)	(0.242)	(0.198)
Sample Size	225,182	225,182	225,182	220,504	221,288	222,267
Chronic Group						
Medicaid Expansion	0.005	0.013	0.009	-0.102	-0.025	-0.065
2014–2018	(0.019)	(0.011)	(0.006)	(0.298)	(0.311)	(0.295)
Sample Size	119,540	119,540	119,540	116,547	117,112	117,333
Non-Chronic Group						
Medicaid Expansion	0.030**	0.029**	0.021*	0.118	0.021	0.346
2014–2018	(0.009)	(0.011)	(0.011)	(0.215)	(0.336)	(0.220)
Sample Size	105,642	105,642	105,642	103,957	104,176	104,934
Chronic Group – High Pre-ACA Chronic Condition Uninsured Rate States						
Medicaid Expansion	0.005	0.021	0.006	-0.200	-0.140	-0.087
2014–2018	(0.030)	(0.014)	(0.009)	(0.381)	(0.477)	(0.292)
Sample Size	64,964	64,964	64,964	63,276	63,628	63,711
Non-Chronic Group – High Pre-ACA Chronic Condition Uninsured Rate States						
Medicaid Expansion	0.019*	0.041***	0.027***	-0.019	-0.273	0.322
2014–2018	(0.009)	(0.012)	(0.009)	(0.203)	(0.353)	(0.280)
Sample Size	55,351	55,351	55,351	54,400	54,573	55,004
Chronic Group – Low Pre-ACA Chronic Condition Uninsured Rate States						
Medicaid Expansion	0.002##	-0.008	0.011	0.072	0.432	0.020
2014–2018	(0.017)	(0.015)	(0.008)	(0.202)	(0.324)	(0.582)
Sample Size	54,576	54,576	54,576	53,271	53,484	53,622
Non-Chronic Group – Low Pre-ACA Chronic Condition Uninsured Rate States						
Medicaid Expansion	0.036***	0.029*	0.019*	-0.047	0.417	0.453
2014–2018	(0.011)	(0.015)	(0.010)	(0.278)	(0.683)	(0.353)
Sample Size	50,291	50,291	50,291	49,557	49,603	49,930

Note: All regressions include state×location type and year×location type fixed effects as well as the controls. Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. *** indicates statistically significant at 1% level; ** 5% level; * 10% level. BRFSS sampling weights are used. We denote statistically significantly different effects for the chronic vs. non-chronic group ### at the 1% level; ## at the 5% level; # at the 10% level. We also denote statistically significant different effects for the chronic (non-chronic) group in states with a high pre-ACA uninsured rate compared to the chronic (non-chronic) group in states with a low pre-ACA uninsured rate with ^^^ at the 1% level, ^^ at the 5% level, and ^ at the 10% level.

V. Discussion

This paper investigates the impact of the ACA Medicaid expansion on access to care and self-assessed health for individuals with and without a chronic health condition between 2014 and 2018. To our knowledge, this is one of the first papers to rigorously compare how low-income individuals with and without a chronic health condition have been impacted by the Medicaid expansion using five years of post-ACA data. Given the well documented untreated acute and non-acute care needs of those with chronic health conditions, it is important to understand how access to care changed due to the ACA Medicaid expansion and how those changes impacted the health of this vulnerable population.

Our baseline results suggest that the ACA Medicaid expansion led to improvements in access to care among those with and without chronic health conditions. While the magnitude of these improvements are mostly larger for those with a chronic health condition, the differences in magnitude are not statistically significant. For example, the ACA Medicaid expansion leads to a 10.1 percentage point increase in the likelihood of having any insurance coverage among the chronic health condition group as compared to a 7.7 percentage point increase among those without a chronic health condition. We also find statistically significant improvements in self-assessed health for those without chronic health conditions. In particular, the ACA Medicaid expansion led to a 3 percentage point increase in the likelihood of reporting good or better health, a 3 percentage point increase in the likelihood of reporting very good or excellent health, and a 2.1 percentage point increase in the likelihood of reporting excellent health among those without chronic health conditions.

Our sub-sample analysis suggests larger gains in access among those with a chronic health condition in states with a high pre-ACA chronic condition uninsured rate. This is important because the conventional wisdom was that even states with relatively less generous public health insurance programs still provided Medicaid coverage for those with the greatest health care need as part of the social safety net prior to the ACA. Our results suggest that pre-ACA Medicaid eligibility criteria in these states may have been excluding some of those with chronic health conditions and provided inadequate health care access through other means, such as charity care. Nevertheless, we find little evidence of improvements in self-reported health after the Medicaid expansion for those with chronic health conditions in states with the largest coverage gains, which may imply that individuals with chronic conditions require more time and care to recover health.

One possible explanation for why we see improvements in self-assessed health among those without chronic health conditions, despite no differential improvement in access for that group in our baseline analysis, may be that they have more capacity for health improvement than those with chronic conditions. It may also be that the marginal benefit of the initial units of medical care for individuals without chronic conditions are prompted to consume are higher than the marginal benefit of the additional units of medical care consumed by those with a chronic health condition due to diminishing returns. Simply put, individuals with non-chronic conditions may be more likely to suffer from previously untreated acute care conditions that are easily curable, while individuals with chronic condition require care that stabilize a condition without immediately improving health. Another explanation may be that our measures of access do not capture changes in the intensity of care received during a medical encounter due to the ACA Medicaid expansion. For example, those without a chronic health condition may be treated more intensively when they visit the doctor now that they have a consistent source of coverage that also reduces the marginal out-of-pocket cost for care. On the other hand, reductions in out-of-pocket cost of care only materialized for a small set of individuals with chronic conditions that didn't have coverage before the ACA.

Compared to previous work focusing on the overall impact of the ACA, our results suggest immediate and lasting improvements in access to care for those with and without a chronic health condition and an increase in the probability of reporting "very good or better" and "excellent" self-assessed health for those without a chronic health condition. These findings are in contrast to work that shows that improvements in access to care are only 50 percent attributable to the Medicaid expansion and that the improvements in self-assessed health are mostly attributed to the non-Medicaid portion of the ACA, e.g. the individual health insurance expansion (Courtemanche et al. 2018a; Courtemanche et al. 2020b). Similar to our work, other recent studies focusing on the Medicaid expansion have also documented immediate positive and lasting improvements in access to care and gradually emerging reduction in mortality among another medically vulnerable population, the near elderly (Miller et al. 2019, Goldin et al. 2019).

Conclusion

This dissertation explores the connection between staffing and patient volume at modern physician practices as well as the impact of recent reforms on safety-net hospitals in Massachusetts and the access to care and self-reported health outcomes of individuals with chronic conditions nationwide. Specifically, the first chapter explores how physician time, the number of non-physician clinical staff, and other non-labor inputs influence how many patients can be seen per week at an office. The second chapter examines how safety-net hospitals in Massachusetts navigated the changes in their revenue streams caused by the state's 2006 reform. Finally, the last chapter investigates the impact of the 2010 Affordable Care Act on the access to care and self-reported health outcomes of chronically ill individuals.

