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Author
Masucci, Alex

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Essays on Physician Billing and Location Decisions

A dissertation submitted in partial satisfaction of the
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by

Alex Masucci

Committee in charge:

Professor Jeffrey Clemens, Chair
Professor Kate Antonovics
Professor Julian Betts
Professor Todd Gilmer
Professor Gaurav Khanna

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The dissertation of Alex Masucci is approved, and it is acceptable in quality and form for publication on microfilm and electronically.

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2021

DEDICATION

To Sam, Mom, Andrew, Kristen, Kelly, Mimi, Papa, Mema, Deeda, Murphy, Dexter, and Sophie.

TABLE OF CONTENTS

LIST OF FIGURES

LIST OF TABLES

ACKNOWLEDGEMENTS

Chapter 1 contains material that is currently being prepared for submission for publication. Clemens, Jeffrey, Leganza, Jonathan M., and Masucci, Alex. “New Medicare Billing Codes: Take-Up and Usage Patterns.” The dissertation author was a primary investigator and an author of this material.

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VITA

Education

2015	Bachelor of Science, Economics, Texas A&M University
2021	Doctor of Philosophy, Economics, University of California San Diego

Employment

2021-	Economist, Amazon

ABSTRACT OF THE DISSERTATION

Essays on Physician Billing and Location Decisions

by

Alex Masucci

Doctor of Philosophy in Economics

University of California San Diego, 2021

Professor Jeffrey Clemens, Chair

Each chapter in this dissertation studies how various aspects of U.S. health policy affect physician behavior. The first two chapters study the introduction and effects of new billing codes for primary care services, and the third chapter studies a program that incentivizes physicians to practice in areas with few physicians.

Chapter 1 studies the take-up of new billing codes introduced by Medicare to compensate physicians for important and underprovided types of primary care: Transitional Care and Chronic Care. We identify significant variation in take-up by geography and by physician type. These patterns provide insight into the processes that determine new code take-up rates. We also find take-up patterns by group size and group type that fit with basic

economic intuition regarding the investments needed to adopt new billing codes.

Chapter 2 studies the extent to which take-up of billing codes for new primary care services complements with and substitutes for codes for other services. We use a panel regression and a matched county-level difference-in-differences design that compares high-intensity counties to low-intensity counties. We find evidence that Transitional Care substitutes for traditional post-discharge visits but complements with other forms of basic primary care as well as the provision of recommended care such as vaccinations and mammograms. We find less evidence that Chronic Care complements with other primary care services, and we find that Chronic Care is associated with a reduction in the provision of Imaging services.

Chapter 3 studies the Health Professional Shortage Area program, which increases the reimbursements paid by Medicare to physicians practicing in areas that are deemed to be experiencing a physician shortage. Using a matched difference-in-differences design, we find that the program has a positive effect on the stock of early-career physicians in designated counties. Furthermore, we find a response only for physicians who attended ranked medical schools. We discuss the policy implications of the fact that later-career physicians do not exhibit a response to the program despite receiving the bonus payments.

Chapter 1

New Medicare Billing Codes: Take-Up and Usage Patterns

Abstract

We document the take-up of two sets of billing codes introduced by Medicare to compensate physicians for important and underprovided types of primary care: Transitional Care Management and Chronic Care Management. We show that there is significant variation in take-up by geography and by physician type. We also identify take-up patterns by group size and group type. These results conform with basic economic intuition for which types of groups are most likely to gain value from billing the new codes, and they are potentially informative for which groups would be useful to target with informational interventions to increase the usage of novel primary care services. Transitional Care exhibits greater usage on the extensive margin, while Chronic Care exhibits greater usage on the intensive margin. This provides interesting context for thinking about the investments required to bill the new codes and the potential for the codes to interact with other services.

1.1 Introduction

Health care payment models shape the financial incentives physicians and hospitals face while delivering care. Payment models can thus have important implications for the health system's efficiency. Importantly, the patterns of service provision that constitute cost-effective health care are not static. That is, efficient health care will tend to evolve dynamically with a population's underlying health needs, with the development of new technologies, and with changes in the organization of medicine. Payment models may also need to adapt to these changes.

Maintaining an efficient health care payment model requires adapting to the health care landscape. To that end, the Centers for Medicare & Medicaid Services (CMS) regularly revises its physician fee schedule to incorporate new billing codes. In this paper, we show that the effects of such reforms can depend on a rich set of factors. In this chapter, we highlight the fact that for new codes to influence patterns of care provision, they must be recognized and adopted by physician practices. In Chapter 2, we show that the impact of new codes on spending and care provision can depend on the extent to which they substitute for or complement with existing services.

To provide evidence on the relevance of code adoption, we analyze the Medicare program's introduction of new codes linked to the management of care for patients with complex conditions. In particular, we analyze the 2013 introduction of new codes for billing Transitional Care Management services and the 2015 introduction of new codes for billing Chronic Care Management services. As discussed in more detail later, both sets of codes were intended to improve incentives for managing the care of patients with high health care needs. The new codes acknowledged the importance of coordinating care across a range of medical specialties for the administration of complicated care plans.

We begin by providing descriptive evidence on patterns in the adoption of the Transitional and Chronic Care Management codes by primary care physicians. Simple time series reveal that the adoption of new codes is a gradual process, suggesting substantial information frictions. We show that the use of both the Transitional and Chronic Care Management codes escalated substantially over the first four years following each code's introduction. We also show significant variation in take-up across space, some of which is correlated with variations in the prevalence of chronic conditions, but much of which is not.

We then explore several dimensions of heterogeneity in new code adoption across physicians. The most striking pattern is that new code take-up is far more rapid among mid-career than among early-career or late-career physicians. This is consistent with an important role for a physician's fluency with code processing, which requires a combination of early-career learning and ongoing investment. In an additional analysis, we show that take-up is strongest among physicians who operate in mid-sized groups that consist entirely of primary care physicians. Overall, the patterns we document are consistent with the idea that the adoption of new codes requires physicians to make investments in their practices' mastery of bill coding, which is a form of entrepreneurial capital.

This branch of our analysis makes multiple contributions to existing literatures. First, a long literature has analyzed the effects of financial incentives on the services physicians provide to their patients. One set of papers in this literature estimates standard impacts of reimbursement levels on the supply of services (see, for example, Alexander and Schnell 2019; Clemens and Gottlieb 2014; Gruber et al. 1999). Other papers have investigated margins including physicians' preferences over taking new patients (Chen 2014; Clemens et al. 2020; Garthwaite 2012), prescription patterns (Carey et al. 2020), and choices over where to establish their practices (Khoury et al. 2021). Relatively recent research on this rich variety of margins has provided evidence that health care becomes more widely accessible, and sometimes to a substantial degree, when physicians are paid more generously to provide it. Research has also demonstrated important roles for factors including intrinsic motivation (Kolstad 2013) and team environments (Chan 2016). We contribute to this literature by showing that the effects of incentives on the supply of services can depend importantly on physicians' awareness of those incentives and on the time horizons over which they adapt. Our analysis points to a novel dimension of physicians' human or entrepreneurial capital, namely their mastery of the billing systems that shape their practices' profitability, and we show that the take-up of the new codes we analyze unfolds quite gradually.

Additionally, while information frictions have received little attention in prior research on physicians' labor supply, they have received substantial attention in other lines of research. Information frictions play an important role, for example, in research on the causes of incomplete take-up of benefits among individuals who are eligible for Medicaid and other forms of public assistance (Aizer 2007; Bhargava and Manoli 2015; Manoli and Turner 2014). Research has also demonstrated an important role for information in shaping responses to the tax code (Chetty and Saez 2013; Chetty et al. 2013).[1] We highlight that information frictions may be important for understanding differences between physicians' short- and long-run responses to non-trivial changes in incentives they face. The complexity of physicians' contracts and reimbursement procedures has been examined elsewhere (Clemens and Gottlieb 2017; Clemens et al. 2017; Gottlieb et al. 2018). Our analysis highlights that the physician workforce's awareness of reforms to the payment models in these contracts can be essential for such reforms to have their intended effects.

The remainder of this paper proceeds as follows. In Section 1.2 we present background information on the introduction of new billing codes for Chronic and Transitional Care Management. In Section 1.3 we present an economic framework to outline the physician-level decision to utilize the new billing codes. In Section 1.4 we describe the data used in our analysis. Section 1.5 presents our analysis of the take-up of these new billing codes, and Section 1.6 concludes.

[1] Adjustment frictions may explain important differences between short- and long-run labor supply elasticities as well as between micro and macro labor supply elasticities (Chetty 2012).

1.2 Background

1.2.1 Primary Care and the Fee for Service Payment System

Primary care physicians play an important role in health care systems. They often serve as initial points of contact for undiagnosed patients and provide continued treatment to patients with health conditions that need to be regularly managed and evaluated. The evidence suggests that strong primary care systems are linked to better population health outcomes across OECD countries (Macinko et al. 2003) and that reorienting health systems towards primary care in general is likely to be beneficial for health outcomes and health care costs (Friedberg et al. 2010). Along these lines, the Centers for Medicare & Medicaid Services has recently "recognized primary care and care coordination as critical components in achieving better care for individuals, better health for individuals, and reduced expenditure growth" (CMS 2012).

Despite playing such an integral role, an emerging body of evidence highlights how primary care physicians often provide services that are left out of the Physician Fee Schedule (PFS). In an important sense, they are thus not paid in full for the services they deliver to patients (Gottschalk and Flocke 2005; Farber et al. 2007; Dyrbye et al. 2012; Tai-Seale et al. 2017). The new codes that we study were intended to address exactly this problem. In the final rule for the Medicare Physician Fee Schedule for 2018, CMS states that the PFS has traditionally not appropriately captured and accounted for services physicians provide in the context of general care coordination and management. The report states: "In the years since 2012, we have acknowledged the shift in medical practice away from an episodic treatment-based approach to one that involves comprehensive patient-centered care management, and have taken steps through rulemaking to better reflect that approach in payment under the PFS. In CY 2013, we established new codes to pay separately for transitional care management (TCM) services. Next, we finalized new coding and separate payment beginning in CY 2015 for chronic care management (CCM) services..." (CMS 2018).

By enacting these new billing codes, CMS has adjusted the PFS by explicitly paying physicians for TCM and CCM services. CMS has done so in order to either compensate doctors more fully for the complex primary care services they were already providing or, where primary care needs were going unmet, to increase incentives for physicians to provide such services. The new billing codes are the result of policy makers aiming to make the provision of primary care more financially attractive (Burton et al. 2017). They capture the essence of a broader CMS agenda to "improve the payment for, and encourage long-term investment in, primary care and care management services" (CMS 2012).

1.2.2 Transitional Care Management

The Transitional Care Management (TCM) codes are designed to pay physicians for the care management services they provide to patients following a discharge out of an inpatient setting, such as a hospital or skilled nursing facility. The goal of these care management services is to reduce preventable readmissions and improve patient health by better coordinating the provision of follow-up care.

CMS introduced two new billing codes for physicians who provide TCM. Billing code 99495 is for Transitional Care services of moderate medical decision complexity. It requires initial communication with the patient (or caregiver) within two days of the patient discharge date as well as a face-to-face visit within 14 days of the discharge. Billing code 99496 is for Transitional Care services of high medical decision complexity. It requires initial communication within two days of the discharge as well as a face-to-face visit within 7 days of the discharge.

These new codes were first eligible to be billed in 2013. The reimbursement rates were set by CMS, taking into consideration the input and feedback from committees and stakeholders such as the American Medical Association RVS Update Committee, and using similar existing codes to guide the rate-making process. In 2013, TCM associated with code

99495 paid roughly $164, which compares favorably to a similar office visit ($107), and TCM associated with code 99496 paid roughly $231, which again is higher than a comparable office visit ($143).[2]

1.2.3 Chronic Care Management

The Chronic Care Management (CCM) codes are designed to pay physicians for care coordination and care management for patients with multiple chronic conditions, such as Alzheimer's disease, dementia, asthma, cancer, cardiovascular disease, chronic obstructive pulmonary disease, depression, diabetes, or hypertension, among others. Chronic conditions are common among Medicare beneficiaries, and spending on patients with these afflictions is substantial. Approximately 85% of U.S. national health care spending is associated with people with chronic conditions (Anderson 2010). Moreover, a recent report analyzing the Medical Expenditure Panel Survey found that 42% of adult Americans had multiple chronic conditions and that the prevalence of multiple chronic conditions was even higher (81%) for Americans 65 years and older (Buttorff et al. 2017).

Recognizing the pressing need for the health care system to provide appropriate care for Medicare patients afflicted with chronic conditions, CMS created the new CCM billing codes. CCM code 99490 pays for care management of at least 20 minutes of clinical staff time per month. Eligible patients are those who have multiple chronic conditions that are expected to last at least twelve months or until death and that create a significant risk of death or functional decline. This code was first eligible to be billed in 2015. As with the TCM codes, payment rates were determined by CMS with input from stakeholders. In 2015, reimbursement was roughly $43.

At first, the process of billing CCM was met with a few burdens and complexities. An initiating office visit was originally required for all patients before commencing CCM, and

[2] We report dollar amounts for reimbursement purposes that correspond to national payments in a non-facility setting, which can be found here: https://www.cms.gov/medicare/physician-fee-schedule/search/overview.

advanced patient consent had to be obtained. In a recent analysis of health care provider interviews, O'Malley et al. 2017 document that some providers reported administrative barriers to billing — such as the need to maintain certified electronic health records and to have the ability to share records with other providers outside their practice — while others reported that the modest reimbursement rate was not sufficient to cover upfront investments in staffing and infrastructure required to provide CCM. With the goal of further increasing the provision of care for patients with chronic conditions, CMS responded to provider concerns. In 2017, CMS relaxed various administrative requirements for billing CCM — such as simplifying patient consent procedures, only requiring initiating office visits for new patients or patients not seen within the previous year, and reducing documentation rules. In the same year, CMS also introduced two additional CCM codes with higher reimbursement rates: code 99487 ($94), for CCM that involves moderate or high complexity medical decision making, and code 99489 ($47), for each 30 minutes of additional CCM time (no matter the complexity).

1.2.4 New Billing Codes in Practice

The implementation of new billing codes that pay physicians for TCM and CCM create financial incentives to provide these services to beneficiaries. However, the extent to which physicians ultimately respond to financial incentives depends on several factors. First, physicians must be aware of the new codes and the rules governing their use. Second, they must weigh the costs and benefits of adjusting their billing and care provision patterns in response to the incentives the codes create. As discussed above, this can require navigating the general administrative complexities associated with billing procedures in the U.S. health care system (Gottlieb et al. 2018).

The effectiveness of new billing codes can be limited by the administrative burdens associated with their use. Adapting to new codes may or may not be worthwhile, if the new codes represent a relatively modest refinement to an otherwise large and complex fee for service payment model. It is thus important to understand the pace of new code adoption, as well as variations in take-up across physicians, physician groups, and geographic regions. As take-up occurs, it is then important to evaluate empirically how the use of new billing codes impacts broader billing patterns and the overall provision of care.

1.3 Economic Framework

Some categories of physicians are better poised than others to make the types of adjustments necessary to bill the Chronic and Transitional Care Management codes. In this section, we will outline some of the basic economic parameters that are pertinent to the decision to incur the up-front costs of building the capacity to bill these codes. This will provide useful context for thinking about the empirical patterns seen in our descriptive analysis in Section 1.5. Our economic framework for thinking about these decisions will highlight some of the differences in take-up of the codes that we might expect to see across physician characteristics, group characteristics, and geography.

As discussed in Section 1.2, taking up the new billing codes can necessitate incurring investment costs due to the time and effort needed to learn about the code requirements and related administrative hurdles. Investment costs could also be directly monetary if creating the capacity to bill the new codes involves paying a billing specialist for their time. One simplified framework for thinking about the decision for a physician to invest in new code infrastructure is to weigh the discounted sum of earnings from the new code against the up-front investment costs. That is, the new code take-up decision is determined by the inequality:

$$\sum_{t=0}^{T_i} \frac{R_{it}}{(1+\delta)^t} - c_i > 0, \tag{1.1}$$

where R_{it} is revenue from the new code earned by physician i in year t, δ is an annual discount rate, T_i is the year of retirement for physician i, and c_i is the individual-specific investment cost required to start billing the new code. Evaluating the take-up decision in this way implies that physician i will take up the new code if the left-hand side of the inequality is positive. This decision framework makes several simplifying assumptions. It assumes that investment decisions are made at the physician level rather than at the group level, assumes that all costs related to billing the new code are paid up front, and omits the consideration of potential opportunity costs of billing the code. Nonetheless, thinking about how these parameters might differ across different types of physicians provides a useful setup for interpreting our findings in Section 1.5.

One idea that is clearly captured by this framework is that late-career physicians, for whom T_i is low, may be less likely to adopt the new billing codes since they have fewer working years left to receive revenue from billing the codes. Thus, we might expect to see physicians in the latter stages of their careers take up the codes at a lower rate. An additional idea related to career stage that is not captured by our simplified framework is the idea that physicians in the earliest career stages may be facing a set of alternative investments in their human capital and billing potential that have particularly high returns as they establish themselves in the profession. These options could "crowd out" investment in the new codes, leading to lower new code take-up among physicians near the beginnings of their careers.

Our framework also captures the simple idea that physicians who have more to gain in potential revenue from the new codes — that is, those for whom R_{it} tends to be high — are more likely to adopt the codes. For instance, physicians practicing in areas with high chronic condition incidence rates among the population of beneficiaries are more likely to face high levels of potential revenue from billing the Chronic and Transitional Care Management codes. We might also expect physicians with a higher share of their business focused on primary care to exhibit higher rates of billing the new codes. Namely, those in groups consisting solely of primary care physicians may be particularly likely to experience revenue gains from these codes. This has an even more striking effect on the investment decision if we shift to thinking about the investment as a group-level decision. At the group level, the new code revenues for all physicians in the group can be weighed against the investment cost, which may not scale up for the marginal within-group physician billing the new code in the same way that revenues do.

We might expect the new code investment decision to vary by group size. The larger the investment cost is, the more scale will be required to reduce average costs to the point where billing the new code is profitable. This dynamic too is most salient if these investment decisions are made at the group level. For this reason, we might expect new code take-up to be higher among groups with the requisite scale for defraying the investment costs. This elicits the idea that new code investment will be most costly for sole practitioners in particular, even relative to small or mid-size groups. Of course, the extent to which a practice's business is focused on primary care may be lower for larger groups, which could work against the effect of scale on new code take-up. It is also possible that group-level coordination or other dynamics result in an equilibrium where only a few physicians perform all of a group's new code billing. This could prevent other physicians at the group from utilizing the code and lead to a lower overall take-up rate for physicians at larger groups.

It could also be the case that larger groups already largely have the infrastructure in place needed to bill the new codes. If physicians at larger groups already meet requirements like maintaining Electronic Health Records and have billing specialists who are at the frontier of updates to Medicare's payment system, these physicians will face a lower individual-specific investment cost c_i than other physicians. This would tend to increase these physicians' propensity to start billing the codes. The same is true for physicians and groups who have already made or are currently making investments related to other non-traditional codes. Past investments in coding infrastructure for other recently introduced codes, billing for which we can identify in our data, may reduce c_i and make new code take-up more likely. Similarly, if the investments required to adopt both Chronic and Transitional

Care Management overlap, the combined discounted revenues from both of these codes could be more likely to outweigh their joint investment costs than is likely to be the case for an individual code.

Other economic concepts can be incorporated into our framework to add more nuance that is useful for explaining the new code take-up decision. For instance, physicians are often modeled as receiving utility from the health of their patients as well as from revenue. It is possible that the health benefits to patients of the Chronic and Transitional Care Management codes make investments in these codes more attractive than they would appear from an accounting of only the monetary benefits of billing the codes. Both health effects and financial incentives could play a role in increasing code take-up among physicians in areas where chronic conditions are particularly prevalent.

We can also introduce a role for information frictions to reduce the take-up of new codes. To decide to bill a new code, a physician must learn about it. If information about the code is not readily available to all physicians, this could lead to substantial geographic variation in take-up. This may also cause take-up to occur gradually over time rather than immediately reaching the end-line billing rate for the new code. The potential for information frictions to add another hurdle to billing new codes raises the possibility that physicians will be more likely to learn about and bill the codes if they practice in a group rather than as a sole practitioner. This effect may be most notable for primary-care-only groups and larger groups, where the likelihood of encountering someone with knowledge of the new coding practices is higher.

Finally, we could explicitly introduce a role for contemporaneous administrative costs and the opportunity cost stemming from services that physicians could be billing instead of the new codes. Documentation and other billing requirements may serve as variable costs of billing the new codes that persist beyond the initial investment cost. Furthermore, rather than billing the new code on top of all business that would have been done otherwise, the new code may crowd out some other billing due to the time constraints of the physician work day. If this is the case, the reduced revenue from these other codes should be evaluated as an opportunity cost of billing the new codes. Both explicit contemporaneous costs and opportunity costs of the new codes should be subtracted from R_{it} in the discounted sum, making the investment in new code adoption less attractive holding all else equal.

We can identify many characteristics of physicians, groups, and practice locations in our data. In the remainder of the paper, we will explore patterns in the take-up of the Chronic and Transitional Care Management codes over time and across these characteristics. We identify significant variation in new code take-up that is particularly interesting given the underlying economics that drives investment-related decisions.

1.4 Data

To study how physicians respond to the introduction of the new Medicare billing codes, we make use of several data sources from CMS. Using three physician-level datasets, we build a panel of physicians from 2012 to 2018 that contain information on physician characteristics and physician billing. We also make use of one additional county-level dataset that contains information on population health.

1.4.1 Constructing the Physician Panel

Our base dataset is the *Medicare Provider Utilization and Payment Data: Physician and Other Supplier* (MPUP). The MPUP is a provider-level panel dataset that covers health care professionals who bill services to Medicare Part B. It spans the years 2012 to 2018. The data are derived from administrative claims data from CMS and allow us to observe almost all physicians who bill Medicare. (Physicians who do not bill any HCPCS code at least 10 times in a given year are omitted from the data for that year.) The MPUP contains unique physician identifiers called National Provider Identifiers (NPIs), information on physician specialties, and information on billing, including importantly billing of the new codes. We focus most of our analysis on primary care physicians (PCPs), which we define to be any physicians with a specialty of Internal Medicine, Family Medicine, General Practice, or Geriatric Medicine.

We supplement these data with two other datasets from CMS, which we link to the MPUP using the unique NPIs. From the *National Plan and Provider Enumeration System* (NPPES), a dataset published by CMS that identifies and enumerates all physicians, we obtain information on physician practice location.[3] This information allows us to study physician groups, which we define as physicians practicing at the same address. Then, from the *Physician Compare* dataset, a dataset CMS publishes to provide patients with information on doctors who accept patients covered by Medicare, we pull information on physician medical school attendance and graduation dates.[4] We use these data to categorize physicians based on medical school ranking (using rankings of medical schools for primary care from the 2018 U.S. News & World Report) and career stage. We define early-career PCPs as those who graduated from medical school 5 to 19 years prior, we define mid-career PCPs as those who graduated from medical school 20 to 39 years prior, and we define late-career PCPs as those who graduated from medical school 40 or more years prior. Our definition of early-career PCPs is driven by the data: very few physicians are assigned an NPI until 5 years after finishing medical school, likely due to time spent in residencies immediately after school. We use our definition to maintain consistency in studying early-career PCPs who have likely completed their residencies. Our chosen threshold for distinguishing between mid- and late-career PCPs is driven by (approximate) age: physicians who attended medical school 39 years prior to the year of observation are likely around 65 years-old, which we use as a natural benchmark to define physicians who are late in their career.

[3]Specifically, the NPPES data record a primary practice location for each physician, for each month. We use practice location as of December for each calendar year.

[4]CMS began publishing the Physician Compare data in 2014. We use all available data from 2014 through 2018 to define medical school and graduation date. The information is time-invariant, and most physicians appear in all waves of the data; however, we are missing information on medical school and graduation date for physicians who appear in our data only before 2014.

After adding the information from the NPPES data and the Physician Compare data to the MPUP, we have a detailed panel dataset of physicians over time.

1.4.2 County-Level Data

We use an additional dataset to facilitate our analysis. The *CMS Chronic Conditions Files* are datasets published by CMS that report county- and HRR-level statistics on the prevalence of, and Medicare spending for, twenty-one different chronic conditions. We use these data to construct a normalized index that reflects the overall prevalence of chronic conditions, which we use as a proxy for patient health. Our index is based on the prevalence of eight major chronic conditions (arthritis, kidney disease, COPD, diabetes, heart failure, hyperlipidemia, hypertension, and ischemic heart disease).[5]

1.4.3 Analysis Sample and Summary Statistics

Our analysis sample is the near-universe of Medicare-billing physicians between 2012 and 2018. We present baseline summary statistics in Table 1.1, which provides a descriptive overview of our data before the implementation of any of the new Medicare billing codes. Of the 175,408 PCPs in our data in 2012, 19.6% are sole practitioners, 15.3% are early-career, and 32.3% attended a ranked medical school. Evaluating and managing patient health makes up a large fraction of PCP billing: average billing for standard office visits amounts to roughly 44% of average total billing for PCPs.

1.5 The Take-Up of New Medicare Billing Codes

In this section we document the take-up of the new Medicare billing codes. First, we show how the adoption of the codes evolved over time. Second, we investigate geographic

[5]More details on the construction of this index and our usage of the address data are available in Section 1.A.

variation in new code usage, exploring how the take-up of the new codes is diffused across space. Third, we analyze how new code usage varies by physician characteristics.

1.5.1 New Code Billing Over Time

Figure 1.1 plots the time series on national billing of both the TCM and CCM codes, and it highlights that the adoption of new codes is a gradual process. Panel A plots total billing, in dollars, for both TCM and CCM, where total billing for TCM (CCM) is defined as the sum of the billing for all relevant new codes classified as TCM (CCM). The graphs show how billing for the new codes ramps up steadily over time, and neither type of new code billing seems to have leveled off over the time horizon of our data.

Panel B plots the fraction of PCPs billing TCM and CCM. A similar pattern emerges: the fraction of PCPs billing the codes increases over time at a relatively stable rate. By 2018, which is six years after the introduction of the TCM codes and four years after the introduction of the CCM codes, 12.3% of PCPs bill TCM and 4.5% of PCPs bill CCM. (For another relevant comparison, note that 8.8% of PCPs bill TCM in 2016, which is four years after the introduction of the codes.)

Panel C plots new code billing as a share of total billing, conditional on billing the new code. For PCPs who bill TCM, the new code billing increases to about 4% of total billing in 2018; for PCPs who bill CCM, the new code billing increases to about 8% of total billing in 2018. While more PCPs bill TCM overall, CCM billing ultimately makes up a greater share of total billing for those PCPs who do bill CCM. This pattern is consistent with the idea of CCM-intensive billing practices emerging as a result of physicians undertaking significant investments in staffing and infrastructure in order to provide care that complies with CCM requirements (O'Malley et al. 2017).

1.5.2 Regional Variation in New Code Billing

There is significant regional variation in the take-up of new codes. We document this in Figure 1.2, which graphs Hospital Referral Region (HRR) new code billing per PCP in 2018 for each of TCM (panel A) and CCM (panel B). TCM billing is much lower in the western parts of the U.S. and is more heavily concentrated in both the Northeast and the Southeast. CCM billing is relatively sparse throughout northern regions and appears to be concentrated all along the southern parts of the U.S., both in the South but also throughout the Southwest. Underlying health conditions across regions explain some of this variation. New code billing is indeed correlated with our constructed chronic condition index, which we illustrate in Figure 1.3; the regression model in panel A explains 19.5% of the variation in HRR-level TCM billing, and the regression model in panel B explains 19.2% of the variation in HRR-level CCM billing.

1.5.3 New Code Billing by Physician Characteristics

Table 1.2 displays new code billing rates by physician characteristics. The table reports the fraction of physicians billing TCM (column (1)) and CCM (column (2)) during 2018, the last year of our data, and each row corresponds to a different physician characteristic. Panel A documents billing rates across specialties and shows that PCPs are much more likely to bill the new codes than non-PCPs. The first row reproduces findings from the time series graphs, whereas the second row shows that billing rates for TCM and CCM are less than 1% for all non-PCPs.

Panel B breaks down billing rates by physician career stage. The likelihood of billing the new codes is higher (for both TCM and CCM) for mid-career PCPs compared to early-career or late-career PCPs. In panel A of Figure 1.4, we provide a more granular look at how billing rates vary over career stage. PCPs with more experience are more likely to adopt the new codes, until reaching the later stages of their career. The declining rate of new code adoption over the latest of career stages is consistent with the idea that physicians approaching retirement will have less time over which they can capture the returns on investments associated with learning how to bill the new codes or how to carry out procedures that will qualify as TCM or CCM.

Panel C of Table 1.2 reports billing rates by physician medical school. The results suggest similar billing rates for PCPs from ranked and unranked schools, although PCPs who attended the highest ranked medical schools appear slightly less likely to bill the new codes than other PCPs. The more granular evaluation of medical school rankings displayed in panel B of Figure 1.4 points to generally similar conclusions, especially for TCM billing. The fact that PCPs who attended the highest ranked medical schools differ in their propensity to adopt the new codes connects to and complements the few existing studies that investigate how physician practice styles differ based on initial medical school quality (Doyle et al. 2010, Schnell and Currie 2018).

Panel D and panel E of Table 1.2 break down billing rates by physician group characteristics. In general, we see that PCPs belonging to groups are more likely to bill the new codes than sole practitioners, except for PCPs in the largest groups, which is consistent with the idea that larger groups face more bureaucratic barriers to providing CCM (O'Malley et al. 2017). Moreover, we see that PCPs in PCP-only groups are particularly likely to bill the new codes, which may reflect stronger incentives to make investments in new code billing for groups composed entirely of physicians for whom the new codes are designed.

Note that large groups themselves are more likely than groups of other sizes to have at least one PCP billing the new code, which we show in Table 1.3, as large groups are made up of more doctors. This suggests that it is not the case that information about the new codes fails to reach large groups at all, but rather that the PCPs who make up the large group tend to be less likely to bill the new codes.

Finally, panel F shows how new code billing rates differ by other billing behaviors. New code billing rates are higher for PCPs who also bill standard office visits. We also see that billing rates for one type of new code are substantially higher than average for PCPs who bill the other new code: 40.9% of PCPs who bill CCM also bill TCM, and 14.8% of PCPs who bill TCM also bill CCM. In addition, we see that billing rates for the new codes are higher for PCPs who also bill Annual Wellness Visits (AWVs), which were first introduced as a part of the Affordable Care Act (ACA) and which aim to provide patients with a standard wellness check and a plan for upcoming preventive care. We explore the relationship between the new codes and AWVs further in Figure 1.5. Panel A illustrates the correlation between AWV billing in 2012 and new code billing in 2018 and points to a relatively weak initial relationship. In contrast, panel B illustrates a shift in the relationship: new code billing in 2018 is strongly correlated with AWV billing in 2018. This is suggestive of a phenomenon we consider more fully in Chapter 2, namely that the adoption of new codes is complementary with the billing and/or provision of additional, related codes. This complementarity may tend to be strongest when the documentation required to bill one code overlaps significantly with the documentation required to bill another code.

Overall, the descriptive facts and patterns presented in this section provide some initial insight into how physicians respond to the implementation of the new codes. Take-up of the new codes occurs gradually over time and across space. In exploring heterogeneity in take-up rates, we find patterns consistent with the idea that the adoption of the new codes requires ongoing learning and investments related to bill coding proficiency. Correlations between new code billing rates and other billing behaviors lead us to the line of analyses conducted in Chapter 2, where we investigate the impact of new code adoption on physician billing and provision of care.

1.6 Conclusion

We provide evidence on take-up patterns for the Chronic Care and Transitional Care Management codes introduced by CMS. For physicians' care provision to respond to payment reform, it is essential that physicians recognize the nature of the payment reform and the incentives it creates, then respond to those incentives in their medical practices. We show that take-up is quite gradual, suggesting an important role for information frictions in mediating the take-up of new billing codes. We also show that take-up is strongest for mid-career PCPs, who have many career years left to reap the benefits of investing in the expertise necessary to bill the new codes now; for physicians practicing in PCP-only groups, who are particularly likely to experience low information frictions and high monetary benefits related to billing the new codes; and physicians practicing in areas where chronic conditions are prevalent among the population of beneficiaries, where the new codes are particularly likely to be beneficial.

