Health Professional Shortage Areas and Physician Location Decisions*

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Abstract

To address regional inequities in access to healthcare, the U.S. government designates primary care Health Professional Shortage Areas (HPSAs). Several programs use these designations to incentivize physicians to practice in areas of need, including a large program through which the Centers for Medicare and Medicaid Services (CMS) provides 10% bonus payments to physicians billing in HPSAs. We use data from CMS and a matched difference-in-differences design to estimate the causal effects of HPSA designations on physician location decisions. We find that designated counties experience an increase in the number of early-career primary care physicians. The increase is driven by physicians who attended ranked medical schools. In contrast, we find no evidence that physicians in later career stages relocate to shortage areas.

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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 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 policy makers alike: some areas have significantly fewer doctors per capita than other areas. Individuals living in so-called "shortage areas" may experience worse health outcomes. Evidence suggests that physician shortages are a key factor in explaining higher mortality rates in rural areas (Gong et al. 2019) and that vulnerable older Americans living in shortage areas are at an increased risk of experiencing preventable hospitalizations (Parchman and Culler 1999).

Consequently, policy makers concerned with regional inequities in health outcomes and unequal access to healthcare strive to identify areas with limited numbers of physicians per capita and to increase resources for residents of these areas. Primary care physicians are an important resource, as stronger primary care systems and primary care physician supply are associated with better population health (Starfield et al. 2005, Macinko et al. 2007). Accordingly, the Health Resources and Services Administration manages official designations of "primary care Health Professional Shortage Areas" (HPSAs) in order to improve access to primary care and incentivize physicians to practice in shortage areas. Notably, 37.7 million people currently live in a geographic area designated as a primary care HPSA (HHS 2023).

Several programs make use of official HPSA designations. One major program is the Health Professional Shortage Area Physician Bonus Program. Through this program, the Centers for Medicare and Medicaid Services (CMS) provides 10% bonus payments for Medicare services furnished by physicians in HPSAs. Other federal programs also use HPSA designations. The National Health Service Corps (NHSC) uses HPSAs to administer federal scholarship and loan repayment programs; a J-1 visa waiver program allows non-U.S. citizen foreign medical graduates to remain in the U.S. to practice in HPSAs; and CMS uses HPSA designations when determining eligibility for the Rural Health Clinic Program, which offers healthcare facilities a bundled payment for primary care services provided by physicians at qualified clinics. These programs are meaningful in size. For instance, while we are not aware of regularly published estimates of the expenditures on the bonus payment program, a recent report by the Government Accountability Office notes that total HPSA bonus payments in 2018 amounted to $145.5 million (GAO 2022). Moreover, funding for the National Health Service Corps has recently increased from $287 million in 2015 to $430 million in 2020 (GAO 2021), and in 2020 there were over 4,700 rural health clinics (CMS 2023). Together these programs, and the overarching federal policy of designating shortage areas, aim to incentivize physicians to practice in areas of need.

In this paper, we evaluate the overall impact of the primary care HPSA designation policy. We ask whether HPSA designations, and the package of program-based incentives that accompany them, 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 data from CMS using unique physician identifiers to create a county-level panel dataset that contains information on physician counts, 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 Resources File.[1] Using this panel 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 driven by 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 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. Our match is based on variables used by the Health Resources and Services Administration to quantitatively assess the severity of shortages. We then use a difference-in-differences framework to compare the stock of PCPs in HPSAs before and after official designations with that of the matched control counties.

Importantly, our data allow us to analyze physician responses separately by career stage. The relevance and strength of the practice location incentives attached to HPSA designations are likely to differ depending on how far along a given physician is in her career. In a broad sense, physicians in early stages of their careers, especially those making initial location decisions after completing residencies, are likely to face substantially lower costs

[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 (Boccuti et al. 2015).

of moving compared to physicians in later stages of their careers, who are more likely to have an established practice. Therefore, in the context of payment increases due to the bonus payment program, one might expect early-career physicians to be more responsive than later-career physicians. Moreover, some of the additional programs that make use of HPSA designations are likely to be more salient and relevant for early-career physicians. For instance, the NHSC scholarship and loan repayment programs may create especially strong incentives for early-career physicians, who might be looking to practice in HPSAs to fulfill scholarship-based service obligations after completing their training, or who might be more likely to have student loans than their later-career peers.

We begin by documenting the impact of designations on counts of PCPs. We find no statistically significant evidence that designations on average affect the total number of PCPs practicing in a county; however, we show that the estimated total impact masks substantial response heterogeneity across career stages. We find that designated counties experience a modest but meaningful increase in the number of early-career PCPs. In contrast, we find no evidence that designated counties experience an increase in the number of later-career PCPs, who make up the vast majority of primary care physicians and drive the estimates for total PCP counts.

The pattern of our dynamic difference-in-differences estimates for early-career PCPs shows a relatively quick rise in the count during the first two years of designation, which then stabilizes at a higher level. Our preferred estimate for quantifying the overall magnitude of the response thus captures the increase in physician counts over the "medium run," after allowing for the brief transition period revealed by the dynamics. Our leading estimate indicates that designated counties experience an increase of approximately 0.111 physicians per 10,000 residents on average, which roughly amounts to 0.65 physicians per county. This modest increase in early-career PCPs represents a 23% increase from a small baseline mean and reflects the fact that the HPSA program focuses on areas with low levels of physicians per capita, such that the increase in the number of PCPs could be quite meaningful for the community in need.

We then investigate which types of early-career PCPs respond to designations. Specifically, we leverage our data to explore whether physician responses differ based on where they attended medical school. Interestingly, we find that the increased counts of early-career physicians are entirely driven by those who attended ranked medical schools. That is, we find that HPSA designations lead to increases in the number of early-career PCPs who attended nationally ranked medical schools, but we find no evidence that designations impact the number of early-career PCPs from other schools. If one considers medical school rankings to proxy for physician quality, then our results indicate that HPSA designations attract early-career, high quality doctors to shortage areas.

Overall, our findings have direct implications for policy makers overseeing the HPSA designation program. First, while we are unable to pin down exactly which HPSA-based program is driving our results, the findings indicate that designations are working to some extent. It could be that our results are largely explained by early-career PCPs being more responsive to the bonus payment program, perhaps due to lower costs associated with relocation. It could instead be that our results are largely explained by the scholarship and loan repayment programs, which are more targeted towards early-career PCPs. Either way, our findings indicate that HPSA designations are attracting some primary care doctors to areas of need. Second, our analysis highlights how further targeting of incentives may be able to improve the cost effectiveness of shortage area programs. For example, under the current bonus payment program, all doctors billing to Medicare in HPSAs receive bonuses. Yet we have found no evidence that later-career PCPs, who make up the vast majority of total PCPs, respond to HPSA designations. This suggests that there may be scope to save costs by targeting payments towards early-career physicians and reducing payments to doctors we find to be inframarginal.

Our 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 key component of physician labor supply: practice location.

Our paper thus relates most closely to other work that investigates physician location decisions, especially in the context of physician shortages.[3] A vast literature in the medical

[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. (2020), 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).

[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). Additional related work studies provider location decisions in other contexts. Two recent papers set in the context of Medicaid expansions are Huh (2021), who finds that expansions can attract dentists to poorer areas, and Huh and Lin (2021), who find that expansions increase counts of obstetricians and gynecologists in urban areas. Another related paper is Polsky et al. (2000), who study how changes in health maintenance organization penetration influence physician decisions to relocate or leave patient care entirely.

and health fields has documented, described, and analyzed physician shortages across the globe, often emphasizing a variety of factors and physician characteristics that predict rural practice locations as well as related programs that might attract physicians to rural and remote areas.[4] Yet 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 related to ours provide new evidence on the topic. Zhou (2017), Falcettoni (2018), and Kulka and McWeeny (2019) 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.[5] 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. Ghosh (2021) also studies state and local loan forgiveness programs using a difference-in-differences design and finds that the programs can induce movement of physicians. We complement and contribute to this strand of the literature by offering causal evidence on the effectiveness of the large, nation-wide HPSA policy. Furthermore, in exploiting our data to study how responses vary by career stage, we 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 government and 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 in human capital and entrepreneurial capital (Clemens et al. 2020), and other papers highlight an important role for financial incentives in shaping the decision to become a doctor (Chen et al. 2020 and Gottlieb et al. 2020).[6] In finding that HPSA designations bring physicians to designated counties, we highlight how government policy can expand access to healthcare in specific geographies and influence the distribution

[4]See, for instance, Brooks et al. (2002) and Lehmann et al. (2008) for literature reviews and Brooks et al. (2003) or Kotzee and Couper (2006) for examples of studies that directly survey physicians about the issues.

[5]For earlier work modeling practice locations, see Hurley (1991), Bolduc et al. (1996), and Holmes (2005).

[6]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).

of health-sector human capital across space.[7]

The rest of this paper is organized as follows. Section 2 describes the policy environment. Section 3 overviews our data sources. Section 4 lays out our empirical strategy. Section 5 presents our results. Section 6 concludes.

2 Policy Environment

2.1 Health Professional Shortage Area Designations

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."[8] 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 several programs are based on this type of designation. 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 the United States, we restrict our attention to HPSAs designated for the primary care discipline. Because the data that we use come from the CMS bonus payment program, which only uses geographic designations, we are only able to study primary care HPSAs designated 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 area designation.

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 both to determine HPSA eligibility status and to calculate a score intended to quantify the severity of the shortage. The score is primarily determined by an

[7]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).

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

area's population-to-provider ratio. For instance, as a general benchmark, HRSA typically considers an area to have a shortage of providers if it has a population-to-provider ratio of 3,500:1 or more. However, the score 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.

Several programs make use of HPSA designations. The Health Professional Shortage Area Physician Bonus Program is a large and important program. Yet additional programs are also centered around HPSA designations. Overall, the designations themselves serve as a way for the federal government to identify areas in need of additional resources, and the HPSA-based programs are the means by which the government attempts to direct resources to these identified areas. Below we describe the relevant programs.