In 2006, Massachusetts passed comprehensive health care reform designed to confer universal insurance coverage by reforming the non-group insurance market, mandating that all individuals purchase insurance, and expanding Medicaid and providing subsidies to help low-income individuals comply with the mandate. The reform also diverted funds previously set aside for safety-net hospitals, with the idea being that once everyone has insurance these supplemental payments would no longer be necessary. Four years later, Congress passed the Affordable Care Act, which contains many of the same features as the Massachusetts reform, including a provision that will soon take effect to reduce safety-net supplemental payments. Both reforms were associated with large increases in the number of insured persons, first in Massachusetts and then across the nation.

Because the increase in the number of insured persons caused by the Affordable Care Act coincided with the onset of the baby boomer generation retiring, many health policy professionals have warned that the US health care system will be unable to meet growing patient demand. The first chapter explores one potential solution to this challenge: physician practices may be able to better optimize their clinical staff to raise patient volume. Using unique, propriety data on doctor offices from 2019, I find that, for many inputs, their marginal productivity has declined. Similarly, while the cross-input elasticities indicate most input pairs have remained either complements or substitutes, the elasticities themselves are also smaller in magnitude. The results suggest that, perhaps as a result of increased managed care and capitated payments, offices have already adjusted the size and skill mix of their clinical workforce and taken other efficiency-improving steps over the past few decades.

The second chapter examines the financial health of safety-net hospitals in Massachusetts and their trends in utilization following the state's 2006 reform. Because the Massachusetts reform is like the national Affordable Care Act, the experience of safety-net hospitals in that state may be indicative of what will happen to safety-net hospitals nationally. Using 17 years of cost report data and a difference-in-differences strategy, I find that the reform reduced patient revenue at the largest safety-net hospitals, and that those hospitals may have tried to compensate for their losses by delivering more services in outpatient settings. However, when I use more lenient definitions to identify safety-net hospitals, the results indicating a negative financial shock disappear. The lesson here may then be that when the federal reductions begin, states should target their remaining funds to those hospitals that serve the greatest number of low-income and uninsured patients, rather than applying the cuts evenly across all hospitals that have historically received supplemental payments. States that did not adopt the Medicaid expansion may also wish to revisit that decision, as that would reduce the number of uninsured patients relying on safety-net care.

The final chapter, co-authored with James Marton and Benjamin Ukert, investigates how the Affordable Care Act impacted chronically ill individuals who may have been ineligible for insurance prior to the reform. This population is also of particular interest because they may require a higher level of medical care compared to the average Medicaid enrollee that the health care system is accustomed to treating. Using five years of post-reform data from the Behavioral Risk Factor Surveillance System and a difference-in-differences strategy, we find large improvements in access to care and self-reported health outcomes for individuals with and without chronic conditions. Although the improvements are generally larger in magnitude for individuals with chronic conditions, the differences are not statistically significant.

There remains work to be done across all the topics covered in this dissertation. The estimates from the first chapter show the average productive relationships of labor and non-labor inputs at physician practices. Since these estimates can be compared to those from previous studies, the results also indicate how these relationships have evolved over time. Future work should focus on connecting the changes observed here to their underlying mechanisms. It also remains unclear what these productive relationships look like at other practice settings.

As for the second chapter, once the federal reductions in supplemental payments begin, researchers should closely monitor the financial health of safety-net hospitals nationally. If these hospitals, particularly the ones in non-expansion states, become insolvent or must scale back operations in ways that adversely affect the communities they serve, then policymakers and other stakeholders will need to know. Finally, regarding the third chapter, more research investigating the insurance coverage and health of vulnerable populations will be needed as the population grows, policies change, and the year-by-year effects of the Affordable Care Act diminish.

Appendix A

Table. Financial Performance Event Study Results for Hospitals Classified as Safety Net under the Restrictive Definition

		(1) Operating Margin	(2) GPSR ($ Million)	(3) Net Inpatient Service Rev ($ Million)	(4) Net Outpatient Service Rev ($ Million)	(5) Total Costs ($ Million)
Pre-Reform Period	$SNH \times 2001$	0	-2.456	-4.798	-16.21	-26.66
		(.)	(135.6)	(14.82)	(30.04)	(53.12)
	$SNH \times 2002$	0	10.54	15.96	-26.41	-3.852
		(.)	(120.4)	(20.36)	(29.21)	(39.80)
	$SNH \times 2003$	1.326	-2.006	20.72	-20.50	5.460
		(2.670)	(104.9)	(13.70)	(21.60)	(31.79)
	$SNH \times 2004$	-0.0903	-6.580	-7.546	-13.87	-1.568
		(1.822)	(79.57)	(11.58)	(15.89)	(28.16)
Transition	$SNH \times 2006$	0.00565	-19.96	4.467	-10.24	2.380
		(0.0163)	(63.21)	(10.33)	(17.45)	(21.55)
	$SNH \times 2007$	0.232	-20.97	3.416	-4.433	4.933
		(1.650)	(58.04)	(12.76)	(17.98)	(21.06)
	$SNH \times 2008$	0.855	-16.62	1.256	-6.457	2.465
		(1.650)	(58.26)	(15.76)	(22.84)	(21.38)
Post-Reform Period	$SNH \times 2009$	-0.505	-41.69	-16.40	-38.41**	-8.824
		(1.903)	(61.09)	(12.59)	(15.68)	(23.35)
	$SNH \times 2010$	1.490	-76.69	-19.86*	-20.77	-18.89
		(1.893)	(55.77)	(11.98)	(17.22)	(23.97)
	$SNH \times 2011$	1.073	-83.31	-27.96***	-25.34	-32.60*
		(1.715)	(54.52)	(9.996)	(16.79)	(19.22)
	$SNH \times 2012$	0.975	-102.8*	-46.01***	-25.17	-37.84*
		(2.102)	(54.83)	(10.10)	(18.00)	(20.34)
	$SNH \times 2013$	0.878	-112.8*	-49.50***	-22.69	-33.76*
		(1.640)	(61.55)	(10.43)	(17.46)	(19.91)
	$SNH \times 2014$	1.058	-101.6	-51.72***	-17.40	-32.02
		(2.154)	(73.30)	(10.06)	(17.78)	(22.00)

-- CONTINUED --

		Operating Margin	GPSR ($ Million)	Net Inpatient Service Rev ($ Million)	Net Outpatient Service Rev ($ Million)	Total Costs ($ Million)
Post-Reform Period	$SNH \times 2015$	-0.372	-161.8**	-56.30***	-46.88***	-16.60
		(1.783)	(69.77)	(12.05)	(15.21)	(26.85)
	$SNH \times 2016$	-1.516	-205.8**	-64.56***	-74.47***	-21.04
		(1.641)	(80.76)	(12.00)	(23.65)	(28.88)
	$SNH \times 2017$	-0.821	-208.6**	-63.06***	-65.37***	-24.74
		(2.882)	(97.59)	(12.51)	(20.85)	(35.15)
	$SNH \times 2018$	0	-156.9	-68.40***	-55.32**	-29.32
		(.)	(176.8)	(15.14)	(25.89)	(40.12)
	Observations	956	1164	1164	1164	1162