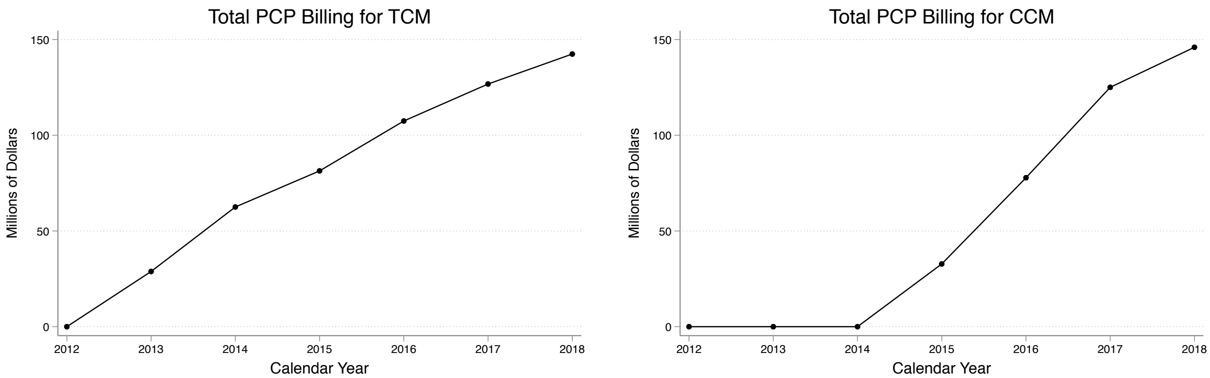
To evaluate the impact of these new billing codes on the primary care landscape, it is also important to know how the codes affect the provision of other services. In the next chapter, we study how billing of the Chronic Care and Transitional Care Management codes interacts with billing for various categories of other medical services. We show evidence of both substitution and complementarity between the new codes and other types of services. These relationships should be accounted for when considering the effect of these new billing codes on levels of care and physician billing behavior.

1.7 Acknowledgements

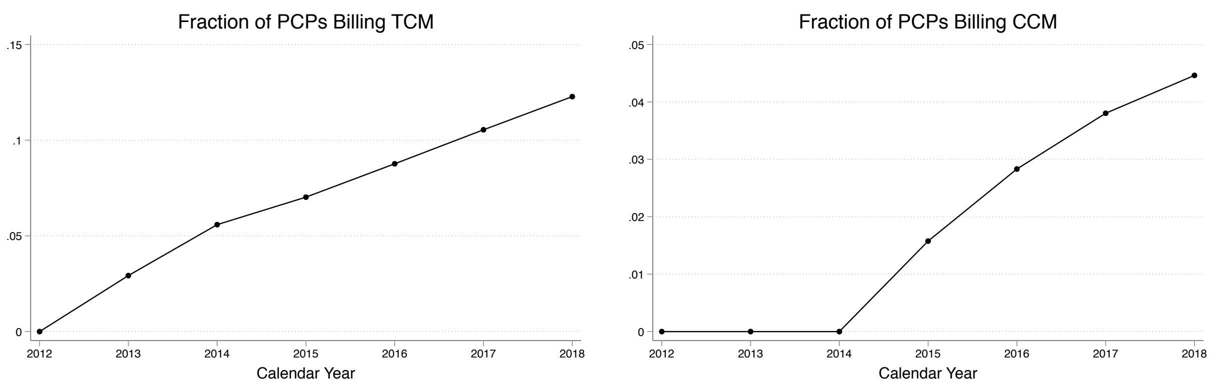
Chapter 1 contains material that is currently being prepared for submission for publication. Clemens, Jeffrey, Leganza, Jonathan M., and Masucci, Alex. “New Medicare Billing Codes: Take-Up and Usage Patterns.” The dissertation author was a primary investigator and an author of this material.

1.8 Figures and Tables

(a) Total New Code Billing



(b) Fraction of PCPs Billing New Codes



(c) New Code Billing as a Share of Total Billing

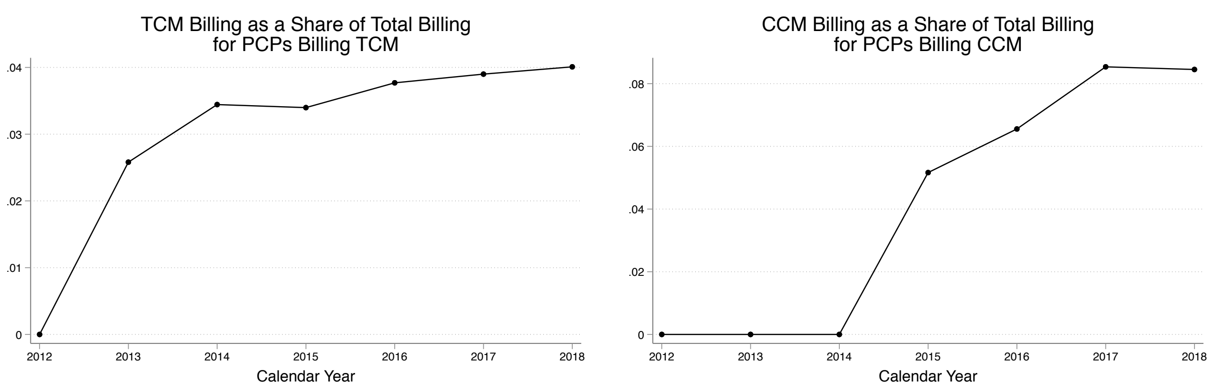
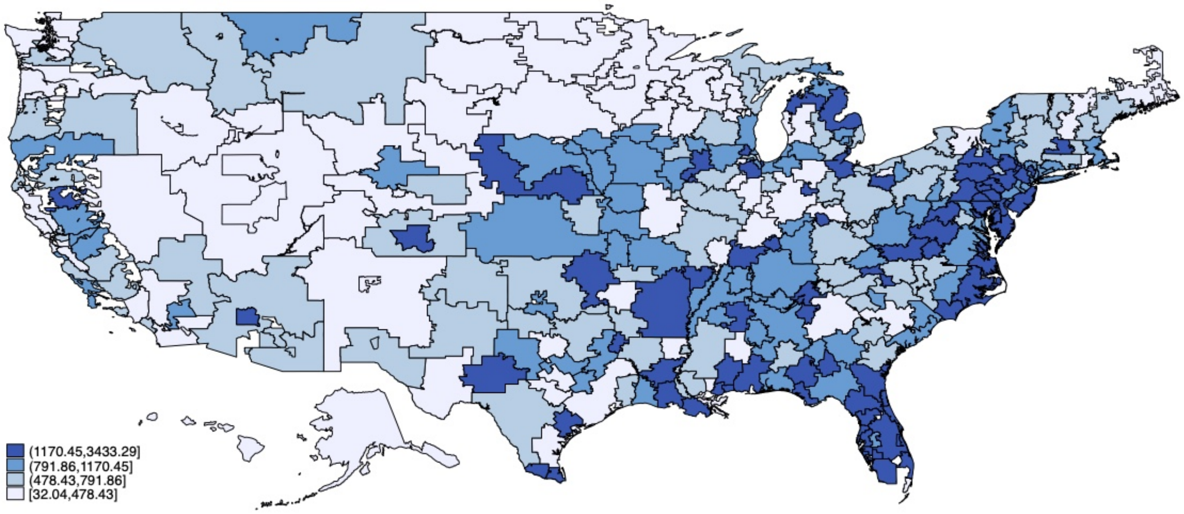


Figure 1.1: Take-Up of New Codes Over Time

Notes: These statistics are obtained from our 2012-2018 sample of physicians. PCPs are defined to be physicians with a specialty of Internal Medicine, Family Practice, General Practice, or Geriatric Medicine.

(a) TCM Hospital Referral Region Billing per PCP in 2018



(b) CCM Hospital Referral Region Billing per PCP in 2018

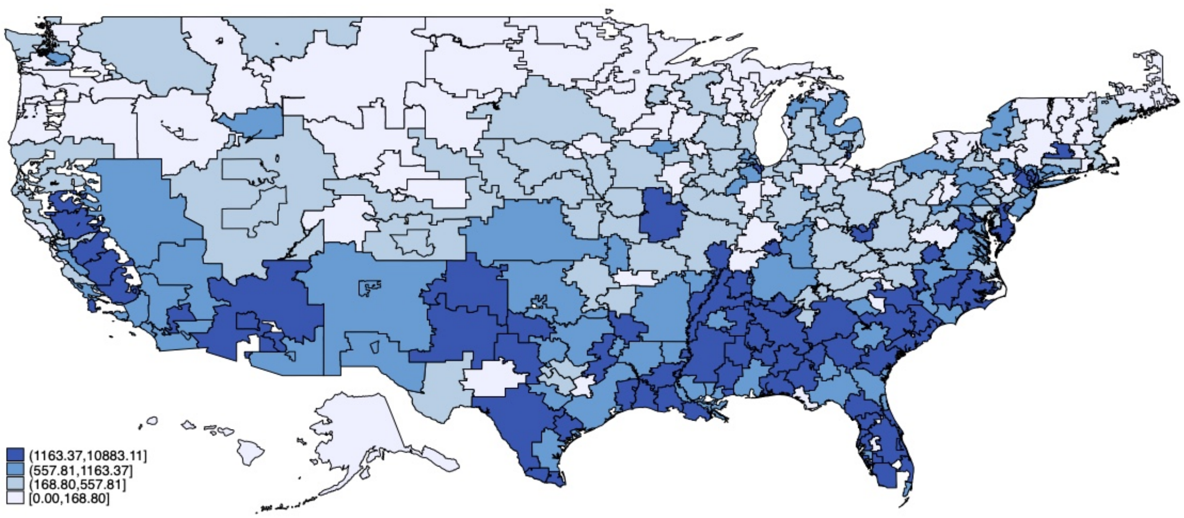
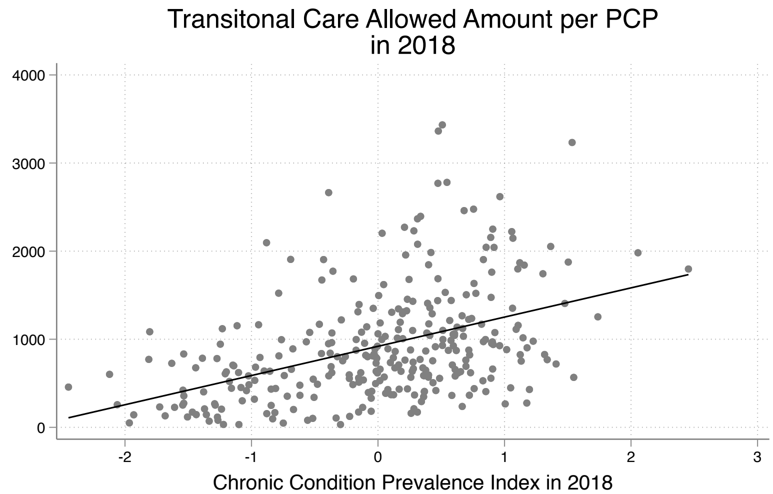


Figure 1.2: Regional Variation in the Take-Up of New Codes

Notes: These heat maps show relative differences in new code billing per PCP at the Hospital Referral Region (HRR) level in 2018, the final year of our sample.

(a) TCM Hospital Referral Region Billing per PCP in 2018



(b) CCM Hospital Referral Region Billing per PCP in 2018

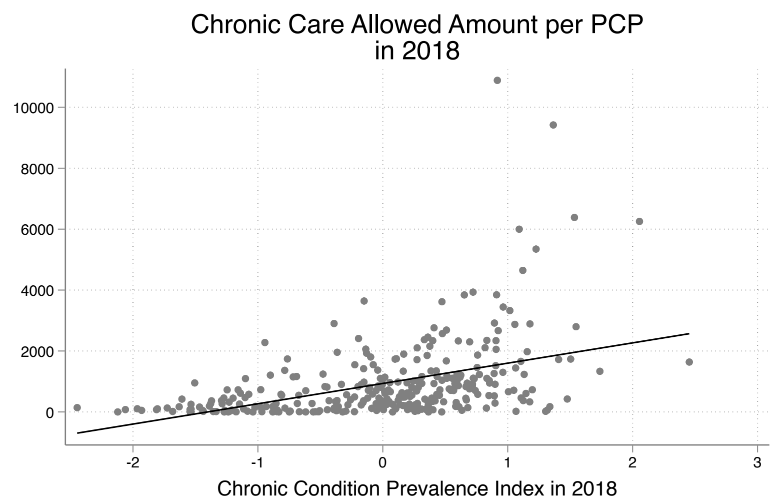
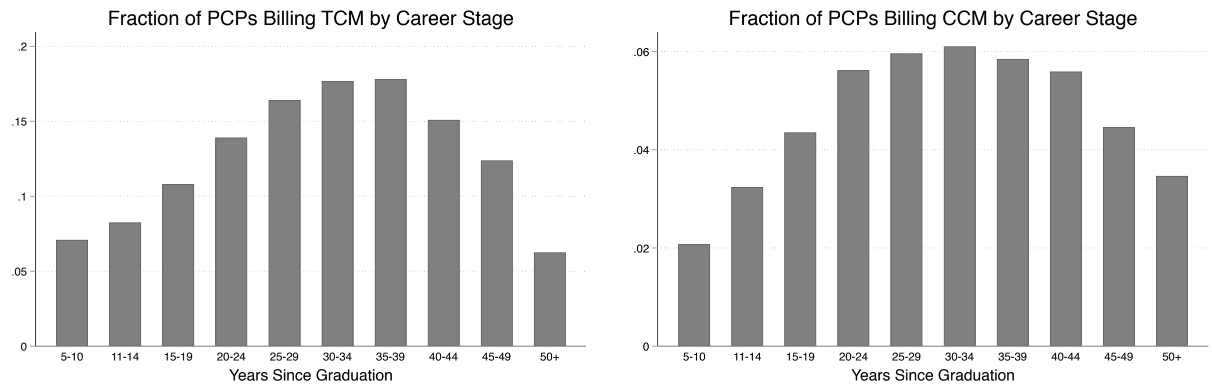


Figure 1.3: Regional Variation in the Take-Up of New Codes by Chronic Condition Prevalence

Notes: These scatter plots show billing for each new code per PCP at the HRR level plotted against a constructed normalized index for chronic condition prevalence in 2018, the final year of our sample. The corresponding regression lines are also plotted. The chronic condition index is constructed by normalizing the prevalence rates of each of eight chronic conditions at the HRR level and averaging these eight values.

(a) New Code Billing by Career Stage



(b) New Code Billing by Medical School Ranking

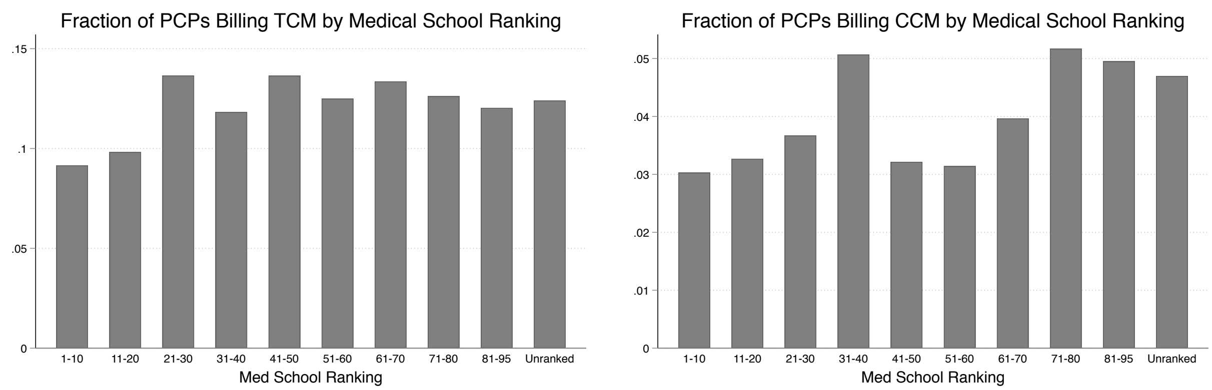
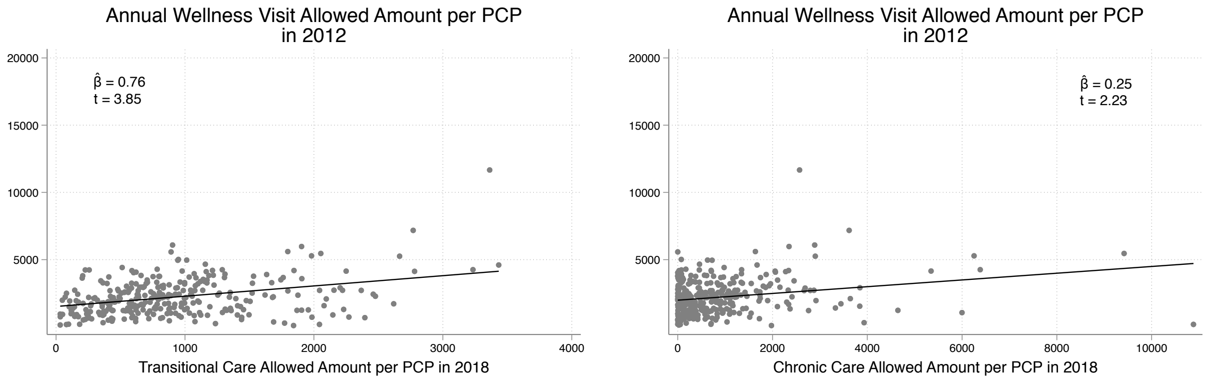


Figure 1.4: New Code Billing in 2018 by Career Stage and Medical School Ranking

Notes: These bar graphs show the fraction of PCPs billing the new codes for different categories of career stage and medical school ranking in 2018, the final year of our sample. Medical School rankings are defined using the 2018 U.S. News & World Report rankings.

(a) Correlations in 2012



(b) Correlations in 2018

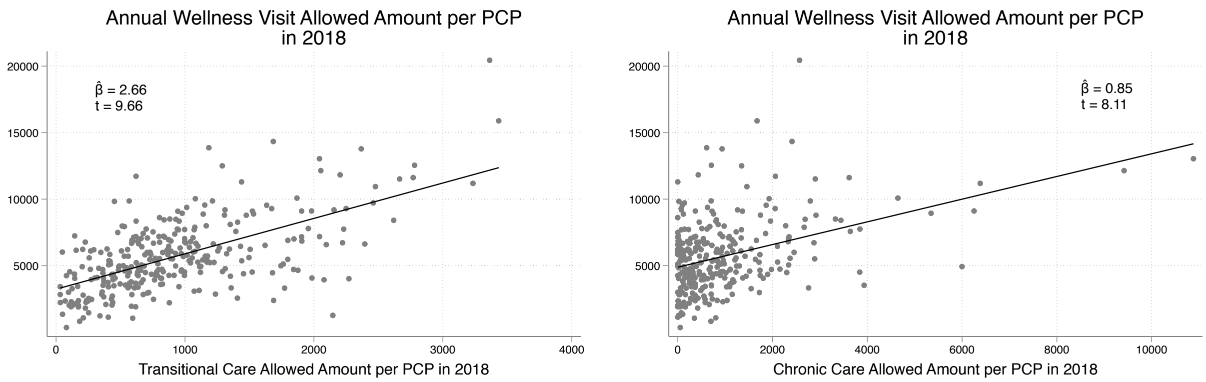


Figure 1.5: Correlations Between New Code Billing and Annual Wellness Visit Billing

Notes: These scatter plots show billing for Annual Wellness Visits per PCP plotted against billing for each new code per PCP at the HRR level. These plots are shown for 2012, the first year of our sample (before either new code was introduced), and 2018, the final year of our sample. The corresponding regression lines are also plotted.

Table 1.1: Summary Statistics Before the Introduction of the New Codes

	Statistic
Panel A. PCP Counts	
Number of PCPs	175,408
Fraction Sole Practitioner	0.196
Fraction Early Career	0.153
Fraction Mid Career	0.654
Fraction Late Career	0.121
Fraction Ranked	0.323
Fraction Unranked	0.677
Panel B. PCP Billing	
Average Total Billing	\$101,967 (139,856)
Average Billing for Office Visits	\$45,186 (57,400)
Average Billing for Annual Wellness Visits	\$2,100 (6,877)

Notes: These summary statistics are obtained from our sample of physicians in 2012. PCPs are defined to be physicians with a specialty of Internal Medicine, Family Practice, General Practice, or Geriatric Medicine. Early-career, mid-career, and late-career physicians are those who graduated from medical school 5-24 years prior, 25-39 years prior, and 40+ years prior, respectively. Medical School rankings are defined using the 2018 U.S. News & World Report rankings.

Table 1.2: Likelihood of Billing New Codes in 2018

	Percent of PCPs Billing TCM	Percent of PCPs Billing CCM	Observations
Panel A. Specialty			
PCPs	12.3%	4.5%	176,676
Non-PCPs	0.6%	0.4%	878,302
Panel B. Career Stage			
Early-Career PCPs	10.0%	3.8%	27,688
Mid-Career PCPs	17.3%	6.0%	109,269
Late-Career PCPs	12.9%	4.9%	30,588
Panel C. Medical School			
PCPs from Ranked Schools	12.0%	3.9%	52,049
PCPs from Unranked Schools	12.4%	4.7%	124,627
PCPs from Top 10 Schools	9.2%	3.0%	5,972
Panel D. Group Size			
Sole Practitioner PCPs	10.8%	5.0%	32,830
Small Group PCPs	15.4%	6.0%	44,915
Mid-Size Group PCPs	15.8%	5.3%	44,511
Large Group PCPs	7.8%	2.2%	54,420
Panel E. Group Size and Composition			
Small PCP-Only Group PCPs	16.5%	6.1%	29,396
Small Non-PCP-Only Group PCPs	13.1%	5.7%	15,519
Mid-Size PCP-Only Group PCPs	17.3%	6.1%	11,552
Mid-Size Non-PCP-Only Group PCPs	15.2%	5.0%	32,959
Large PCP-Only Group PCPs	10.3%	3.2%	533
Large Non-PCP-Only Group PCPs	7.8%	2.2%	53,887
Panel F. Other Billing Behaviors			
PCPs Billing Standard Office Visits	16.6%	5.7%	129,495
PCPs Billing the Other New Code	40.9%	14.8%	21,692 | 7,883
PCPs Billing Annual Wellness Visits	27.3%	9.3%	67,803

Notes: These new code billing propensities are obtained from our sample of physicians in 2018, the final year of our sample. PCPs are defined to be physicians with a specialty of Internal Medicine, Family Practice, General Practice, or Geriatric Medicine. Early-career, mid-career, and late-career physicians are those who graduated from medical school 5-24 years prior, 25-39 years prior, and 40+ years prior, respectively. Medical School rankings are defined using the 2018 U.S. News & World Report rankings. Small groups, mid-size groups, and large groups are groups with 2-5 practitioners, 6-20 practitioners, and 20+ practitioners, respectively.

Table 1.3: Group-Level Likelihood of Billing New Codes in 2018

	Percent of Groups Billing TCM	Percent of Groups Billing CCM	Observations
Sole Practitioners	10.8%	5.0%	32,830
Small Group	21.3%	8.5%	26,801
Mid-Size Group	31.2%	11.5%	12,254
Large Group	36.8%	15.9%	4,821

Notes: These group-level new code billing propensities are obtained from our sample of physicians in 2018, the final year of our sample. We include all groups that have at least one PCP. PCPs are defined to be physicians with a specialty of Internal Medicine, Family Practice, General Practice, or Geriatric Medicine. A group is defined to be billing the new code if at least one physician at the group bills the code in 2018.

1.A Appendix: Additional Data Details

1.A.1 MPUP, NPPES, and Physician Compare

The *Medicare Provider Utilization and Payment Data* (MPUP) has address data for the practice of each physician in the data set. CMS obtains this data from the *National Plan and Provider Enumeration System* (NPPES) data and merges it into the MPUP claims data before publishing it. However, each year of the raw MPUP data actually contains physicians' addresses from the end of the calendar year following the given year of claims data. The exception to this is the 2012 MPUP data, for which physicians' addresses were taken from the end of calendar year 2014. We download the NPPES files and overwrite the address variables in each year of the MPUP data with the address variables in the NPPES file from December of the year in which the claims in the MPUP data occurred. That is, we fix the raw input data so that the physician addresses reflect where they practiced during that year of claims data.

The main use of the address data in our paper is to define physician groups. We define a physician group as any physicians that practice at the same address in a given year. Before defining groups, we do some basic changes to the street address variables to align observations where the same address may have been typed in different ways. Namely, we remove all punctuation, and we convert all address suffixes recognized by the U.S. Postal Service to their standard abbreviations (e.g. "STREET" becomes "ST").

We also use data from the *Physician Compare* database to provide us with information on graduation year and medical school at the physician level. For any conflicts between the values of these variables for the same physician across different years of Physician Compare data, which are rare, we use the most recent non-missing value. Some physicians are missing data on these variables: 7.9% of physicians in our panel in 2018 are missing their graduation year, and the same fraction are missing their medical school. We do not define these physicians as belonging to any career stage or as having attended a ranked or unranked medical school.

1.A.2 Constructing the Chronic Care Index

We construct an index that reflects the overall prevalence of eight chronic conditions at the county level for each year. These are conditions that are often experienced by the elderly. We show that this variable is correlated with new code take-up in Figure 1.3. The data on the prevalence of these chronic conditions come from the *CMS Chronic Conditions Files*.

The eight conditions included in our baseline index are arthritis, kidney disease, COPD, diabetes, heart failure, hyperlipidemia, hypertension, and ischemic heart disease. For each year, we normalize the prevalence rate of each of these conditions by subtracting that year's mean of the prevalence rate and dividing this difference by that year's standard deviation of the prevalence rate. This gives us eight normalized values reflecting how many standard deviations above the mean each county is in terms of each of the eight conditions. The mean of these eight values for each county gives us our baseline normalized chronic condition prevalence index. Our findings are robust to chronic condition indices that include more chronic conditions than the eight included in our baseline index.

County-level chronic condition prevalence rates are sometimes missing in the CMS data. The number of counties in our unmatched sample that are missing data in 2018 (out of a total of 3,078 counties) is 2 for arthritis, 2 for kidney disease, 6 for COPD, 2 for diabetes, 4 for heart failure, 2 for hyperlipidemia, 2 for hypertension, and 2 for ischemic heart disease. Rates of missing data are lower for our matched sample. When data is missing for a condition in a given county, we impute the condition's prevalence rate using the beneficiary-weighted average of that condition's prevalence rate in the other counties in the same Hospital Referral Region with non-missing data for that condition.

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Chapter 2

Primary Care Service Interactions: Evidence from the Take-Up of New Medicare Billing Codes

Abstract

We use geographic variation in the take-up of codes for new primary care services to estimate the effect of these services on other types of care. Our main analysis consists of a panel regression and a matched county-level difference-in-differences design that compares high-intensity counties to low-intensity counties. We find evidence that Transitional Care substitutes for traditional post-discharge visits to an extent but also likely represents an increase in real care provided. We also find that Transitional Care complements with other basic primary care services such as office visits and Medicare's more recently introduced Annual Wellness Visits, resulting in higher overall levels of traditional Evaluation & Management services. We find less evidence of complementary care with Chronic Care, and Chronic Care is associated with a reduction in the provision of Imaging services. Finally, we find suggestive evidence that introduction of the new codes resulted in greater provision of recommended care such as vaccinations and mammograms.

2.1 Introduction

Changes to the medical billing landscape can alter the financial incentives faced by physicians in ways that have implications for the provision of various types of primary care services. In Chapter 1, we investigate and document the take-up patterns of new billing codes for Chronic and Transitional Care Management, noting significant differences in take-up across physician type and geography. In this chapter, we analyze the relationships between billing for these new codes and billing for other services. That is, we explore the extent to which the new codes serve as complements and substitutes for other services. Importantly, patterns of complementarity and substitutability can involve subtle mixes of changes in bill coding, on the one hand, and real care provision, on the other.

We analyze patterns of complementarity and substitutability between the adoption of new codes and either the billing or provision of additional services. We use a set of complementary regression frameworks to study the effects of new code adoption at the county level. We begin by estimating fixed effect models that exploit all panel variation in the intensity with which new codes are billed across counties. For our estimates to capture the causal effect of new code take-up, the key assumption we must make is that differential rates of new code take-up were uncorrelated with differential counterfactual trends in overall health care needs and utilization. We provide evidence on the validity of this assumption using two sets of robustness analyses. First, we show that our initial estimates are robust to whether we control for a rich set of time-varying demographic and health characteristics of the Medicare beneficiaries in each county. Second, we estimate event study models for which we match high and low take-up counties on the basis of baseline levels of health care utilization. Together, these analyses provide evidence that our estimates are unaffected by divergent trends associated with variations in population health and health care utilization either at baseline or over the course of our sample.

We begin our analysis of patterns of care complementarity and substitutability by presenting relatively clear illustrations of code substitution, on the one hand, and code complementarity on the other. First, we find that the adoption of Transitional Care Management services has partially crowded out the billing of standard office visits during the weeks following hospital discharges. Second, we find that both Transitional and Chronic Care Management services quite strongly predict increases in the provision of Annual Wellness Visits. Finally, we find that Transitional Care Management services predict a much broader increase in the billing of "complex" office visit codes. Below, we emphasize two policy relevant aspects of these findings.

After illustrating the phenomena of substitution and complementarity, we analyze the relationship between new code billing and billing patterns across broad categories of care. We find that Transitional Care Management billing predicts substantial increases in overall care provision. These increases occur primarily within Evaluation & Management services and a category of "Other" services that includes vaccinations, but not in the provision of Procedures, Imaging, or Tests. By contrast, Chronic Care Management billing predicts no net increase in overall billing and a modest decrease in the provision of Imaging services. These findings suggest that the Chronic Care Management code may, in large part, have rationalized the coding of services that had previously been delivered and billed using less lucrative codes. By contrast, the Transitional Care Management codes appear to generate substantial increases in patients' interactions with their doctors. This is consistent with CMS's goal in introducing the Transitional Care Management codes, namely to enhance the coordination of care for discharged patients with complex conditions. Finally, we find that the adoption of the Transitional Care Management codes predicts increases in influenza vaccinations, pneumonia vaccinations, and mammograms, which are strongly recommended services. The care complementarities associated with Transitional Care Management ramp up over time, reflecting the gradual take-up of the new code itself. While a comprehensive cost-benefit analysis is beyond this paper's scope, our findings illustrate the relevance and potential importance of several nuanced pieces of the puzzle.

Our analysis makes contributions to existing literatures on related topics. We show that patterns of complementarity and substitutability in bill coding and service provision can play important roles in shaping a payment reform's effects on overall cost and care delivery. Our analysis provides clear illustrations demonstrating that new service codes introduced by Medicare can both complement and substitute for existing service codes. We emphasize two policy relevant aspects of these findings. First, code substitution and code complementarity will tend to impact claims-dependent systems of quality measurement and/or risk adjustment. Carey 2017, for example, shows that the entry of new drugs creates challenges for risk-adjustment models.[1] The introduction of new service codes can have similar implications. Second, the existence of complementarities in real service provision can have straightforward effects on both the cost and health benefits of introducing new service codes. A comprehensive analysis of the costs and benefits of introducing new codes must account for these spillovers.

The remainder of this paper proceeds as follows. In Section 2.2 we recap fundamental background information about the introduction of the new Chronic and Transitional Care Management billing codes. In Section 2.3 we describe the data used in our analyses. In Section 2.4 we present our empirical research designs for estimating the effects of new billing code take-up on other billing and service provision. Section 2.5 presents the results of these analyses, and Section 2.6 concludes.

2.2 Background

A full discussion of the background, motivation, and introduction of the new billing codes that we study in this paper is provided in Section 1.2 of Chapter 1. the Transitional

[1] New drugs alter affected patients' expected costs, which changes their profitability net of risk adjustment. Carey 2017 finds that the design of Medicare Part D plans responds to the incentives associated with the introduction of new drugs. Geruso et al. 2019 and Lavetti and Simon 2018 provide related evidence on the strategic responses of firms to the incentives created by risk adjustment mechanisms for drug benefits. Brown et al. 2014 develop related findings in the context of Medicare Advantage plans.

Care Management (TCM) and Chronic Care Management (CCM) codes were introduced to appropriately compensate physicians for specific types of care that were not being adequately provided to the population of Medicare beneficiaries. The TCM codes were introduced in 2013 to allow physicians to charge for visits with a beneficiary within two weeks of a discharge from the hospital that are focused on managing post-discharge care and coordinating care with other health providers if necessary. The initial CCM code was introduced in 2015 to compensate physicians for visits intended to manage care related to chronic conditions for beneficiaries with multiple chronic conditions. Additional CCM codes were introduced in 2017 to pay physicians greater amounts for CCM visits that involved greater length or more complex medical decision making. In the remainder of this paper, we study the effects of billing for TCM and CCM on billing for other categories of services.

2.3 Data

As described in Section 1.4 of Chapter 1, we construct a panel of physicians from 2012 to 2018 using the *Medicare Provider Utilization and Payment Data*, the *National Plan and Provider Enumeration System*, and the *Physician Compare* database. For the analysis below, we aggregate this data to the county level and merge in data on the prevalence of chronic conditions in each county from the *CMS Chronic Conditions Files.*

We use data on patient demographics and health care utilization from two additional county-level datasets. From the *CMS Geographic Variation Public Use File*, a dataset that CMS publishes for researchers and policy makers to assess geographic variation in health care services, we extract information on basic demographics. Specifically, we utilize county-level variables that report total beneficiary counts, the percent of beneficiaries that are female, the percent of beneficiaries that are eligible for Medicaid, and the average age of beneficiaries. The latter three of these variables as well as our constructed index of chronic condition prevalence serve as controls in some specifications of our regressions.