2.2 Health Professional Shortage Area Physician Bonus Program

The Centers for Medicare and Medicaid Services provides 10% bonus payments for Medicare services furnished by physicians in primary care geographic HPSAs designated by December 31 of the previous year. All physicians billing in HPSAs are eligible for the bonus payments, which provide a direct financial incentive to practice in shortage areas. Eligibility for payments depends only on the overall designation status of an area, and it does not depend on the score-based severity of the shortage.

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 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 (and because CMS relies primarily on their own list of HPSA ZIP codes), we use designations that result in automatically-billed HPSA ZIP codes as our source of variation.

2.3 Scholarship and Loan Repayment Programs

The National Health Service Corps (NHSC) maintains two federal programs, a scholarship program and a loan forgiveness program, that also make use of HPSA designations. The NHSC Scholarship Program provides students in primary care training with scholarships in exchange for commitments to providing primary care health services in an NHSC-approved site in a HPSA. The program provides up to four years of financial support, which includes payments for tuition and fees, payments for other education-related costs, and a modest monthly stipend. The service commitment is between two to four years, depending on how many years of financial support are received.[9] Full-time students who are U.S. citizens are eligible to apply for the program, which has traditionally had enough funds to award scholarships to only around 10% of new applicants.

The NHSC Loan Repayment Program offers opportunities for primary care physicians working in HPSAs to have their student loans repaid. The program offers loan repayment awards in exchange for two years of service. The full-time program awards participating physicians up to $50,000 in loan repayments, and a part-time version of the program awards physicians up to $25,000 in loan repayments. Physicians may then have the option of continuing their service, once their initial contract has ended, in exchange for an additional loan repayment, subject to administrator discretion and the availability of funds. To be eligible, physicians must work at an NHSC-approved site in a HPSA. Physicians apply to the program, and awards are handed out according to the availability of funds and a priority structure.[10] We note that nurse practitioners and physician assistants are also eligible for both the NHSC scholarship and loan repayment programs, which is a policy feature that we will revisit in an extension of our main analysis.

In addition to these national programs, individual states, localities, and other entities can administer additional scholarship and loan repayment programs. Some of these programs can make use of HPSA designations for determining eligibility criteria. For example, the Indiana Primary Care Scholarship Program is sponsored by the Indiana University School of Medicine and offers scholarships to Indiana students committed to practicing primary care in shortage areas. Similarly, the California State Loan Repayment Program offers assistance with loan repayment for primary care physicians practicing in HPSAs in California.[11] The existence of programs like these two can create additional incentives to locate in shortage areas.

[9] The full-time service commitment is two years for those who receive one or two years of financial support, it is three years for those who receive three years of support, and it is four years for those who receive four years of support. A half-time option with years of service obligations that are doubled is also available.

[10] To determine priority, the program uses previous participation in the NHSC Scholarship Program, HPSA score severity, and then other applicant characteristics, such as training, experience, and the likelihood of remaining in a HPSA.

[11] For an overview of these specific programs, see information collected for the Rural Health Information Hub: https://www.ruralhealthinfo.org/funding/3521 and https://www.ruralhealthinfo.org/funding/3293.

2.4 J-1 Visa Waiver Program

The U.S. administers a J-1 nonimmigrant visa exchange visitor program, which allows individuals to visit the United States for a defined time period. J-1 visas are often used by visitors obtaining medical training in the U.S., and it is common for visitors to face a two-year home residency requirement after finishing their visit. However, it is possible to obtain a J-1 visa wavier that eliminates the two-year home residency requirement, which permits the visitor to remain in the U.S. The U.S. Department of Health and Human Services (HHS) manages the visitor program related to health research and clinical care. Physicians holding J-1 visas can obtain a J-1 visa waiver in exchange for delivering healthcare services for three years in a primary care HPSA. Formally, interested agencies submit an application to HHS. HHS can then submit an approval recommendation to the U.S. Department of State, who can then submit an approval recommendation to the U.S. Citizens and Immigration Service, who officially grants the waivers.

2.5 Rural Health Clinic Program

The Rural Health Clinic program certifies clinics located in rural areas to receive enhanced Medicare and Medicaid reimbursement. In exchange for providing outpatient primary care services in rural areas, Rural Health Clinics (RHCs) are eligible for an all-inclusive reimbursement rate, based on their costs per patient, that can result in greater than usual reimbursement rates. To be certified as an RHC, a clinic must be located in a non-urban area, and it must be located in a shortage or underserved area. An RHC must also employ either a nurse practitioner or a physician assistant. Being located in a geographic primary care HPSA satisfies the shortage/underserved area requirement.

2.6 Affordable Care Act Programs

The Patient Protection and Affordable Care Act (ACA) established programs that target physician supply, some of which either directly use or are related to HPSA designations. One direct incentive involves the creation of a loan forgiveness program for physicians with pediatric specialties who provide care in a HPSA. As our data consist of Medicare-billing physicians, we may not be ideally situated to pick up effects due to this incentive, although we do include physicians with pediatric specialties in our main analysis of primary care physicians. Another direct incentive comes from the creation of an additional 10% bonus payment program for general surgeons who perform certain surgeries in a HPSA. Although we are focused on primary care physicians, in an extension we investigate the impact of designations on physicians in other specialties, where this additional incentive for surgeons is relevant. The ACA also aimed to strengthen existing programs that use HPSA designations with funding. For example, the ACA permanently authorized funding for the NHSC program and it established grants for rural health clinics.

Finally, it is worth noting that the ACA created the Primary Care Incentive Payment Program, which from 2011 through 2015 provided physicians who billed predominantly primary care services with 10% bonus payments. While these bonus payments were not tied to HPSA designations, we note that the first few years of our study period are years during which these other bonus payments were taking place.

2.7 Summary of Incentives

The combination of these programs means that the primary care geographic HPSA designations that we study represent a bundled treatment. The automatic bonus payments from CMS through the Health Professional Shortage Area Physician Bonus Program provide a major direct financial incentive to all physicians. However, the other more-targeted programs also use primary care geographic HPSA designations and provide additional incentives. For instance, the NHSC programs are likely to provide stronger incentives for early-career PCPs. We will not be able to disentangle the effects of one program from another with our data.

Moreover, a related and important contextual issue to keep in mind is that the data we use to define HPSA designations come from the CMS bonus payment program. We discuss the data in more detail below, but this means that we only study geographic primary care HPSAs that result in automatic bonus payments, as these are the designations that we see in these data. We are not able to study population HPSAs and facility HPSAs. We are also unable to study geographic HPSAs that do not result in an automatically-billed HPSA ZIP code, which can happen if, for example, a designation is made for a geographic unit (say, a township) and there are no ZIP codes that are entirely contained within that township. The bonus payment program uses only geographic HPSAs, but the other programs generally make use of population or facility HPSAs as well.

How prevalent are these other types of designations? Scannell et al. (2021) study the evolution of HPSA designations and show that most counties in the U.S. that are not fully designated as HPSAs are partly designated in some capacity, defined as containing at least one of a population HPSA, a facility HPSA, or a sub-county level geographic HPSA. They also show that the number of these partly-designated counties has increased due to the Shortage Designation Modernization Project of 2014, which aimed to streamline the designation process. Their results indicate that many counties in our difference-in-differences analysis (both treatment and control) are quite likely to have either a population HPSA, a facility HPSA, or some smaller, sub-county geographic unit designated as a HPSA, throughout our analysis time horizon.

These factors mean that we use our difference-in-differences setup to study specifically how primary care geographic HPSA designations that result in automatic bonus payments impact physician counts. We do so under the current policy environment, where there exist several HPSA-based programs and where some sub-areas of counties in our analysis are already exposed to other types of designations. The specific designations that we study reflect an increase in the incentives to locate within a county (our treatment group consists of counties that previously contained no automatically-billed HPSA ZIP codes), but the presence of these other types of designations makes it difficult to quantify the magnitude of the increase in incentives in terms of the overall exposure to any type of HPSA status. We provide more context on these issues when interpreting our results in Section 5, after discussing our data and empirical strategy in detail.

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 overview the data sources, highlight our approach to creating the county panel, and discuss key variables for our analysis. Appendix B provides additional details.

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 level from 2012 to 2017.[12] 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

[12]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.

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 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, which is central to our analysis. We use the NPPES data to consistently define physician location in each year t as their primary location in the NPPES data as of December of year t.[13] Physicians are first recorded in the NPPES data when they apply for and are assigned an NPI, which is required of health care providers to begin billing and for use in electronic transfers of information. In the application process, physicians self-report their information, including practice location, and CMS attempts to verify social security numbers and that the address provided is valid (Bindman 2013). Physicians are then responsible for updating their information.

The updating of addresses is especially important for our analysis, as we are interested in capturing both initial location decisions of physicians starting their careers and relocation decisions. To the extent that addresses are stale or updated infrequently, we will have challenges picking up the effects of HPSA designations on movement of established physicians from one location to another, which would limit our ability to detect effects for physicians later in their careers. Importantly, CMS requires providers to update their NPPES information, including practice location, within 30 days of a change (CMS 2004). While it is not clear how well physicians comply with this policy, some research has investigated physician location information across datasets and has shown the NPPES data to compare favorably. DesRoches et al. (2015) compare NPPES data to data from the American Medical Association Masterfile and to data from the SK&A physician file. The authors verify physician information with phone calls and find that the NPPES data had the highest rates of correct address information, with especially high rates for physicians in internal medicine. They note that there are incentives to keep this information updated because NPPES addresses are used for billing. They conclude that the NPPES data has better coverage and "appears to provide accurate, up-to-date contact information for physicians billing public and private payers." Even so, the data is not perfectly accurate. We stress that the presence of stale addresses should have a larger impact on our ability to measure effects on later-career physi-

[13]The MPUP data itself contains information on practice location, which comes from the NPPES data that we use, but the variable in the MPUP dataset is updated to capture the location of physicians in the subsequent calendar year of NPPES data, rather than the year of observation.

cians than early-career physicians, but overall we view the relative strength of the NPPES data against the alternatives as encouraging.

Linking information from the MPUP and NPPES datasets yields panel data for 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 graduation dates and medical school attended, which allows us to analyze physician 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 HPSA policy 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.