Note: Standard error in parentheses. All regressions include hospital and year fixed effects. Pre-reform period includes fiscal years 2001–2005 (2003–2005 for operating margin). Transition period includes fiscal years 2006–2008. Post-reform period includes fiscal years 2009–2018 (2009–2017 for operating margin). Fiscal year 2005 is omitted to serve as reference year. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix B

Table. Utilization Event Study Results for Hospitals Classified as Safety Net under the Restrictive Definition

		(1) Total Inpatient Discharges	(2) Total Outpatient Visits	(3) Inpatient Service Rev Per Discharge	(4) Outpatient Service Rev Per Visit	(5) ER Visits
Pre-Reform Period	$SNH \times 2001$	-442.4	64326.0	-78.68	29.41	-3116.1
		(788.4)	(90586.3)	(604.4)	(90.45)	(6683.4)
	$SNH \times 2002$	528.9	11201.6	347.0	-35.73	1218.7
		(765.3)	(58943.4)	(815.4)	(112.2)	(3617.7)
	$SNH \times 2003$	157.3	614.4	685.7	11.05	2228.7
		(618.3)	(56629.2)	(523.1)	(137.0)	(2809.5)
	$SNH \times 2004$	87.34	21668.8	-839.4	-18.81	-3833.5
		(570.8)	(53148.9)	(615.0)	(139.5)	(4175.5)
Transition	$SNH \times 2006$	292.9	3308.9	85.69	-78.48	2159.0
		(516.5)	(34122.7)	(443.4)	(76.72)	(3033.4)
	$SNH \times 2007$	346.2	17201.2	119.2	-56.18	3378.5
		(554.4)	(34334.2)	(551.1)	(82.20)	(3100.4)
	$SNH \times 2008$	-52.10	35220.8	131.7	-81.84	5400.9*
		(463.0)	(30272.8)	(617.1)	(85.55)	(3272.0)
Post-Reform Period	$SNH \times 2009$	-297.4	48803.2*	-927.5**	-131.0	7924.9**
		(483.7)	(28679.2)	(449.5)	(92.70)	(3585.7)
	$SNH \times 2010$	-990.8	45877.5	-826.6**	-131.1*	10333.9***
		(606.6)	(28656.0)	(381.7)	(71.43)	(3605.9)
	$SNH \times 2011$	-1231.5**	52666.2*	-529.2	-272.6**	9055.2**
		(503.7)	(29526.5)	(922.9)	(124.6)	(3683.7)
	$SNH \times 2012$	-1695.8***	68349.1**	-1514.8***	-163.5**	9967.1***
		(378.8)	(33174.3)	(438.8)	(67.08)	(3349.0)
	$SNH \times 2013$	-1449.7***	71377.0*	-2226.7***	-150.0**	9363.5***
		(421.1)	(37281.9)	(530.0)	(68.14)	(3079.6)
	$SNH \times 2014$	-1618.3***	88308.4*	-2309.1***	-165.4**	9992.5***
		(467.7)	(46833.5)	(439.4)	(70.30)	(3175.7)

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		Total Inpatient Discharges	Total Outpatient Visits	Inpatient Service Rev Per Discharge	Outpatient Service Rev Per Visit	ER Visits
Post-Reform Period	$SNH \times 2015$	-2218.2***	108301.3**	-2147.9***	-318.9***	11325.7***
		(492.2)	(43064.0)	(515.8)	(78.38)	(3421.3)
	$SNH \times 2016$	-2287.4***	115340.7**	-2721.9***	-420.4***	12345.1***
		(584.3)	(46303.9)	(552.4)	(88.44)	(3471.0)
	$SNH \times 2017$	-2207.5***	123947.1**	-3064.1***	-433.0***	9805.0***
		(618.1)	(52110.0)	(715.2)	(91.42)	(3617.6)
	$SNH \times 2018$	-2226.1***	65000.3	-2918.5***	-455.2***	-3347.5
		(697.9)	(50632.6)	(752.1)	(92.83)	(9419.4)
	Observations	1176	1176	1163	1144	1162

Note: Standard error in parentheses. All regressions include hospital and year fixed effects. Pre-reform period includes fiscal years 2001–2005. Transition period includes fiscal years 2006–2008. Post-reform period includes fiscal years 2009–2018. Fiscal year 2005 is omitted to serve as reference year. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix C

Table. Financial Performance Event Study Results for Hospitals Classified as Safety Net under the CMS Definition

		(1) Operating Margin	(2) GPSR ($ Million)	(3) Net Inpatient Service Rev ($ Million)	(4) Net Outpatient Service Rev ($ Million)	(5) Total Costs ($ Million)
Pre-Reform Period	$SNH \times 2001$	0	-186.5	-37.20	-60.99*	-63.39
		(.)	(213.4)	(28.64)	(33.34)	(49.83)
	$SNH \times 2002$	0	-127.0	-11.50	-59.03*	-32.52
		(.)	(199.2)	(27.65)	(30.70)	(43.29)
	$SNH \times 2003$	0.175	-86.78	-14.67	-44.98*	-23.09
		(1.649)	(181.2)	(27.99)	(26.63)	(41.31)
	$SNH \times 2004$	-1.397	-52.71	-30.83	-28.70	-14.21
		(1.224)	(160.9)	(23.84)	(22.38)	(35.35)
Transition	$SNH \times 2006$	-0.116	18.26	11.59	-8.093	20.04
		(1.123)	(136.1)	(16.76)	(19.17)	(28.41)
	$SNH \times 2007$	-1.067	42.98	15.32	-23.15	31.16
		(1.244)	(127.5)	(16.04)	(28.23)	(25.90)
	$SNH \times 2008$	-0.230	114.8	35.62**	7.081	50.65**
		(1.431)	(111.3)	(16.06)	(19.08)	(25.14)
Post-Reform Period	$SNH \times 2009$	-0.139	206.3*	40.12**	7.851	77.24***
		(1.263)	(108.4)	(16.89)	(18.64)	(28.43)
	$SNH \times 2010$	1.062	229.1**	15.07	26.47	70.15**
		(1.254)	(110.4)	(15.92)	(21.53)	(27.58)
	$SNH \times 2011$	0.241	227.0**	27.47	11.46	66.05**
		(1.258)	(114.0)	(17.66)	(17.30)	(28.86)
	$SNH \times 2012$	-2.041	229.2*	23.74	9.369	68.88**
		(1.496)	(122.6)	(18.54)	(18.07)	(31.65)
	$SNH \times 2013$	-0.902	252.2*	16.16	15.26	55.04**
		(1.265)	(135.3)	(18.98)	(17.72)	(27.04)
	$SNH \times 2014$	-0.0599	279.7**	19.02	31.74	62.88**
		(1.638)	(140.3)	(17.11)	(20.48)	(28.05)