Finally, we use the *Dartmouth Atlas Post-Discharge Events* data from 2010 to 2017, which provide county-level rates of the incidence of various health care-related events experienced by beneficiaries after being discharged from the hospital. These rates are calculated as a percentage of all hospital discharges in the county in each year. This data set provides particularly relevant outcomes for studying the effect of Transitional Care Management, since this service is directly used to provide managed care for patients after a hospital discharge.

2.4 Empirical Framework for Analyzing the Effects of New Code Adoption

In our analysis we use a complementary set of regression frameworks to estimate the effects of new code take-up on broader patterns of bill coding and care provision. Within each framework, we implement robustness checks to gauge the relevance of threats to interpreting the estimated relationships between new code billing and outcomes of interest as causal. The first regression framework we consider exploits all panel variation in the intensity with which new codes are billed at the county level. That is, we estimate the equation below, where c denotes counties and t denotes years:

$$\begin{aligned} Outcome_{c,t} = \beta_1 NewCodeBillingperPCP_{c,t} + X_{c,t}\gamma \\ + \alpha_{1c}County_c + \alpha_{2t}Time_t + \varepsilon_{c,t}. \end{aligned} \tag{2.1}$$

Equation (2.1) controls for county fixed effects ($County_c$), time fixed effects ($Time_t$), and time-varying county characteristics ($X_{c,t}$).

When we estimate equation (2.1), the primary coefficient of interest is β_1, which describes the relationship between the outcome of interest and the dollar value of new code billing per primary care physician. For β_1 to be an unbiased estimate of the effect of new code adoption, new code billing would need to be as good as randomly distributed. This may, of course, seem implausible given that the new codes are intended for use when patients have chronic conditions, and will thus be more intensively used in counties where many patients have such conditions. Here, it is crucial that the new codes did not exist during the first year of our sample, which allows us to use county fixed effects to effectively control for baseline variations in counties' outcomes. The key assumption is that variations in the intensity with which new codes were adopted were uncorrelated with other sources of divergence in counties' outcomes. Our robustness analyses are designed to provide checks for the relevance of threats to this key assumption.

A first set of robustness checks we implement operate within the basic estimation framework described by equation (2.1). We explore the robustness of our estimates to whether we control for time-varying county characteristics that describe the health of the Medicare population. Specifically, we construct indices for the prevalence of a variety of chronic conditions. We then estimate regressions both with and without these covariates included in $X_{c,t}$, which provides evidence on whether our estimates are sensitive to controlling for proxies for variation in the evolution of the patient population's health.

In a second set of robustness checks, we transition from equation (2.1) to an event study estimator. For the event-study approach, we divide counties into groups based on the intensity with which they adopted the new billing codes. High intensity adopters are implicitly our "treatment" group while low intensity adopters and non-adopters are implicitly our "control" group.[2] Using this grouping of counties, we then estimate regressions of the form:

$$\begin{aligned} Outcome_{c,t} = \sum_{p(t)\neq 0} \beta_{p(t)} HighIntensityNewCoder_c \times EventYear_{p(t)} + X_{c,t}\gamma \\ + \alpha_{1c} County_c + \alpha_{2t} Time_t + \varepsilon_{c,t}. \end{aligned} \tag{2.2}$$

[2]Specifically, we first drop counties that do not meet a size threshold of having over 10 total PCPs in 2012. We then order the remaining counties in our sample by average post-implementation annual new code billing per PCP. The top half of these counties is defined as the treatment group, and the bottom half is defined as the control group.

In equation (2.2), we interact a set of "event time" dummy variables with an indicator for whether a county was a high intensity adopter of the new billing code. The event time dummy variables are coded to correspond with specific numbers of years relative to the new code's introduction, which corresponds with 2015 when we analyze the Chronic Care Management codes and 2013 when we analyze the Transitional Care Management codes. We omit the interaction for the time period describing the year immediately prior to the new code's introduction, which we define as year p(t) = 0. The coefficients of interest can thus be interpreted as differential changes in the outcome of interest from the year prior to the new code's introduction to the reference year. For reference years less than 0, the point estimates thus provide evidence on whether divergent trends in the outcome had occurred prior to the new code's introduction. This provides evidence on the relevance of concerns related to divergent pre-existing trends. Estimates for years following the new code's introduction track the dynamics with which the outcome subsequently evolved.

Note that equation (2.2) provides a natural check for one of the sources of bias that might be relevant to our estimate of β_1 in equation (2.1). The absence of divergent pre-existing trends would provide evidence that outcomes of interest were on parallel paths in the high take-up counties relative to low take-up counties. One might still worry, however, that differential shocks may have occurred in later years in ways that correlate with high intensity take-up. This motivates us to implement one additional check.

As shown in Section 1.5 of Chapter 1, high intensity take-up of the new codes is, as was intended, correlated with the prevalence of chronic conditions. More generally, high take-up is correlated with more intensive utilization of services. We thus implement a final robustness check that limits our sample to high and low intensity take-up counties with similar levels of overall health care utilization at baseline. That is, we match "high" and "low" intensity counties on their baseline total allowed amounts.[3] By estimating equation

[3]The treatment group in the matched strategy is identical to the treatment group that is defined using the unmatched strategy. To generate our control group, we match to each treatment county the three counties from the unmatched control group that are closest to it in terms of total PCP billing per PCP in the year

(2.2) on the resulting matched sample, we provide a final check for the problem of divergent pre-existing trends in high vs. low take-up counties with populations that had similar levels of overall utilization at baseline.

2.5 Analysis of the Effects of New Code Adoption on Subsequent Coding and Care Provision

In this section we present the results of our analysis. For each outcome of interest, we report estimates for both equation (2.1)) and equation (2.2). In the main text we present estimates of equation (2.1) both without and without the inclusion of demographic and health related covariates. In the main text we also present event study estimates of equation (2.2) using our matched sample of counties. Estimates for equation (2.2) using the unmatched sample are shown in the Appendix. We are careful to differentiate between outcomes for which our estimates are robust across this set of specifications and outcomes for which our findings exhibit sensitivity.

In Section 2.5.1, we show the difference in new code take-up between the treatment and control groups in our event study estimation strategy. In Section 2.5.2, we present evidence from a clear case in which the new billing codes acted as a (partial) substitute for other service codes. In Section 2.5.3, we present evidence from clear cases in which the adoption of new codes was complementary to the provision and billing of additional services. In Section 2.5.4, we present a more comprehensive analysis of the effects of the adoption of the Transitional Care Management and Chronic Care Management codes on a broader set of coding and care provision outcomes. Finally, in Section 2.5.5, we present analyses of the effects of new code adoption on proxies for patient receipt of best practice care.

before the implementation of the new code of interest.

2.5.1 New Code Billing by Treatment Status

Figure 2.1 presents an analysis of TCM and CCM billing for the counties we identify as high take-up counties (the "treatment" group) relative to the counties we identify as low take-up counties (the "control" group) for our matched event-study design. We see that the treatment groups for both new codes exhibit a widening gap in new code usage relative to the control groups. For TCM, the gap appears to stabilize in 2018, the final year of our sample and the sixth year of usage for that code, leaving treatment counties with about $1,500 more in TCM billing per PCP. For CCM, the gap is about $2,000 per PCP in 2018. This is the fourth year of CCM availability, and the gap between the treatment and control groups has not clearly leveled off at this point. For our event study analyses, these differences in new code billing between the "treatment" and "control" counties can be viewed as similar to an underlying "first stage;" the magnitude of the differential utilization of the new codes should be kept in mind for scaling the variations we observe in the outcomes we analyze in Sections 2.5.2 through 2.5.5.

2.5.2 A Case of Code Substitution

In this section we begin our analysis of the effect of the new codes on overall billing and service delivery. An initial effect of interest involves the possibility that the introduction of new billing codes may lead to substitution away from other service codes. This could involve either real changes in service provision or pure coding substitution. The TCM and CCM codes were introduced to improve compensation for physicians who are responsible for designing and implementing complex care management plans. One possibility is that, prior to the new codes' introduction, physicians may have billed more basic office visit codes that, in a relevant sense, would have undercompensated them for the work performed. The transition to new codes may also come with increases in the intensity of a fixed number of patient-physician interactions. (Later, we consider the possibility of complementary care, in which the adoption of new codes alters care management plans and, as a result, increases the number of patient-physician interactions.)

Figure 2.2 presents an illustrative example of code substitution in practice. Panel A shows the fraction of beneficiaries that have a traditional office visit within two weeks of discharge from the hospital, split by treatment status for TCM.[4] Panel B shows the fraction of beneficiaries with a traditional post-discharge office visit with a Primary Care Physician specifically, as opposed to with a Nurse Practitioner or Physician Assistant. Note that these variables come from the Dartmouth Atlas of Health Care, which allows us to track this particular set of outcomes from 2010 through 2017, whereas the bulk of our analysis involves variables that extend from 2012 through 2018. Both panels reveal a clear relative decline, comparing "treatment" and "control" counties, in the fraction of beneficiaries receiving a traditional post-discharge office visit in the treatment group relative to the control group. By 2017, this amounts to a 4 percentage point decline in the fraction of beneficiaries receiving these visits and a decline of 6 percentage points in the fraction of beneficiaries receiving these visits from PCPs in particular. The pre-treatment trends in both outcomes are flat, indicating that these outcomes were not diverging across our treatment and control counties prior to the treatment counties' take-up of the TCM code.

Table 2.1 shows estimates of β_1 from equation (2.1) for these outcomes. These specifications regress the outcome of interest on the county-level volume of new code billing per PCP; the analysis thus allows us to exploit all of the available variation in our county-level panel to estimate the effects of new code billing. We find that an additional thousand dollars of TCM billing per PCP predicts a 1.2 percentage point reduction in the fraction of beneficiaries receiving a traditional post-discharge visit with any care provider. We similarly estimate a 1.9 percentage point reduction in the fraction of beneficiaries receiving such a visit from a PCP. Table 2.A.1 shows the year-by-year point estimates, and we see that the

[4]The visits included in this variable are those corresponding to HCPCS codes 99201-99205, 99211-99215, 99381-99387, 99391-99397, 99241-99245, and 99271-99275.

magnitude of the estimates rises over time, in particular for the latter outcome, as new code take-up expands in prevalence.

Figure 2.2 and Table 2.1 provide clear evidence of a case of code substitution. Office visits provided within two weeks of a hospital discharge, as tracked by the Dartmouth Atlas, would convert quite readily into TCM services. We emphasize two additional points of interest with respect to this particular instance of code substitution. First, the amount of code substitution appears to be quite modest, implying a non-trivial net increase in post-discharge visits. For example, our estimates imply a reduction of one traditional post-discharge visit with a PCP for every 17 additional TCM visits.[5] This implies that while TCM is substituting for some care that was previously being provided, the bulk of TCM visits represent an increase in real post-discharge care that is taking place because of the introduction of the TCM codes. On this point, we note that a key purpose of TCM services is to aid in coordinating a patient's care across providers. Successful management of this sort may manifest itself through a non-trivial increase in the total quantity of care delivered. We consider such effects directly in Sections 2.5.3 and 2.5.4.

Second, we highlight that both code substitution and code complementarity can have subtle implications for claims-dependent measures of care quality and claims-dependent measures of risk adjustment. Metrics like those analyzed here, namely post-discharge visits with primary care physicians, are sometimes interpreted as measures of care quality. Our estimates reveal that a stagnant measure of post-discharge care, meaning a measure constructed entirely from the codes for standard office visits, would have penalized the physicians or health systems who were quickest to adopt the TCM code. This highlights that quality metrics that use billing codes to assess a provider's compliance with recommended care de-

[5]To arrive at these figures, we take $1,000 of new code billing per PCP, multiply it by the average county-level stock of PCPs in our sample (58.42), and divide the resulting figure by the average billed amount for a TCM visit ($190.08). This tells us that $1,000 of additional new code billing per PCP represents 307.34 TCM visits on average. The average county-level count of discharged patients in our sample is 965.67. Comparing 1.2-percentage-point and 1.9-percentage-point declines in this figure to the total increase in TCM visits with which these declines are associated yields the figures above.

livery must adapt to changes in coding systems. Changes in coding patterns can also have implications for risk adjustment models that take prior years' coding patterns as inputs.[6]

2.5.3 A Case of Complementary Coding and Service Provision

We might also expect the new codes to serve as complements to other primary care services. This could happen for distinct reasons that, as above, may blend changes in real service provision with changes in coding practices. For instance, on the one hand, the TCM codes were explicitly intended to reimburse services that would help with the coordination of post-discharge care. Once successfully integrated into physicians' practices, TCM billing might thus result directly in an increase in patients' contact with other physicians and an increase in care that would have otherwise not been provided. On the other hand, the need to integrate the new codes into a practice could lead a physician group to simply update their coding procedures more generally, or perhaps to hire a coding specialist, which could lead to changes in billing that come from reclassifying care that would have otherwise been provided.

An example of a service that acts as a complement to both TCM and CCM is the Annual Wellness Visit. The Annual Wellness Visit was first introduced as a part of the Affordable Care Act. It describes an office visit during which a patient receives a standard wellness check and works with a physician to plan for upcoming preventive care. In Figure 2.3 we see that the introduction of TCM is associated with an increase in the billing of Annual Wellness Visits in the treatment counties relative to the control counties. According to Table 2.2, this relationship amounts to an additional $1.28 of Annual Wellness Visit billing per PCP for every additional dollar of TCM billing per PCP. We estimate a far

[6]This point relates quite directly to insights from Carey 2017. Carey points out that because new drugs alter affected patients' expected costs, their introduction can alter patients' relative profitability to drug plans when risk adjustment is based on prior years' claims. Carey shows further that the design of Medicare Part D plans is responsive to these incentives. Geruso et al. 2019, Lavetti and Simon 2018, and Brown et al. 2014 also provide evidence that firms respond strategically to the incentives created by risk adjustment mechanisms.

more modest complementarity of 13 cents in Annual Wellness Visit billing per PCP for each dollar of CCM billing per PCP (see Panel B of Table 2.2). The magnitude of the relationship between Annual Wellness Visits and TCM is particularly interesting given that the codes are meant to serve the complementary purpose of expanding the quantity and quality of primary care received by Medicare beneficiaries.

The implications of the complementarity of TCM with another recently introduced code should be considered in the context of its complementarity with traditional office visits. Table 2.3 shows the relationship between both new codes and the billing of office visits of 5 levels of complexity, where level 5 represents the most intensive office visits.[7] We see in Table 2.3 that TCM complements office visits overall and that this is primarily driven by an additional \$2.35 in level 4 office visit billing per PCP for each dollar of TCM billing per PCP. This relationship is shown in the event study specification in Figures 2.4 and 2.5 as well. The event studies for the unmatched sample (Figures 2.A.4 and 2.A.5) reinforce the positive relationship between TCM billing and complexity level 4 visits, although we do not estimate a statistically significant effect for total office visit billing for this sample.

The fact that there is some evidence of a relationship between CCM and Annual Wellness Visits, but not of one between CCM and complex office visits, may indicate that some of the Annual Wellness Visit complementarity stems from learning about coding and adopting more sophisticated billing practices that take advantage of the high payments available for recently introduced codes. But the strong link between TCM and complex office visits also suggests that there is a real care complementarity that is occurring independent of any increase in coding sophistication, and some of this additional care may be spilling over into Annual Wellness Visits. A mix of these stories is also plausible, where physicians provide more primary care visits in general and more intently provide an Annual Wellness Visit for each of their patients due to increased coding knowledge.

[7]The office visit complexity categories are defined by factors such as the degree of detail of the medical history and examination involved, the complexity of the medical decision making, and the length of the visit.

Increased coding sophistication could also partially explain the complementarity of TCM with traditional office visits that are of high complexity. Of course, this relationship could simply reflect the needs of the patients that TCM-billing PCPs are seeing. But it is also plausible that greater knowledge about coding, whether provided by a billing specialist or otherwise, would lead to an increase in the share of office visits provided that are classified as the more complex and higher-paying levels. Our estimates in Table 2.3 indicate a complementarity between TCM and level 5 office visits that is similar to the level 4 complementarity in terms of its percentage increase from the baseline. Additionally, while our estimates lack precision, the point estimate for level 3 office visits is negative and relatively large. It is possible that office visits for TCM-billing physicians are being moved up in terms of their billed complexity. Physicians may be changing the real care provided during their visits to target the higher-paying complexity levels, or they may simply be billing higher complexity levels for visits that already would have met the necessary requirements.

2.5.4 The Overall Effects of New Code Adoption on Patterns of Coding and Care Provision

In this section we describe the effects of new code adoption on overall patterns of physician billing and care provision. The associations between TCM take-up, CCM take-up, and broad categories of billing are shown in Table 2.4. TCM exhibits substantial complementarity with the overall volume of services provided by PCPs. The estimates in Table 2.4 are reinforced by the event study evidence in panel A of Figure 2.6. In total, each dollar of TCM billing predicts an additional \$5.24 of additional billing per PCP. Of this total, \$3.62 comes from Evaluation & Management services, which encompasses many of the most basic and essential primary care services. The bulk of this increase is driven by Annual Wellness Visits and office visits, as described above. The "Other" category contains codes that are not easily categorized. As shown below, this includes a complementarity between TCM services and the delivery of flu and pneumonia vaccines.

In contrast with TCM, we find that CCM billing predicts little net increase in overall billing. Like TCM billing, CCM billing predicts increases in “Other” billing, including vaccinations. Unlike TCM billing, CCM billing predicts no net increase in Evaluation & Management services. Interestingly, CCM billing predicts a modest but statistically significant decline in Imaging billing. We present event study evidence for the Evaluation & Management and Imaging categories in Figures 2.7 and 2.8. The corresponding estimates for the outcomes in this section for the unmatched sample, which exhibit less precision than do those for the matched sample, are shown in Figures 2.A.6-2.A.8.

The evidence presented thus far describes care provided by PCPs only. To assess the impacts of TCM and CCM billing on total care provision, we must also consider the care provided by physicians in other specialties. Appendix Tables 2.A.2 and 2.A.3 present the relevant evidence. Evidence on billing by non-PCP physicians reinforces key aspects of our findings on the care delivered by PCPs. Counties with high levels of TCM billing exhibit increases in the provision of care by non-PCPs, which augments the increase in care provision by PCPs. Once again, increases in Evaluation & Management services and “Other” services account for the majority of the overall increase. For CCM, we see no net increase in care provided by non-PCPs. Further, we see that declines in the utilization of Imaging services by PCPs are reinforced by declines in the utilization of Imaging services by non-PCPs.

The evidence summarized above reveals that the adoption of the TCM codes is associated with a substantial increase in overall care provision. This is consistent with the rationale for TCM services, which was to improve incentives for the coordination of post-discharge care. TCM services predict substantial increases in office visits by both PCPs and non-PCPs, suggesting systematic increases in patients’ contact with their physicians after discharge. Importantly, these increases cannot be purely a function of coding, as the billing of additional Evaluation & Management services requires additional office visits to take place. In contrast with TCM billing, CCM billing predicts essentially no change in overall care billing. We take this as suggestive that CCM billing may, in large part, involve a rationalization of the coding for services that had previously been billed as moderately less lucrative codes. Take-up of CCM codes predicts modest shifts in care provision. This includes a reduction in Imaging services and an increase in Other services, including rates of vaccination.

2.5.5 New Code Provision and Receipt of Recommended Care

In this section we discuss the effect of the new codes on distinct examples of recommended preventive care to beneficiaries. In Table 2.5 we show evidence of complementarity of TCM with billing for flu vaccinations and billing for pneumonia vaccinations. This amounts to 18 cents of flu vaccine billing per PCP and 43 cents of pneumonia vaccination billing per PCP for each dollar of TCM billed per PCP. The corresponding event study graphs are shown in Figures 2.9 and 2.10. These examples are striking for a couple of reasons. These services represent unambiguous increases in care provision in that, in contrast with shifts in the complexity of office visits, billing for vaccinations cannot plausibly arise through the reclassification of care that was already being delivered. Additionally, flu vaccination in particular is a basic primary care service that is nearly universally recommended for the elderly.

Flu vaccination billing ramps up temporally for the treatment group as TCM take-up and usage increases. For pneumonia vaccinations, there is evidence of an increase in the first two years of TCM availability. In 2015, the Advisory Committee on Immunization Practices updated its recommendations to significantly expand the portion of the elderly whom it recommends to be vaccinated against pneumonia. The resulting spike in pneumonia vaccinations in 2015 hits the treatment group to a greater extent than the control group. The difference between the groups subsides from this peak but remains present through the last year of our sample. As with our evidence on broader categories of care provision, the evidence of the effects of CCM billing is mixed and points towards low levels of care complementarity. The contrast between TCM and CCM is, once again, quite striking.

In Table 2.5 we also see evidence of complementarity between TCM and billing for mammograms. The point estimate of 2 cents of mammogram billing per PCP for each dollar of TCM billing per PCP, while small in magnitude, is precisely estimated relative to a comparatively small baseline mean. This complementarity is reflected in the event study in panel A of Figure 2.11 as well. The relationship between TCM and the primary care services mentioned in this section mesh well with the fact that TCM seems to drive higher levels of Evaluation & Management services overall. The increases in provision of these services, however, provides clear evidence that TCM causes additional real care to take place that does not simply represent a re-coding of care that was already being provided before the introduction of TCM.

2.6 Discussion and Conclusion

Maintaining an efficient health care payment system requires adapting to changes in the health care landscape. In recent years, this has required confronting the challenge of designing and managing care plans, in particular for patients with complex conditions. In this context, we analyze the U.S. Medicare program's introduction of new billing codes for the provision of Chronic Care Management and Transitional Care Management. Our analysis points to and assesses several economic margins that can complicate the jobs of insurance administrators as they design and implement such reforms.

We show why the successful implementation of basic payment reforms requires attending to a broad set of issues including informational frictions, substitution across billing codes, and complementarities in both code billing and care provision. We provide evidence that the billing codes we analyze substitute for some baseline service billing, while complementing and augmenting others. These patterns of substitution and complementarity have implications for the total cost of new code implementation as well as for the overall impact of new codes on the care received by patients. Each of these outcomes can be important for understanding the new code's financial costs and health care benefits. We show, for example, that the total care billed by PCPs rises by roughly $5 for each dollar of Transitional Care Management billed to Medicare. This additional spending comes with additional service provision including Annual Wellness Visits, additional office visits, vaccinations, and mammograms.

A complete analysis of the costs and benefits of payment reform must assess its impacts on health as well as expenditure. While we analyze the relationship between new code adoption and several indicators of "recommended" care, we do not provide a comprehensive cost-benefit analysis. A full cost-benefit analysis would require long-run evidence on patient-level outcomes, which is beyond the scope of our study.

The Chronic and Transitional Care Management codes we analyze fit into a long-running effort by the Centers for Medicare and Medicaid Services to improve the rewards for providing primary care. These codes constitute an important tool in policy makers' toolkits, namely the ability to expand the set of services that are recognized and rewarded within fee-for-service payment schedules. In addition to the issues of take-up, substitution, and complementarity that we emphasize, we conclude by highlighting longer-run margins of interest. A crucial question for the payment reforms we analyze is how they shape the overall returns to specializing in primary care. Over the long run, reforms that increase the returns to practicing in primary care will tend to achieve their objectives if they induce more medical school students to make primary care their chosen specialty.

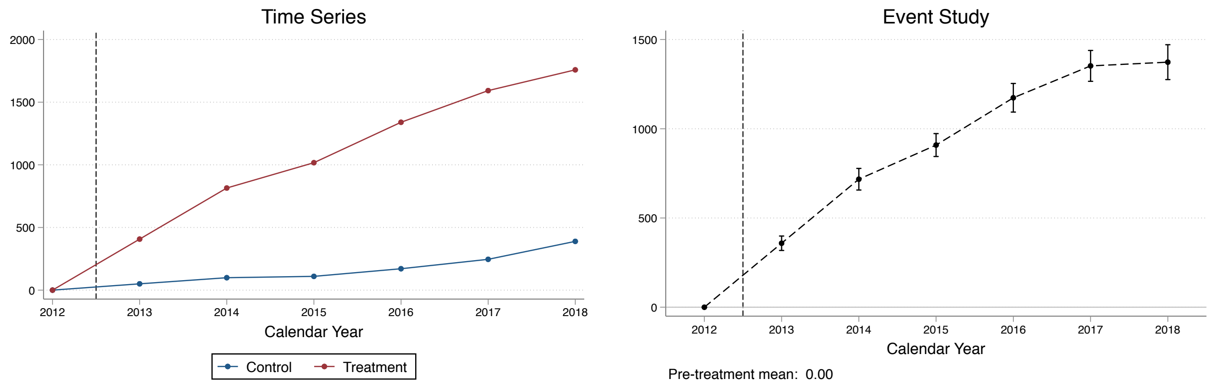
2.7 Acknowledgements

Chapter 2 contains material that is currently being prepared for submission for publication. Clemens, Jeffrey, Leganza, Jonathan M., and Masucci, Alex. "Primary Care Service

Interactions: Evidence from the Take-Up of New Medicare Billing Codes." The dissertation author was a primary investigator and an author of this material.

2.8 Figures and Tables

(a) Transitional Care Management Billing per PCP, by TCM Treatment Group



(b) Chronic Care Management Billing per PCP, by CCM Treatment Group

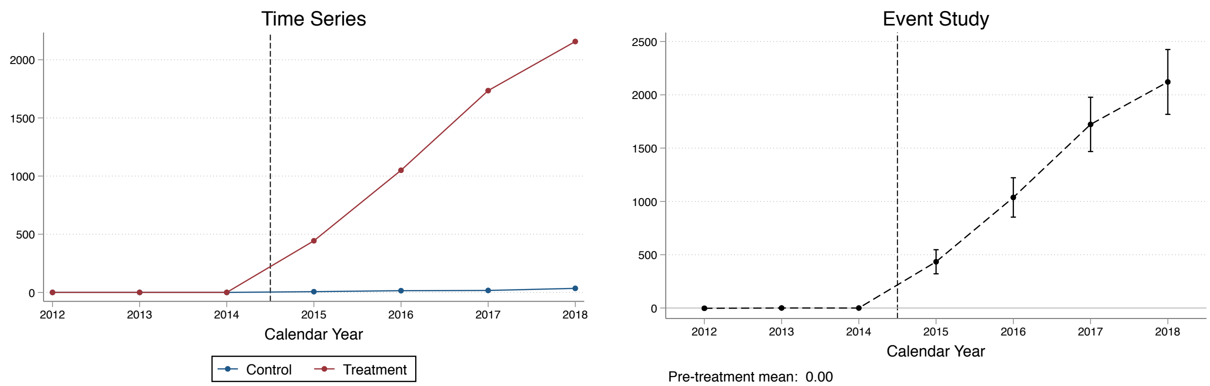
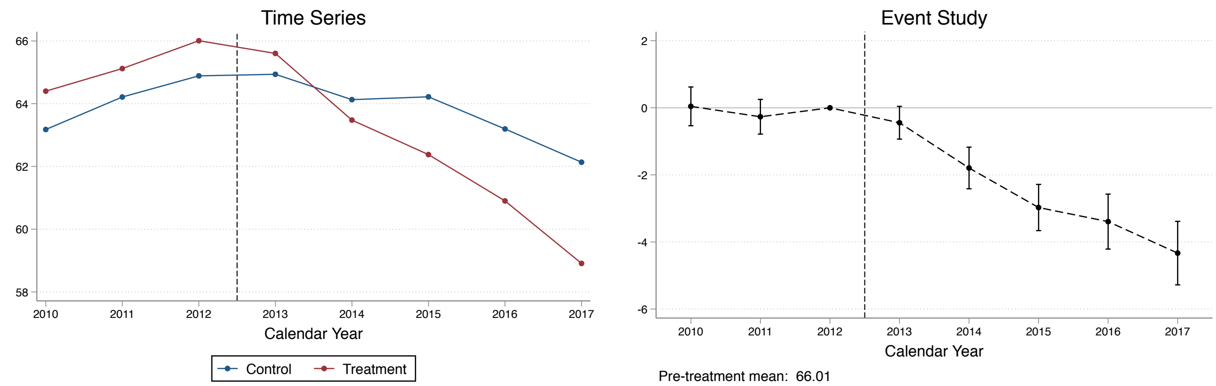


Figure 2.1: New Code Allowed Amount by Treatment Status

Notes: This figure shows estimates for $\beta_{p(t)}$ from equation (2.2). The sample in panel A is our matched sample of counties where treatment is defined by Transitional Care Management take-up, and the sample in panel B is our matched sample of counties where treatment is defined by Chronic Care Management take-up. In each panel, estimates of $\beta_{p(t)}$ are shown in the right-hand graph and the corresponding time series are shown in the left-hand graph. Data is at the county-year level. The dependent variable in panel A is the county-level allowed amount for Transitional Care Management in units of dollars billed by PCPs per PCP. The dependent variable in panel B is the county-level allowed amount for Chronic Care Management in units of dollars billed by PCPs per PCP. All regressions include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level.

(a) Fraction of Beneficiaries with a Traditional Post-Discharge Ambulatory Visit, by TCM Treatment Group



(b) Fraction of Beneficiaries with a Traditional Post-Discharge Visit with a PCP, by TCM Treatment Group

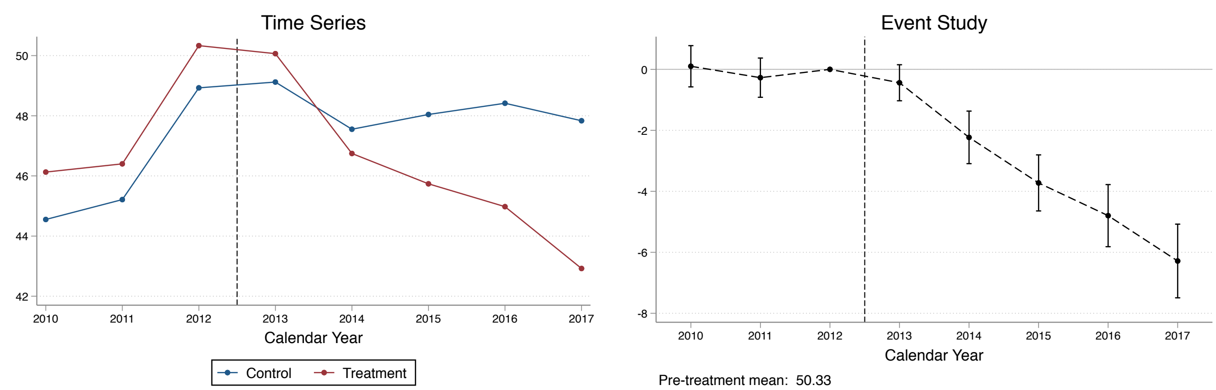
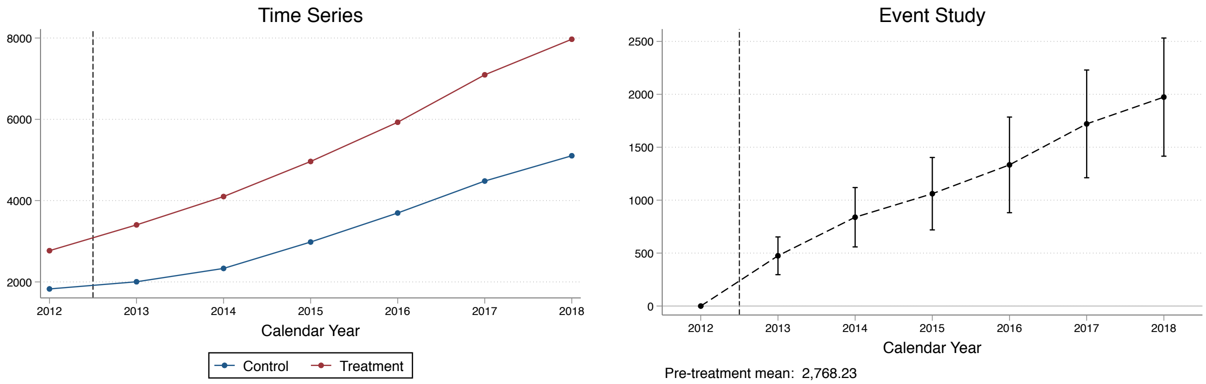


Figure 2.2: An Example of Code Substitution

Notes: This figure shows estimates for $\beta_{p(t)}$ from equation (2.2), estimated from our matched sample of counties where treatment is defined by Transitional Care Management take-up. In each panel, estimates of $\beta_{p(t)}$ are shown in the right-hand graph and the corresponding time series are shown in the left-hand graph. Data is at the county-year level. The dependent variable in panel A is the county-level fraction of beneficiaries with a traditional office visit within 14 days of a hospital discharge. The dependent variable in panel B is the county-level fraction of beneficiaries with a traditional office visit with a PCP within 14 days of a hospital discharge. Traditional office visits are defined in the data to include HCPCS codes 99201-99205, 99211-99215, 99381-99387, 99391-99397, 99241-99245, and 99271-99275. PCPs are defined in the data as any practitioners with a specialty of Internal Medicine, Family Practice, General Practice, or Geriatric Medicine. The denominator for both of these variables is the number of discharges in the given county-year. All regressions include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level.