The main drawback of the Physician Compare dataset is 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 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).[14] 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 A.1.

We merge together these three physician-level datasets and then aggregate the data up to the county level by simply counting the number of physicians in each county in each year. After doing this, we are left with a county-level panel dataset that contains various counts of primary care Medicare-billing physicians and spans the years 2012 to 2017. Below, in the

[14]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.

key variables section, we describe explicitly the types of physician counts that we study.

Finally, into our newly-constructed panel we merge data 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 use variables from the *Area Health Resources File* (AHRF), which contains a range of county-level, health-related variables derived from the American Medical Association Masterfile and county-level demographic and economic variables derived from the American Community Survey. Linking together all of the data sources, we are left with a county panel containing information on population demographics, economic conditions, HPSA designations, and counts of Medicare-billing primary care physicians.

3.2 Key Variables

The main outcome variables of interest for our analysis are per capita counts of primary care physicians. We focus on PCPs by studying doctors with specialties of "family practice," "general practice," "internal medicine," "geriatric medicine," or "pediatric medicine," which make up the typical primary care specialties. We analyze the evolution of total PCP counts in counties across time, but we also break down the total stock into counts by career stage. In any given year, we define early-career PCPs 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 define mid-career physicians as those graduating medical school between 10 and 30 years ago, and we define late-career physicians (some of whom could respond to designations without relocating by delaying retirement) as those graduating medical school more than 30 years ago.

We also analyze physician counts by medical school ranking. 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 by medical school ranking to roughly proxy for physician quality. 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,

[15] In any given year, the data contain a 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, likely due to time spent in residency without an NPI.

we use the 2018 rankings of medical 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. One disadvantage of the Physician Compare data used to define these counts of physicians is that many doctors report "Other" as their medical school. In our sample, about 36% of PCPs report "Other." We classify these physicians as unranked in our leading analysis. However, we note that this category might be composed largely of physicians from international medical schools, so in the appendix we analyze separately counts of physicians who report "Other" and counts of physicians who report a named medical school that is not on the list of ranked medical schools. We find that including "Other" physicians in our definition of unranked physicians does not impact the results.

We use several additional variables in our matched difference-in-differences design. In particular, we define our treatment variable based on whether or not a county contains at least one automatically-billed designated HPSA ZIP code.[16] 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. We employ three more variables from the AHRF specifying the population, unemployment rate, and median household income of counties as control variables in a robustness check. In Section 4, we describe specifically how these variables enter our design.

4 Empirical Strategy

Our goal is to estimate the causal effect of HPSA designations on PCP counts. An ideal experiment would randomly assign HPSA designations to some counties and track the counts of physicians in these counties compared to the undesignated counties. While we cannot run such an ideal experiment, we can estimate causal effects by using a difference-in-differences framework to compare outcomes for a treatment group of counties that become designated to counterfactual outcomes derived from a control group of counties that are not designated.

Our setting presents two main challenges for a difference-in-differences framework. First, there is a conceptual challenge related to which counties should serve as control counties. A potentially-naive framework would compare all designated counties (i.e., the treatment group) to all counties that are not designated (i.e., the control group). Such a comparison

[16]While some counties are only "partially" designated, meaning only some of their ZIP codes are automatically billed HPSAs, the majority of designated counties in our sample are fully designated. There are 75 (35%) partially designated counties in our analysis data. We assess the robustness of our results to the exclusion of partially designated counties in Section 5.5.

is not without problems though, as counties designated as HPSAs are likely quite different in observable and unobservable ways from counties that are not designated. Indeed, we show later that physician counts in this potentially-naive control group of counties trend quite differently from physician counts in counties that become HPSAs, which raises concerns about the validity of an estimator that uses all non-designated counties as a control group. Second, there is an implementation challenge related to the fact that not all HPSA designations in our data occur at the same time. Recent work has shown that traditional two-way fixed effect regression models can result in biased estimates when there is staggered adoption of treatment and when treatment effects are heterogeneous, as the regressions can incorporate problematic comparisons that use already-treated groups as control groups (e.g. Goodman-Bacon 2021). For this reason, we need to use an approach that accounts for issues associated with staggered adoption, and we need to avoid using a traditional two-way fixed effect model applied to our entire county panel dataset.

To overcome these two challenges, we use a matching procedure to select a control group of counties that are similar to HPSA counties, and we use a "stacked" difference-in-differences framework (similar to, e.g., Cengiz et al. 2019 or Deshpande and Li 2019) to ensure that we are not using a regression framework that results in problematic comparisons and biased estimates. Specifically, we define and study designation events for the 217 different counties that we see become designated as HPSAs during the time horizon of our data. For each of these different designation events, we create a mini dataset that consists of the designated treated county and a matched group of clean control counties who are never designated during our analysis time horizon. We append these mini datasets together to create one stacked dataset, which we then analyze using simple difference-in-differences estimating equations.

4.1 Matched County Design

First, we describe our approach to selecting counties to serve as control counties. We use a matching procedure based on Deryugina et al. (2018), who study the long-run effects of Hurricane Katrina, to match to each individual treated county three control counties. We assign the matched control counties a placebo designation year equal to the actual designation year of their corresponding treated county.

To select the three control counties for each designated county, we use as our set of matching variables $\mathbf{X}_{ct}$ three variables defined over a baseline time period: number of active physicians per capita, annual percentage change in active physicians per capita, and percent of the population below the federal poverty line. We match on these variables (from the

AHRF) defined during 2010 and 2011, which are baseline years that predate our analysis sample time horizon and correspond to two or three years before 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 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 only of clean 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 to 2011.[17] That is,

$$\text{Closeness}(c^*, c) = \sum_{t=2010}^{2011} \sum_{x_{ct} \in \mathbf{X}_{ct}} \left(\frac{x_{ct} - x_{c^*,t}}{\sigma_{x_t}} \right)^2 . \tag{1}$$

In addition to the variables included in the closeness measure, we match on region, given that the existing literature indicates that geography influences physician residential choices (Burfield et al. 1986, Chen et al. 2010). Specifically, we define four regions corresponding to the Northeast, South, Midwest, and West, and we stipulate that a designated county can only be matched to control counties that are in its geographic region. The three counties from the pool of potential controls with the smallest values of the closeness measure for a given treatment county are included in the control group with placebo designation years equal to the actual designation year of the treatment county to which they are matched.

We show that our results are robust to changing the matching procedure in Section 5.5. Importantly, we find similar results when we use an alternative matching strategy that does not match on baseline physician counts and trends in physician counts, but rather matches only on area characteristics that are less directly linked to our outcomes of interest.

4.2 Analysis Sample

To construct our analysis sample, we start with the 217 counties that we see become designated between 2013 and 2017. We observe 96 designations in 2013, 45 designations in 2014, 39 designations in 2016, 12 designations 2016, and 25 designations in 2017. For each

[17]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.

of these 217 designation events, we use the matching method described above to generate a mini dataset that contains observations of the designated county and the three matched control counties that come from the sample of counties that are never designated as HPSAs between 2012 and 2017. We note that counties are allowed to be matched to more than one treatment county and are thus allowed to be a part of more than one designation event. After creating our 217 mini difference-in-differences datasets, we append them to arrive at our stacked analysis sample. The resulting sample includes county-year observations of the 217 counties that are designated and that make up the overall treatment group, as well as county-year observations of the 651 matched clean control counties that are never designated and that make up the overall control group. Note that because we allow matched control counties to be part of more than one designation event, the stacked dataset contains multiple copies of some of the observations of control counties. Of the 651 matched control counties that appear in the analysis sample, 470 are unique.[18]

Figure 1 provides a map of our analysis sample and illustrates the geographic variation in HPSA designation status that we study. Most of the counties designated over our time period are in the Midwest or the South. Just under 60% of counties in our treatment group are located in either Georgia, Indiana, Iowa, Kentucky, Missouri, Ohio, or Texas. Non-HPSA counties from these states make up a good deal of the control group as well, but states such as Kansas, Nebraska, and Oklahoma are also well-represented in our sample of control counties.

The map shows that treatment and control counties are often in close proximity. This setup has a number of advantages from an empirical perspective and, as we show below, performs well when assessing parallel pre-trends in outcomes. However, one drawback is that our design will overstate the magnitude of the impact of HPSA designations if designations induce physicians who would have otherwise located in one of the control group counties to instead move to one of the treatment group counties, because the estimates measure changes in the differences in physician counts between the treatment group and the control group. Alternatively, our design will not overstate magnitudes if designations induce physicians who would have otherwise located in counties outside of our quasi-experiment to locate in treatment counties. Because our analysis set is reasonably small compared to the full set of places that physicians could choose to locate, we expect any resulting bias to be minor; nonetheless, in the robustness section we make adjustments to our control group to attempt to mitigate this potential issue, and we find results similar to our leading approach.

[18] 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 counties that are always designated and study only designated counties for which we see the year before and year of designation.

Table 1 presents summary statistics for the treatment and matched control groups. The table reports means and standard errors estimated during the year preceding actual or placebo designation. We test for differences in means between the two groups. There are no statistically significant differences in means of variables that we use in our matching procedure or in means of other county characteristics (current population, median household income, and the unemployment rate). However, after matching we still see that HPSAs have on average lower counts of primary care physicians (per 10,000 residents at baseline), which comprise our main outcome variables.[19]

Figure 2 builds on this assessment of our matching procedure with a graphical examination. Consider panel A. The solid line depicts the average total count of PCPs in HPSAs, where time on the horizontal axis is relative to designation year. The stock of physicians in HPSA counties declines leading up to the designation year, as expected. In contrast, the dotted line depicts the average total count of PCPs for the potentially-naive control group that consists of all other counties. Relative time for this comparison group is defined by matching to each HPSA all other counties (instead of only counties selected by our matching procedure), 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 does not trend in parallel with HPSAs before designations. Finally, the dashed line plots the stock of physicians over time for our matched control group. After matching, we have a control group of counties that, while still different in levels, trends in parallel with HPSAs over the pre-period.[20]

[19]For additional context, Appendix Table A.1 presents summary statistics for the sample of counties that are always designated throughout our time period, which are not used in our analysis. The HPSA counties that we study look broadly similar to the always-designated counties, although the always-designated sample contains some outliers that have especially large populations, such as Los Angeles County in California and Cook County in Illinois.