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		Operating Margin	GPSR ($ Million)	Net Inpatient Service Rev ($ Million)	Net Outpatient Service Rev ($ Million)	Total Costs ($ Million)
Post-Reform Period	$SNH \times 2015$	-0.824	288.8*	13.16	12.04	65.45**
		(1.296)	(149.6)	(17.95)	(19.63)	(29.12)
	$SNH \times 2016$	-0.708	341.4*	25.12	9.274	81.02**
		(1.393)	(174.3)	(23.12)	(26.66)	(34.24)
	$SNH \times 2017$	-0.481	392.3**	19.22	21.22	91.63**
		(1.879)	(195.5)	(24.04)	(28.65)	(40.51)
	$SNH \times 2018$	0	442.7**	15.95	30.53	85.18**
		(.)	(217.3)	(27.06)	(30.51)	(42.80)
	Observations	956	1164	1164	1164	1162

Note: Standard error in parentheses. All regressions include hospital and year fixed effects. Pre-reform period includes fiscal years 2001–2005 (2003–2005 for operating margin). Transition period includes fiscal years 2006–2008. Post-reform period includes fiscal years 2009–2018 (2009–2017 for operating margin). Fiscal year 2005 is omitted to serve as reference year. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix D

Table. Utilization Event Study Results for Hospitals Classified as Safety Net under the CMS Definition

		(1) Total Inpatient Discharges	(2) Total Outpatient Visits	(3) Inpatient Service Rev Per Discharge	(4) Outpatient Service Rev Per Visit	(5) ER Visits
Pre-Reform Period	$SNH \times 2001$	-854.7	29235.1	-670.5	-81.43	92.51
		(756.9)	(56458.9)	(1159.7)	(122.8)	(4187.2)
	$SNH \times 2002$	-536.2	-4913.3	-17.55	-108.0	2654.2
		(726.5)	(43539.8)	(1064.9)	(120.3)	(2706.5)
	$SNH \times 2003$	-154.6	1409.9	-152.0	-31.59	3036.9
		(613.3)	(41799.9)	(1067.6)	(115.3)	(2421.3)
	$SNH \times 2004$	-190.2	1666.1	-1085.9	13.38	-2167.1
		(540.5)	(36480.0)	(898.5)	(106.9)	(3199.3)
Transition	$SNH \times 2006$	181.1	-1281.5	219.1	-40.37	1279.4
		(495.4)	(26584.8)	(660.4)	(87.28)	(2762.6)
	$SNH \times 2007$	327.1	9967.2	370.9	-81.24	3185.8
		(512.5)	(25312.4)	(701.2)	(81.81)	(2941.2)
	$SNH \times 2008$	163.4	18815.8	670.1	-7.297	5001.0*
		(483.3)	(24617.1)	(648.0)	(79.02)	(2953.4)
Post-Reform Period	$SNH \times 2009$	379.1	21629.7	135.5	42.71	4613.5
		(529.1)	(22588.4)	(594.7)	(95.50)	(2993.8)
	$SNH \times 2010$	95.01	24229.2	-460.1	18.36	9238.5***
		(523.6)	(23153.8)	(610.1)	(87.08)	(2728.5)
	$SNH \times 2011$	84.23	29595.3	-396.3	-120.3	7141.2**
		(659.5)	(24235.2)	(641.8)	(128.9)	(2963.4)
	$SNH \times 2012$	-289.2	37290.9	-195.5	-39.32	8062.5***
		(524.0)	(25578.7)	(611.7)	(77.32)	(3003.9)
	$SNH \times 2013$	-608.6	26374.6	-457.5	-3.262	6450.9**
		(469.3)	(26509.4)	(677.3)	(74.41)	(2775.6)
	$SNH \times 2014$	-1202.9**	35118.8	79.64	-6.840	7718.1***
		(608.4)	(31200.2)	(875.4)	(76.89)	(2782.3)

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		Total Inpatient Discharges	Total Outpatient Visits	Inpatient Service Rev Per Discharge	Outpatient Service Rev Per Visit	ER Visits
Post-Reform Period	$SNH \times 2015$	-1289.7**	15506.7	-96.58	-30.08	9823.7***
		(621.1)	(36972.5)	(796.1)	(107.7)	(2913.7)
	$SNH \times 2016$	-1454.1**	65965.0*	36.85	-241.6**	14792.6***
		(626.6)	(37439.2)	(970.7)	(115.3)	(3527.0)
	$SNH \times 2017$	-1466.1**	50758.5	-85.74	-160.9	13268.0***
		(665.3)	(35503.0)	(1171.8)	(112.5)	(3736.8)
	$SNH \times 2018$	-1638.2**	41493.0	172.4	-233.9*	5831.6
		(754.4)	(38475.0)	(1339.3)	(130.5)	(6711.6)
	Observations	1176	1176	1163	1144	1162

Note: Standard error in parentheses. All regressions include hospital and year fixed effects. Pre-reform period includes fiscal years 2001–2005. Transition period includes fiscal years 2006–2008. Post-reform period includes fiscal years 2009–2018. Fiscal year 2005 is omitted to serve as reference year. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix E

Table. Financial Performance Event Study Results for Hospitals which Self-Reported Receiving Supplemental Payments

		(1) Operating Margin	(2) GPSR ($ Million)	(3) Net Inpatient Service Rev ($ Million)	(4) Net Outpatient Service Rev ($ Million)	(5) Total Costs ($ Million)
Pre-Reform Period	$SNH \times 2001$	0	-81.36	-7.728	-15.41	-18.74
		(.)	(129.2)	(17.33)	(25.63)	(33.54)
	$SNH \times 2002$	0	-62.82	0.730	-17.25	-11.40
		(.)	(117.9)	(15.66)	(23.54)	(29.00)
	$SNH \times 2003$	-0.00652	-40.70	-6.525	-11.63	-9.162
		(1.513)	(106.3)	(15.14)	(21.35)	(26.98)
	$SNH \times 2004$	-0.894	-10.59	-7.077	-6.469	0.391
		(1.013)	(94.92)	(13.45)	(18.55)	(23.55)
Transition	$SNH \times 2006$	0.0408	13.52	5.761	-0.853	5.115
		(0.950)	(81.03)	(10.14)	(14.81)	(19.04)
	$SNH \times 2007$	0.0205	28.20	6.985	-3.543	9.831
		(1.026)	(74.96)	(9.570)	(15.71)	(16.77)
	$SNH \times 2008$	0.510	52.29	13.26	4.577	14.17
		(1.069)	(69.48)	(9.588)	(13.41)	(16.73)
Post-Reform Period	$SNH \times 2009$	0.0749	80.43	11.54	1.209	22.79
		(1.010)	(66.38)	(10.06)	(13.79)	(17.76)
	$SNH \times 2010$	-0.136	99.70	7.301	8.057	21.52
		(1.241)	(66.42)	(9.416)	(14.03)	(17.53)
	$SNH \times 2011$	-2.126*	97.17	11.28	-5.528	13.50
		(1.184)	(67.75)	(9.938)	(13.05)	(17.54)
	$SNH \times 2012$	0.00989	101.5	16.04	-2.599	18.85
		(1.267)	(71.98)	(10.72)	(13.78)	(19.08)
	$SNH \times 2013$	-1.891*	138.7*	16.74	-0.528	23.04
		(1.103)	(80.16)	(10.80)	(14.12)	(18.98)
	$SNH \times 2014$	-2.269	148.6*	11.06	8.778	20.88
		(1.696)	(84.85)	(10.64)	(15.30)	(19.88)