(a) Annual Wellness Visit Billing per PCP, by TCM Treatment Group



(b) Annual Wellness Visit Billing per PCP, by CCM Treatment Group

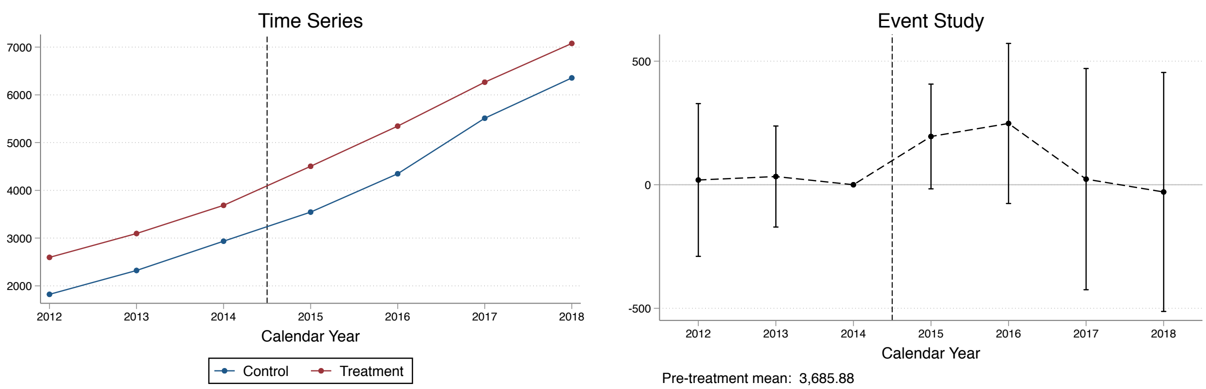
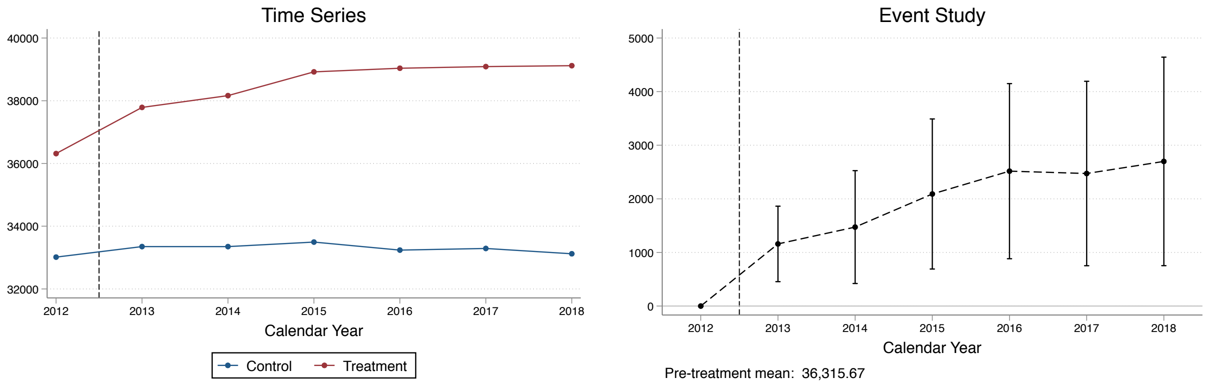


Figure 2.3: An Example of Complementarity with Another Recently Introduced Code

Notes: This figure shows estimates for $\beta_{p(t)}$ from equation (2.2). The sample in panel A is our matched sample of counties where treatment is defined by Transitional Care Management take-up, and the sample in panel B is our matched sample of counties where treatment is defined by Chronic Care Management take-up. In each panel, estimates of $\beta_{p(t)}$ are shown in the right-hand graph and the corresponding time series are shown in the left-hand graph. Data is at the county-year level. The dependent variable is the county-level allowed amount for Annual Wellness Visits in units of dollars billed by PCPs per PCP. All regressions include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level.

(a) Complexity Level 4 Office Visit Billing per PCP, by TCM Treatment Group



(b) Complexity Level 4 Office Visit Billing per PCP, by CCM Treatment Group

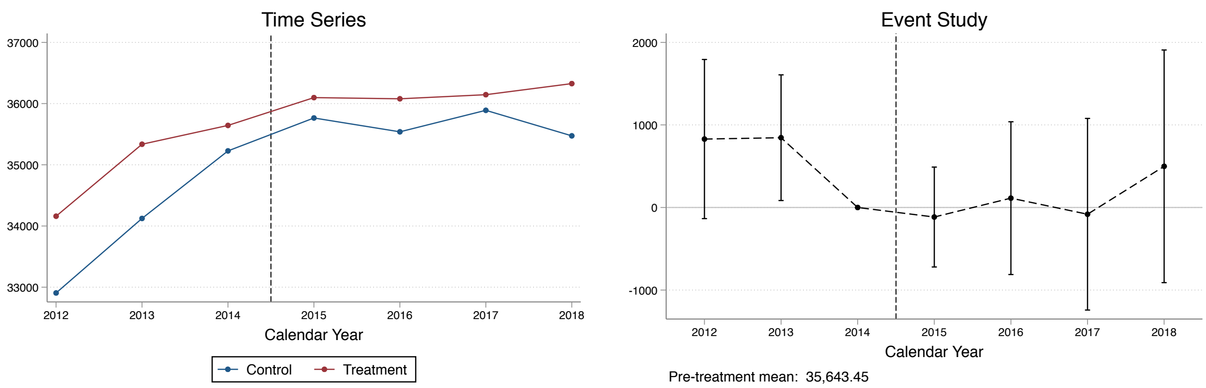
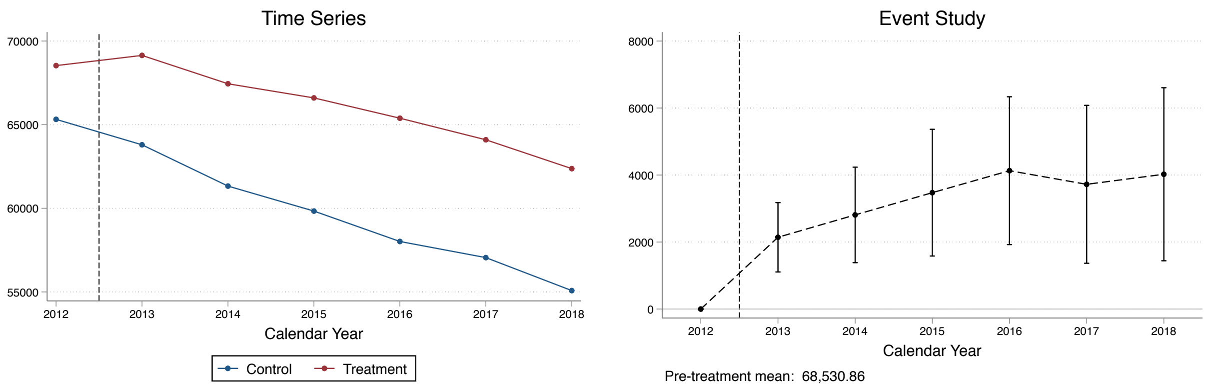


Figure 2.4: An Example of Complementarity with a Traditional Primary Care Code (Complexity Level 4 Office Visit Billing)

Notes: This figure shows estimates for $\beta_{p(t)}$ from equation (2.2). The sample in panel A is our matched sample of counties where treatment is defined by Transitional Care Management take-up, and the sample in panel B is our matched sample of counties where treatment is defined by Chronic Care Management take-up. In each panel, estimates of $\beta_{p(t)}$ are shown in the right-hand graph and the corresponding time series are shown in the left-hand graph. Data is at the county-year level. The dependent variable is the county-level allowed amount for complexity level 4 office visits in units of dollars billed by PCPs per PCP. All regressions include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level.

(a) Total Office Visit Billing per PCP, by TCM Treatment Group



(b) Total Office Visit Billing per PCP, by CCM Treatment Group

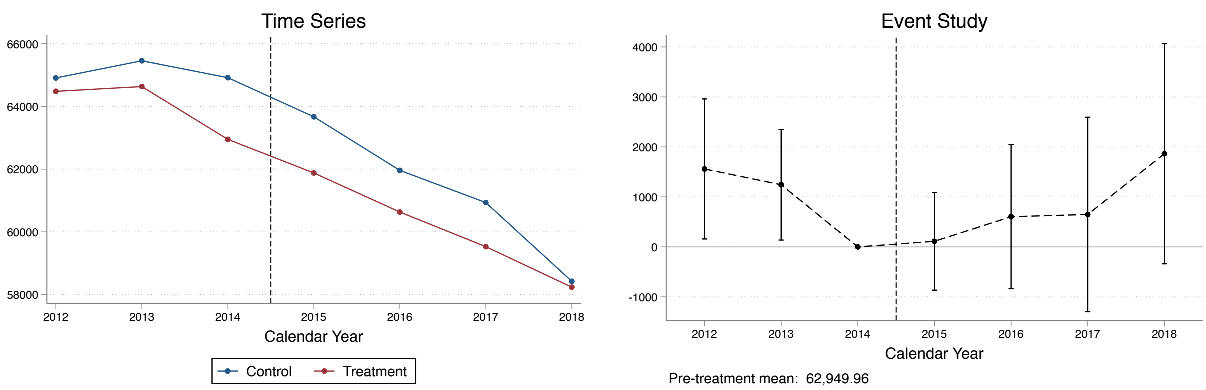
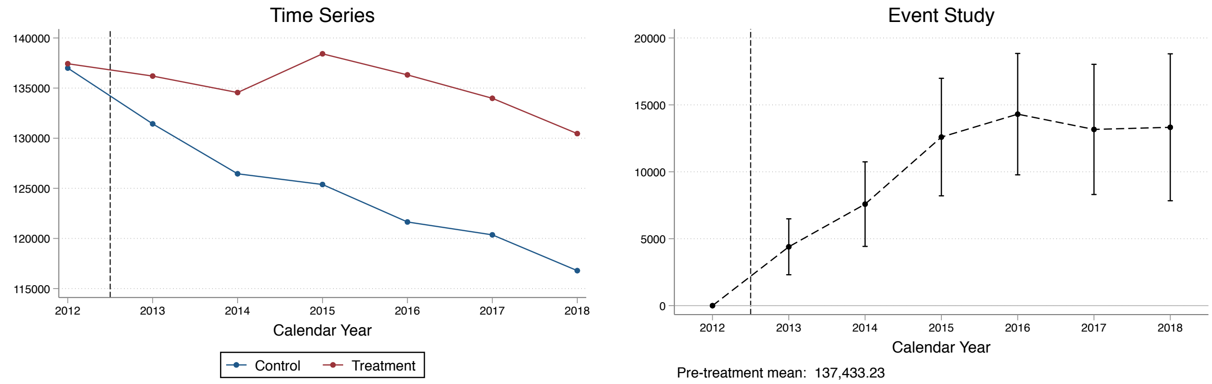


Figure 2.5: An Example of Complementarity with a Traditional Primary Care Code (Total Office Visit Billing)

Notes: This figure shows estimates for $\beta_{p(t)}$ from equation (2.2). The sample in panel A is our matched sample of counties where treatment is defined by Transitional Care Management take-up, and the sample in panel B is our matched sample of counties where treatment is defined by Chronic Care Management take-up. In each panel, estimates of $\beta_{p(t)}$ are shown in the right-hand graph and the corresponding time series are shown in the left-hand graph. Data is at the county-year level. The dependent variable is the county-level allowed amount for office visits in units of dollars billed by PCPs per PCP. All regressions include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level.

(a) Total Billing per PCP, by TCM Treatment Group



(b) Total Billing per PCP, by CCM Treatment Group

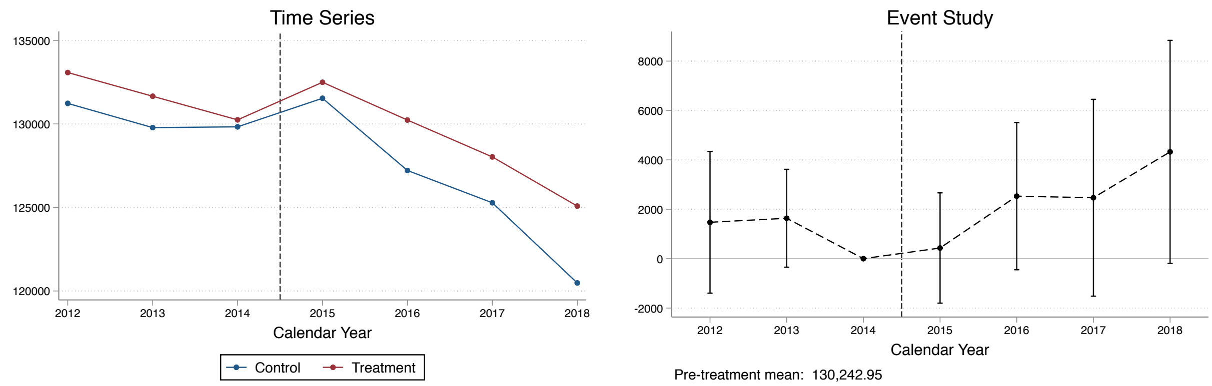
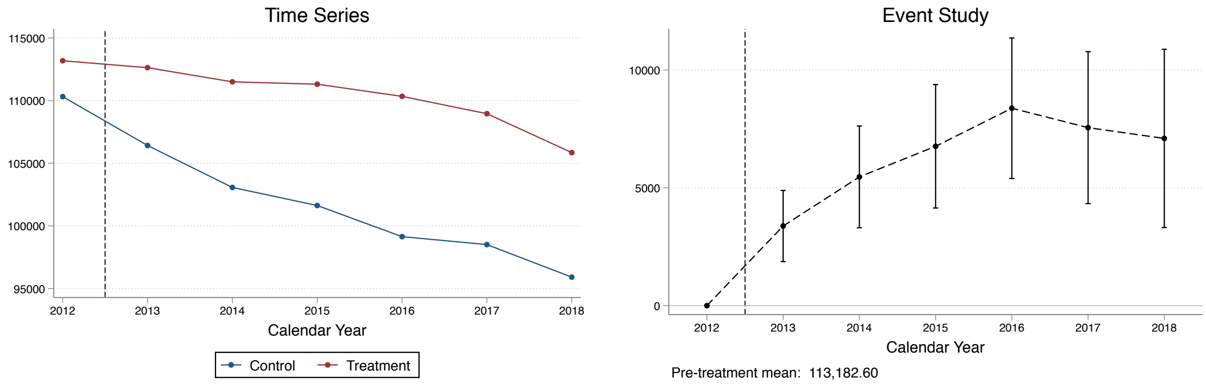


Figure 2.6: Total Billing

Notes: This figure shows estimates for $\beta_{p(t)}$ from equation (2.2). The sample in panel A is our matched sample of counties where treatment is defined by Transitional Care Management take-up, and the sample in panel B is our matched sample of counties where treatment is defined by Chronic Care Management take-up. In each panel, estimates of $\beta_{p(t)}$ are shown in the right-hand graph and the corresponding time series are shown in the left-hand graph. Data is at the county-year level. The dependent variable is the county-level total allowed amount in units of dollars billed by PCPs per PCP. All regressions include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level.

(a) Evaluation & Management Billing per PCP, by TCM Treatment Group



(b) Evaluation & Management Billing per PCP, by CCM Treatment Group

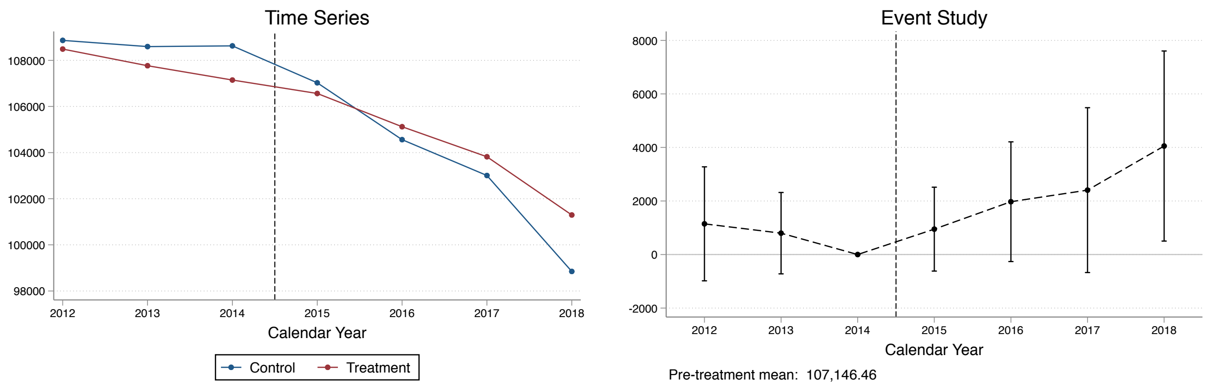
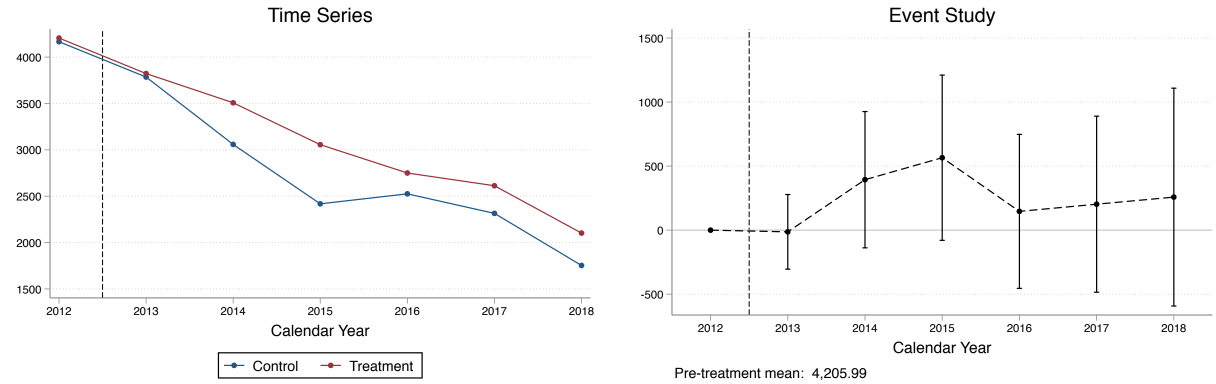


Figure 2.7: Evaluation & Management Billing

Notes: This figure shows estimates for $\beta_{p(t)}$ from equation (2.2). The sample in panel A is our matched sample of counties where treatment is defined by Transitional Care Management take-up, and the sample in panel B is our matched sample of counties where treatment is defined by Chronic Care Management take-up. In each panel, estimates of $\beta_{p(t)}$ are shown in the right-hand graph and the corresponding time series are shown in the left-hand graph. Data is at the county-year level. The dependent variable is the county-level allowed amount for Evaluation & Management services in units of dollars billed by PCPs per PCP. All regressions include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level.

(a) Imaging Billing per PCP, by TCM Treatment Group



(b) Imaging Billing per PCP, by CCM Treatment Group

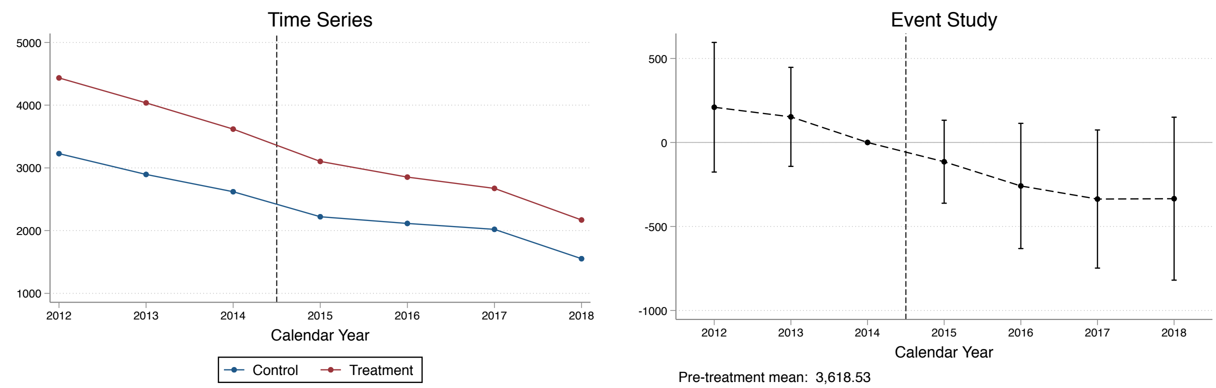
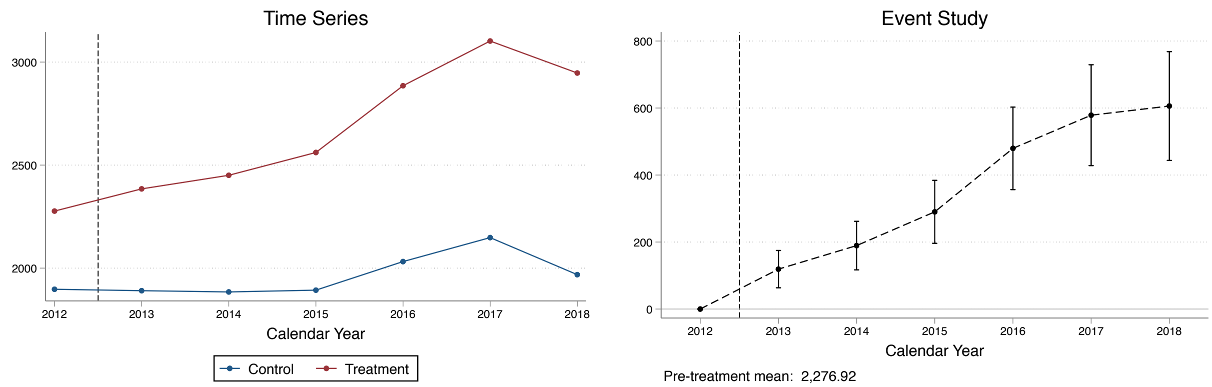


Figure 2.8: Imaging Billing

Notes: This figure shows estimates for $\beta_{p(t)}$ from equation (2.2). The sample in panel A is our matched sample of counties where treatment is defined by Transitional Care Management take-up, and the sample in panel B is our matched sample of counties where treatment is defined by Chronic Care Management take-up. In each panel, estimates of $\beta_{p(t)}$ are shown in the right-hand graph and the corresponding time series are shown in the left-hand graph. Data is at the county-year level. The dependent variable is the county-level allowed amount for Imaging services in units of dollars billed by PCPs per PCP. All regressions include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level.

(a) Influenza Vaccination Billing per PCP, by TCM Treatment Group



(b) Influenza Vaccination Billing per PCP, by CCM Treatment Group

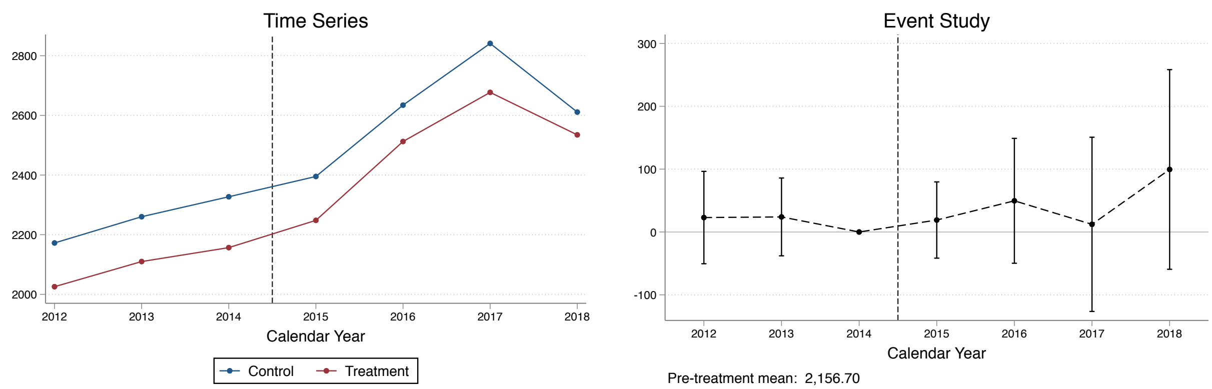
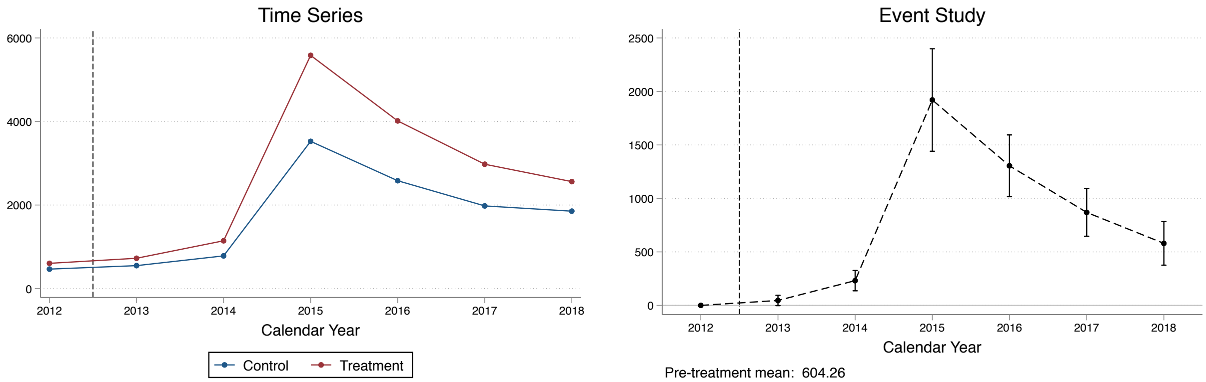


Figure 2.9: An Example of Complementarity with Recommended Primary Care Services: Influenza Vaccination

Notes: This figure shows estimates for $\beta_{p(t)}$ from equation (2.2). The sample in panel A is our matched sample of counties where treatment is defined by Transitional Care Management take-up, and the sample in panel B is our matched sample of counties where treatment is defined by Chronic Care Management take-up. In each panel, estimates of $\beta_{p(t)}$ are shown in the right-hand graph and the corresponding time series are shown in the left-hand graph. Data is at the county-year level. The dependent variable is the county-level allowed amount for influenza vaccinations in units of dollars billed by PCPs per PCP. All regressions include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level.

(a) Pneumonia Vaccination Billing per PCP, by TCM Treatment Group



(b) Pneumonia Vaccination Billing per PCP, by CCM Treatment Group

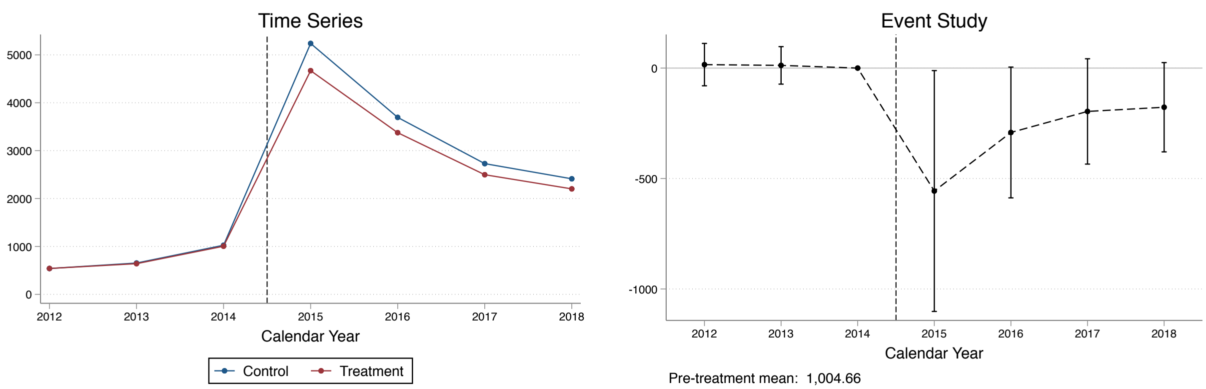
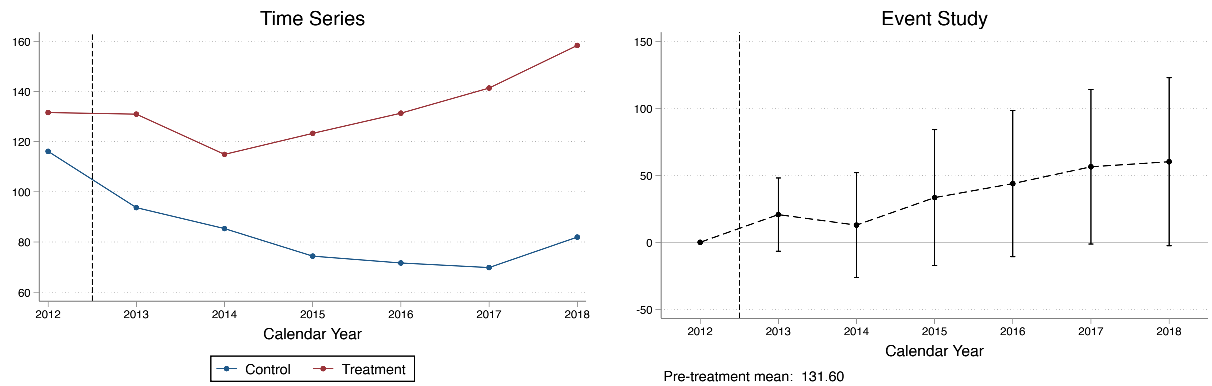


Figure 2.10: An Example of Complementarity with Recommended Primary Care Services: Pneumonia Vaccination

Notes: This figure shows estimates for $\beta_{p(t)}$ from equation (2.2). The sample in panel A is our matched sample of counties where treatment is defined by Transitional Care Management take-up, and the sample in panel B is our matched sample of counties where treatment is defined by Chronic Care Management take-up. In each panel, estimates of $\beta_{p(t)}$) are shown in the right-hand graph and the corresponding time series are shown in the left-hand graph. Data is at the county-year level. The dependent variable is the county-level allowed amount for pneumonia vaccinations in units of dollars billed by PCPs per PCP. All regressions include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level.

(a) Mammogram Billing per PCP, by TCM Treatment Group



(b) Mammogram Billing per PCP, by CCM Treatment Group

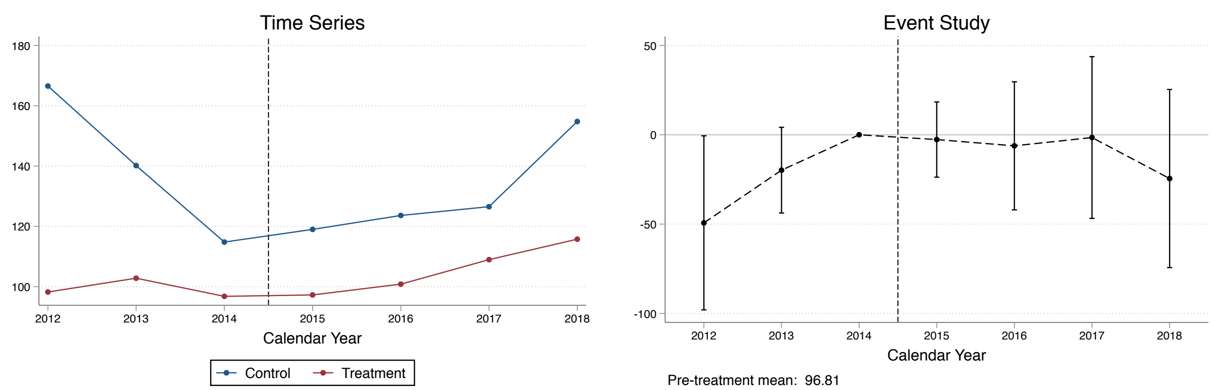


Figure 2.11: An Example of Complementarity with Recommended Primary Care Services: Mammograms

Notes: This figure shows estimates for $\beta_{p(t)}$ from equation (2.2). The sample in panel A is our matched sample of counties where treatment is defined by Transitional Care Management take-up, and the sample in panel B is our matched sample of counties where treatment is defined by Chronic Care Management take-up. In each panel, estimates of $\beta_{p(t)}$ are shown in the right-hand graph and the corresponding time series are shown in the left-hand graph. Data is at the county-year level. The dependent variable is the county-level allowed amount for mammograms in units of dollars billed by PCPs per PCP. All regressions include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level.

Table 2.1: Post-Discharge Outcomes

New Code	Fraction of Beneficiaries with a Traditional Post-Discharge Visit	Fraction of Beneficiaries with a Traditional Post-Discharge Visit with a PCP	Controls
Transitional Care Management (Thousands of $ per PCP)			
	-1.15***	-1.87***	No
	(.15)	(.19)	
	-1.21***	-1.93***	Yes
	(.15)	(.20)	
Dependent Mean	63.5	48.1	
N	16,760	16,051	

Notes: This table shows estimates for β_1 from equation (2.1). Data is at the county-year level and these data span the years 2012-2017. The independent variable is the county-level allowed amount for Transitional Care Management in units of thousands of dollars billed by PCPs per PCP. Dependent variables are county-level rates where the denominator is the annual number of discharged patients. All regressions include year-level and county-level fixed effects. Regressions with controls include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level.