[20]Here we note that we lack data on amenities, an important factor in location choice. Certain amenities are likely correlated with household income, which we show to be similar across our treatment and matched control groups. While other amenities, such as weather or coastal proximity, might not be as correlated with income, we also match on geography, making it likely that our treatment and control counties are, at least to some extent, broadly similar in terms of amenities based on climate and geography. Finally, we show evidence in support of the parallel trends assumption, so for amenities to bias our estimates, it would need to be the case that amenities change differentially across treatment and control groups right around the timing of designations. For some reference, Appendix Figure A.2 analyzes the evolution of county characteristics (which we use as control variables in a robustness check) around HPSA designation events, using the dynamic difference-in-differences specification described below, and shows that these characteristics are not changing differentially around designations.

4.3 Difference-in-Differences Estimating Equations

To analyze the effect of designations, we use standard difference-in-differences estimating equations applied to our stacked dataset. Specifically, to document the dynamic impacts, we first estimate event study equations of the form:

$$y_{ctd} = \alpha + \beta treat_{cd} + \sum_{\tau \neq -1} \gamma_\tau 1[ETime_{td} = \tau] + \sum_{\tau \neq -1} \delta_\tau (treat_{cd} \times 1[ETime_{td} = \tau]) + \varepsilon_{ctd}, \tag{2}$$

where c indexes county, t indexes calendar year, d indexes the mini dataset and designation event to which the observation corresponds, y_{ctd} is an outcome for county c in year t as a part of designation event d (e.g., the number of Medicare-billing PCPs per 10,000 county residents at baseline), $treat_{cd}$ is an indicator that county c in designation event d is a treated county, $ETime_{td}$ is the number of years relative to (actual or placebo) HPSA designation for (treated and control) counties that are part of designation event d, and ε_{ctd} is an error term. 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 time period. 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 extend through 2017; however, we pool together observations three or more years away from designation due to low observation counts.

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 (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 also allows us to evaluate how the stock of doctors evolves over time, during the post-period. That is, results from estimating equation (2) shed light on how immediate or delayed, as well as how persistent or temporary, any physician responses to designations might be. We allow these dynamics to guide us when quantifying the overall magnitudes and assessing the statistical significance of our results. Specifically, we make use of two additional estimating equations to capture the mean treatment effect of designations. We estimate a standard pooled difference-in-differences equation,

$$y_{ctd} = \alpha + \beta treat_{cd} + \gamma post_{td} + \delta(treat_{cd} \times post_{td}) + \varepsilon_{ctd}, \tag{3}$$

where $post_{td}$ is an indicator that equals one if year t is a post-designation (or post-placebo-designation) year for designation event d, and δ is the parameter of interest. We also estimate an equation that splits the post-period into two periods, a short-run period and a medium-run period. That is, we estimate

$$\begin{aligned} y_{ctd} = \alpha + \beta treat_{cd} &+ \gamma^{SR} post_{td}^{SR} + \gamma^{MR} post_{td}^{MR} \\ &+ \delta^{SR}(treat_{cd} \times post_{td}^{SR}) + \delta^{MR}(treat_{cd} \times post_{td}^{MR}) + \varepsilon_{ctd}, \end{aligned} \tag{4}$$

where $post_{td}^{SR}$ is a (post-period short-run) indicator that equals one if year t for designation event d is in the year of the designation, and $post_{td}^{MR}$ is a (post-period medium-run) indicator that equals one if year t for designation event d is after the immediate year of designation. Our decision to estimate equation (4) is informed by the dynamic estimates, which indicate that quantifying the overall impact of designations is best done by analyzing the counts of physicians in a county after allowing for the stock to evolve over a brief, short-run, transition period. We thus prefer "medium-run" estimates from equation (4) when summarizing the overall impact of HPSA designations, although we report standard "pooled estimates" from equation (3) as well.

We use these simple regressions as our primary specifications. In the robustness section, we explore the sensitivity of our estimates to alternative versions of the estimating equations. Specifically, we add county-year control variables to the regressions, we add year fixed effects to control for national-level developments that may influence physician location decisions, and we use more flexibly-controlled versions of our estimating equations by replacing the treatment county indicators with county-by-designation-event specific fixed effects.

5 Results

5.1 Impact of HPSA Designations on Primary Care Physician Counts

We begin by analyzing raw means of key outcome variables. Figure 2 plots average PCP counts for HPSAs and non-HPSAs, around the time of actual or placebo designation years. These plots of means provide an initial gauge for the impact of HPSA designations on PCP counts. Panel A illustrates the evolution of total PCP counts. Comparing the HPSA counties depicted by the solid line to the matched control counties depicted by the dashed line, we see little evidence that designations impact the total number of PCPs practicing in a county. However, panels B, C, and D highlight that the total counts mask substantial response heterogeneity. The panels generally illustrate that while the parallel trends assumption for counts of physicians by career stages does not hold when comparing HPSA counties to all non-HPSA counties, the assumption does hold when comparing HPSA counties to their matched controls. Panel B then shows that, after designation, average counts of early-career PCPs in HPSAs increase relative to the matched control group, whereas panels C and D show that, even after designation, the average counts of mid-career and late-career PCPs in HPSAs seem to track the counts in the control group. Because the majority of doctors are not early-career doctors, later-career doctors drive the patterns for the total counts. These initial graphs emphasize the importance of analyzing physicians separately by career stage.

To quantify relevant magnitudes and assess the statistical significance of responses to HPSA designations, we move away from the graphs of raw means, and we use our matched difference-in-differences design. Recall that we study a binary treatment by defining a county as a treated HPSA if it contains at least one automatically-billed HPSA ZIP code. While most of our treatment counties are fully designated, some are only partially designated in the sense that they contain other ZIP codes that are not automatically-billed HPSAs. To help with interpreting the reduced form results that follow, Figure 3 graphically presents a "first stage." That is, the graph illustrates exposure to treatment by plotting point estimates from estimating equation (2) on the percent of ZIP codes within a county that are designated as automatically-billed HPSAs.

The graph shows a mechanical increase in HPSA exposure around the time of designations as we have defined them. The fact that the increase in the percentage of designated ZIP codes remains roughly constant over time means that treatment intensity does not seem to fluctuate much on average, which should be kept in mind when interpreting the patterns of the dynamic treatment effects. The fact that the average percent of HPSA ZIP codes for treatment counties compared to control counties elevates to 77%, instead of 100%, reflects the presence of some partially designated counties. This fraction can be helpful for interpreting our results discussed below. Specifically, we report "reduced form" estimates that capture the impact of any automatically-billed HPSA ZIP code within a county on the counts of PCPs, but these estimates do not take into consideration the partially-designated counties and are therefore likely underestimating the effects of designation to some extent. If a given partially designated county contained even more automatically-billed HPSA ZIP codes, then there would be even more incentives to locate in the county.

Thus, to account for the fraction of ZIP codes designated, we can scale our reduced form estimates by 0.77. However, we do so with caution, for two main reasons. First, this scaling exercise assumes that the impact of designations on physician counts would increase linearly with the fraction of ZIP codes within a county that are designated as automatically-billed HPSAs. We note that within-county ZIP codes could differ from one another such that designating one ZIP code has a different impact than designating another. Second, recall from Section 2.7 that the prevalence of population HPSAs, facility HPSAs, or other sub-county geographic designations mean that many counties in our analysis are likely already subject to at least some HPSA-based incentives. Therefore, our first stage estimation does not reflect the true difference in overall exposure to any type of HPSA status. For example, if treated counties were already subject to facility HPSA designations, then the true first stage effect would be smaller, and the implied effect of the designations that we study would be even larger.[21] With these factors in mind, our preferred estimates are the reduced form effects, which capture the impact of a county gaining at least one automatically-billed HPSA ZIP code.

Figure 4 presents results from estimating equation (2) separately for early-career, mid-career, and late-career PCPs. The graphs plot the estimates of the δ_τ's and their 95% confidence intervals. In our leading regression specifications, we use outcome variables that are normalized per 10,000 population at baseline year 2011, and we winsorize outcomes at the 95th percentile to reduce the influence of outliers. Analyzing the pattern of the point estimates allows us to assess the validity of the identifying assumption and examine the dynamic impacts of designations. Panel A 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

[21] To provide more context, we turn to the AHRF data, which allows us to investigate these other types of designations to a limited extent. The data contain information on HPSA designations for a few years that overlap with our analysis time horizon, specifically 2015–2017. So, for our designation events in 2016 and 2017, we can assess the extent to which treated and control counties in our analysis sample already contained some other type of HPSA status before the designation events that we study occur. For 2016, there are 12 treated counties. According to the AHRF data, 8 of them already had either a population HPSA, a facility HPSA, or another type of geographic HPSA (that did not result in any automatically-billed HPSA ZIP codes) in 2015. Similarly, there are 36 unique matched control counties in these 2016 designation events, and 30 of them contained at least one of the other types of designations that we do not study in 2015. For 2017 designation events, there are 25 treated (73 unique control) counties, and 24 (58) of them already contained some other type of designation that we do not study in either 2015 or 2016.

before the year of designation, which lends support to the parallel trends assumption. After designation, we see a 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 before stabilizing at the new level.

In contrast, panels B and C show no evidence of responses from mid-career and late-career physicians, respectively. None of the point estimates are statistically distinguishable from zero, and the graphs show no discernible patterns or trends. The lack of evidence supporting a response from mid- or late-career physicians could be consistent with these groups facing greater costs of relocating from established practices, although we note that late-career physicians approaching retirement ages could respond without having to incur costs associated with relocation, as they could delay retirement and continue to work in their same practice.