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		Operating Margin	GPSR ($ Million)	Net Inpatient Service Rev ($ Million)	Net Outpatient Service Rev ($ Million)	Total Costs ($ Million)
Post-Reform Period	$SNH \times 2015$	-0.699	186.9**	17.36	6.781	38.34*
		(1.075)	(95.18)	(11.91)	(16.39)	(21.52)
	$SNH \times 2016$	-0.146	207.5*	22.50	2.886	41.68
		(1.179)	(113.6)	(14.56)	(20.64)	(25.75)
	$SNH \times 2017$	0.644	245.0*	17.00	13.17	43.84
		(1.560)	(130.5)	(14.82)	(24.65)	(29.31)
	$SNH \times 2018$	0	290.5*	20.66	7.100	55.07
		(.)	(149.5)	(17.53)	(28.08)	(37.13)
	Observations	956	1164	1164	1164	1162

Note: Standard error in parentheses. All regressions include hospital and year fixed effects. Pre-reform period includes fiscal years 2001–2005 (2003–2005 for operating margin). Transition period includes fiscal years 2006–2008. Post-reform period includes fiscal years 2009–2018 (2009–2017 for operating margin). Fiscal year 2005 is omitted to serve as reference year. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix F

Table. Utilization Event Study Results for Hospitals which Self-Reported Receiving Supplemental Payments

		(1) Total Inpatient Discharges	(2) Total Outpatient Visits	(3) Inpatient Service Rev Per Discharge	(4) Outpatient Service Rev Per Visit	(5) ER Visits
Pre-Reform Period	$SNH \times 2001$	-1075.4	19824.9	221.1	-171.6	2353.7
		(718.3)	(27397.6)	(660.8)	(111.1)	(3319.6)
	$SNH \times 2002$	-315.6	17886.7	426.4	-174.4	2829.0
		(567.8)	(23968.6)	(618.8)	(106.6)	(2914.9)
	$SNH \times 2003$	-137.7	8279.5	-40.00	-122.9	1212.4
		(538.1)	(24783.4)	(616.2)	(95.37)	(2892.5)
	$SNH \times 2004$	350.6	29552.1	-703.7	-134.6	2588.8
		(423.7)	(20056.5)	(563.4)	(89.40)	(3548.7)
Transition	$SNH \times 2006$	238.0	9064.7	-66.94	-33.84	2937.2
		(393.7)	(17286.0)	(463.0)	(86.15)	(3377.0)
	$SNH \times 2007$	146.3	13996.6	167.2	-83.76	2995.4
		(395.2)	(15594.8)	(587.8)	(87.03)	(3373.3)
	$SNH \times 2008$	58.74	14166.0	179.1	-34.05	1672.1
		(357.7)	(14558.6)	(455.9)	(79.62)	(2981.1)
Post-Reform Period	$SNH \times 2009$	-9.571	15658.9	-303.9	-48.54	1216.5
		(365.8)	(13911.8)	(476.0)	(84.36)	(3136.3)
	$SNH \times 2010$	-206.4	18447.5	-681.6	-41.15	4816.5*
		(382.8)	(14075.5)	(468.7)	(77.96)	(2709.0)
	$SNH \times 2011$	-196.7	13405.6	-1116.9*	69.72	3868.8
		(402.0)	(14662.2)	(595.1)	(155.5)	(2750.1)
	$SNH \times 2012$	-167.5	24932.9*	-560.6	-68.63	4963.5*
		(352.1)	(15051.7)	(502.3)	(77.94)	(2761.6)
	$SNH \times 2013$	-61.15	20414.6	-887.7	-67.79	7841.1***
		(406.1)	(15896.6)	(540.6)	(74.45)	(2707.9)
	$SNH \times 2014$	-306.1	24504.4	-1393.9**	-38.04	9385.3***
		(435.2)	(17191.0)	(623.6)	(77.45)	(2808.6)