Table 2.2: Annual Wellness Visits

New Code	Annual Wellness Visit Billing per PCP	Controls
Transitional Care Management ($ per PCP)		
	1.30^{***}	No
	(.11)	
	1.28^{***}	Yes
	(.11)	
Chronic Care Management ($ per PCP)		
	$.14^{**}$	No
	(.06)	
	$.13^{**}$	Yes
	(.06)	
Dependent Mean	3,051.80	
N	20,262	

Notes: This table shows estimates for β_1 from equation (2.1). Data is at the county-year level and these data span the years 2012-2018. The independent variable is the county-level allowed amount for Transitional Care Management in units of dollars billed by PCPs per PCP. Similarly, the dependent variable is the county-level allowed amount for Annual Wellness Visits billed by PCPs per PCP. All regressions include year-level and county-level fixed effects. Regressions with controls include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level.

Table 2.3: Office Visits by Complexity

New Code	Total Office Visit Billing per PCP	Level 1 Office Visit Billing per PCP	Level 2 Office Visit Billing per PCP	Level 3 Office Visit Billing per PCP	Level 4 Office Visit Billing per PCP	Level 5 Office Visit Billing per PCP	Controls
Transitional Care Management ($ per PCP)							
	2.02***	-.03**	-.03*	-.51	2.40***	.18***	No
	(.61)	(.01)	(.02)	(.37)	(.40)	(.07)	
	2.05***	-.03**	-.02	-.45	2.35***	.19***	Yes
	(.62)	(.01)	(.02)	(.37)	(.40)	(.07)	
Chronic Care Management ($ per PCP)							
	.07	.01	-.01	-.06*	.15	-.02	No
	(.12)	(.01)	(.004)	(.04)	(.09)	(.01)	
	.08	.01	-.003	-.04	.13	-.01	Yes
	(.12)	(.01)	(.004)	(.04)	(.09)	(.01)	
Dependent Mean	48,611.14	327.64	1,008.83	18,486.90	26,401.64	2,386.13	
N	20,262	20,262	20,262	20,262	20,262	20,262	

Notes: This table shows estimates for β_1 from equation (2.1). Data is at the county-year level and these data span the years 2012-2018. The independent variable is the county-level allowed amount for Transitional Care Management in units of dollars billed by PCPs per PCP. Similarly, the dependent variable is the county-level allowed amount for the specified outcome billed by PCPs per PCP. All regressions include year-level and county-level fixed effects. Regressions with controls include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level.

Table 2.4: PCP Billing by Category

New Code	Total Billing per PCP	Evaluation & Management Billing per PCP	Procedures Billing per PCP	Imaging Billing per PCP	Tests Billing per PCP	Durable Medical Equipment Billing per PCP	Other Billing per PCP	Controls
Transitional Care Management ($ per PCP)								
	5.24***	3.68***	.18	-.13	.13	.01	1.44***	No
	(1.20)	(.82)	(.12)	(.21)	(.11)	(.03)	(.16)	
	5.24***	3.62***	.17	-.11	.13	.01	1.42***	Yes
	(1.21)	(.83)	(.12)	(.21)	(.11)	(.02)	(.16)	
Chronic Care Management ($ per PCP)								
	.27	.21	-.01	-.09**	-.03	.002	.19**	No
	(.29)	(.20)	(.02)	(.04)	(.05)	(.006)	(.08)	
	.27	.21	-.01	-.09**	-.03	.003	.19**	Yes
	(.29)	(.21)	(.02)	(.04)	(.05)	(.006)	(.08)	
Dependent Mean	99,675.41	82,169.14	3,706.79	1,997.22	5,456.28	40.30	6,305.68	
N	20,262	20,262	20,262	20,262	20,262	20,262	20,262	

Notes: This table shows estimates for β_1 from equation (2.1). Data is at the county-year level and these data span the years 2012-2018. The independent variable is the county-level allowed amount for Transitional Care Management in units of dollars billed by PCPs per PCP. Similarly, the dependent variable is the county-level allowed amount for the specified outcome billed by PCPs per PCP. All regressions include year-level and county-level fixed effects. Regressions with controls include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level. TCM and CCM belong to the Evaluation & Management category, but the new code of interest for each regression is excluded from this category as well as from Total billing. The dependent means for the first two columns are the average of the means that result from dropping each new code, which are approximately the same.

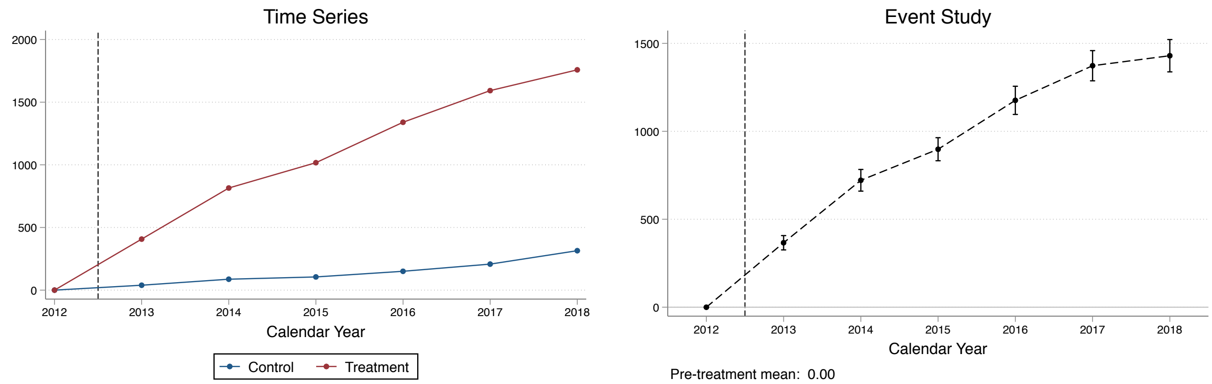
Table 2.5: Recommended Care

New Code	Influenza Vaccination Billing per PCP	Pneumonia Vaccination Billing per PCP	Mammogram Billing per PCP	Controls
Transitional Care Management ($ per PCP)				
	.18***	.43***	.02**	No
	(.03)	(.06)	(.008)	
	.18***	.43***	.02**	Yes
	(.03)	(.06)	(.008)	
Chronic Care Management ($ per PCP)				
	.01	.04***	-.003	No
	(.01)	(.01)	(.004)	
	.01	.03***	-.003	Yes
Dependent Mean	1,810.14	1,544.92	83.22	
N	20,262	20,262	20,262	

Notes: This table shows estimates for β_1 from equation (2.1). Data is at the county-year level and these data span the years 2012-2018. The independent variable is the county-level allowed amount for Transitional Care Management in units of dollars billed by PCPs per PCP. Similarly, the dependent variable is the county-level allowed amount for the specified outcome billed by PCPs per PCP. All regressions include year-level and county-level fixed effects. Regressions with controls include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level.

2.A Appendix: Additional Figures and Tables

(a) Transitional Care Management Billing per PCP, by TCM Treatment Group



(b) Chronic Care Management Billing per PCP, by CCM Treatment Group

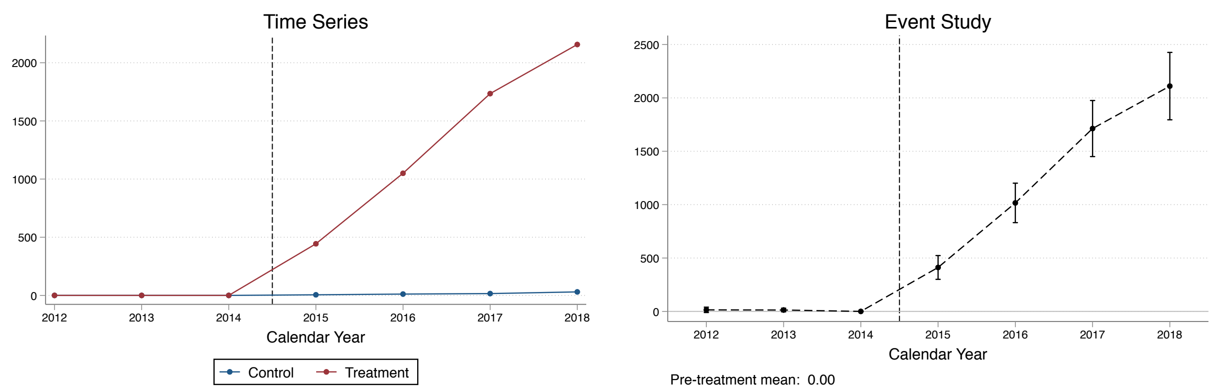
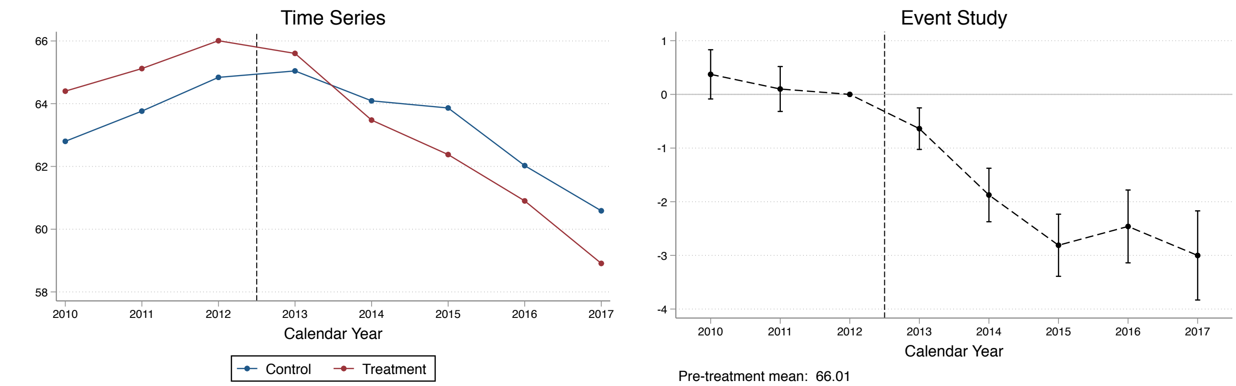


Figure 2.A.1: New Code Allowed Amount by Treatment Status — Unmatched Sample

Notes: This figure shows estimates for $\beta_{p(t)}$ from equation (2.2). The sample in panel A is our unmatched sample of counties where treatment is defined by Transitional Care Management take-up, and the sample in panel B is our unmatched sample of counties where treatment is defined by Chronic Care Management take-up. In each panel, estimates of $\beta_{p(t)}$ are shown in the right-hand graph and the corresponding time series are shown in the left-hand graph. Data is at the county-year level. The dependent variable in panel A is the county-level allowed amount for Transitional Care Management in units of dollars billed by PCPs per PCP. The dependent variable in panel B is the county-level allowed amount for Chronic Care Management in units of dollars billed by PCPs per PCP. All regressions include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level.

(a) Fraction of Beneficiaries with a Traditional Post-Discharge Ambulatory Visit, by TCM Treatment Group



(b) Fraction of Beneficiaries with a Traditional Post-Discharge Visit with a PCP, by TCM Treatment Group

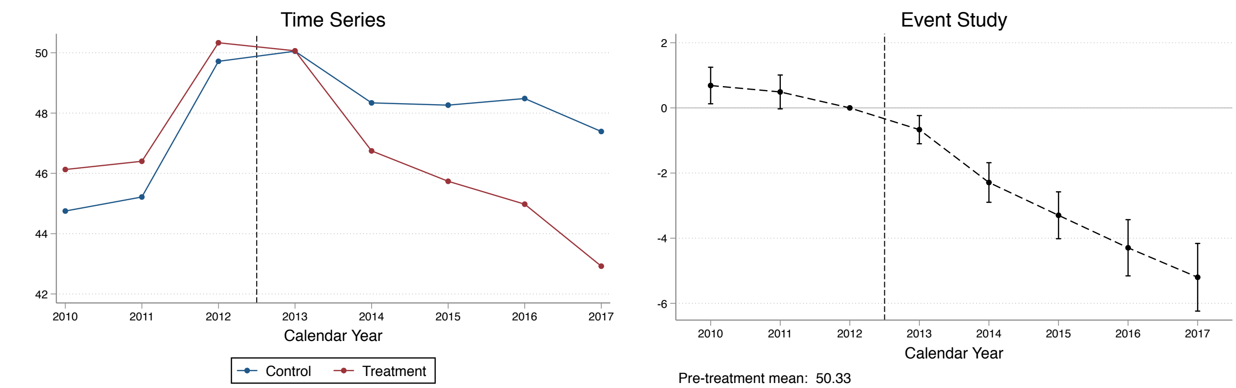
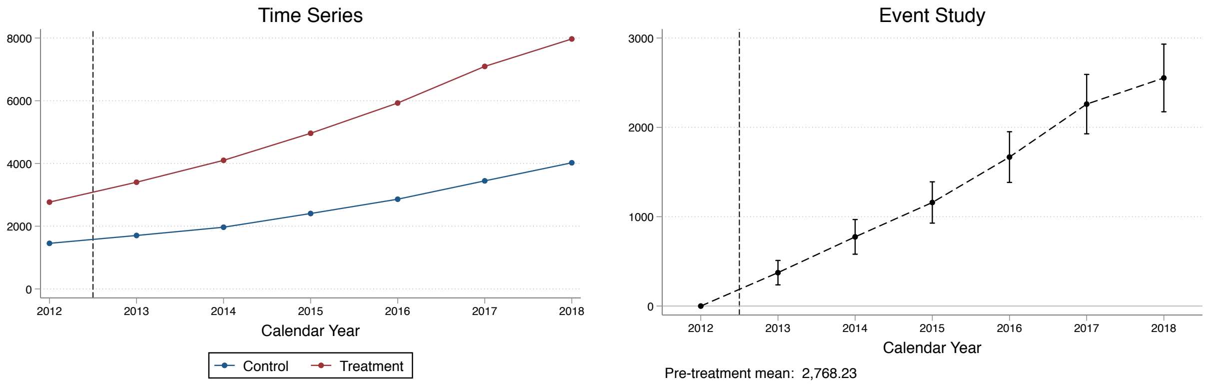


Figure 2.A.2: An Example of Code Substitution — Unmatched Sample

Notes: This figure shows estimates for $\beta_{p(t)}$ from equation (2.2), estimated from our unmatched sample of counties where treatment is defined by Transitional Care Management take-up. In each panel, estimates of $\beta_{p(t)}$ are shown in the right-hand graph and the corresponding time series are shown in the left-hand graph. Data is at the county-year level. The dependent variable in panel A is the county-level fraction of beneficiaries with a traditional office visit within 14 days of a hospital discharge. The dependent variable in panel B is the county-level fraction of beneficiaries with a traditional office visit with a PCP within 14 days of a hospital discharge. Traditional office visits are defined in the data to include HCPCS codes 99201-99205, 99211-99215, 99381-99387, 99391-99397, 99241-99245, and 99271-99275. PCPs are defined in the data as any practitioners with a specialty of Internal Medicine, Family Practice, General Practice, or Geriatric Medicine. The denominator for both of these variables is the number of discharges in the given county-year. All regressions include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level.

(a) Annual Wellness Visit Billing per PCP, by TCM Treatment Group



(b) Annual Wellness Visit Billing per PCP, by CCM Treatment Group

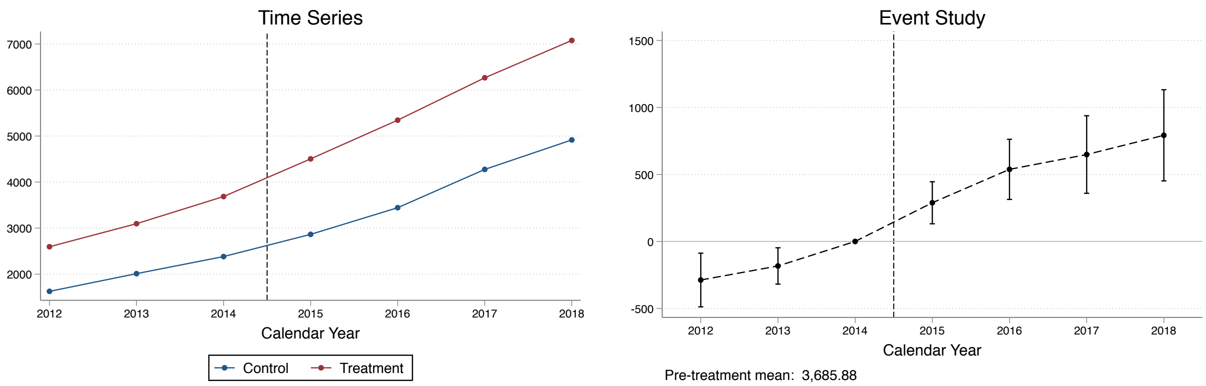
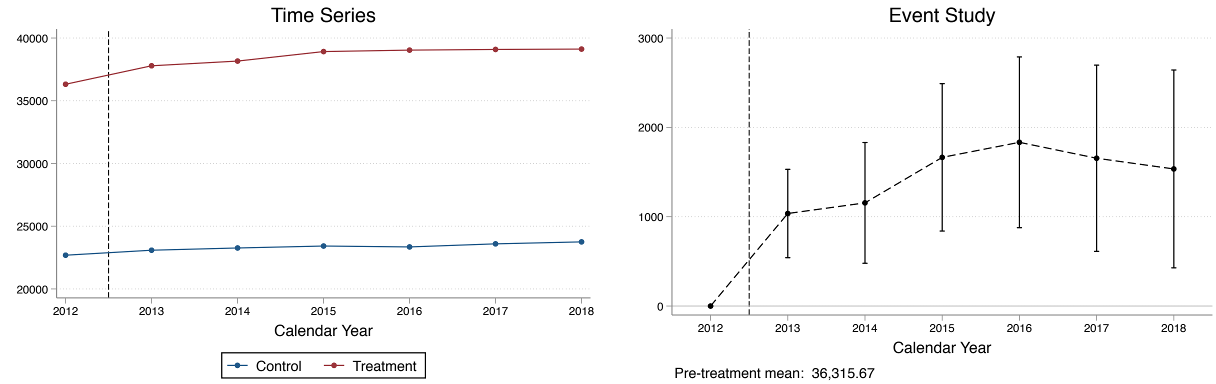


Figure 2.A.3: An Example of Complementarity with Another Recently Introduced Code — Unmatched Sample

Notes: This figure shows estimates for $\beta_{p(t)}$ from equation (2.2). The sample in panel A is our unmatched sample of counties where treatment is defined by Transitional Care Management take-up, and the sample in panel B is our unmatched sample of counties where treatment is defined by Chronic Care Management take-up. In each panel, estimates of $\beta_{p(t)}$ are shown in the right-hand graph and the corresponding time series are shown in the left-hand graph. Data is at the county-year level. The dependent variable is the county-level allowed amount for Annual Wellness Visits in units of dollars billed by PCPs per PCP. All regressions include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level.

(a) Complexity Level 4 Office Visit Billing per PCP, by TCM Treatment Group



(b) Complexity Level 4 Office Visit Billing per PCP, by CCM Treatment Group

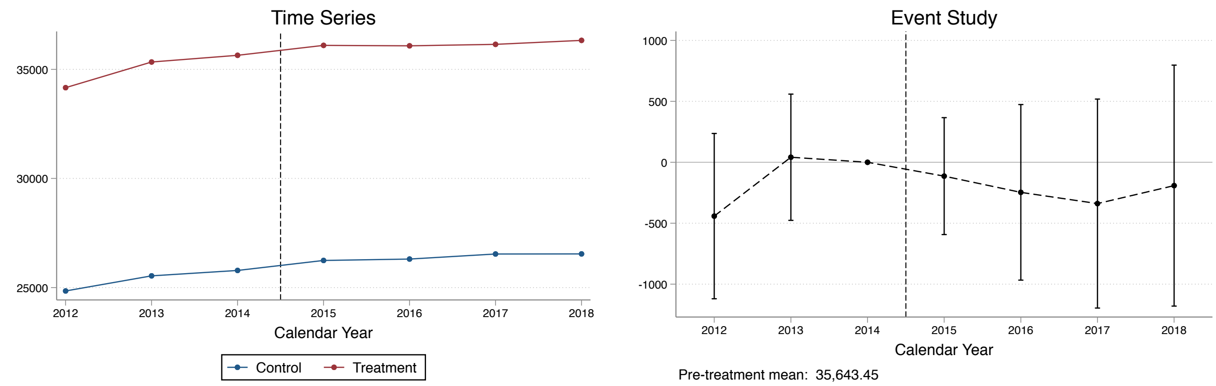
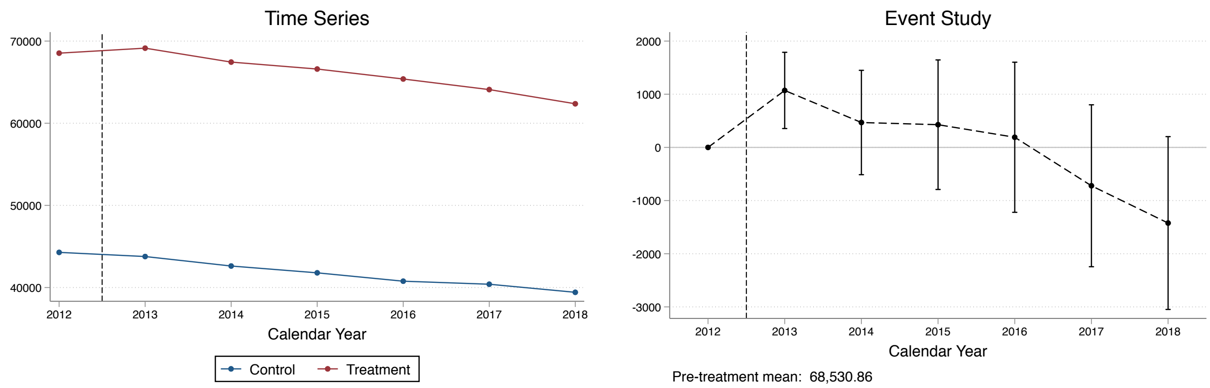


Figure 2.A.4: An Example of Complementarity with a Traditional Primary Care Code — Unmatched Sample (Complexity Level 4 Office Visit Billing)

Notes: This figure shows estimates for $\beta_{p(t)}$ from equation (2.2). The sample in panel A is our unmatched sample of counties where treatment is defined by Transitional Care Management take-up, and the sample in panel B is our unmatched sample of counties where treatment is defined by Chronic Care Management take-up. In each panel, estimates of $\beta_{p(t)}$ are shown in the right-hand graph and the corresponding time series are shown in the left-hand graph. Data is at the county-year level. The dependent variable is the county-level allowed amount for complexity level 4 office visits in units of dollars billed by PCPs per PCP. All regressions include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level.

(a) Total Office Visit Billing per PCP, by TCM Treatment Group



(b) Total Office Visit Billing per PCP, by CCM Treatment Group

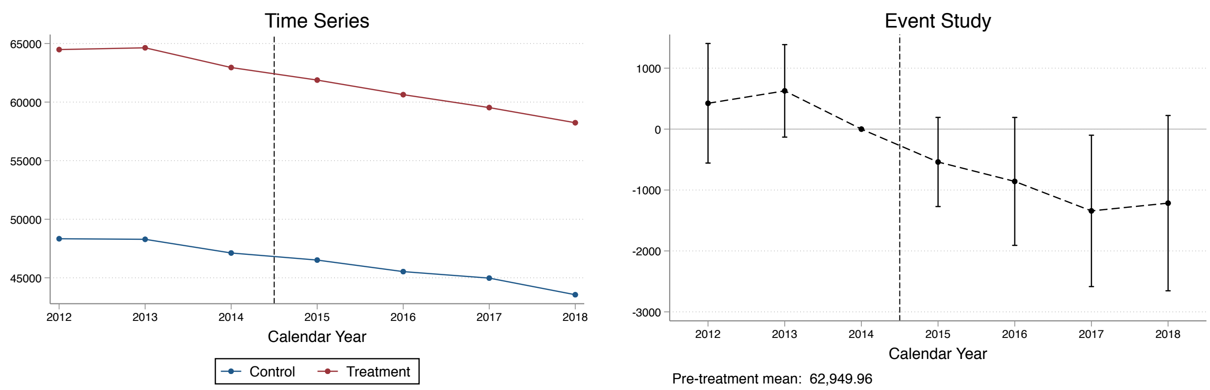
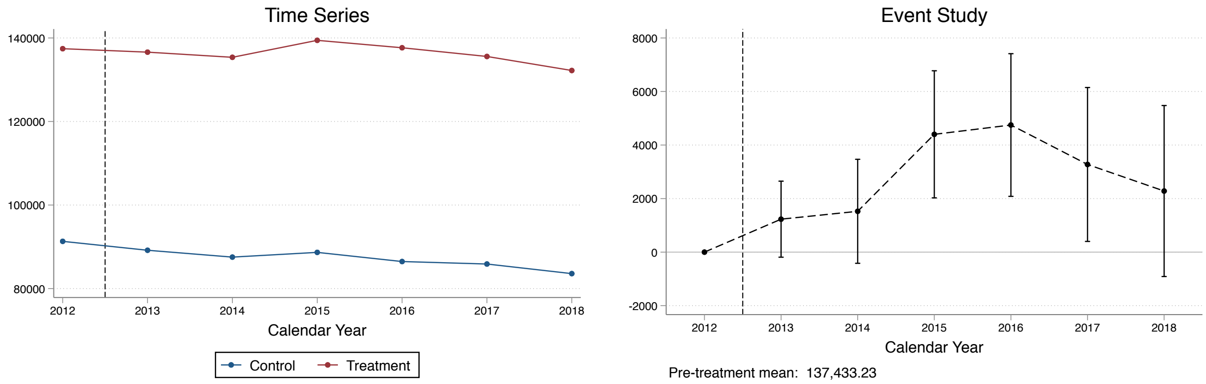


Figure 2.A.5: An Example of Complementarity with a Traditional Primary Care Code — Unmatched Sample (Total Office Visit Billing)

Notes: This figure shows estimates for $\beta_{p(t)}$ from equation (2.2). The sample in panel A is our unmatched sample of counties where treatment is defined by Transitional Care Management take-up, and the sample in panel B is our unmatched sample of counties where treatment is defined by Chronic Care Management take-up. In each panel, estimates of $\beta_{p(t)}$ are shown in the right-hand graph and the corresponding time series are shown in the left-hand graph. Data is at the county-year level. The dependent variable is the county-level allowed amount for office visits in units of dollars billed by PCPs per PCP. All regressions include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level.

(a) Total Billing per PCP, by TCM Treatment Group



(b) Total Billing per PCP, by CCM Treatment Group

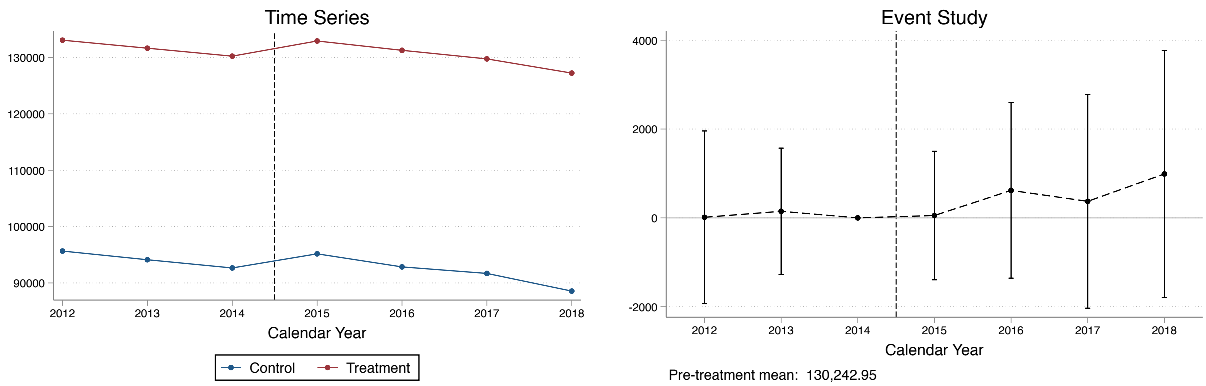
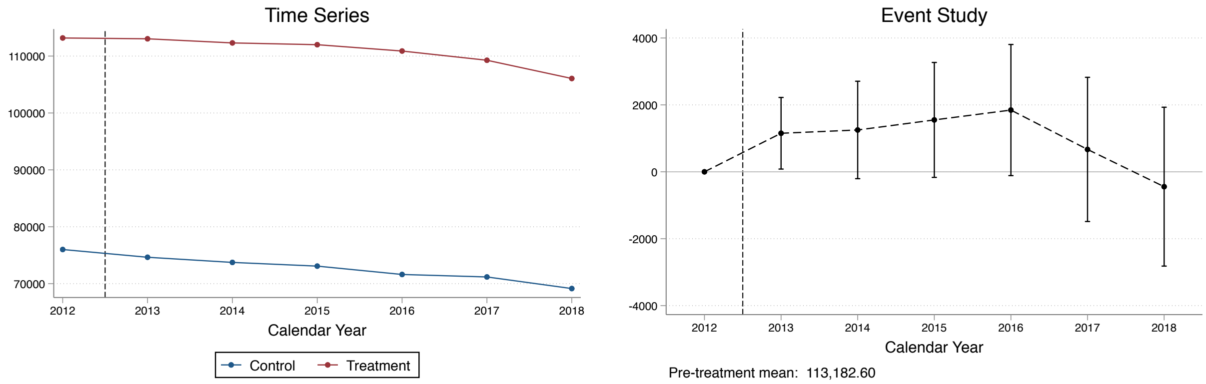


Figure 2.A.6: Total Billing — Unmatched Sample

Notes: This figure shows estimates for $\beta_{p(t)}$ from equation (2.2). The sample in panel A is our unmatched sample of counties where treatment is defined by Transitional Care Management take-up, and the sample in panel B is our unmatched sample of counties where treatment is defined by Chronic Care Management take-up. In each panel, estimates of $\beta_{p(t)}$ are shown in the right-hand graph and the corresponding time series are shown in the left-hand graph. Data is at the county-year level. The dependent variable is the county-level total allowed amount in units of dollars billed by PCPs per PCP. All regressions include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level.

(a) Evaluation & Management Billing per PCP, by TCM Treatment Group



(b) Evaluation & Management Billing per PCP, by CCM Treatment Group

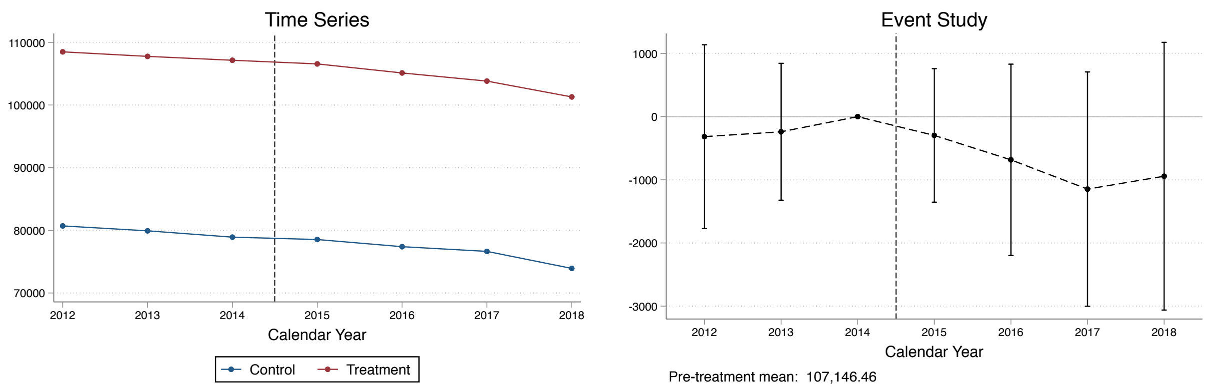
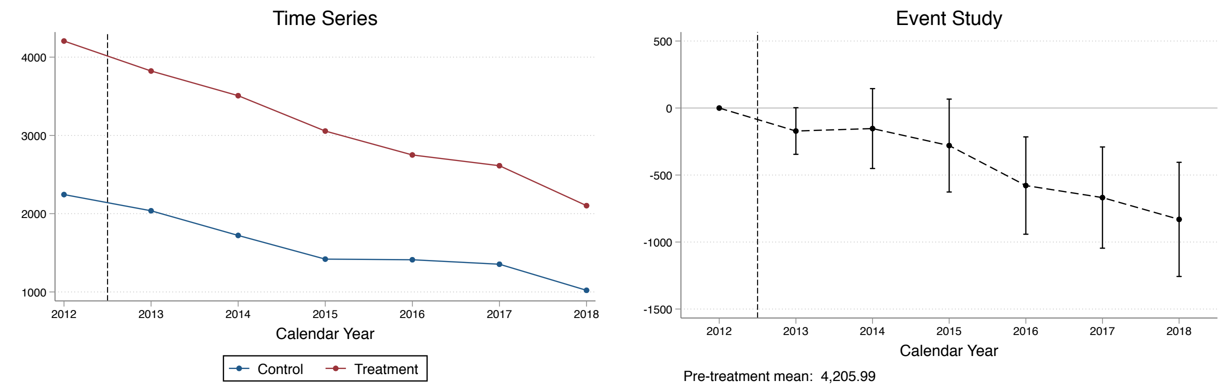


Figure 2.A.7: Evaluation & Management Billing — Unmatched Sample

Notes: This figure shows estimates for $\beta_{p(t)}$ from equation (2.2). The sample in panel A is our unmatched sample of counties where treatment is defined by Transitional Care Management take-up, and the sample in panel B is our unmatched sample of counties where treatment is defined by Chronic Care Management take-up. In each panel, estimates of $\beta_{p(t)}$ are shown in the right-hand graph and the corresponding time series are shown in the left-hand graph. Data is at the county-year level. The dependent variable is the county-level allowed amount for Evaluation & Management services in units of dollars billed by PCPs per PCP. All regressions include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level.