Table 2 reports results from estimating equations (3) and (4) to quantify magnitudes. Panel A reports the corresponding results for PCP counts by career stages. The first column reports the medium-run estimates, which quantify the effect of HPSA designation on the stock of doctors after the brief transition period. HPSA designations lead to a statistically significant average increase of 0.111 early-career PCPs per 10,000 (s.e. 0.058). This estimate amounts to a 23% increase when compared to the mean of 0.49 in the period before designation. Given that the average population of a treated county in our sample is around 59,000, the estimate translates to an increase of approximately 0.65 more doctors per county, on average. Scaling the estimate by 0.77 to account for the first stage discussed above would indicate that a county moving from no automatically-billed HPSA ZIP codes to entirely automatically-billed HPSA ZIP codes experiences an increase of $\frac{0.111}{0.77} = 0.144$ early-career PCPs per 10,000 population, which translates to roughly 0.85 more doctors per county. The second column reports the pooled estimates, which are based on the entire post period (including the transition year seen in the dynamics), thus resulting in a smaller point estimate for early-career PCPs.

The point estimates for mid-career and late-career physicians are not statistically distinguishable from zero. They are also much smaller in magnitude than those for early-career physicians, and the means before designation are larger. Finally, panel B reports estimates for the total counts of PCPs. As mid-career and late-career PCPs, who do not appear to be responsive to designations, make up the vast majority of total PCPs, the point estimates in panel B are not statistically distinguishable from zero.

5.2 Primary Care Physician Responses by Medical School Ranking

Given the responsiveness of early-career PCPs 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 roughly proxy for physician quality, we use medical school rankings, and we analyze separate counts of early-career PCPs by whether or not the doctors reported attending one of the 95 medical schools that received a primary care ranking from U.S. News & World Report.

Figure 5 presents the dynamic effects on the stock of early-career doctors, split by ranked and unranked medical schools. 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 panel A and panel B that the entire post-designation increase in early-career PCPs 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 response over the entire period.[22]

Corresponding point estimates are presented in Table 3. Panel A summarizes responses of early-career PCPs. The estimates for early-career ranked PCPs resemble those for the total number of early-career PCPs, and are more precisely estimated. The medium-run estimate in column (1) indicates that treated counties gain 0.099 early-career ranked PCPs per 10,000 population on average following HPSA designation, which corresponds to about 0.58 physicians in the average treated county, a 40% increase from a small mean. Accounting for partially-designated counties, our scaling exercise would indicate an increase of 0.129 early-career ranked PCPs, which corresponds to roughly 0.76 more doctors per county. Point estimates for early-career unranked PCPs are much smaller and indistinguishable from zero. Panels B and C show no statistical evidence that ranked or unranked PCPs later in their careers respond to designations. Taken together, our sets of results indicate that HPSA designations lead to increased counts of early-career primary care physicians who attended

[22] Appendix Figure A.3 presents results for analyzing separately counts of PCPs who reported attending a medical school of "Other" and counts of PCPs who reported a named medical school that is not on the list of ranked medical schools. The graphs show no evidence of an impact on either outcome and highlight that the null results for unranked physicians are not driven by those reporting a medical school of "Other."

ranked medical schools.

Before assessing the robustness of our main sets of results in Section 5.5, we first extend our analysis with two supplementary exercises. First, we investigate the impact of HPSA designations on physicians in specialties other than primary care. Second, we investigate the impact of counties losing HPSA designations.

5.3 Impacts on Physicians in Other Specialties and on Other Providers

Our main analysis is naturally centered on primary care physicians, as the designations we study are "primary care" HPSAs, and shortages of primary care physicians in particular are often the focus of policy makers and stakeholders concerned about access to healthcare. However, we note that designations can create incentives for physicians in other specialties as well. For example, while the NHSC Loan Repayment Program and the NHSC Scholarship Program base eligibility on the primary care specialty specifically, the CMS HPSA Physician Bonus Program does not limit bonus payments to primary care physicians. We also note that designations can create incentives for non-physician providers, specifically nurse practitioners (NPs) and physician assistants (PAs), who have recently been providing an increasing proportion of healthcare visits for patients using Medicare (Patel et al. 2023). While eligibility for the bonus payment program is limited to physicians, the NHSC does provide scholarships and loan forgiveness to NPs and PAs.

To thus provide a more comprehensive assessment of HPSA designations, here we consider physicians in other specialties, as well as NPs and PAs. Specifically, we first study the effects of HPSA designations on counts of doctors who are non-PCPs, and we next study the effects of designations on NPs and PAs. These additional analyses serve two purposes. First, they provide direct evidence on whether these providers respond to designations and location incentives. Second, they allow us to further examine the relative strength of the various HPSA-tied programs in general. Unlike PCPs, who face the full bundle of designation-based programs, these other providers are not eligible for both bonus payments and the loan forgiveness and scholarship programs. The responses (or lack of responses) from these other providers can therefore provide insight on which specific programs might be driving our main results for PCPs, though we note that PCPs and other providers are different and thus could respond differently when facing the same types of incentives.

We first study counts of doctors who are non-PCPs. Appendix Table A.2 and Appendix Figure A.4 present the results. We find no evidence of an impact on counts of physicians in other specialties. On the one hand, a lack of response by non-PCPs, who are eligible for the bonus payments, could be a piece of evidence in support of the interpretation that the more targeted NHSC Loan Repayment and Scholarship Programs are key programs influencing PCP responses to HPSAs. On the other hand, it could also be that the bonus payment program is more effective for PCPs than non-PCPs. For instance, perhaps non-PCPs face greater costs of moving due to factors such as needing to be close to a hospital.

Next, we study the effects of HPSA designations on counts of NPs and PAs. Appendix Table A.3, Appendix Figure A.5, and Appendix Figure A.6 present the results. Overall, we find little to no evidence that these other types of providers, who are eligible for the loan repayment and scholarship programs, respond to HPSA designations. We find no statistically significant evidence of an impact on counts of PAs in any career stage. For NPs, we find no evidence of a response from early-career or mid-career providers. We do find some evidence that HPSA designations lead to an increase of 0.012 late-career NPs per 10,000 population, although this estimate translates into a small increase of 0.07 nurse practitioners per county on average. Because NPs and PAs are not eligible for the bonus payment program, if we had found strong evidence of a response from them, it could have suggested that the non-bonus-payment programs are driving our main findings for PCPs. Alternatively, if we had found no evidence of a response at all, it could have been a piece of evidence suggesting that the bonus payment program is important for driving PCP responses. In the end, we hesitate to draw firm conclusions about the relative strength of the various HPSA programs from the evidence that we find in this additional analysis.

5.4 Impact of HPSA De-Designations on Primary Care Physician Counts

We can also study the effects of losing HPSA designation. Losing a designation results in an undoing of the incentives that arise from HPSA-based programs and may influence physician decisions about where to locate. For example, we have shown that designations can induce early-career physicians to locate in counties. When a currently-designated county loses its designation, there may be early-career physicians looking to establish a practice in a HPSA who would have otherwise chosen to practice in the county, but who instead opt to locate in a different county that maintains its HPSA status.

To investigate the impact of de-designations, we continue to use our matching procedure and stacked difference-in-differences framework detailed above, but we instead define and study de-designation events. Specifically, we construct a treatment group of counties that completely lose HPSA designation at some point between 2013 and 2017. That is, we define a treated county as one that changes from having at least one ZIP code designated as a

HPSA to then having zero ZIP codes designated as a HPSA. For each of these counties, we then construct a mini dataset that includes the treated county as well as a control group of three matched counties that, in contrast, maintain their HPSA designations throughout our analysis period. Just as in our main design, we stack these mini datasets and use our difference-in-differences estimating equations to compare the evolution of PCP counts for treated counties to counts in their matched control counties.

Table 4 presents the main regression results and Appendix Figure A.9 shows the graphical evidence depicting the dynamic impacts, including the analogous first stage graph that shows how the percent of ZIP codes that are designated changes. There is a mechanical decline in the percent of ZIP codes designated during the year that a county loses its HPSA designation status, but this percentage point decline starts to recover during the post period years, indicating that some of the de-designated counties become at least partially designated again. While this pattern should be kept in mind when interpreting the results, we ultimately find no statistical evidence that de-designations impact PCP counts. The graphical results do not indicate any clear evidence of responses, and while the point estimates for early-career PCPs and late-career PCPs are negative, the estimates for mid-career physicians are positive, and in general standard errors prevent us from ruling out meaningful effect sizes.

5.5 Robustness and Specification Checks

We assess the robustness of our results along three broad dimensions. For simplicity, we focus on the effects of gaining HPSA designation and on five main outcome variables: early-career PCPs, early-career PCPs from ranked schools, early-career PCPs from unranked schools, mid-career PCPs, and late-career PCPs.

5.5.1 Robustness to Regression Specification

First, we assess the robustness of our estimates to various specification choices. The first 7 rows of Table 5 report the results.[23] In row A, we reproduce the leading estimates for ease of comparison. In rows B through D, we vary the approach to censoring the data for outliers. Point estimates are similar if we winsorize more stringently, less stringently, or not at all, though we tend to experience precision gains when winsorizing more of the data.

In row E, we add control variables to the leading specification. Specifically, we add county-year control variables for population, population squared, the unemployment rate,

[23] Appendix Table A.4 presents corresponding results for the pooled estimates.

and median household income (flexibly, using indicators for median household income bins of $5,000), as well as year fixed effects to account for national-level developments over time that may impact physician location decisions. The estimates are very similar to the leading estimates.[24]

In row F, we adjust our leading specification by estimating more flexibly-controlled regressions that replace the treatment variable indicator with county-by-designation-event specific fixed effects. Note that including these fixed effects is identical to including county fixed effects for the treatment counties, but since control counties can be matched to multiple designation events, we include a separate fixed effect for each instance of a duplicated control county, according to the mini difference-in-differences dataset to which it belongs. In general, the magnitudes are smaller when using county-by-designation-event fixed effects instead of a treatment group indicator. The estimate for early-career PCPs is no longer statistically significant at the 10% level, but it is well within the confidence interval of the leading estimate. The estimate for early-career ranked PCPs is statistically significant at the 5% level and more similar in magnitude to its leading estimate. For a graphical inspection, Appendix Figure A.7 presents the dynamic difference-in-differences results for this robustness check. The patterns of the point estimates are consistent with the leading graphs and indicate increases in the stock of early-career, especially early-career ranked, PCPs after designation events. Finally, in row G of the table, we add to the specification that uses county-by-designation-event fixed effects by also including the control variables and year fixed effects that are analyzed in row E. As before, these additional control variables do not impact the estimates.