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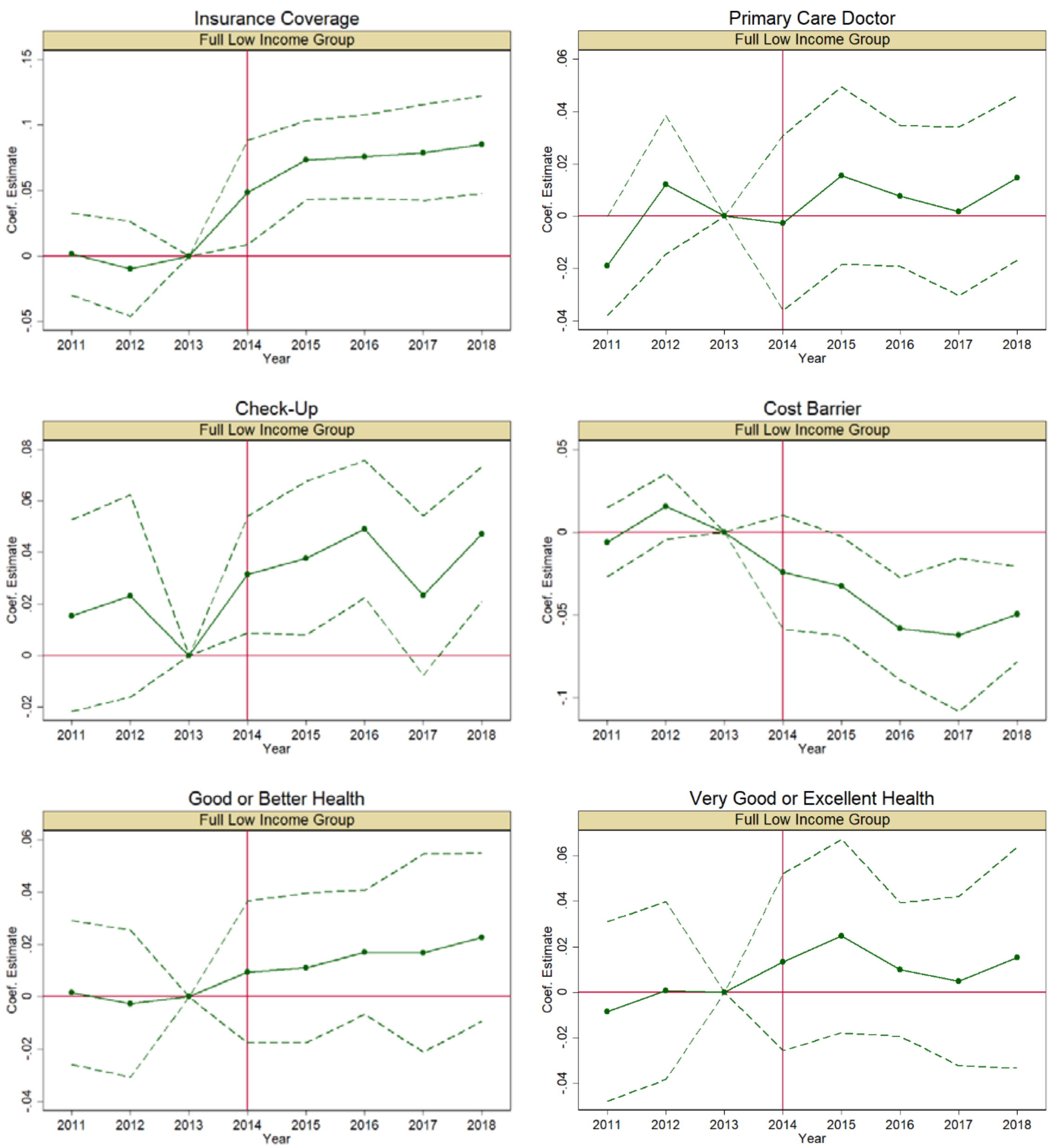
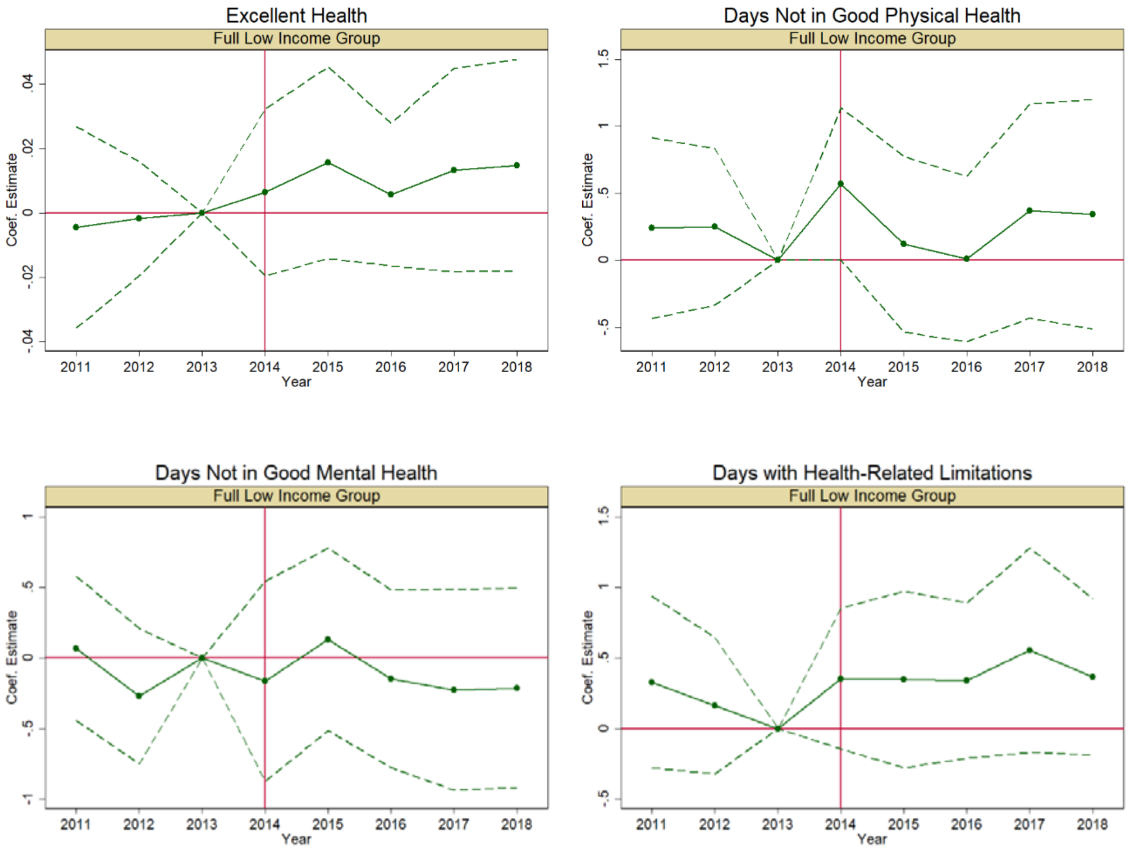
		Total Inpatient Discharges	Total Outpatient Visits	Inpatient Service Rev Per Discharge	Outpatient Service Rev Per Visit	ER Visits
Post-Reform Period	$SNH \times 2015$	-101.1	32777.0*	-1059.4*	-130.6	10365.5***
		(458.5)	(19192.3)	(569.9)	(98.83)	(3183.4)
	$SNH \times 2016$	-75.13	68310.7***	-1561.7**	-256.4**	12612.3***
		(493.9)	(20400.8)	(629.4)	(117.2)	(3450.3)
	$SNH \times 2017$	-107.6	63960.1***	-2205.2***	-260.6**	13591.8***
		(523.9)	(19371.6)	(791.7)	(120.7)	(3356.1)
	$SNH \times 2018$	-278.9	53751.3**	-1765.7**	-337.8***	12182.7***
		(585.5)	(22891.8)	(763.2)	(125.7)	(4259.8)
	Observations	1176	1176	1163	1144	1162

Note: Standard error in parentheses. All regressions include hospital and year fixed effects. Pre-reform period includes fiscal years 2001–2005. Transition period includes fiscal years 2006–2008. Post-reform period includes fiscal years 2009–2018. Fiscal year 2005 is omitted to serve as reference year. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix G

Figure. Full Low-Income Sample Event Study Results for all Outcomes




Excellent Health
Full Low Income Group
Coef. Estimate
-.04
-.02
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2011
2012
2013
2014
2015
2016
2017
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Year
Days Not in Good Physical Health
Full Low Income Group
Coef. Estimate
-.5
0
.5
1
1.5
2011
2012
2013
2014
2015
2016
2017
2018
Year
Days Not in Good Mental Health
Full Low Income Group
Coef. Estimate
-1
-.5
0
.5
1
2011
2012
2013
2014
2015
2016
2017
2018
Year
Days with Health-Related Limitations
Full Low Income Group
Coef. Estimate
-.5
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1
1.5
2011
2012
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2014
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2016
2017
2018
Year

Appendix H

Table. Summary Statistics for Control Variables by State Medicaid Expansion Status and Chronic Condition Status