(a) Imaging Billing per PCP, by TCM Treatment Group



(b) Imaging Billing per PCP, by CCM Treatment Group

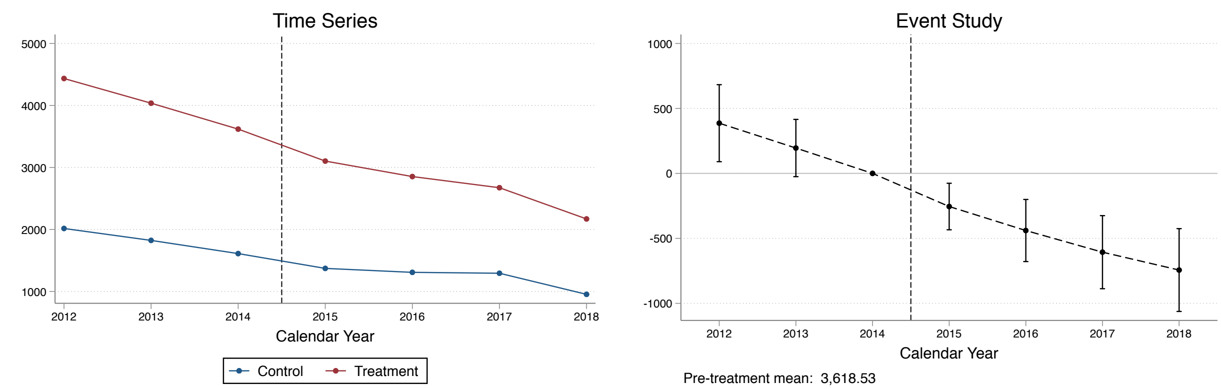
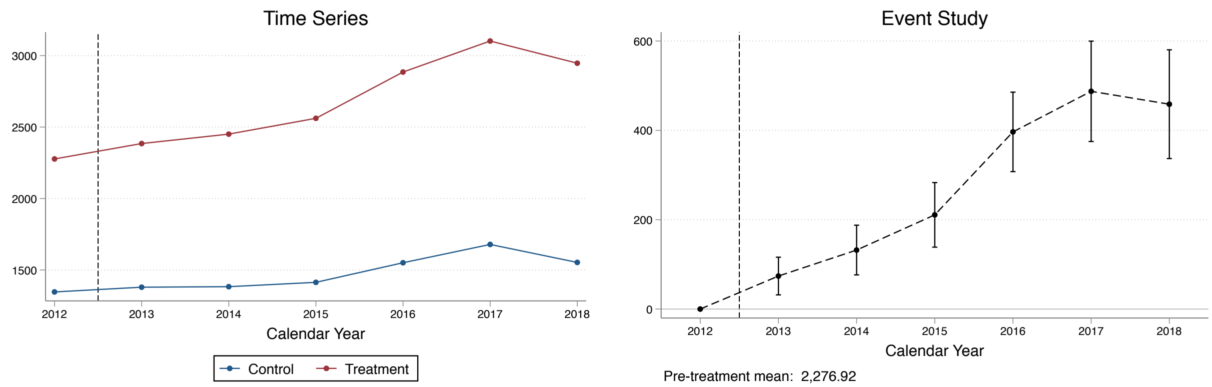


Figure 2.A.8: Imaging Billing — Unmatched Sample

Notes: This figure shows estimates for $\beta_{p(t)}$ from equation (2.2). The sample in panel A is our unmatched sample of counties where treatment is defined by Transitional Care Management take-up, and the sample in panel B is our unmatched sample of counties where treatment is defined by Chronic Care Management take-up. In each panel, estimates of $\beta_{p(t)}$ are shown in the right-hand graph and the corresponding time series are shown in the left-hand graph. Data is at the county-year level. The dependent variable is the county-level allowed amount for Imaging services in units of dollars billed by PCPs per PCP. All regressions include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level.

(a) Influenza Vaccination Billing per PCP, by TCM Treatment Group



(b) Influenza Vaccination Billing per PCP, by CCM Treatment Group

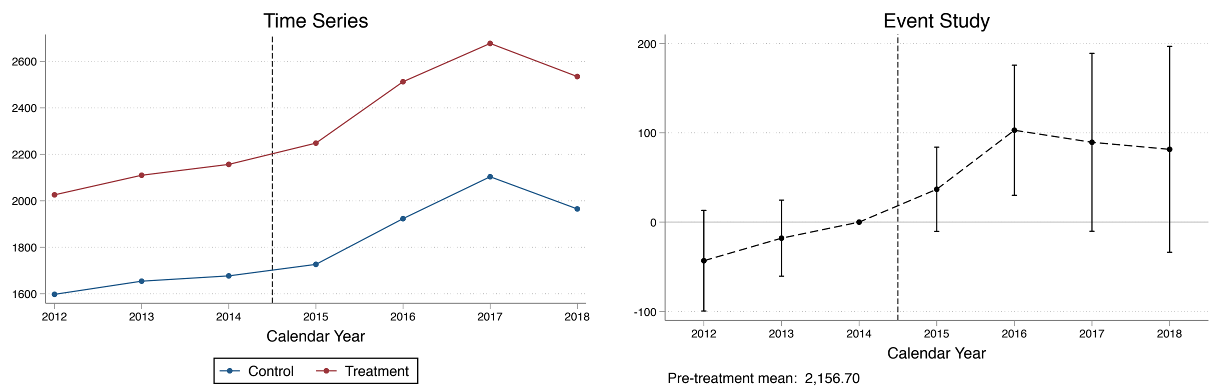
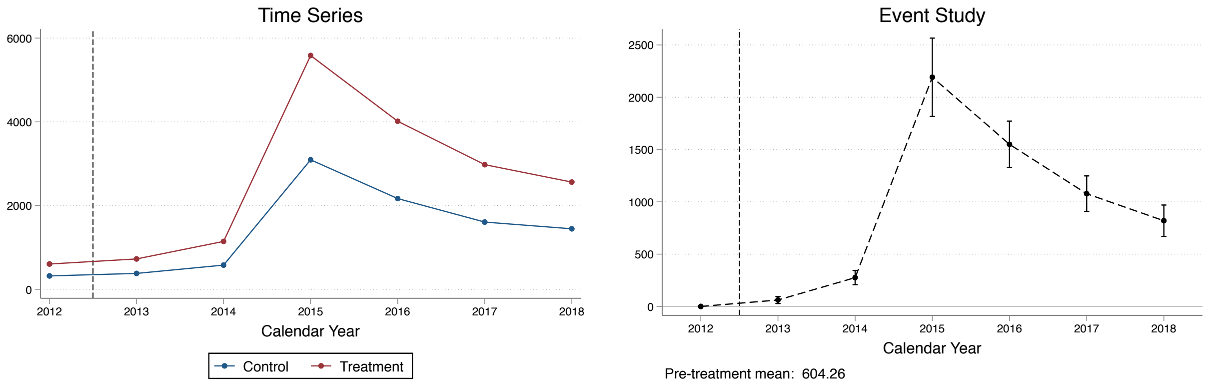


Figure 2.A.9: An Example of Complementarity with Recommended Primary Care Services: Influenza Vaccination — Unmatched Sample

Notes: This figure shows estimates for $\beta_{p(t)}$ from equation (2.2). The sample in panel A is our unmatched sample of counties where treatment is defined by Transitional Care Management take-up, and the sample in panel B is our unmatched sample of counties where treatment is defined by Chronic Care Management take-up. In each panel, estimates of $\beta_{p(t)}$ are shown in the right-hand graph and the corresponding time series are shown in the left-hand graph. Data is at the county-year level. The dependent variable is the county-level allowed amount for influenza vaccinations in units of dollars billed by PCPs per PCP. All regressions include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level.

(a) Pneumonia Vaccination Billing per PCP, by TCM Treatment Group



(b) Pneumonia Vaccination Billing per PCP, by CCM Treatment Group

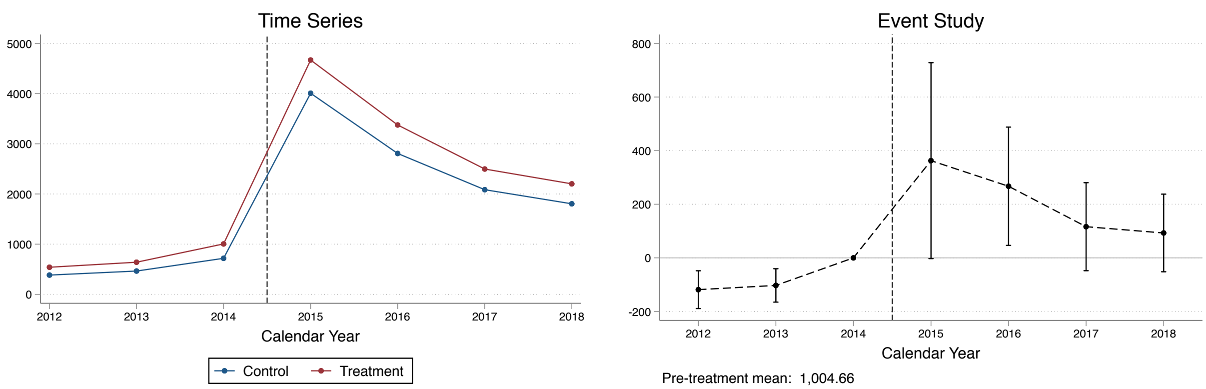
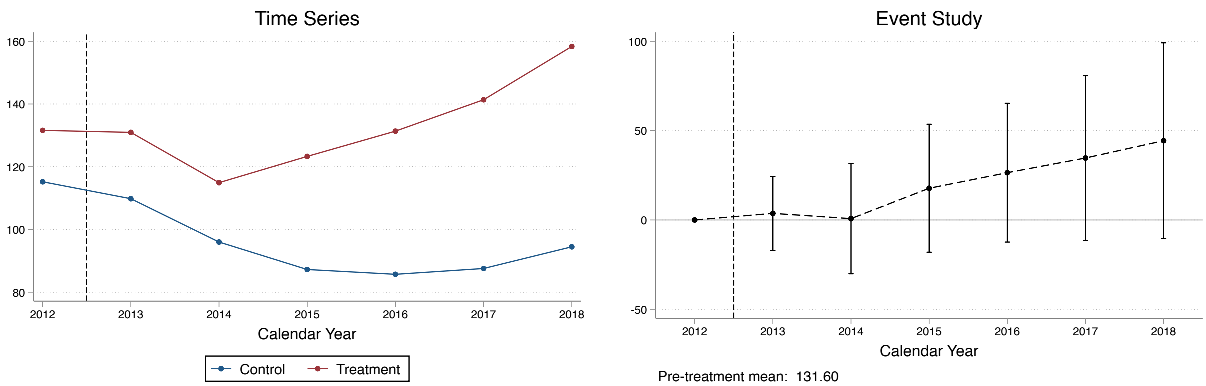


Figure 2.A.10: An Example of Complementarity with Recommended Primary Care Services: Pneumonia Vaccination — Unmatched Sample

Notes: This figure shows estimates for $\beta_{p(t)}$ from equation (2.2). The sample in panel A is our unmatched sample of counties where treatment is defined by Transitional Care Management take-up, and the sample in panel B is our unmatched sample of counties where treatment is defined by Chronic Care Management take-up. In each panel, estimates of $\beta_{p(t)}$ are shown in the right-hand graph and the corresponding time series are shown in the left-hand graph. Data is at the county-year level. The dependent variable is the county-level allowed amount for pneumonia vaccinations in units of dollars billed by PCPs per PCP. All regressions include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level.

(a) Mammogram Billing per PCP, by TCM Treatment Group



(b) Mammogram Billing per PCP, by CCM Treatment Group

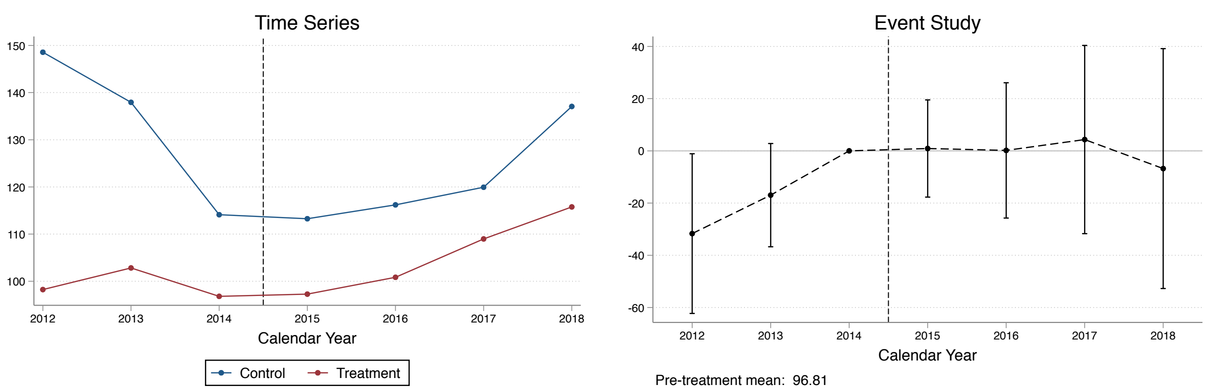


Figure 2.A.11: An Example of Complementarity with Recommended Primary Care Services: Mammograms — Unmatched Sample

Notes: This figure shows estimates for $\beta_{p(t)}$ from equation (2.2). The sample in panel A is our unmatched sample of counties where treatment is defined by Transitional Care Management take-up, and the sample in panel B is our unmatched sample of counties where treatment is defined by Chronic Care Management take-up. In each panel, estimates of $\beta_{p(t)}$ are shown in the right-hand graph and the corresponding time series are shown in the left-hand graph. Data is at the county-year level. The dependent variable is the county-level allowed amount for mammograms in units of dollars billed by PCPs per PCP. All regressions include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level.

Table 2.A.1: Post-Discharge Outcomes — Annual Estimates

New Code	Year	Fraction of Beneficiaries with a Traditional Post-Discharge Visit	Fraction of Beneficiaries with a Traditional Post-Discharge Visit with a PCP	Controls
Transitional Care Management (Thousands of $ per PCP)				
	2013	-.33	-.17	Yes
		(.29)	(.40)	
	2014	-1.18***	-1.08***	Yes
		(.15)	(.19)	
	2015	-1.23***	-1.02***	Yes
		(.17)	(.21)	
	2016	-.90***	-1.31***	Yes
		(.18)	(.23)	
	2017	-1.06***	-1.54***	Yes
		(.14)	(.17)	

Notes: This table shows estimates for β_1 from a version of equation (2.1) that is estimated separately on each year of data. Data is at the county level and these data span the years 2012-2017. The independent variable is the county-level allowed amount for Transitional Care Management in units of dollars billed by PCPs per PCP. Dependent variables are county-level rates where the denominator is the annual number of discharged patients. Regressions with controls include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are heteroskedasticity-robust.

Table 2.A.2: Non-PCP Billing by Category — Outcomes Scaled per PCP

New Code	Total Billing per PCP	Evaluation & Management Billing per PCP	Procedures Billing per PCP	Imaging Billing per PCP	Tests Billing per PCP	Durable Medical Equipment Billing per PCP	Other Billing per PCP	Controls
Transitional Care Management ($ per PCP)								
	7.31**	2.97***	.60	1.62	-.32	-.04**	2.47*	No
	(3.27)	(.73)	(.85)	(2.32)	(.22)	(.01)	(1.29)	
	7.03**	2.84***	.52	1.63	-.28	-.04***	2.36*	Yes
	(3.28)	(.73)	(.81)	(2.31)	(.22)	(.01)	(1.37)	
Chronic Care Management ($ per PCP)								
	-.23	.07	-.11	-.20**	-.06	-.003	.07	No
	(.69)	(.31)	(.22)	(.08)	(.05)	(.01)	(.30)	
	-.33	.03	-.13	-.19**	-.06	-.003	.004	Yes
	(.70)	(.313)	(.23)	(.08)	(.05)	(.01)	(.31)	
Dependent Mean	311,501.64	100,090.68	70,524.22	21,079.84	16,930.18	212.21	102,664.52	
N	20,262	20,262	20,262	20,262	20,262	20,262	20,262	

Notes: This table shows estimates for β_1 from equation (2.1). Data is at the county-year level and these data span the years 2012-2018. The independent variable is the county-level allowed amount for Transitional Care Management in units of dollars billed by PCPs per PCP. The dependent variable is the county-level allowed amount for the specified outcome billed by non-PCPs per PCP. All regressions include year-level and county-level fixed effects. Regressions with controls include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level. TCM and CCM belong to the Evaluation & Management category, but the new code of interest for each regression is excluded from this category as well as from Total billing. The dependent means for the first two columns are the average of the means that result from dropping each new code, which are approximately the same.

Table 2.A.3: Non-PCP Billing by Category — Outcomes Scaled per PCP at Baseline

New Code	Total Billing per PCP	Evaluation & Management Billing per PCP	Procedures Billing per PCP	Imaging Billing per PCP	Tests Billing per PCP	Durable Medical Equipment Billing per PCP	Other Billing per PCP	Controls
Transitional Care Management ($ per PCP)								
	5.47**	1.82***	.21	.94	-.28	-.04**	2.81***	No
	(2.34)	(.51)	(.50)	(1.68)	(.21)	(.01)	(1.04)	
	5.02**	1.63***	.09	.95	-.25	-.04***	2.65**	Yes
	(2.36)	(.52)	(.48)	(1.68)	(.21)	(.01)	(1.12)	
Chronic Care Management ($ per PCP)								
	.41	.26	-.0002	-.17**	-.03	-.003	.35	No
	(.49)	(.25)	(.20)	(.07)	(.04)	(.008)	(.27)	
	-.32	.22	-.01	-.16**	-.03	-.003	.30	Yes
	(.50)	(.25)	(.21)	(.07)	(.04)	(.009)	(.28)	
Dependent Mean	305,763.18	98,411.30	70,229.07	20,993.41	17,037.88	215.28	98,876.24	
N	20,262	20,262	20,262	20,262	20,262	20,262	20,262	

Notes: This table shows estimates for β_1 from equation (2.1). Data is at the county-year level and these data span the years 2012-2018. The independent variable is the county-level allowed amount for Transitional Care Management in units of dollars billed by PCPs per PCP. The dependent variable is the county-level allowed amount for the specified outcome billed by non-PCPs divided by the number of PCPs in the county in 2012. All regressions include year-level and county-level fixed effects. Regressions with controls include controls for average age, percent of beneficiaries that are female, percent of beneficiaries that are Medicaid-eligible, and a normalized index of chronic condition prevalence. Standard errors are clustered at the county level. TCM and CCM belong to the Evaluation & Management category, but the new code of interest for each regression is excluded from this category as well as from Total billing. The dependent means for the first two columns are the average of the means that result from dropping each new code, which are approximately the same.

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Chapter 3

Health Professional Shortage Areas and Physician Location Decisions

Abstract

To address geographic disparities in healthcare provision, the U.S. government designates primary care Health Professional Shortage Areas (HPSAs), and the Centers for Medicare and Medicaid Services (CMS) provide 10% bonus payments to physicians billing in these areas. We use administrative data from CMS and a matched difference-in-differences design to study the effects of shortage area designations on physician location decisions. We find that counties designated as HPSAs experience a 23% increase in the number of early-career primary care physicians. The increase is driven entirely by physicians who attended ranked medical schools. However, we find no evidence that physicians in later career stages relocate to shortage areas. Overall, our findings suggest that targeting incentive payments towards newer physicians may improve the effectiveness and cost-efficiency of policies aimed at addressing physician shortages.

3.1 Introduction

There exists wide regional variation in healthcare spending and utilization, as well as health outcomes across the United States (Skinner 2011). While the literature seeks to understand and debates the relative importance of supply side factors versus demand side factors in causing this phenomenon, a closely-related fact has captured the interest of researchers and policymakers alike: some areas have significantly fewer doctors per capita than other areas. Individuals living in these so-called "shortage areas" may face higher costs of obtaining medical treatment and may be less likely to seek preventive care.

To address potential problems associated with the presence of physician shortages, the U.S. government identifies areas in need and attempts to increase resources available to residents of these areas. A particularly prominent policy aims to improve access to primary care through financially incentivizing physicians to practice in areas deemed to have too few doctors. Specifically, the Health Resources and Services Administration works with state agencies to manage official designations of Health Professional Shortage Areas (HPSAs), and through the Centers for Medicare and Medicaid Services (CMS), physicians receive a 10 percent bonus payment on the Medicare services they bill in designated HPSAs.

In this paper, we ask whether Health Professional Shortage Area designations influence the location decisions of primary care physicians (PCPs). To answer this question, we study the effect of a county being designated as a HPSA on the stock of Medicare-billing primary care doctors practicing in that county. We first link together several sources of administrative data from CMS using unique physician identifiers to create a county-level panel dataset that contains information on physician counts (by doctor characteristics such as graduation date and medical school attended), as well as HPSA designation status. We then supplement these data, which capture the near-universe of physicians who bill Medicare Part B, with county-level information from the Area Health Resource File.[1] Using this panel

[1] Note that the vast majority of primary care physicians bill to Medicare; more than 90% of non-pediatric primary care physicians accept Medicare patients (Kaiser Foundation 2015).

dataset, which spans the years 2012 to 2017, we employ a matched difference-in-differences design to identify the causal effect of HPSA designations on the stock of Medicare-billing PCPs.

We use a matching strategy in order to overcome a significant challenge associated with studying the impact of shortage area designations. To identify causal effects, one needs a valid counterfactual for the evolution of PCP counts in HPSA counties. Yet designations are not random; they are in part directly due to declines in the number of physicians practicing in a county. Thus comparing a control group of all non-HPSA counties with a treatment group of HPSA counties is unlikely to be a credible approach. Our matching strategy, which uses variables defined over a baseline time period that capture information directly relevant for official shortage area designations, addresses this concern by selecting counties similar to HPSAs to serve as controls.

Specifically, to each county designated as a HPSA during our analysis time period, we match similar counties that are not designated as HPSAs. We then use a difference-in-differences framework to compare the stock of PCPs in HPSAs before and after the official designation with that of the matched control counties. Importantly, we exploit our data to analyze physician responses separately by career stage. Early-career physicians likely making initial location decisions after completing their residencies may face substantially lower costs of moving compared to later-career physicians, and the degree to which they respond may have particularly important consequences for evaluating the efficacy of the program, if physicians who locate in shortage areas tend to continue practicing there for the duration of their career. We also use information on medical school rankings to proxy for physician quality, and we assess whether physician responses differ along this dimension.

Our main result is that designated counties experience an increase in the number of early-career PCPs. The pattern of our dynamic difference-in-differences estimates suggests a relatively quick rise in the count of early-career physicians during the first two years of designation, which then stabilizes at a higher level. Our preferred estimate indicates that designated counties experience an average increase of approximately 0.114 physicians per 10,000 residents, which roughly amounts to 0.67 physicians per county and represents a 23% increase off of a modest baseline mean. We then show that the increase is entirely driven by an influx of early-career PCPs who attended ranked medical schools, perhaps reflecting the ability of the program to attract high-quality physicians to areas in need.

In contrast, we find no evidence of an increase in counts of later-career physicians, who are likely more settled and may face higher costs of relocating already-established practices. Our results are consistent with the notion that bonus payments for billing in HPSAs may be more attractive to newer physicians—who are likely already considering (re)location decisions as it relates to the timing of recently completed residencies or initial career trajectories.

Our findings have direct implications for policy. The 10 percent bonus payment attached to HPSAs is provided to all PCPs billing services to Medicare, but the majority of these are later-career doctors, who we find to be generally unresponsive. A more effective and cost-efficient way to increase physician counts in underserved areas may be to target a higher percentage bonus payment at the subset of physicians we find to be responsive. For instance, using a simple and stylized policy exercise, we show that a 20 percent bonus payment offered to PCPs who relocate to a HPSA in the first 10 years of their career may induce even more movement of early-career physicians than the current program while substantially reducing overall payments to inframarginal doctors who would practice in a HPSA under either regime.

This paper relates broadly to the large literature that studies physician responses to financial incentives, often analyzing how payment rates and prices impact provision of care (e.g., Ellis and McGuire 1986, McGuire and Pauly 1991, McGuire 2000, and Chandra et al. 2011) and physician labor supply more generally (e.g., Nicholson and Propper 2011).[2] We contribute to this literature by providing new evidence on how financial incentives impact a

[2] For additional work in the U.S. setting, see Hadley and Reschovsky (2006), Clemens and Gottlieb (2014), Alexander (2015), Johnson and Rehavi (2016), Clemens et al. (2018), and Gottlieb et al. (2020). For evidence from other countries, see Sørensen and Grytten (2003), Kantarevic et al. (2008), Devlin and Sarma (2008), Sarma et al. (2010), and Brekke et al. (2017).

key component of physician labor supply: practice location.

We thus relate most closely to other papers that investigate physician location decisions, especially in the context of physician shortages.[3] Despite the importance and policy-relevance of the topic, there is limited causal evidence informing the issues. In a review of research on shortage area programs, Bärnighausen and Bloom (2009) discuss several observational studies and conclude that, mostly due to selection effects, none allow for credible causal inference. More recently, a series of working papers develop models of physician location decisions, simulate the effects of various incentive policies designed to combat shortages, and find generally that physicians are not very responsive to financial and salary incentives (Zhou 2017, Falcettoni 2018, and Kulka and McWeeny 2019).[4] Of these papers, Kulka and McWeeny (2019) is the most similar to ours, as they complement their structural analysis with a reduced-form evaluation of state-level student loan forgiveness programs and find small positive effects. We contribute to this strand of the literature by offering causal evidence on the effectiveness of a large, nation-wide program designed to address shortage areas through direct monetary payments. Furthermore, in exploiting our data to study how responses vary by career stage, we are able to uncover evidence that early-career PCPs are more responsive to shortage area designations.

Finally, our findings connect to an important discussion in the literature on how payment policies influence the overall capacity of the healthcare system, particularly as it relates to the allocation of human capital to and within the health sector. Existing work shows that Medicare policy can increase investments in medical technology (Finkelstein 2007, Acemoglu and Finkelstein 2008, and Clemens and Gottlieb 2014) as well as physician on-the-job investments (Clemens et al. 2018), and other papers highlight an important role for financial incentives in shaping the decision to become a doctor (Chen et al. 2020 and Gottlieb

[3] More generally, papers have documented factors such as the location and type of medical training as influencing practice locations (e.g., Burfield et al. 1986 and Chen et al. 2010). Another related paper set in a different context is Huh (2018), who finds that Medicaid expansions can attract dentists to poorer areas.

[4] These papers advance earlier work that modeled physician location decisions in the U.S. (Hurley 1991 and Holmes 2005) and Canada (Bolduc et al. 1996).

et al. 2020).[5] In finding that the HPSA program brings physicians to designated counties, we present evidence of a government payment policy expanding access to healthcare in specific geographies and influencing the distribution of health-sector human capital across space.[6]

The rest of this paper is organized as follows. Section 3.2 provides an exposition of the policy environment. Section 3.3 describes the data sources and highlights how we construct our dataset. Section 3.4 lays out our matched difference-in-differences framework. Section 3.5 presents our results. Section 3.6 discusses policy implications. We conclude in Section 3.7.

3.2 Policy Environment

Overview of Health Professional Shortage Areas. The Health Resources and Services Administration (HRSA), which is an agency of the United States Department of Health and Human Services, strives to "improve health outcomes and address health disparities through access to quality services, a skilled health workforce, and innovative, high-value programs."[7] In order to bring federal resources to people in need, HRSA creates shortage designations. Health Professional Shortage Areas (HPSAs) are one type of shortage designation, and it is this particular type on which CMS bases their Medicare bonus payment program.[8] HPSA designations can be made for three disciplines (primary care, mental health, and dental health) at three different levels (geographic area, population group, and facilities). Because primary care physicians (PCPs) play such a central role in the provision of healthcare in

[5] Another set of related papers show that specialty choice may also be influenced by financial incentives (e.g., Sloan 1970, Bazzoli 1985, Hurley 1991, Nicholson and Souleles 2001, Nicholson 2002, Bhattacharya 2005, Gagné and Léger 2005, and Sivey et al. 2012).

[6] Our analysis thus also connects to the influential research concerned with assessing causes and implications of regional differences in healthcare utilization, expenditures, and physician practice styles (e.g. Fisher et al. 2003a, Fisher et al. 2003b, Sutherland et al. 2009, Gottlieb et al. 2010, Song et al. 2010, Zuckerman et al. 2010, Skinner 2011, Finkelstein et al. 2016, Molitor 2018, and Cutler et al. 2019).

[7] See their mission statement on the following website: `https://www.hrsa.gov/about/index.html`.

[8] Other types of shortage area designations maintained by HRSA include: Medically Underserved Areas (MUAs), Medically Underserved Populations (MUPs), and Governor's Designated Secretary Certified Shortage Areas for Rural Health Clinics.

the United States, and because the CMS Medicare incentive payment program that we study in this paper does not apply to population group or facility shortage designations, we restrict our attention to HPSAs designated for the primary care discipline at the geographic level. Unless otherwise specified, hereafter we use the more general terms, "HPSAs" and "designations," to refer to this specific type of shortage designation.

HPSA Designation Process. While HRSA manages and grants HPSA designations, the responsibility to identify potential shortage areas falls on state Primary Care Offices (PCOs), who generally submit applications on behalf of geographic areas in their state to HRSA. State PCOs do not all operate in the same manner. For instance, depending on the PCO, areas identified as potential HPSAs can be census tracts, minor civil divisions (e.g., townships), or entire counties. Nonetheless, once HRSA receives an application, they work with the applying PCO to gather objective data used to both determine HPSA eligibility status and to calculate a score intended to quantify the severity of the shortage.[9] The score is primarily determined by an area's population-to-provider ratio, but it also depends on the fraction of the population below the federal poverty line, an infant health index, and travel time to the nearest source of care outside of the proposed HPSA. While the actual score may be informative for programs beyond the scope of our paper, the Medicare bonus payments provided by CMS depend only on overall designation status, and they do not depend on the score-based severity of the shortage.

Medicare Bonus Payments from CMS. The Centers for Medicare and Medicaid Services provide 10 percent bonus payments on Medicare services furnished by physicians in primary care geographic HPSAs designated by December 31 of the previous year. Bonuses are paid quarterly and are generated automatically when physicians provide services in a CMS-maintained list of HPSA ZIP codes, which consists of ZIP codes that fall entirely within a designated HPSA (e.g., all ZIP codes completely contained in a county that is

[9]As a general benchmark, HRSA typically considers an area to have a shortage of providers if they have a population to provider ratio of 3,500:1 or more.

a designated HPSA). Physicians providing services in designated areas not on the CMS-maintained ZIP code list can still receive the HPSA bonus payment by appending a modifier to their claims; these physicians are responsible for determining the HPSA status of their area based on tools provided by HRSA. Due to the data availability discussed in Section 3.3 (and because CMS relies primarily on their own list of HPSA ZIP codes), we use as our source of variation designations that result in automatically-billed HPSA ZIP codes. The 10% bonus payment program produces the major incentive for locating in HPSAs and applies to all physicians in HPSAs; though for some groups of doctors, other related programs may interact with designations to create additional incentives.[10]

3.3 Data

To analyze the impact of HPSA designations on the location decisions of Medicare-billing PCPs, we draw on five main data sources to assemble a detailed, county-level, panel dataset. In this section, we provide an overview of the data sources, highlight our approach to creating the county panel, and discuss key variables for our analysis.

3.3.1 Data Sources and Creating the County Panel

To construct a county panel suitable for our analysis, we start by linking together three physician-level datasets developed by CMS. The first, *Medicare Provider Utilization and Payment Data: Physician and Other Supplier* (MPUP), contains detailed information on Medicare services provided by healthcare professionals at the physician-code-location

[10] A variety of smaller federal incentive programs aim to bring physician and non-physician healthcare providers to shortage areas. For example, loan forgiveness and scholarship programs through the National Health Service Corps (NHSC) and the NURSE Corps, Rural Health Clinic Programs through CMS, and the J-1 visa waiver program for foreign medical graduates may use HPSA criteria to determine eligibility in their contexts. Some primary care physicians may also participate in these programs and thus may face additional incentives above and beyond the bonus program. In addition, most states have some form of a loan forgiveness program for practicing in rural areas (Kulka and McWeeny 2019) which could potentially interact with HPSA designations. For more information on HPSA designations in general and additional related programs, see `https://bhw.hrsa.gov/shortage-designation/hpsas`.

level from 2012–2017.[11] It is based on CMS administrative claims data for Medicare Part B fee-for-service beneficiaries, and it represents the near-universe of Medicare billing physicians. Only Medicare-billing doctors who do not bill any HCPCS code at least 10 times in a given year are omitted from the data for that year. Of note, more than 90% of non-pediatric primary care physicians accept Medicare patients (Boccuti et al., 2015). We extract from this dataset the unique physician identification numbers, National Provider Identifiers (NPIs), of Medicare-billing doctors and information regarding their specialty. From annual disseminations of a second physician-level dataset, the *National Plan and Provider Enumeration System* (NPPES), we extract information on the primary practice location for the Medicare-billing physicians.[12] Linking these two datasets yields panel data for Medicare-billing physicians spanning the years 2012 to 2017, with information on physician specialty and practice location.