5.5.2 Robustness to Sample Selection

Second, we assess the robustness of our estimates to issues related to sample selection. In row H of Table 5, we address partial county designations. We include only counties that are fully designated in the treatment group, meaning that 100% of ZIP codes in the county are designated. Our leading strategy defines counties as a HPSA if they contain any HPSA ZIP code, so this alternative sample excludes a number of counties that do contain valid HPSAs within them, but it provides estimates for a sample of counties where every doctor in each treatment county is located in a designated area. Point estimates are not meaningfully

[24]We note that one potential concern with including additional control variables is the possibility that the controls could themselves respond to treatment, but Appendix Figure A.2 shows no evidence that the unemployment rate, median household income, or population respond to HPSA designation.

different for this sample, although the standard errors are larger, likely due to decreased sample size.

In rows I and J, we speak to potential issues related to overstating magnitudes due to the close proximity of treatment and control counties. As mentioned earlier, if physicians induced by designations to locate in a treatment county would have otherwise located in a control county, then while our design would correctly capture a positive effect, the magnitudes of the estimates would be overstated. So long as the set of analysis counties is small relative to the full set of counties where physicians might practice, any resulting bias is likely to be small. For some reference, there are over 3,000 counties in the U.S., and our leading sample includes 217 treatment counties plus 651 control counties. In row I though, we study a smaller sample by only matching two control counties to each treatment county. This restriction further reduces the size of our analysis sample compared to the full set of counties. Results are similar to our leading specification. To take another approach, in row J, we adjust our main analysis sample by requiring that control counties cannot be in the same state as the treatment county to which they are matched. The idea here is to limit the extent to which the counterfactual counts of physicians provided by the control group is itself influenced by designation-induced physician moves away from nearby control group counties to treatment counties. Again the point estimates are similar to our leading estimates.

In row K, we drop counties designated in 2017 (and their corresponding matched control counties) from the analysis, because these counties have only one post-period year (the designation year), but our primary focus is on the medium-run, defined as the years after the initial designation year. The point estimates are generally larger for this subsample that excludes the 2017 designations.

Finally, in row L, we take into consideration HPSA designations made outside our study period. Our physician data and analysis time horizon span the years 2012 to 2017. However, it is possible that counties in our sample could have been designated previously, which could influence the counts of physicians during our analysis time horizon. For example, consider a county that was historically a HPSA, but became de-designated right before our study period begins and then designated again during our study period. To the extent that these situations occur, our estimates could understate the true effect of a HPSA designation, if the physician counts in these treatment counties were already elevated due to the historic HPSA status before the observed designation event. To investigate this possibility, we use data on automatically-billed HPSA ZIP codes from 2010 and 2011, before our physician data and study period begin. Of the 217 counties in our treatment group, we see that 44 had a designation in either 2010 or 2011. Note that these counties must have lost their designations in 2012, because we define our treatment group as those changing from no designation to some designation between 2012 and 2017. Similarly, of the 470 unique counties in our control group, 73 had a designation in either 2010 or 2011, and these counties thus lost their designations by the start of our study period in 2012.

Our estimates in row L come from a subsample that excludes all of these counties that were previously designated in either 2010 or 2011.[25] Specifically, we first drop these counties from our base county panel dataset, and we then create a new stacked dataset using our matching procedure and estimate our difference-in-differences equations using this stacked dataset. The estimates are thus picking up the effects of automatically-billed HPSA designations for an analysis sample consisting of both treatment and control counties that were not designated for a longer period of time before our study period begins. The key point estimates for early-career PCPs and ranked early-career PCPs are slightly larger than the leading estimates.

5.5.3 Robustness to Variables Used in the Matching Procedure

Third, we assess the robustness of our estimates to the variables on which we match. We report results for the medium-run estimates in Table 6.[26] Daw and Hatfield (2018) show how matching on pre-period outcomes can result in regression to the mean bias in difference-in-differences designs when treatment status is correlated with outcome levels, so we analyze how our results change when we relax our matching strategy to avoid matching on baseline variables that are so closely correlated with our outcomes. Column (1) reproduces the leading estimates for comparability. Recall that we match on geography as well as physician counts, trends in physician counts, and poverty rates defined during 2010 and 2011, a baseline time period that occurs before our analysis time horizon begins. Column (2) relaxes the matching strategy by not matching on physician trends, and column (3) relaxes the matching strategy by not matching on physician counts. Point estimates are not sensitive to either of these changes. Finally, we match only on variables that are less-directly-linked to our main outcomes. Specifically, in column (4) we match only on geography, poverty rate, median household income, and population, again defined during the baseline time period.

[25]We additionally exclude 3 counties (2 from our treatment group and 1 from our control group) for which we could not determine a 2010 or 2011 designation status, because these counties were missing from the Department of Housing and Urban Development ZIP-code-to-county crosswalks (see Appendix B) in 2010 and 2011, likely because the pre-2012 crosswalks use earlier Census geographies than the 2012–2017 crosswalks.

[26]Appendix Table A.5 presents corresponding results for the pooled estimates.

We find results similar to our main estimates. For a visual assessment, Appendix Figure A.8 graphs the dynamic difference-in-differences results for this alternative matching strategy. The patterns displayed in our leading analysis hold.

6 Conclusion

Some areas have significantly fewer physicians per capita than other areas. Policy makers are concerned with this inequity in access to care across geographies and related disparities in health outcomes. To address these issues, the Health Professional Shortage Area (HPSA) policy designates shortage areas and attempts to increase counts of physicians in these areas.

The HPSA policy is a large, important, decades-old policy; however, there is a paucity of evidence on its effectiveness, likely due to significant empirical challenges such as data availability and identification issues related to the fact that designations are not random. In this paper, we confront these challenges by combining several sources of data to construct a suitable analysis dataset and by using a matched difference-in-differences design to study the causal effects of primary care geographic HPSA designations on location decisions of physicians. Overall, our results indicate that these designations induce early-career primary care physicians (PCPs) to practice in shortage areas. We find that designations lead to an average increase of roughly 0.65 early-career PCPs per county, which is driven by physicians who attended ranked medical schools. In contrast, we find no evidence that designations change location decisions of later-career PCPs (or physicians in other specialties).

Our study is not without limitation. For one, as discussed earlier, our data from the CMS Health Professional Shortage Area Physician Bonus Program limits the extent of our study to automatically-billed primary care geographic HPSAs. We also focus only on location outcomes, whereas a complete analysis of costs and benefits would call for the study of additional outcomes beyond the scope of this paper, such as physician responses along the intensive margin (e.g. effort, quality of care, or the number of services provided) as well as population health outcomes.

Moreover, an important question remains: what mechanism best explains our results? On the one hand, our findings might be consistent with a strong role for the scholarship and loan forgiveness programs. These programs are targeted towards early-career PCPs, whom we find to be responsive. It could be that the opportunity to apply for loan repayments is quite attractive for these physicians, perhaps especially for those who attended ranked medical schools, if graduates from ranked schools also tend to incur more medical school debt. On the other hand, our findings could also be consistent with early-career PCPs being more responsive to the bonus payment program. PCPs in later career stages are likely to face higher costs of relocating, due to difficulties associated with moving an established practice, than early-career PCPs, who are less likely to have formed their own practice and may be making initial location decisions anyway after recently completing residencies. Thus these early-career physicians may be more likely to find the benefits of practicing in a HPSA for an increase in remuneration to outweigh the costs. Among other potential explanations, the differences in responses between ranked and unranked medical school graduates could be due to differences in information dissemination regarding HPSA-based programs or differences in intrinsic motivation to alleviate geographic shortages in care. Of course, it could be that it is some combination of the programs and incentives that drive the results.

Despite the limitations, our study provides new evidence on how a major policy impacts a key outcome, and our results have implications for policy makers. Our results indicate that, under the current program and policy environment, primary care geographic HPSA designations are able to attract early-career PCPs to areas in need. Moreover, our results underscore the importance of targeting shortage area program incentives and highlight how additional targeting could potentially lead to gains in cost-effectiveness. Specifically, we have found no evidence that HPSA designations impact the location decisions of later-career PCPs or physicians in other specialties. Yet these doctors make up the bulk of the physician labor force, and the bonus payment program (which is not targeted and applies to all physicians) is therefore directing substantial funds to doctors we find to be inframarginal. If it is the case that the bonus payment program is playing a key role in driving the responses of early-career PCPs, then there could be scope to improve the effectiveness of the program and still reduce costs by targeting even higher bonus payments towards PCPs who recently graduated medical school. Finally and more broadly, our findings show how geographically targeted government healthcare policies can be used as a tool by policy makers to influence the allocation of primary care physicians—who constitute a critical component of healthcare systems—across space.