		Medicaid Expansion		Non-Expansion	
	Full Sample	Chronic Group	Non-Chronic Group	Chronic Group	Non-Chronic Group
Age 25–29	0.138	0.101	0.165	0.098	0.168
	(0.345)	(0.301)	(0.371)	(0.297)	(0.373)
Age 30–34	0.160	0.123	0.185	0.118	0.197
	(0.367)	(0.328)	(0.388)	(0.323)	(0.398)
Age 35–39	0.119	0.095	0.134	0.099	0.136
	(0.323)	(0.293)	(0.340)	(0.299)	(0.343)
Age 40–44	0.117	0.113	0.118	0.122	0.115
	(0.321)	(0.316)	(0.323)	(0.327)	(0.319)
Age 45–49	0.089	0.117	0.070	0.111	0.070
	(0.285)	(0.321)	(0.255)	(0.314)	(0.256)
Age 50–54	0.093	0.140	0.055	0.148	0.058
	(0.291)	(0.347)	(0.229)	(0.355)	(0.234)
Age 55–59	0.067	0.110	0.034	0.121	0.030
	(0.250)	(0.313)	(0.181	(0.326)	(0.170)
Age 60–64	0.052	0.095	0.021	0.090	0.020
	(0.221)	(0.294)	(0.143)	(0.286)	(0.141)
Female	0.582	0.600	0.559	0.611	0.575
	(0.493)	(0.490)	(0.497)	(0.488)	0.494
Black	0.192	0.178	0.148	0.247	0.235
	(0.394)	(0.383)	(0.355)	(0.431)	(0.424)
Hispanic	0.363	0.280	0.468	0.214	0.403
	(0.481)	(0.449)	(0.499)	(0.410)	(0.491)
White	0.371	0.460	0.292	0.479	0.312
	(0.483)	(0.498)	(0.455)	(0.500)	(0.463)
Married	0.357	0.325	0.359	0.367	0.380
	(0.479)	(0.468)	(0.480)	(0.482)	(0.485)
High school degree	0.319	0.317	0.311	0.333	0.325
	(0.466)	(0.465)	(0.463)	(0.471)	(0.468)
Some College	0.240	0.246	0.232	0.244	0.245
	(0.427)	(0.431)	(0.422)	(0.429)	(0.430)
College graduate	0.064	0.059	0.073	0.051	0.063
	(0.244)	(0.235)	(0.261)	(0.220)	(0.244)
One child	0.177	0.187	0.178	0.176	0.168
	(0.382)	(0.390)	(0.382)	(0.380)	(0.374)
Two children	0.241	0.208	0.259	0.213	0.273
	(0.428)	(0.406)	(0.438)	(0.410)	(0.446)
Three children	0.154	0.114	0.177	0.122	0.189
	(0.361)	(0.318)	(0.382)	(0.327)	(0.392)

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		Medicaid Expansion		Non-Expansion	
	Full Sample	Chronic Group	Non-Chronic Group	Chronic Group	Non-Chronic Group
Four children	0.077	0.057	0.084	0.064	0.097
	(0.266)	(0.233)	(0.278)	(0.245)	(0.296)
Unemployed	0.214	0.217	0.211	0.220	0.211
	(0.410)	(0.412)	(0.408)	(0.414)	(0.408)
Unemployment rate	8.350	8.610	8.884	7.745	7.663
	(1.638)	(1.527)	(1.622)	(1.485)	(1.501)
Student	0.081	0.055	0.109	0.046	0.095
	(0.273)	(0.227)	(0.312)	(0.210)	(0.293)
Income 10k to less	0.206	0.204	0.209	0.222	0.194
than 15k	(0.405)	(0.403)	(0.406)	(0.416)	(0.395)
Income 15k to less	0.185	0.147	0.196	0.156	0.233
than 20k	(0.388)	(0.354)	(0.397)	(0.363)	(0.423)
Income 20k to less	0.120	0.089	0.131	0.096	0.158
than 25k	(0.326)	(0.285)	(0.338)	(0.294)	(0.365)
Income 25k to less	0.021	0.015	0.022	0.015	0.028
than 35k	(0.142)	(0.122)	(0.148)	(0.121)	(0.165)
Income 35k to less	0.000	0.000	0.000	0.000	0.000
than 50k	(0.006)	(0.008)	(0.008)	(0.000)	(0.000)
Income 50k to less	0.000	0.000	0.000	0.000	0.000
than 75k	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Income more than	0.000	0.000	0.000	0.000	0.000
75k	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)

Note: Standard deviations in parentheses.

Appendix I

Table. Event Study of the Impact of the ACA Medicaid Expansion—Chronic Group

	Insurance Coverage	Primary Care Doctor	Check-Up	Cost Barrier
Medicaid Expansion in 2011	-0.019 (0.017)	-0.066*** (0.014)	-0.015 (0.015)	0.027 (0.019)
Medicaid Expansion in 2012	-0.036* (0.018)	-0.013 (0.014)	-0.025 (0.016)	0.030** (0.015)
Medicaid Expansion in 2014	0.062** (0.028)	-0.009 (0.022)	0.028 (0.017)	-0.015 (0.019)
Medicaid Expansion in 2015	0.077*** 0.021)	0.002 (0.029)	-0.001$^{\wedge\wedge}$ (0.023)	-0.047* (0.027)
Medicaid Expansion in 2016	0.060** (0.023)	-0.041$^{*\wedge}$ (0.021)	0.005 (0.021)	-0.040** (0.016)
Medicaid Expansion in 2017	0.081*** (0.024)	0.003 (0.012)	0.003 (0.012)	-0.039* (0.022)
Medicaid Expansion in 2018	0.070*** (0.023)	0.017 (0.021)	-0.001 (0.024)	-0.006 (0.020)

	Good or Better Health	Very Good or Excellent Health	Excellent Health	Days Not in Good Physical Health	Days Not in Good Mental Health	Days with Health-Related Limitations
Medicaid Expansion in 2011	0.003 (0.030)	-0.013 (0.024)	-0.002 (0.007)	0.568 (0.497)	0.531 (0.513)	0.282 (0.568)
Medicaid Expansion in 2012	-0.019 (0.015)	-0.000 (0.024)	0.007 (0.008)	0.655 (0.559)	-0.146 (0.410)	0.488 (0.516)
Medicaid Expansion in 2014	-0.009 (0.019)	0.004 (0.016)	-0.005 (0.009)	1.174*** (0.475)	-0.034 (0.557)	0.605 (0.449)
Medicaid Expansion in 2015	-0.001 (0.022)	0.013 (0.020)	0.018$^{\wedge}$ (0.012)	0.442 (0.521)	0.135 (0.633)	0.247 (0.687)
Medicaid Expansion in 2016	0.001 (0.022)	0.020 (0.018)	0.014$^{\wedge}$ (0.009)	-0.364$^{\wedge\wedge\wedge}$ (0.542)	0.193 (0.469)	0.052 (0.453)
Medicaid Expansion in 2017	-0.003 (0.028)	-0.003 (0.023)	0.013 (0.012)	0.676 (0.482)	0.352 (0.584)	0.708 (0.673)
Medicaid Expansion in 2018	0.013 (0.025)	0.006 (0.024)	0.000 (0.009)	0.678 (0.524)	0.655 (0.601)	0.509 (0.492)

Note: BRFSS sampling weights are used. All regressions include state×location type and year×location type fixed effects as well as the controls. Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. *** indicates statistically significant at 1% level; ** 5% level; * 10% level. In addition, we denote statistically significantly different effects in a post expansion year relative to 2014 by $^{\wedge\wedge\wedge}$ at 1% level; $^{\wedge\wedge}$ at 5% level; $^{\wedge}$ at 10% level.