The third physician-level dataset we employ is the *Physician Compare* dataset, which CMS began publishing in 2014 for the use of patients who wish to gather information about doctors who accept Medicare. From these data we extract physician graduation dates and medical school attendance, which allows us to analyze doctor responses by career stage and quality of medical school (as proxied for by medical school rankings). The ability to incorporate this information in our analysis is important for policy. For example, the effectiveness of the program in alleviating concerns regarding the provision of medical care in the longer run may depend on the types of physicians ultimately induced to locate in shortage areas.

[11] Specifically, one observation in the dataset is defined by (1) a National Provider Identifier, the unique physician identification number, (2) a Healthcare Common Procedure Coding System (HCPCS) code, which are specific codes detailing the procedure undertaken by the physician, and (3) place of service.

[12] The MPUP does contain information on practice location; however, the variables contained in this dataset are not suitable for our analysis. Specifically, location variables in the MPUP data are updated to be the location of the physician in the subsequent calendar year. For example, the 2014 MPUP data contain billing information for physicians who billed Medicare in 2014, but the location variable captures locations at the end of the 2015 calendar year. It is for this reason that we use the NPPES data to accurately define physician location for the calendar years for which we have billing information. We define location as a physician's primary practice location in December of the year of observation.

The main drawback of the Physician Compare dataset lies in the fact that it is a snapshot in time of currently-billing physicians. While we make use of all available archived data from 2014 onward, we do not have a snapshot of the Medicare-billing physicians before the initial publication of the data in 2014. For the most part, this drawback is rather harmless, as the information pulled from Physician Compare (i.e. graduation year and medical school) is time-invariant, and most doctors in our panel of Medicare-billing physicians appear in all waves of the data. However, after we link the Physician Compare data to our panel data, graduation year and medical school are mechanically missing for physicians that practice and bill to Medicare *only* in 2012 or 2013 (because those doctors are never observed in a year for which Physician Compare exists).[13] While it is perhaps more likely that the physicians who are observed only in 2012 and/or 2013 are late-career physicians who have retired by 2014, our leading analysis does not count these physicians as belonging to any career stage (and it also does not count them as having attended ranked or unranked medical schools). We show that the rate of missing data does not differ significantly between the treatment group and the control group before or after designation in Appendix Figure 3.A.3.

After linking together the three physician-level data sources, we aggregate the data up to the county level. That is, we create a county-level dataset with counts of primary care Medicare-billing physicians spanning the years 2012 to 2017.[14] Finally, into our newly-constructed panel we merge data derived from two more sources. First, for information regarding HPSA status, we use the official, CMS-maintained list of ZIP codes that define automatically billed HPSAs. We aggregate this data up to the county level by simply counting the number of HPSA ZIP codes in a county. Second, for more information on county characteristics, we pull variables from the *Area Health Resources File* (AHRF), which contains a wide range of county-level, health-related variables derived from the American

[13] There are 16,873 (7.23%) primary care physicians who only appear in the data in 2012 and 2013, overall, and 2,563 (6.63%) in our analysis counties.

[14] We define a doctor as a primary care physician if her specialty is any of the following: "family practice," "general practice," "internal medicine," "geriatric medicine," or "pediatric medicine."

Medical Association Masterfile and county-level demographic and economic variables derived from the American Community Survey. Linking together all of the data sources, we create a county panel containing information on population demographics, economic conditions, HPSA designations, and the stock of Medicare-billing primary care physicians.

3.3.2 Key Variables

The outcome variables of interest for our analysis are per-capita counts of Medicare-billing primary care physicians. We analyze the evolution of the total count of these doctors in counties across time, but we also break down the stock of physicians into counts by career stage and by quality of medical school. In any given year, we define early-career PCPs, who may have higher elasticities governing their labor supply (and practice location) decisions, as those who graduated from medical school 5 to 10 years prior. Our definition of early-career physicians intends to capture those likely making initial location decisions for their practice after completing their residencies. Our choice of 5 years after graduating is also driven by the data: the vast majority of physicians are not assigned an NPI until about 5 years after finishing medical school.[15] We then define later-career PCPs as those who graduated more than 10 years ago.

We also analyze physician counts by quality of medical school. HRSA designates shortage areas with the goal of bringing resources to areas in need. From a policy perspective, the types of physicians the program brings in may have important consequences. We therefore break down counts of physicians along this dimension. Specifically, we study counts of PCPs who attended ranked medical schools separately from counts of PCPs who attended unranked medical schools. To define the relevant variables, we use the 2018 rankings of med-

[15] In any given year, the data contain a very small number of physicians who report having graduated less than 5 years earlier. The counts of physicians by medical school cohort do not approach the typical cohort size until 5 years after graduation. This is because physicians typically spend their years immediately after graduation completing their residency and likely do not yet have an NPI. To maintain a consistent interpretation of our definition of early-career physicians, we exclude from our count of early-career PCPs the handful of physicians in the data who are not likely to have completed their residency by defining early-career PCPs as those graduating 5 to 10 years earlier.

ical schools for primary care from the U.S. News & World Report, and we consider a medical school to be ranked if it is any one of the 95 schools receiving an official ranking.[16]

We use several additional variables in our matched difference-in-differences design. In particular, we define our treatment variables based on whether or not a county contains at least one automatically-billed designated HPSA ZIP code.[17] We also use county-level variables from the AHRF indicating the total number of active physicians per capita and the percent of the population below the federal poverty line to carry out our matching procedure, and we employ three more variables from the AHRF specifying the population, unemployment rate, and median household income of counties as controls. In Section 3.4, we describe specifically how these variables enter our design.

3.4 Empirical Strategy

Our goal is to estimate the causal effect of HPSA designations on physician location decisions. An ideal experiment would randomly assign HPSA designations to some counties and track the counts of physicians in these counties compared to a control group of non-designated counties. A potentially-naive difference-in-differences framework that aims to approximate this ideal would involve the comparison of designated counties (i.e., the treatment group), in which 10% bonus payments are made to Medicare-billing PCPs, to counties that are not designated (i.e., the control group), in which there are no 10% bonus payments for Medicare-billing PCPs. Such a comparison is not without problems, as counties designated as HPSAs are likely very different in observable and unobservable ways than counties that are not designated.

[16] About 36% of PCPs in the sample report a medical school of "Other," which we classify as unranked. Some PCPs reporting "Other" may have attended medical school outside of the U.S.

[17] While some counties are only "partially" HPSA-designated, meaning only some of its zip codes are on the CMS list of automatically billed HPSAs, the majority of HPSA-designated counties in our sample are fully designated. There are 79 (36.4%) partially designated counties in our analysis data. Of those, 20% are at least 50% designated. We assess the robustness of our results to the exclusion of partially designated counties in Section 3.5.2.

Indeed, Figure 3.1 illustrates exactly this concern. The solid line depicts the average count of PCPs in HPSAs, where time on the x-axis is relative to designation year. The stock of physicians in HPSA counties tends to fall leading up to the designation year, which is not unexpected. In contrast, the dotted line depicts the average count of PCPs for the potential control group that consists of all other counties. Relative time for this comparison group is defined by matching to each HPSA all other counties, and then assigning a placebo designation year to the comparison counties equal to the actual designation year for the HPSA county to which they are matched. The stock of physicians in all other counties is not falling in the years before placebo designation, which would raise concerns about the validity of a straightforward difference-in-differences estimator.

For these reasons, we use a matched difference-in-differences approach to select a control group of non-designated counties that are more similar to HPSAs. In Section 3.4.1, we detail our procedure for selecting the control group and discuss our analysis sample. In Section 3.4.2, we describe the specifics of how we implement our matched difference-in-differences design.

3.4.1 Matched County Design

Matching Procedure. To select our control group, we borrow a matching procedure from Deryugina et al. (2018) to identify counties that are similar to our treatment group comprised of HPSAs.[18] We match to each treated county three control counties, and we assign the matched controls a placebo designation year equal to the actual designation year of their corresponding treated county.

To select the three control counties for each treated county, we use as our set of matching variables $\mathbf{X}_{ct}$ three variables defined at a baseline: number of active physicians per capita, annual percentage change in active physicians per capita, and percent of the

[18] Deryugina et al. (2018) study the long-run effects of Hurricane Katrina; we broadly base our matching procedure off of the one they employ, which selects cities similar to New Orleans.

population below the federal poverty line. We use these variables (pulled from the AHRF) from 2010 and 2011, which corresponds to two or three years before any of the earliest designations that we study. HRSA uses both the stock of physicians and the poverty rate to determine the score of proposed HPSAs, and designations are largely due to declines in physician counts; therefore, we view these variables as a reasonable and natural benchmark set on which to match.

For each treated county, we use our matching variables to compute a measure of "closeness" to each potential control county, where the pool of potential controls consists of the counties that are never designated as HPSAs in our sample period. To compute the closeness between a treatment county c^* and a control county c, we sum the squared difference between counties of each variable $x_{ct} \in \mathbf{X}_{ct}$ (normalized by that variable's standard deviation in the pool of counties σ_{x_t}) across both years in the baseline period 2010–2011.[19] That is,

$$\text{Closeness}(c^*, c) = \sum_{t=2010}^{2011} \sum_{x_{ct} \in \mathbf{X}_{ct}} \left(\frac{x_{ct} - x_{c^*,t}}{\sigma_{x_t}} \right)^2 . \tag{3.1}$$

In addition to the variables included in the closeness measure, matching on region is important given that the existing literature has indicated that geography has an influence on physician residential choices (Burfield et al., 1986; Chen et al., 2010). For this reason, we stipulate that a treatment county can only be matched to control counties that are in its geographic region.[20] The three counties from the pool of potential controls with the smallest value of this match measure for a given treatment county are included in the control sample with a placebo designation year equal to the actual designation year of the treatment county to whom they are matched.

We probe the robustness of our results to changing different aspects of the matching

[19]Note that while the other match variables are defined for both 2010 and 2011, the percentage change in number of physicians is only calculated for the annual change from 2010 to 2011 since these are our designated baseline years. Thus, the closeness measure includes two values for the stock of active physicians, two values for the poverty rate, and one value for the percentage change in active physicians.

[20]We define four distinct regions roughly corresponding to South, Northeast, Midwest, and West.

procedure in Section 3.5.2. Specifically, we vary the combination of baseline variables used to construct the match, and we vary the number of control counties matched to each treatment county.

Analysis Sample. The treatment group consists of the 217 counties that we see become designated between 2013 and 2017. The matching method described above generates a control group from the sample of counties that are never designated as HPSAs between 2012 and 2017. Three counties are matched to each treatment county to serve as controls, and counties are allowed to be matched to more than one treatment county; the resulting analysis sample thus includes 651 control counties, 470 of which are unique.[21]

Table 3.1 presents summary statistics for descriptive variables, for the treatment and control groups separately. The statistics come from the year preceding (actual or placebo) designation. The table shows that HPSAs generally look similar to control counties in terms of descriptive observables, although they are less populous and have slightly fewer physicians. Figure 3.1 makes it clear that the matched sample improves upon the non-matched sample in terms of assessing the validity of a difference-in-differences estimator through examination of parallel pre-trends. The dashed line plots the average counts of PCPs in our control group constructed using the matching procedure. The group experiences a decline in the stock of PCPs before placebo designation year similar to that in HPSAs, which allows us to more confidently use the evolution of PCP counts in the control group as a counterfactual for the evolution of PCP counts in the treatment group.

3.4.2 Implementation

We use the matching procedure described above to construct a suitable control group for counties within-whom an automatically-billed, primary care geographic HPSA is designated. To then analyze the effect of designations, we use a standard difference-in-differences

[21] Our panel is unbalanced due to the fact that the number of lead and lag years we see for a county depends on the year it was treated. By design, we exclude those counties that are always designated and study only those designated counties for which we see the year before and year of designation.

framework. Specifically, to document the dynamic impacts, we estimate the following equation:

$$y_{ct} = \alpha + \beta treat_c + \sum_{\tau \neq -1} \gamma_\tau I_\tau + \sum_{\tau \neq -1} \delta_\tau treat_c \times I_\tau + Z_{ct}\theta + \varepsilon_{ct}, \tag{3.2}$$

where y_{ct} is an outcome for county c in year t (e.g., the total number of Medicare-billing PCPs per 10,000 county residents), $treat_c$ is an indicator that equals one for counties receiving a designation over our sample period, the I_τ's are indicators for years relative to (actual or placebo) designation, Z_{ct} is a vector of controls, and the δ_τ's are the parameters of interest, which capture the average difference in y between the treatment and control groups relative to the omitted year.[22]

The identifying assumption asserts that, in the absence of HPSA designations, the stock of Medicare-billing PCPs in treated counties would have evolved in parallel with that in control counties. Analyzing the estimated δ_τ's from equation (3.2) provides an assessment on the validity of the design; specifically, we test whether the δ_τ's for $\tau < 0$ are different from zero, which would indicate the presence of pre-trends and might raise concerns regarding our difference-in-differences approach. Encouragingly, we consistently find no evidence of pre-trends that might invalidate the design.

Estimating the fully dynamic specification permits an evaluation of the key parallel trends assumption, but it also shows how the stock of doctors evolves over time; that is, results from estimating equation (3.2) shed light on how immediate or delayed, as well as how persistent or temporary, any physician responses to designations might be. After assessing the dynamic impact of HPSA designations, to better quantify the magnitudes of the mean

[22]Based on our data, $\tau \in \{-5, -4, \ldots, 4\}$ because the earliest year we can observe a change from not designated to designated is 2013 and our data goes through 2017; however, we pool together observations three or more years away from designation due to low observation counts.

treatment effect, we estimate the usual difference-in-differences estimating equation:

$$y_{ct} = \alpha + \beta treat_c + \gamma post_{ct} + \delta(treat_c \times post_{ct}) + Z_{ct}\theta + \varepsilon_{ct}, \tag{3.3}$$

where $post_{ct}$ is an indicator that equals one if for county c year t is a post-designation (or post-placebo-designation) year and δ is the parameter of interest.

Finally, while estimating equation (3.3) pools all pre-period years together and all post-period years together in order to quantify the overall effect, we employ one related additional specification. Guided by the graphical analysis of the dynamic impact, we split the post-designation period into two: a short-run period and a medium-run period. Specifically, we estimate

$$\begin{aligned} y_{ct} = \alpha &+ \beta treat_c + \gamma^{SR} post_{ct}^{SR} + \gamma^{MR} post_{ct}^{MR} \\ &+ \delta^{SR}(treat_c \times post_{ct}^{SR}) + \delta^{MR}(treat_c \times post_{ct}^{MR}) + Z_{ct}\theta + \varepsilon_{ct}, \end{aligned} \tag{3.4}$$

where $post_{ct}^{SR}$ is a (post-period short-run) indicator that equals one if for county c year t is in the year of the designation, and $post_{ct}^{MR}$ is a (post-period medium-run) indicator that equals one if for county c year t is after the immediate year of designation. Estimating equation (3.4) allows us to split up the post period and quantify short-run and medium-run effects, captured by δ^{SR} and δ^{MR} respectively. We often highlight the medium run estimates, which capture the impact on counts of doctors practicing in a county after allowing for the stock to evolve over a brief transition period.

3.5 Results

In this section, we first discuss our main results. We then discuss various robustness and specification checks. In general, we lead our analysis with graphical representations of dynamic effects before quantifying average magnitudes. In our leading regression specifications, all outcome variables are normalized per 10,000 population at baseline and winsorized at the 95th percentile, and we include county-level controls for household income, population, and the unemployment rate.[23]

3.5.1 Main Results

Figure 3.2 presents the results of estimating equation (3.2) for early-career and later-career PCPs.[24] The estimates for each parameter δ_τ are plotted along with 95% confidence intervals. These point estimates allow us to assess the validity of the identifying assumption and examine dynamic impacts.

The left-hand-side graph presents estimates of the impact of HPSA designation on counts of early-career doctors. The point estimates for δ_τ where $\tau < 0$ are not statistically different from zero and do not appear to be trending in any direction before the year of designation, which lends support to the parallel trends assumption. After designation, we see a relatively quick rise in the stock of these physicians practicing in HPSAS relative to non-HPSAs. The point estimate in year 0 is slightly elevated, whereas each of the point estimates on the indicators for the later post periods are positive and very similar to one another. The pattern of the dynamic estimates is consistent with a brief transition period over which the stock of doctors increases in response to the reform before stabilizing at the new level; this pattern also motivates a particular focus on the medium run estimates, which will quantify the effect of the policy on the stock of doctors after this brief transition period. Results from estimating equations (3.3) and (3.4) to quantify magnitudes are reported in Table 3.2. Column (1) summarizes the responses of early-career doctors. Panel A shows a statistically significant average medium-run increase of 0.114 early-career doctors per 10,000

[23]We measure baseline population in 2011. We include as controls indicators for $5,000 average household-income bins, current population, current population squared, and the unemployment rate.

[24]The corresponding graphs of raw means for these outcomes can be found in Appendix Figure 3.A.1. As defined in Section 3.3.2, early-career PCPs are those who graduated 5 to 10 years ago.

(s.e. 0.0570). This estimate corresponds to an increase of about 23% when compared to the baseline mean of 0.49 in the period before designation, and given that the average population of a treated county in our sample is around 59,000, it translates to approximately 0.67 more doctors per county on average. Panel B reports the average treatment effect for the entire post period, which includes the transition year as seen in the dynamics, thus resulting in a slightly smaller point estimate.

In contrast, the right-hand-side graph of Figure 3.2 shows no evidence of responses from later-career physicians. None of the dynamic point estimates are statistically distinguishable from zero, and the graph shows no discernible pattern or trend. Column (2) of Table 3.2 presents estimates for later-career PCPs; the magnitudes of the point estimates are comparatively smaller than those for early-career physicians, and the baseline mean is larger. At face value, the standard pooled difference-in-differences estimate for this outcome would represent a 0.13% increase in later-career doctor counts.

These results are consistent with PCPs in later career stages facing higher barriers to relocating. The cost of leaving behind a business that has already been established may be high, especially when considered with any implicit costs of moving to a potentially less desirable area. PCPs at the beginning of their career, however, might have fewer professional ties binding them to a given area, particularly when making initial location decisions after completing residencies.

Given the responsiveness of early-career doctors to HPSA designation, one may wonder which types of physicians are most likely to be induced to practice in a HPSA—in particular, whether they tend to be of higher or lower quality. Successfully attracting doctors to HPSAs that are young and high quality may increase both the quantity and quality of care in medically underserved areas. To proxy for physician quality, we use medical school rankings, and we analyze separate counts of early-career PCPs by whether the doctors attended a medical school that is included in the 2018 U.S. News Primary Care medical school rankings.

The dynamic effects on the stock of early-career doctors, split up by ranked and unranked medical schools, are presented in Figure 3.3, with corresponding graphs of means in Appendix Figure 3.A.2. First, we note the impacts in pre-designation years (on both counts of ranked and unranked doctors) are statistically indistinguishable from zero and do not exhibit any concerning trend. Next, we can see from comparing the left-hand-side graph and the right-hand-side graph that the entire post-designation increase in early-career doctors is driven by those who attended ranked medical schools. The dynamics for ranked physicians point to the same brief transition period followed by a period of stability, whereas the dynamics for unranked physicians reveal a lack of responses over the entire period. Corresponding point estimates are presented in Table 3.3; the estimates for early-career ranked doctors resemble those for the total number of early-career doctors, and are more precisely estimated. The medium run estimate indicates that treated counties gain 0.100 early-career, ranked PCPs per 10,000 population on average following HPSA designation (column (1) of Panel A), which corresponds to about 0.59 doctors in the average treated county, a 40% increase off of a small baseline mean. Mean treatment effects for early-career unranked physicians are much smaller and indistinguishable from zero (column (2)). Unfortunately, we lack the data to further investigate underlying mechanisms that could explain this dichotomy. Among other potential explanations, it could be that information about HPSAs is more widely disseminated at ranked schools, that students from these schools graduate with more debt, or that these doctors are more motivated to alleviate geographic shortages in care.

Lastly, to provide a gauge for the overall impact of designations, we present estimates on the per capita stock of all Medicare-billing PCPs. Figure 3.4 shows no evidence that designations have an impact on total PCP counts. This is not surprising, as the majority of PCPs are later-career PCPs, whom we have found to be unresponsive to HPSA status. We quantify corresponding magnitudes in Table 3.4. Columns (2) and (3) report separate estimates for the total stocks of ranked and unranked PCPs, both of which are statistically indistinguishable from zero.

3.5.2 Robustness and Specification Checks

We assess the robustness of our results along several dimensions. For simplicity, we focus on treatment effects from estimating equation (3.3) and medium run effects from estimating equation (3.4), for each of our main outcome variables: early-career PCPs; early-career PCPs from ranked schools; early-career PCPs from unranked schools; and later-career PCPs.

First, we probe the sensitivity of our results to various regression specifications. Table 3.5 displays results for the medium run effects, and Table 3.6 displays results for the mean treatment effects over all post-designation years. Each table is constructed as follows. Row A reproduces the baseline estimates. Rows B through D vary the approach to censoring the data for outliers. Rows E and F assess the sensitivity to inclusion of control variables. Overall, across both tables, we see that our results are not too sensitive to the choice of winsorization; point estimates are similar if we winsorize more stringently, winsorize less stringently, or do not winsorize at all, though we tend to experience precision gains when winsorizing more of the data. Further, results appear robust to both omitting all of the control variables as well as adding additional controls (year and state fixed effects).

Second, we assess the robustness of our results to removing partially designated counties from our treatment group. Appendix Table 3.B.1 reports point estimates for the medium run effects as well as overall pooled estimates. The first column reproduces our baseline estimates from studying all partially designated counties, and the remaining three columns report estimates from studying only counties that are at least 10%, 50%, and 100% designated. The point estimates remain generally consistent across columns. Results for later-career PCPs seem to vary more than others, though the effects are relatively small and are never statistically distinguishable from zero. We note that the number of observations drops by about 36% from column (1) to column (4).

Third, we vary our matching strategy. Appendix Table 3.B.2 reports results from altering the number of control counties that we match to each treatment county. Point estimates are broadly stable, though those for later-career PCPs appear more sensitive. Appendix Table 3.B.3 reports results from changing the variables used in our matching procedure. Column (1) reproduces estimates from our leading procedure. Column (2) does not match on the baseline trends in physician counts, and column (3) does not match on the baseline number of physicians. Column (4) matches only on geography and poverty rate. Column (5) matches on the baseline level of physicians along with a baseline trend in the poverty rate, rather than using the trend in physician counts. Overall our results appear mostly stable, especially the results on early-career ranked PCPs, and alternative matching procedures may address potential concerns about matching on both baseline levels and trends of physicians while also selecting a control group of counties that are themselves not designated over our time period.

3.6 Policy Discussion

Responsiveness to HPSA designation varies significantly by career stage: there is evidence for an increase in the stock of early-career PCPs, but no evidence of any effect for PCPs in later career stages. The 10% HPSA bonus payments are made to all physicians regardless of career stage, and the majority of PCPs in HPSA-designated counties in our sample are later-career PCPs. Thus, millions of dollars in bonus payments are spent on doctors who the empirical evidence suggests are unlikely to change their practice location in response to the program. The cost effectiveness of the HPSA bonus payment program may be improved by targeting the incentive payment exclusively to those who do respond, namely early-career PCPs.[25] In this case, even a bonus payment higher than 10% could result in a

[25] Note that these targeted groups can feasibly be identified by policymakers, as career stages are defined by readily observable physician characteristics: graduation date and age.

lower cost per additional PCP in shortage areas and an overall lower cost of the program.

To illustrate this, we walk through a simple policy analysis that compares the estimated cost effectiveness of the 10% bonus payment program to that of a hypothetical alternative program that offers larger bonus payments to only early-career PCPs. This exercise requires some caveats, as we make a handful of simplifying assumptions. Importantly, we assume that the entirety of the effect of HPSA designation on the stock of early-career PCPs stems from the bonus payments. However, other programs connected to HPSA designations as well as potential interactions between private insurance payments and HPSA status may contribute to the total incentives associated with designations.[26] We also focus just on the costs and effects of the program for PCPs, even though all physicians practicing in HPSAs receive the bonus payments. We make back-of-the-envelope calculations that take our point estimates at face value and assume that effects scale linearly with the size of the bonus payments. Our aim is to conduct a simple yet informative exercise that draws from our main findings to highlight policy implications.

Focusing on our analysis sample of 217 designated counties, in the year before treatment, the average designated county has 0.49 early-career PCPs and 3.15 later-career PCPs per 10,000. Taking the point estimates in Panel B of Table 3.2 at face value, the stock of early-career PCPs becomes 0.59 per 10,000 in the average post-treatment year while the stock of later-career PCPs remains unchanged. The claims data imply post-treatment bonus payments to PCPs totaling $226,900 per year per county, resulting in an annual cost of $2,268,600 per additional PCP per 10,000 in the average HPSA-designated county.[27]

[26]The 10% bonus payment is a salient and major incentive that impacts all doctors in HPSAs, and our estimates come from studying designations defined using CMS data on automatically-billed HPSAs. To the extent that official HPSA designations interact with other various government programs related to shortage areas though, there could be additional incentives for locating in a HPSA. For instance, most states maintain loan forgiveness programs for practicing in rural areas, some of which may use criteria related to official HPSA designations. (See Kulka and McWeeny (2019) for a more detailed discussion of state loan forgiveness programs.) Additionally, to the extent that private insurance companies follow the lead of Medicare (Clemens and Gottlieb 2017, Clemens et al. 2017) and offer bonus payments for providing services in shortage areas, the direct financial incentives for locating in a HPSA could be even greater.

[27]The figure of $2,268,600 per year for 1 additional PCP per 10,000 comes from dividing the average annual bonus payment at the county level ($226,900) by the average increase in early-career PCPs attributed to

Suppose instead that a 20% bonus payment is offered to all early-career PCPs who practice in a HPSA-designated county. The bonus payment would remain available to these PCPs as long as the county remains designated, while no bonus would be paid to PCPs who graduated from medical school more than 10 years before the time of designation. Assuming that the response scales linearly with respect to the size of the bonus payment, the stock of early-career PCPs would increase to 0.69 per 10,000 following treatment and the stock of later-career PCPs would remain constant at 3.15 per 10,000. So the new regime would be predicted to yield 0.20 additional PCPs per 10,000, but (according to the claims data) at a reduced total annual cost of $57,100 per county, or $285,600 per additional PCP per 10,000.[28] This amounts to nearly an eight-fold decrease in costs per PCP.

As explained above, we make several simplifying assumptions in arriving at these results. Most notably, if HPSA incentives other than the 10% bonus payments are contributing to the increase in early-career PCPs, we may be overestimating the reduction in costs per additional PCP that would result from altering the bonus payment program as described. Nonetheless, it seems likely that there is significant scope for reducing costs and improving the effectiveness of the bonus payment program by adjusting it to target the subset of physicians we find to be responsive to relocation incentives.

3.7 Conclusion

This paper studies how physician location decisions respond to 10 percent Medicare bonus payments for practicing in "shortage areas." We find that while the majority of primary

HPSA designation (about 0.1 PCPs per 10,000). Note that the MPUP dataset omits line items for services provided by an NPI to 10 or fewer beneficiaries in a given year, so all cost figures slightly understate the true totals.

[28] While this analysis assumes no effect of HPSA designation for later-career PCPs, note that the proposed regime of targeted 20% payments would result in increased cost-effectiveness even under less generous assumptions. For instance, we could assume a positive effect of 10% bonus payments on later-career PCPs of 0.26 PCPs per 10,000, which is the top of the 95% confidence interval on the point estimate for this career group. In this case the cost per an additional PCP per 10,000 under the standard 10% bonus payment program would be $630,200, still greater than the $285,600 under our proposed targeted 20% bonus payment program.

care physicians do not appear to respond to the policy, an important subset of doctors do respond. Designated counties, on average, experience an increase in the stock of early-career physicians that amounts to roughly 23% and corresponds to about 0.67 more doctors per county. Results indicate that this increase occurs rather quickly, is stable over time, and is driven by increases in counts of PCPs who attended ranked medical schools.

Our findings can inform policymakers tasked with alleviating physician shortages. Accounting for response heterogeneity by career stage of doctors might improve the cost-effectiveness of bonus payment programs. For instance, to avoid paying bonuses to infra-marginal physicians already located in shortage areas, an alternative program offered solely to physicians in the first 10 years of their career that pays an even greater bonus amount for Medicare procedures provided in HPSAs might attract more doctors and reduce costs.

3.8 Acknowledgements

Chapter 3, contains material that has been submitted for publication. Khoury, Stephanie, Leganza, Jonathan M., and Masucci, Alex. “Health Professional Shortage Areas and Physician Location Decisions.” The dissertation author was a primary investigator and an author of this material.

3.9 Figures and Tables

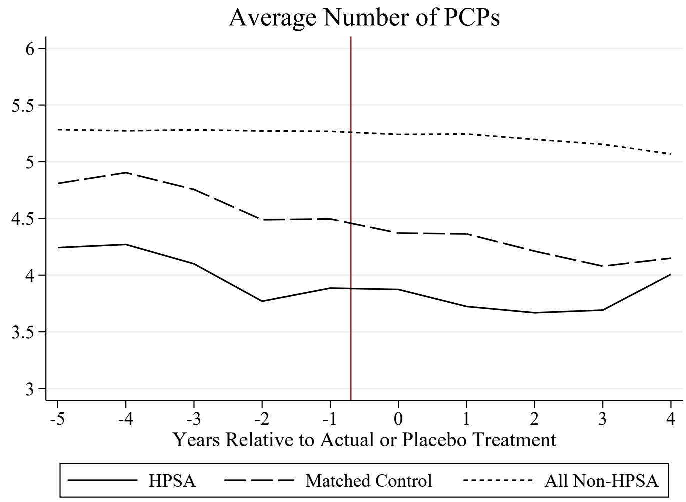

Figure 3.1: Average Number of PCPs for HPSA and Non-HPSA Counties

Notes: This graph plots the average number of PCPs per 10,000 population for treatment HPSA counties and potential non-HPSA control counties around actual or placebo designation year. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The matched control sample consists of the non-HPSA counties that are matched to HPSA counties using the method described in Section 3.4. The unmatched control sample consists of all counties that are never designated as a HPSA during 2012–2017, assigned as controls to and given placebo designation years from all counties in the treatment sample. Three control counties are matched to each treatment county, resulting in 217 treatment counties, 651 matched control counties (470 of which are unique), and 1,606 unmatched control counties. The x-axis shows the years relative to actual or placebo HPSA designation.

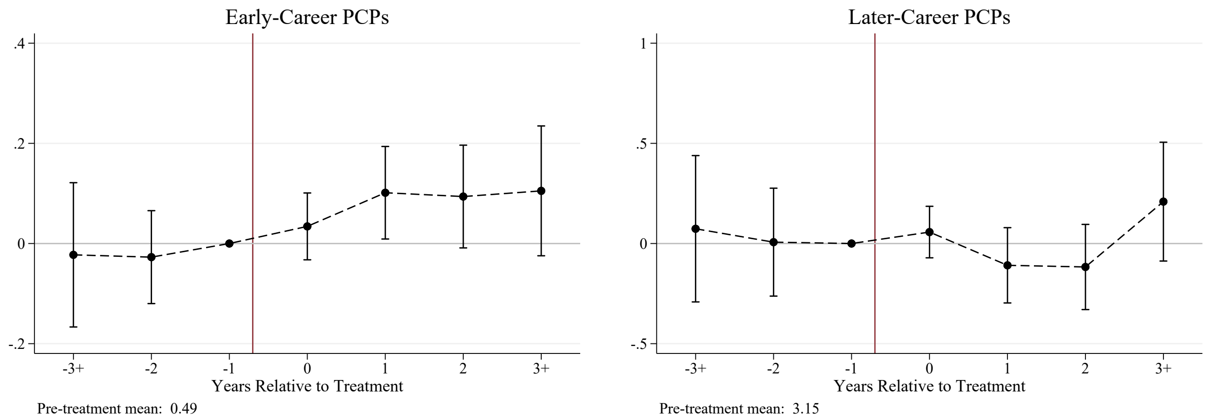

Figure 3.2: Impact of HPSA Designation on PCP Counts by Career Stage

Notes: These graphs plot the point estimates of the δ_τ's and their 95% confidence intervals from estimating equation (3.2), where the outcome y_{ct} is the stock of PCPs in the indicated career stage per 10,000 population in a county. Early-career PCPs are those graduating 5-10 years earlier and later-career PCPs are those graduating more than 10 years earlier. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The control sample consists of all counties that are never designated as a HPSA during 2012–2017 and are matched to the treatment counties using the matching procedure described in Section 3.4.1. Three control counties are matched to each treatment county, resulting in 217 treatment counties and 651 control counties. 470 of the 651 control counties are unique, as control counties can be matched to multiple treatment counties. The x-axis shows the years relative to HPSA designation. Controls for unemployment rate, median household income, and population at the county-year level are included in each regression.