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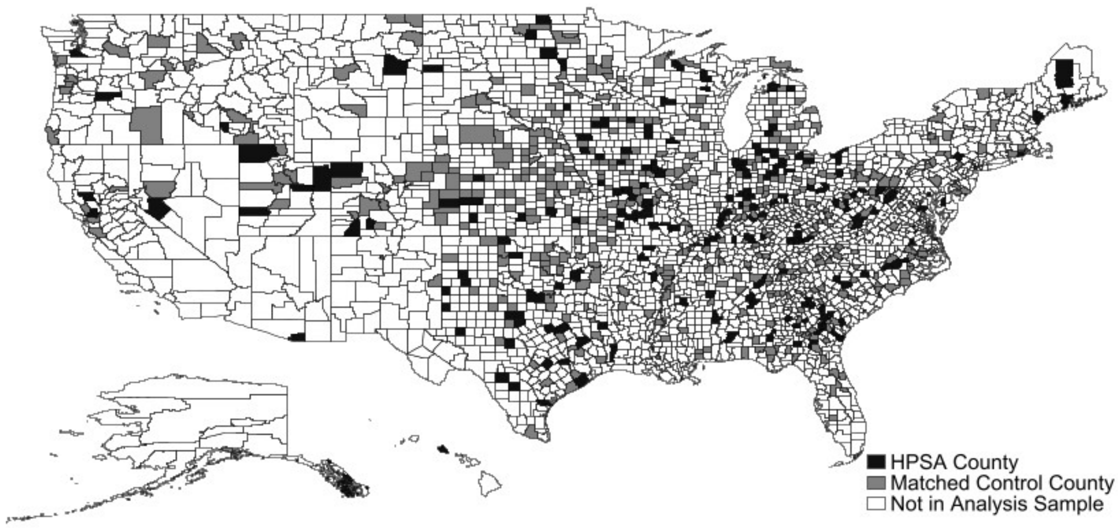
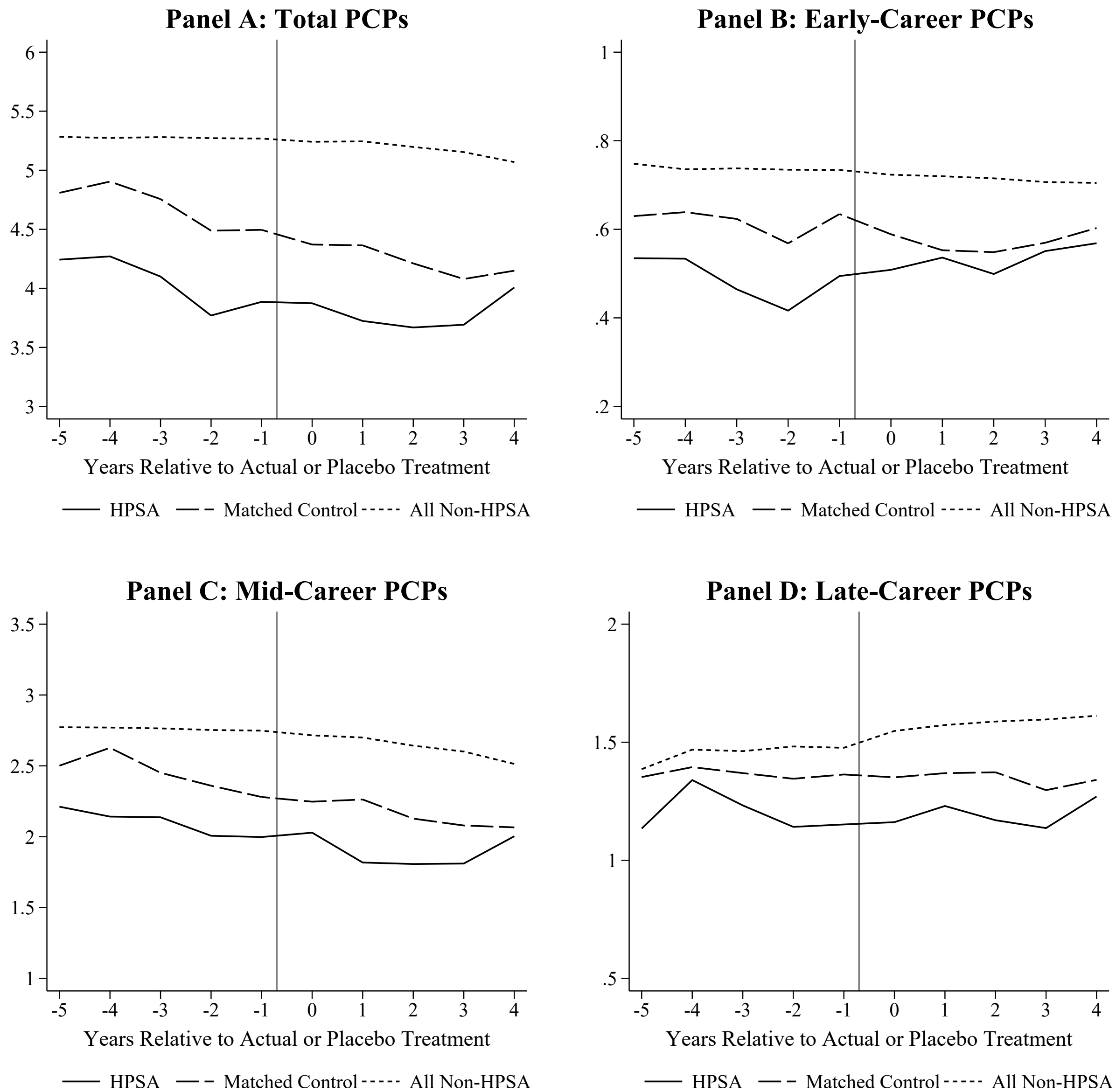
Figure 1: Geographic Variation in HPSA Designation Status



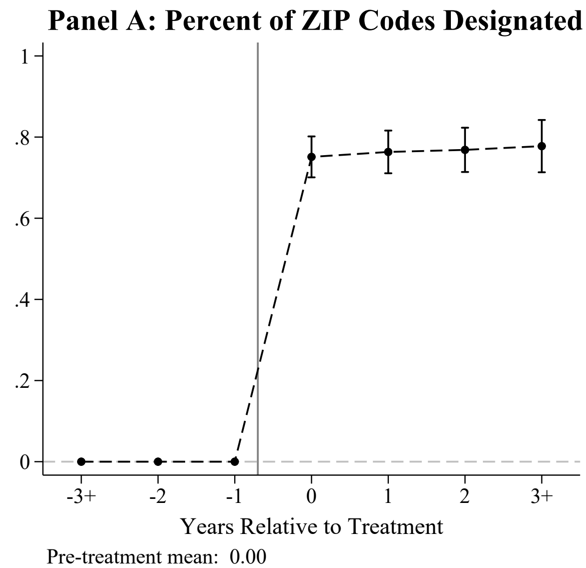
Notes: This map illustrates the geographic variation in Health Professional Shortage Area (HPSA) designation status of the type that we study. The dark counties are our treatment group. They are counties that become designated as a geographic primary care HPSA in some year between 2013 and 2017, where we define a county as designated if it contains a ZIP code on the CMS bonus payment program list of automatically billed HPSAs. The gray counties are our control group. They are non-HPSA counties that are matched to HPSA counties using the matching method described in Section 4.

Figure 2: Average Number of Primary Care Physicians in HPSA and Non-HPSA Counties



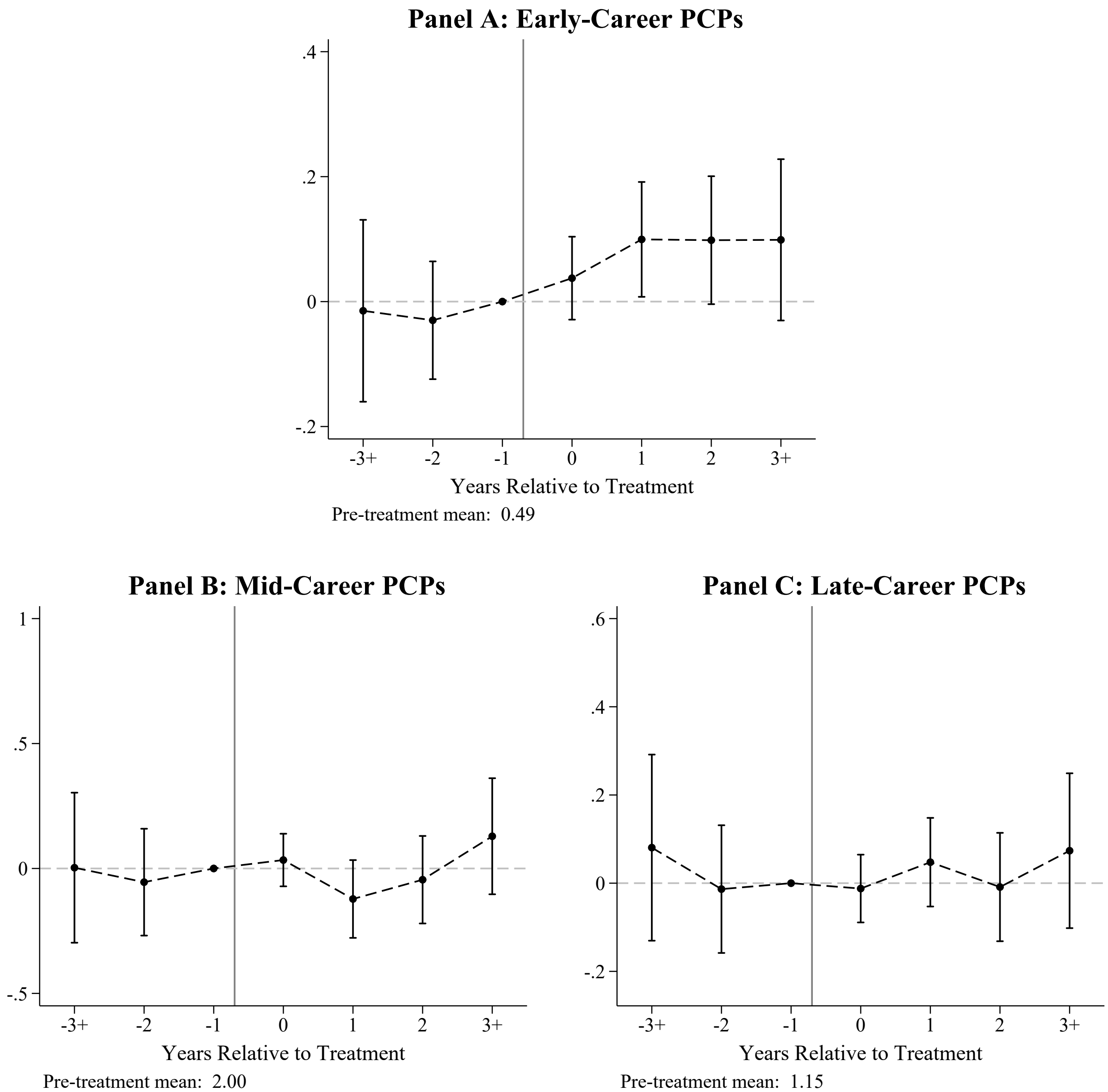
Notes: These graphs plot the average number of primary care physicians (PCPs) per 10,000 population around actual or placebo designation year for treatment HPSA counties, for unmatched potential controls, and for matched controls. The treatment sample consists of all counties that become designated as a primary care HPSA in some year between 2013 and 2017. The unmatched control sample consists of all counties that are never designated as a HPSA during 2012 to 2017, assigned as controls to and given placebo designation years from all counties in the treatment sample. The matched control sample consists of the non-HPSA counties that are matched to HPSA counties using the matching method described in Section 4.