Appendix J

Table. Event Study of the Impact of the ACA Medicaid Expansion—Non-Chronic Group

	Insurance Coverage	Primary Care Doctor	Check-Up	Cost Barrier		
Medicaid Expansion in 2011	0.019 (0.020)	0.018 (0.015)	0.036 (0.024)	-0.0398*** (0.014)		
Medicaid Expansion in 2012	0.014 (0.037)	0.035 (0.022)	0.058* (0.029)	-0.006 (0.018)		
Medicaid Expansion in 2014	0.040 (0.028)	0.005 (0.020)	0.031** (0.015)	-0.038 (0.023)		
Medicaid Expansion in 2015	0.071*** (0.019)	0.029* (0.016)	0.066***^ (0.017)	-0.029 (0.020)		
Medicaid Expansion in 2016	0.087***^^ (0.024)	0.048***^ (0.018)	0.078***^ (0.022)	-0.077***^ (0.020)		
Medicaid Expansion in 2017	0.077*** (0.024)	0.001 (0.020)	0.038* (0.019)	-0.081***^ (0.025)		
Medicaid Expansion in 2018	0.098***^^ (0.024)	0.019 (0.020)	0.089***^^ (0.024)	-0.086***^^ (0.017)		
	Good or Better Health	Very Good or Excellent Health	Excellent Health	Days Not in Good Physical Health	Days Not in Good Mental Health	Days with Health-Related Limitations
Medicaid Expansion in 2011	0.004 (0.011)	0.001 (0.020)	-0.003 (0.023)	-0.046 (0.360)	-0.358 (0.290)	0.265 (0.250)
Medicaid Expansion in 2012	0.013 (0.017)	0.007 (0.020)	-0.007 (0.013)	-0.108 (0.163)	-0.420 (0.267)	-0.178 (0.171)
Medicaid Expansion in 2014	0.033* (0.018)	0.027 (0.025)	0.018 (0.021)	-0.002 (0.271)	-0.328 (0.435)	0.046 (0.228)
Medicaid Expansion in 2015	0.030* (0.016)	0.039* (0.022)	0.017 (0.021)	-0.278 (0.328)	0.001 (0.366)	0.265 (0.306)
Medicaid Expansion in 2016	0.041** (0.016)	0.011 (0.020)	0.003 (0.017)	0.098 (0.245)	-0.545 (0.374)	0.346 (0.379)
Medicaid Expansion in 2017	0.035 (0.023)	0.014 (0.021)	0.015 (0.024)	0.049 (0.424)	-0.633 (0.435)	0.338 (0.288)
Medicaid Expansion in 2018	0.032** (0.013)	0.030 (0.027)	0.029 (0.025)	-0.068 (0.477)	-0.967* (0.489)	0.151 (0.296)

Note: BRFSS sampling weights are used. All regressions include state×location type and year×location type fixed effects as well as the controls. Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. *** indicates statistically significant at 1% level; ** 5% level; * 10% level. In addition, we denote statistically significantly different effects in a post expansion year relative to 2014 by ^^^ at 1% level; ^^ at 5% level; ^ at 10% level.

Appendix K

Table. Specification Checks for Health Care Access

	Insurance Coverage	Primary Care Doctor	Check-Up	Cost Barrier
Drop Cell Phone				
Medicaid Expansion 2014–2018	0.084***	0.026*	0.034**	-0.044***
	(0.020)	(0.014)	(0.016)	(0.016)
Exclude 19–25 Year-Olds				
Medicaid Expansion 2014–2018	0.092***	0.007	0.026**	-0.050***
	(0.016)	(0.010)	(0.011)	(0.011)
Drop ACA Early Expanders				
Medicaid Expansion 2014–2018	0.092***	0.021**	0.029**	-0.052***
	(0.017)	(0.010)	(0.012)	(0.010)
Drop ACA Late Expanders				
Medicaid Expansion 2014–2018	0.083***	0.022**	0.025*	-0.058***
	(0.020)	(0.009)	(0.015)	(0.014)
Drop Sample Weights				
Medicaid Expansion 2014–2018	0.102***	0.023***	0.036***	-0.054***
	(0.016)	(0.007)	(0.009)	(0.009)
200% FPL Threshold				
Medicaid Expansion 2014–2018	0.052***	0.017**	0.022**	-0.030***
	(0.011)	(0.008)	(0.009)	(0.006)

Note: BRFSS sampling weights are used. All regressions include state×location type and year×location type fixed effects as well as the controls Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. *** indicates statistically significant at 1% level; ** 5% level; * 10% level.

Appendix L

Table. Specification Checks for Self-Assessed Health

	Good or Better Health	Very Good or Excellent Health	Excellent Health	Days Not in Good Physical Health	Days Not in Good Mental Health	Days with Health-Related Limitations
Drop Cell Phone						
Medicaid Expansion 2014–2018	0.036***	0.027*	0.014	0.025	-0.306	-0.397
	(0.012)	(0.014)	(0.009)	(0.258)	(0.283)	(0.349)
Exclude 19–25 Year-Olds						
Medicaid Expansion 2014–2018	0.025**	0.020	0.010	0.032	-0.063	0.052
	(0.011)	(0.013)	(0.008)	(0.235)	(0.0251)	(0.211)
Drop ACA Early Expanders						
Medicaid Expansion 2014–2018	0.016	0.019	0.015**	0.016	-0.043	0.111
	(0.010)	(0.012)	(0.007)	(0.240)	(0.251)	(0.188)
Drop ACA Late Expanders						
Medicaid Expansion 2014–2018	0.018	0.036***	0.023***	0.020	-0.181	0.251
	(0.015)	(0.010)	(0.008)	(0.291)	(0.335)	(0.232)
Drop Sample Weights						
Medicaid Expansion 2014–2018	0.015**	0.014**	0.007*	-0.197	-0.236	-0.090
	(0.007)	(0.005)	(0.004)	(0.178)	(0.157)	(0.157)
200% FPL Threshold						
Medicaid Expansion 2014–2018	-0.000	0.007	0.006	0.118	0.073	0.094
	(0.005)	(0.008)	(0.007)	(0.132)	(0.175)	(0.129)

Note: BRFSS sampling weights are used. All regressions include state×location type and year×location type fixed effects as well as the controls. Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. *** indicates statistically significant at 1% level; ** 5% level; * 10% level.

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Vita

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