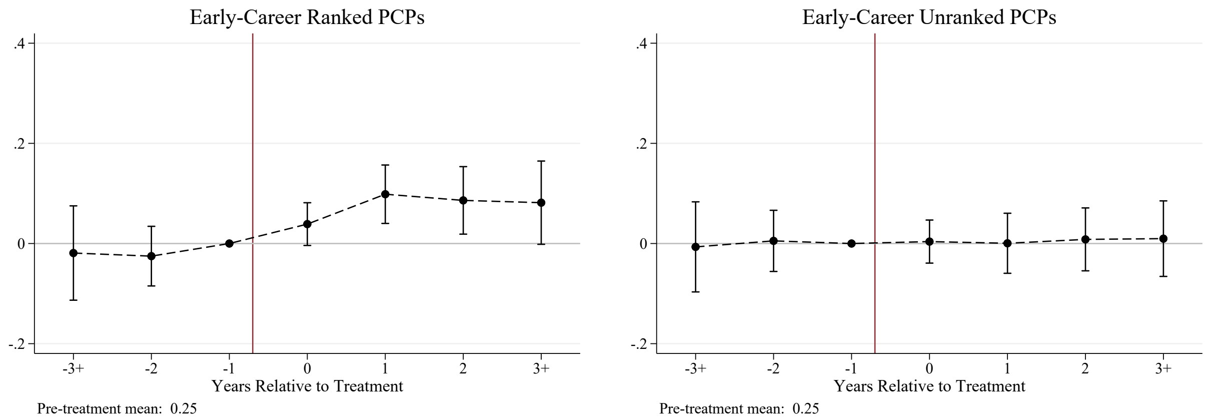

Figure 3.3: Impact of Designation on Early-Career PCP Counts by Medical School Rank

Notes: These graphs plot the point estimates of the δ_τ's and their 95% confidence intervals from estimating equation (3.2), where the outcome y_{ct} is the stock of early-career PCPs who attended ranked or unranked medical schools per 10,000 population in a county. Early-career PCPs are those graduating 5-10 years earlier. The 95 schools included in the 2018 U.S. News Primary Care medical school rankings are defined as ranked, and all other medical schools are defined as unranked. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The control sample consists of all counties that are never designated as a HPSA during 2012–2017 and are matched to the treatment counties using the matching procedure described in Section 3.4.1. Three control counties are matched to each treatment county, resulting in 217 treatment counties and 651 control counties. 470 of the 651 control counties are unique, as control counties can be matched to multiple treatment counties. The x-axis shows the years relative to HPSA designation. Controls for unemployment rate, median household income, and population at the county-year level are included in each regression.

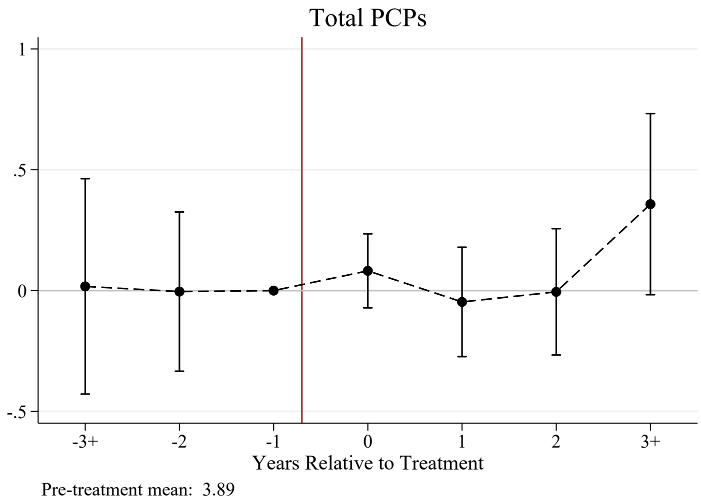

Figure 3.4: Impact of HPSA Designation on Total PCP Counts

Notes: This graph plots the point estimates of the δ_τ's and their 95% confidence intervals from estimating equation (3.2), where the outcome y_{ct} is the stock of PCPs per 10,000 population in a county. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The control sample consists of all counties that are never designated as a HPSA during 2012–2017 and are matched to the treatment counties using the matching procedure described in Section 3.4.1. Three control counties are matched to each treatment county, resulting in 217 treatment counties and 651 control counties. 470 of the 651 control counties are unique, as control counties can be matched to multiple treatment counties. The x-axis shows the years relative to HPSA designation. Controls for unemployment rate, median household income, and population at the county-year level are included in each regression.

Table 3.1: Summary Statistics for Descriptive Variables

	Treatment			Control		
	$\tau = -1$			$\tau = -1$		
	mean	min	max	mean	min	max
Physicians Per 10k	9.95	0.00	87.63	10.40	0.00	89.65
Percent Persons in Poverty	17.3	4.2	42.0	17.4	7.2	44.8
Population	58,969	690	1,265,111	67,568	589	1,919,402
Unemployment Rate	7.3	1.8	20.0	6.9	2.1	16.9
Median Household Income	44,479	22,834	86,703	44,161	23,837	110,843
Observations		217			651	

Notes: This table presents summary statistics for the analysis sample. Statistics are presented separately for the treatment group and the control group. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The control sample consists of all counties that are never designated as a HPSA during 2012–2017 and are matched to the treatment counties using the matching procedure described in Section 3.4.1. Three control counties are matched to each treatment county, resulting in 217 treatment counties and 651 control counties. 470 of the 651 control counties are unique, as control counties can be matched to multiple treatment counties. Data for each variable in the table is obtained for each county from the Area Health Resources File in the year before treatment for treatment counties and the year before the assigned treatment year for control counties. Physicians Per 10k (and its percentage change) and Percent Persons in Poverty are the variables used in the matching procedure to determine the closeness of eligible control counties to treatment counties.

Table 3.2: Impact of HPSA Designation on PCP Counts by Career Stage

	Early-Career PCPs	Later-Career PCPs
	(1)	(2)
Panel A. Split Post-Period		
$treat_c \times post_{ct}^{SR}$	0.0476	0.0349
	(0.0431)	(0.0947)
$treat_c \times post_{ct}^{MR}$	0.114**	-0.0091
	(0.0570)	(0.146)
Panel B. Pooled Post-Period		
$treat_c \times post_{ct}$	0.0968*	0.0040
	(0.0509)	(0.128)
Dep. Mean	0.49	3.15
Clusters	687	687
Observations	5208	5208

Notes: This table presents the point estimates of δ^{SR} and δ^{MR} from estimating equation (3.4) in Panel A, and the point estimate of δ from estimating equation (3.3) in Panel B, where the outcome y_{ct} is the stock of PCPs in the indicated career stage per 10,000 population in a county. Early-career PCPs are those graduating 5-10 years earlier and later-career PCPs are those graduating more than 10 years earlier. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The control sample consists of all counties that are never designated as a HPSA during 2012–2017 and are matched to the treatment counties using the matching procedure described in Section 3.4.1. Three control counties are matched to each treatment county, resulting in 217 treatment counties and 651 control counties. 470 of the 651 control counties are unique, as control counties can be matched to multiple treatment counties. Controls for unemployment rate, median household income, and population at the county-year level are included in each regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.3: Impact of Designation on Early-Career PCPs by Medical School Rank

	Early-Career Ranked PCPs (1)	Early-Career Unranked PCPs (2)
Panel A. Split Post-Period		
$treat_c \times post_{ct}^{SR}$	0.0507*	0.0045
	(0.0278)	(0.0264)
$treat_c \times post_{ct}^{MR}$	0.100***	0.0069
	(0.0361)	(0.0335)
Panel B. Pooled Post-Period		
$treat_c \times post_{ct}$	0.0873***	0.0063
	(0.0323)	(0.0299)
Dep. Mean	0.25	0.25
Clusters	687	687
Observations	5208	5208

Notes: This table presents the point estimates of δ^{SR} and δ^{MR} from estimating equation (3.4) in Panel A, and the point estimate of δ from estimating equation (3.3) in Panel B, where the outcome y_{ct} is the stock of early-career PCPs who attended ranked or unranked medical schools per 10,000 population in a county. Early-career PCPs are those graduating 5-10 years earlier. The 95 schools included in the 2018 U.S. News Primary Care medical school rankings are defined as ranked, and all other medical schools are defined as unranked. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The control sample consists of all counties that are never designated as a HPSA during 2012–2017 and are matched to the treatment counties using the matching procedure described in Section 3.4.1. Three control counties are matched to each treatment county, resulting in 217 treatment counties and 651 control counties. 470 of the 651 control counties are unique, as control counties can be matched to multiple treatment counties. Controls for unemployment rate, median household income, and population at the county-year level are included in each regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.4: Impact of HPSA Designation on PCPs by Medical School Rank

	Total PCPs	Ranked PCPs	Unranked PCPs
	(1)	(2)	(3)
Panel A. Split Post-Period			
$treat_c \times post_{ct}^{SR}$	0.0786	0.0381	0.0322
	(0.115)	(0.0917)	(0.0803)
$treat_c \times post_{ct}^{MR}$	0.121	0.163	-0.0106
	(0.180)	(0.136)	(0.118)
Panel B. Pooled Post-Period			
$treat_c \times post_{ct}$	0.111	0.131	0.0008
	(0.157)	(0.120)	(0.105)
Dep. Mean	3.89	1.89	1.88
Clusters	687	687	687
Observations	5208	5208	5208

Notes: This table presents the point estimates of δ^{SR} and δ^{MR} from estimating equation (3.4) in Panel A, and the point estimate of δ from estimating equation (3.3) in Panel B. The outcome y_{ct} is the stock of PCPs per 10,000 population in a county in column 1, and this outcome is split up into PCPs who attended ranked or unranked medical schools in columns 2 and 3. The 95 schools included in the 2018 U.S. News Primary Care medical school rankings are defined as ranked, and all other medical schools are defined as unranked. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The control sample consists of all counties that are never designated as a HPSA during 2012–2017 and are matched to the treatment counties using the matching procedure described in Section 3.4.1. Controls for unemployment rate, median household income, and population at the county-year level are included in each regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.5: Robustness of Medium-Run Estimates to Alternative Regression Specifications

	Early-Career PCPs (1)	Early-Career Ranked PCPs (2)	Early-Career Unranked PCPs (3)	Later-Career PCPs
A. Leading Specification	0.114**	0.100***	0.00694	-0.0091
	(0.0431)	(0.0361)	(0.0335)	(0.146)
B. Winsorize Less	0.115*	0.116**	0.0059	0.0485
	(0.0691)	(0.0529)	(0.0380)	(0.161)
C. Winsorize More	0.107*	0.0772***	0.0047	-0.0501
	(0.0490)	(0.0293)	(0.0288)	(0.136)
D. No Winsorizing	0.113	0.116**	-0.0027	0.0477
	(0.0712)	(0.0570)	(0.0418)	(0.168)
E. No Controls	0.111*	0.0988***	0.0025	-0.0134
	(0.0578)	(0.0360)	(0.0344)	(0.150)
F. More Controls	0.109*	0.0979***	0.0055	-0.0422
	(0.0553)	(0.0341)	(0.0327)	(0.144)
Clusters	687	687	687	687
Observations	5208	5208	5208	5208

Notes: This table presents point estimates of δ^{MR} from estimating equation (3.4) for the main outcomes as we vary the regression specification. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The control sample consists of all counties that are never designated as a HPSA during 2012–2017 and are matched to the treatment counties using the matching procedure described in Section 3.4.1. Three control counties are matched to each treatment county, resulting in 217 treatment counties and 651 control counties. 470 of the 651 control counties are unique, as control counties can be matched to multiple treatment counties. Row A reproduces our baseline estimates. Row B winsorizes outcome variables at the 99th percentile. Row C winsorizes outcome variables at the 90th percentile. Row D does not winsorize outcome variables. Row E drops controls from the regression. Row F adds year and state fixed effects to the regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.6: Robustness of Pooled Estimates to Alternative Regression Specifications

	Early-Career PCPs (1)	Early-Career Ranked PCPs (2)	Early-Career Unranked PCPs (3)	Later-Career PCPs
A. Leading Specification	0.0968*	0.0873***	0.00625	0.0349
	(0.0509)	(0.0323)	(0.0299)	(0.0947)
B. Winsorize Less	0.0969	0.0987**	0.0043	0.0604
	(0.0616)	(0.0478)	(0.0341)	(0.141)
C. Winsorize More	0.0902**	0.0659**	0.0048	-0.0368
	(0.0436)	(0.0261)	(0.0256)	(0.119)
D. No Winsorizing	0.0989	0.103**	-0.0041	0.0605
	(0.0641)	(0.0521)	(0.0374)	(0.147)
E. No Controls	0.0946*	0.0865***	0.0027	0.0000
	(0.0515)	(0.0322)	(0.0307)	(0.131)
F. More Controls	0.0924*	0.0851***	0.0052	-0.0233
	(0.0495)	(0.0306)	(0.0291)	(0.125)
Clusters	687	687	687	687
Observations	5208	5208	5208	5208

Notes: This table presents point estimates of δ from estimating equation (3.3) for the main outcomes as we vary the regression specification. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The control sample consists of all counties that are never designated as a HPSA during 2012–2017 and are matched to the treatment counties using the matching procedure described in Section 3.4.1. Three control counties are matched to each treatment county, resulting in 217 treatment counties and 651 control counties. 470 of the 651 control counties are unique, as control counties can be matched to multiple treatment counties. Row A reproduces our baseline estimates. Row B winsorizes outcome variables at the 99th percentile. Row C winsorizes outcome variables at the 90th percentile. Row D does not winsorize outcome variables. Row E drops controls from the regression. Row F adds year and state fixed effects to the regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.A Appendix: Additional Figures and Tables

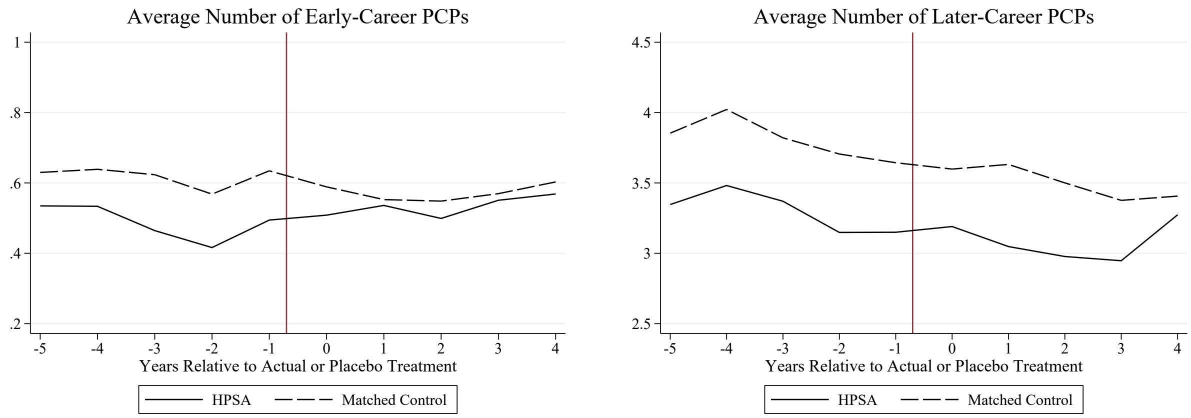

Figure 3.A.1: Average PCP Counts by Career Stage

Notes: These graphs plot the average number of PCPs in the indicated career stage per 10,000 population in a county in the sample of treatment HPSA counties and the non-HPSA control counties around actual or placebo treatment. Early-career PCPs are those graduating 5-10 years earlier and later-career PCPs are those graduating more than 10 years earlier. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The control sample consists of all counties that are never designated as a HPSA during 2012–2017 and are matched to the treatment counties using the matching procedure described in Section 3.4.1. Three control counties are matched to each treatment county, resulting in 217 treatment counties and 651 control counties. 470 of the 651 control counties are unique, as control counties can be matched to multiple treatment counties. The x-axis shows the years relative to HPSA designation.

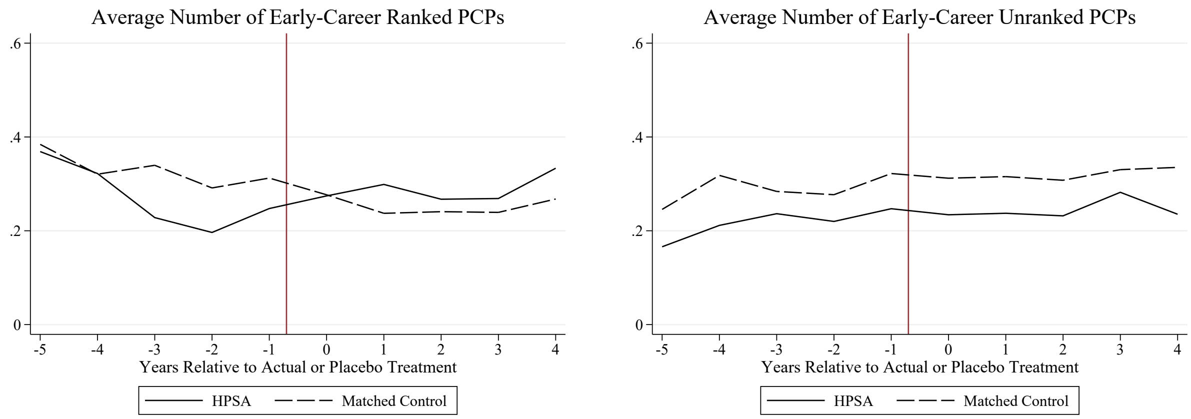

Figure 3.A.2: Average Early-Career PCP Counts by Medical School Rank

Notes: These graphs plot the average number of early-career PCPs who attended ranked or unranked medical schools per 10,000 population in a county in the sample of treatment HPSA counties and the non-HPSA control counties around actual or placebo treatment. Early-career PCPs are those graduating 5-10 years earlier. The 95 schools included in the 2018 U.S. News Primary Care medical school rankings are defined as ranked, and all other medical schools are defined as unranked. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The control sample consists of all counties that are never designated as a HPSA during 2012–2017 and are matched to the treatment counties using the matching procedure described in Section 3.4.1. Three control counties are matched to each treatment county, resulting in 217 treatment counties and 651 control counties. 470 of the 651 control counties are unique, as control counties can be matched to multiple treatment counties. The x-axis shows the years relative to HPSA designation.

Table 3.A.1: Dynamic Impact of Designations on PCP Counts by Career Stage

	(1) Early-Career PCPs	(2) Later-Career PCPs
$treat_c \times -5$	0.00993	0.0390
	(0.131)	(0.299)
$treat_c \times -4$	0.0368	0.0938
	(0.104)	(0.246)
$treat_c \times -3$	-0.0623	0.0764
	(0.0683)	(0.186)
$treat_c \times -2$	-0.0272	0.00735
	(0.0473)	(0.137)
$treat_c \times -1$	0	0
	0	0
$treat_c \times 0$	0.0342	0.0572
	(0.0340)	(0.0655)
$treat_c \times 1$	0.101**	-0.109
	(0.0471)	(0.0958)
$treat_c \times 2$	0.0938*	-0.117
	(0.0523)	(0.108)
$treat_c \times 3$	0.126*	0.145
	(0.0657)	(0.156)
$treat_c \times 4$	0.0747	0.398**
	(0.0770)	(0.190)
Clusters	687	687
Observations	5208	5208

Notes: This table presents the δ_τ point estimates from estimating equation (3.2), where the outcome y_{ct} is the stock of PCPs in the indicated career stage per 10,000 population in a county. Early-career PCPs are those graduating 5–10 years earlier and later-career PCPs are those graduating more than 10 years earlier. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The control sample consists of all counties that are never designated as a HPSA during 2012–2017 and are matched to the treatment counties using the matching procedure described in Section 3.4.1. Three control counties are matched to each treatment county, resulting in 217 treatment counties and 651 control counties. 470 of the 651 control counties are unique, as control counties can be matched to multiple treatment counties. Controls for unemployment rate, median household income, and population at the county-year level are included in each regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.A.2: Dynamic Impact of Designations on Early-Career PCPs by Medical School Rank

	(1) Early-Career Ranked PCPs	(2) Early-Career Unranked PCPs
$treat_c \times -5$	0.0536	-0.0282
	(0.0830)	(0.0713)
$treat_c \times -4$	0.0435	-0.00875
	(0.0751)	(0.0626)
$treat_c \times -3$	-0.0733*	0.00129
	(0.0426)	(0.0433)
$treat_c \times -2$	-0.0252	0.00529
	(0.0304)	(0.0311)
$treat_c \times -1$	0	0
	0	0
$treat_c \times 0$	0.0387*	0.00388
	(0.0217)	(0.0219)
$treat_c \times 1$	0.0984***	0.000520
	(0.0297)	(0.0306)
$treat_c \times 2$	0.0861**	0.00830
	(0.0343)	(0.0320)
$treat_c \times 3$	0.0789*	0.0286
	(0.0409)	(0.0403)
$treat_c \times 4$	0.0855	-0.0179
	(0.0520)	(0.0434)
Clusters	687	687
Observations	5208	5208

Notes: This table presents the δ_τ point estimates from estimating equation (3.2), where the outcome y_{ct} is the stock of early-career PCPs who attended ranked or unranked medical schools per 10,000 population in a county. Early-career PCPs are those graduating 5–10 years earlier. The 95 schools included in the 2018 U.S. News Primary Care medical school rankings are defined as ranked, and all other medical schools are defined as unranked. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The control sample consists of all counties that are never designated as a HPSA during 2012-2017 and are matched to the treatment counties using the matching procedure described in Section 3.4.1. Three control counties are matched to each treatment county, resulting in 217 treatment counties and 651 control counties. 470 of the 651 control counties are unique, as control counties can be matched to multiple treatment counties. Controls for unemployment rate, median household income, and population at the county-year level are included in each regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.A.3: Dynamic Impact of Designations on PCPs by Medical School Rank

	(1) Total PCPs	(2) Ranked PCPs	(3) Unranked PCPs
$treat_c \times -5$	0.0804	0.0154	0.145
	(0.360)	(0.280)	(0.275)
$treat_c \times -4$	0.115	0.0422	0.102
	(0.300)	(0.252)	(0.213)
$treat_c \times -3$	-0.0497	-0.0400	0.0287
	(0.224)	(0.175)	(0.147)
$treat_c \times -2$	-0.00367	0.00363	0.0234
	(0.168)	(0.124)	(0.105)
$treat_c \times -1$	0	0	0
	0	0	0
$treat_c \times 0$	0.0818	0.0361	0.0578
	(0.0780)	(0.0560)	(0.0520)
$treat_c \times 1$	-0.0469	0.0889	-0.0911
	(0.115)	(0.0792)	(0.0777)
$treat_c \times 2$	-0.00539	0.0773	-0.00838
	(0.133)	(0.0943)	(0.0906)
$treat_c \times 3$	0.262	0.246*	0.0833
	(0.186)	(0.127)	(0.121)
$treat_c \times 4$	0.498**	0.331*	0.171
	(0.236)	(0.170)	(0.141)
Clusters	687	687	687
Observations	5208	5208	5208

Notes: This table presents the δ_τ point estimates from estimating equation (3.2), where the outcome y_{ct} is the stock of PCPs per 10,000 population in a county in column 1, and this outcome is split up into PCPs who attended ranked or unranked medical schools in columns 2 and 3. The 95 schools included in the 2018 U.S. News Primary Care medical school rankings are defined as ranked, and all other medical schools are defined as unranked. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013-2017. The control sample consists of all counties that are never designated as a HPSA during 2012-2017 and are matched to the treatment counties using the matching procedure described in Section 3.4.1. Three control counties are matched to each treatment county, resulting in 217 treatment counties and 651 control counties. 470 of the 651 control counties are unique, as control counties can be matched to multiple treatment counties. Controls for unemployment rate, median household income, and population at the county-year level are included in each regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

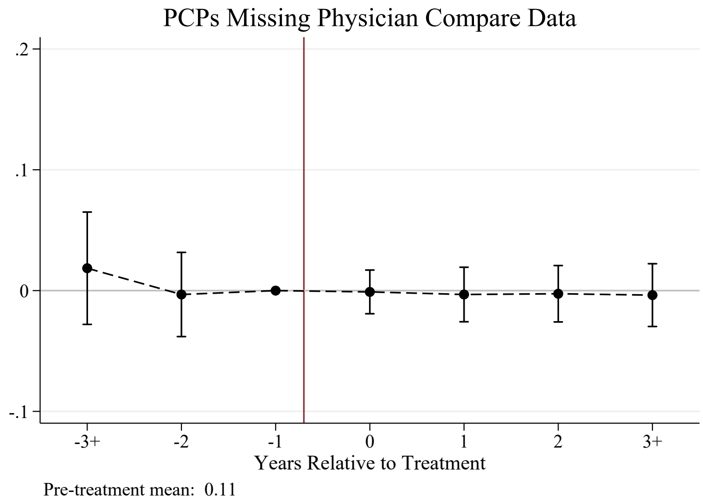

Figure 3.A.3: PCPs Missing Data Relative to Designation

Notes: This graph plots the point estimates of the δ_τ's and their 95% confidence intervals from estimating equation (3.2), where the outcome y_{ct} is the stock of PCPs per 10,000 population in a county that are missing data on graduation year or medical school from the Physician Compare dataset. Almost all PCPs missing data on one of these variables are also missing data on the other variable. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The control sample consists of all counties that are never designated as a HPSA during 2012–2017 and are matched to the treatment counties using the matching procedure described in Section 3.4.1. Three control counties are matched to each treatment county, resulting in a sample size of 217 treatment counties and 651 control counties. 470 of the 651 control counties are unique, as control counties can be matched to multiple treatment counties. The x-axis shows the years relative to HPSA designation. Controls for unemployment rate, median household income, and population at the county-year level are included in the regression.

3.B Appendix: Additional Robustness Checks

Table 3.B.1: Robustness to Partially Designated County Inclusion

	HPSA > 0% (1)	HPSA > 10% (2)	HPSA > 50% (3)	HPSA = 100% (4)
Panel A. Medium Run Estimates				
Early-Career PCPs	0.114**	0.0946	0.112	0.101
	(0.0570)	(0.0600)	(0.0688)	(0.0721)
Early-Career Ranked PCPs	0.100***	0.102***	0.110**	0.105**
	(0.0361)	(0.0386)	(0.0439)	(0.0454)
Early-Career Unranked PCPs	0.0069	-0.0114	-0.0108	-0.0099
	(0.0335)	(0.0349)	(0.0399)	(0.0425)
Later-Career PCPs	-0.0091	-0.0561	-0.108	-0.184
	(0.146)	(0.151)	(0.161)	(0.168)
Panel B. Pooled Estimates				
Early-Career PCPs	0.0968*	0.0789	0.0893	0.0779
	(0.0509)	(0.0537)	(0.0624)	(0.0655)
Early-Career Ranked PCPs	0.0873***	0.0902***	0.0947**	0.0893**
	(0.0323)	(0.0347)	(0.0400)	(0.0415)
Early-Career Unranked PCPs	0.0063	-0.0118	-0.0114	-0.0110
	(0.0299)	(0.0312)	(0.0356)	(0.0379)
Later-Career PCPs	0.0040	-0.0444	-0.0937	-0.161
	(0.128)	(0.132)	(0.142)	(0.148)
Obs.	5,208	4,728	3,696	3,312

Notes: This table presents the point estimates of δ^{MR} from estimating equation (3.4) in Panel A and the point estimates of δ from estimating equation (3.3) in Panel B, for the main outcome variables as we vary the definition of HPSA designation. The columns designate the level at which a county must be designated to be included in the treatment group as a HPSA. Column (1) reproduces our preferred definition of designation, which includes all partially designated counties as treated counties. Columns (2), (3), and (4) include as treatment counties those with at least 10 percent, 50 percent, and 100 percent of zip codes designated, respectively. Controls for unemployment rate, median household income, and population at the county-year level are included in each regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.B.2: Robustness to Number of Matched Control Counties

	$n_{control} = 1$ (1)	$n_{control} = 2$ (2)	$n_{control} = 3$ (3)	$n_{control} = 4$ (4)	$n_{control} = 5$ (5)
Panel A. Medium Run Estimates					
Early-Career PCPs	0.0932	0.107*	0.114**	0.0995*	0.0986*
	(0.0688)	(0.0613)	(0.0570)	(0.0560)	(0.0533)
Early-Career Ranked PCPs	0.101**	0.106***	0.100***	0.0946***	0.0925***
	(0.0415)	(0.0392)	(0.0361)	(0.0350)	(0.0336)
Early-Career Unranked PCPs	-0.00993	-0.00563	0.00694	0.00131	0.00276
	(0.0409)	(0.0359)	(0.0335)	(0.0333)	(0.0323)
Later-Career PCPs	0.118	0.0464	-0.00913	-0.0220	-0.0586
	(0.187)	(0.160)	(0.146)	(0.139)	(0.135)
Panel B. Pooled Estimates					
Early-Career PCPs	0.0768	0.0870	0.0968*	0.0839*	0.0832*
	(0.0614)	(0.0547)	(0.0509)	(0.0500)	(0.0477)
Early-Career Ranked PCPs	0.0858**	0.0901**	0.0873***	0.0833***	0.0809***
	(0.0371)	(0.0352)	(0.0323)	(0.0313)	(0.0301)
Early-Career Unranked PCPs	-0.0075	-0.0059	0.0063	0.0003	0.0014
	(0.0369)	(0.0320)	(0.0299)	(0.0296)	(0.0287)
Later-Career PCPs	0.0932	0.0512	0.0040	-0.0049	-0.0418
	(0.165)	(0.141)	(0.128)	(0.122)	(0.118)

Notes: This table presents the point estimates of δ^{MR} from estimating equation (3.4) in Panel A and the point estimates of δ from estimating equation (3.3) in Panel B, for the main outcome variables as we vary the number of controls matched to each treatment county. Column (3) reproduces our preferred matching procedure, in which we match 3 controls to each treatment county. Controls for unemployment rate, median household income, and population at the county-year level are included in each regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.B.3: Robustness to Match Variables

	(1)	(2)	(3)	(4)	(5)
Panel A. Medium Run Estimates					
Early-Career PCPs	0.114**	0.0961*	0.0996*	0.0556	0.0690
	(0.0570)	(0.0537)	(0.0597)	(0.0600)	(0.0503)
Early-Career Ranked PCPs	0.100***	0.0943***	0.0862**	0.0809**	0.0857***
	(0.0361)	(0.0344)	(0.0375)	(0.0373)	(0.0319)
Early-Career Unranked PCPs	0.0069	-0.0047	0.0116	-0.0249	-0.0208
	(0.0335)	(0.0321)	(0.0355)	(0.0352)	(0.0326)
Later-Career PCPs	-0.0091	-0.0539	-0.0761	-0.122	-0.103
	(0.146)	(0.142)	(0.146)	(0.146)	(0.137)
Panel B. Pooled Estimates					
Early-Career PCPs	0.0968*	0.0797*	0.0793	0.0441	0.0629
	(0.0509)	(0.0478)	(0.0534)	(0.0537)	(0.0449)
Early-Career Ranked PCPs	0.0873***	0.0802***	0.0719**	0.0724**	0.0735**
	(0.0323)	(0.0307)	(0.0336)	(0.0335)	(0.0287)
Early-Career Unranked PCPs	0.0063	-0.0031	0.0083	-0.0272	-0.0146
	(0.0299)	(0.0284)	(0.0314)	(0.0310)	(0.0285)
Later-Career PCPs	0.0040	-0.0417	-0.0569	-0.0953	-0.0842
	(0.128)	(0.124)	(0.128)	(0.127)	(0.120)
Match Variables					
# Physicians	✓	✓	✗	✗	✓
%Δ Physicians	✓	✗	✓	✗	✗
Poverty Rate	✓	✓	✓	✓	✗
%Δ Poverty Rate	✗	✗	✗	✗	✓
Geographic Region	✓	✓	✓	✓	✓

Notes: This table presents the point estimates of δ^{MR} from estimating equation (3.4) in Panel A and the point estimates of δ from estimating equation (3.3) in Panel B, for the main outcome variables as we vary the variables used in the matching procedure. Column (1) reproduces our preferred matching procedure, in which we match on the baseline variables corresponding to the level of total physicians, trends in total physicians, and the poverty rate. Column (2) does not match on the baseline trends in physician counts. Column (3) does not match on the baseline number of physicians. Column (4) excludes both baseline trends and numbers of total physicians from the match. Column (5) matches on the baseline number of total physicians along with a baseline trend in the poverty rate. Controls for unemployment rate, median household income, and population at the county-year level are included in each regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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