Figure 3: Exposure to HPSA Designation over Time



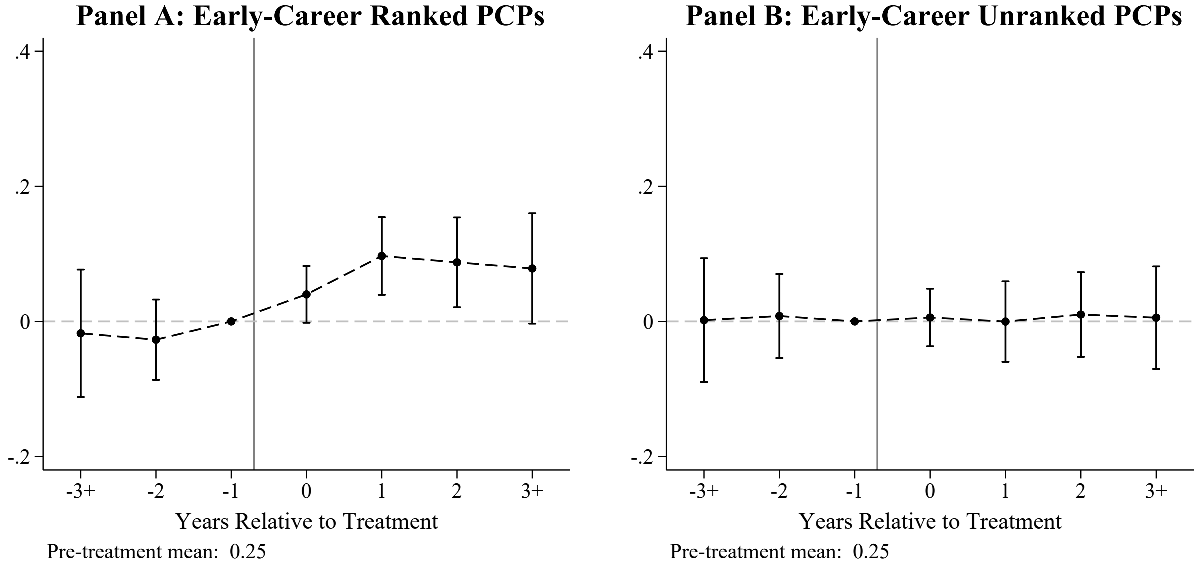
Notes: This graph illustrates how the fraction of county ZIP codes designated as an automatically-billed primary care geographic HPSA evolves around the timing of the designations that we study. Most of the treatment counties that we study (65%) are fully designated, meaning that 100% of the ZIP codes within the county have been designated; however, some are only partially designated. The graph plots point estimates of the δ_τ's and their 95% confidence intervals from estimating equation (2) on the fraction of ZIP codes within a county that are designated, with standard errors clustered at the county level. The figure shows that on average, after designation, approximately 77% of the ZIP codes within treatment counties are designated as HPSAs.

Figure 4: Impact of HPSA Designation on Counts of Primary Care Physicians by Career Stage



Notes: These graphs plot the dynamic impact of HPSA designation on primary care physician (PCP) counts per 10,000 population by career stage. The graphs plot point estimates of the δ_τ's and their 95% confidence intervals from estimating equation (2). Standard errors are clustered at the county level.

Figure 5: Impact of HPSA Designation on Counts of Early-Career Primary Care Physicians by Medical School Rank



Notes: These graphs plot the dynamic impact of HPSA designation on early-career primary care physician (PCP) counts per 10,000 population by rank of medical school attended. The graphs plot point estimates of the δ_τ's and their 95% confidence intervals from estimating equation (2). Standard errors are clustered at the county level.

Table 1: Summary Statistics and Balance Test Before Actual or Placebo Designation Year

	HPSAs	Matched Control
	Mean	Mean
	(1)	(2)
Panel A: Outcome Variables (County Panel)		
Total PCPs Per 10,000	3.89***	4.50
	(0.17)	(0.13)
Early-Career PCPs Per 10,000	0.49**	0.63
	(0.05)	(0.04)
Early-Career Ranked PCPs Per 10,000	0.25	0.31
	(0.04)	(0.03)
Early-Career Unranked PCPs Per 10,000	0.25*	0.32
	(0.03)	(0.03)
Mid-Career PCPs Per 10,000	2.00**	2.28
	(0.10)	(0.08)
Mid-Career Ranked PCPs Per 10,000	1.00	1.17
	(0.08)	(0.06)
Mid-Career Unranked PCPs Per 10,000	1.00	1.11
	(0.07)	(0.05)
Late-Career PCPs Per 10,000	1.15**	1.36
	(0.07)	(0.06)
Late-Career Ranked PCPs Per 10,000	0.56**	0.70
	(0.04)	(0.04)
Late-Career Unranked PCPs Per 10,000	0.59	0.67
	(0.05)	(0.04)
Panel B: Other Variables (AHRF)		
Total Physicians Per 10,000	9.95	10.40
	(0.78)	(0.45)
Percent Persons in Poverty	17.27	17.42
	(0.45)	(0.42)
Population	58,969	67,569
	(9,967)	(8,372)
Median Household Income	44,480	44,161
	(692)	(531)
Unemployment Rate	7.27	6.86
	(0.21)	(0.16)
Number of Counties	217	651

Notes: This table presents summary statistics for the analysis sample during the year immediately preceding actual or placebo designation. Means are reported separately for the treatment group and the control group, and standard errors clustered at the county level are in parentheses. Panel A presents means for our outcome variables, which come from our constructed county panel dataset. Panel B presents means for other variables, including variables used in our match as well as covariates, which come from the Area Health Resources File (AHRF). We test for statistically significant differences between the treatment group and the matched control group.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Impact of HPSA Designation on Counts of Primary Care Physicians

	Medium-Run Estimate (1)	Pooled Estimate (2)	Dependent Mean (3)
Panel A: PCP Counts by Career Stage			
Early-Career PCPs	0.111*	0.095*	0.49
	(0.058)	(0.052)	
Mid-Career PCPs	0.011	0.021	2.00
	(0.122)	(0.108)	
Late-Career PCPs	0.021	0.007	1.15
	(0.081)	(0.073)	
Panel B: Total PCP Counts			
Total PCPs	0.114	0.106	3.89
	(0.184)	(0.161)	
Clusters	687	687	
Observations	5,208	5,208	

Notes: This table presents difference-in-differences estimates of the impact of HPSA designation on primary care physician (PCP) counts per 10,000 population. Panel A presents estimates for counts by career stage. Panel B presents estimates for counts of total PCPs. Column (1) reports our preferred medium-run estimates; that is, it reports estimates of δ^{MR} from equation (4). Column (2) reports pooled estimates; that is, it reports estimates of δ from equation (3). Standard errors are clustered at the county level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Impact of HPSA Designation on Counts of Primary Care Physicians by Medical School Rank

	Medium-Run Estimate (1)	Pooled Estimate (2)	Dependent Mean (3)
Panel A: Early-Career PCP Counts			
Early-Career Ranked PCPs	0.099***	0.087***	0.25
	(0.036)	(0.032)	
Early-Career Unranked PCPs	0.003	0.003	0.25
	(0.034)	(0.031)	
Panel B: Mid-Career PCP Counts			
Mid-Career Ranked PCPs	0.070	0.068	1.00
	(0.094)	(0.083)	
Mid-Career Unranked PCPs	-0.067	-0.058	1.00
	(0.082)	(0.072)	
Panel C: Late-Career PCP Counts			
Late-Career Ranked PCPs	0.005	-0.012	0.56
	(0.059)	(0.053)	
Late-Career Unranked PCPs	0.025	0.030	0.59
	(0.054)	(0.048)	
Clusters	687	687	
Observations	5,208	5,208	

Notes: This table presents difference-in-differences estimates of the impact of HPSA designation on primary care physician (PCP) counts per 10,000 population. Panel A presents estimates for counts of early-career PCPs by medical school rank. Panel B presents estimates for counts of mid-career PCPs by medical school rank. Panel C presents estimates for counts of late-career PCPs by medical school rank. Column (1) reports our preferred medium-run estimates; that is, it reports estimates of δ^{MR} from equation (4). Column (2) reports pooled estimates; that is, it reports estimates of δ from equation (3). Standard errors are clustered at the county level.

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

Table 4: Impact of HPSA De-Designation on Counts of Primary Care Physicians

	Medium-Run Estimate (1)	Pooled Estimate (2)	Dependent Mean (3)
Panel A: PCP Counts by Career Stage			
Early-Career PCPs	-0.063	-0.052	0.68
	(0.061)	(0.053)	
Mid-Career PCPs	0.077	0.069	2.45
	(0.137)	(0.115)	
Late-Career PCPs	-0.084	-0.068	1.44
	(0.083)	(0.071)	
Panel B: Total PCP Counts			
Total PCPs	-0.155	-0.128	4.85
	(0.220)	(0.186)	
Clusters	529	529	
Observations	4,008	4,008	

Notes: This table presents difference-in-differences estimates of the impact of de-designation on primary care physician (PCP) counts per 10,000 population. Panel A presents estimates for counts by career stage. Panel B presents estimates for counts of total PCPs. Column (1) reports medium-run estimates; that is, it reports estimates of δ^{MR} from equation (4). Column (2) reports pooled estimates; that is, it reports estimates of δ from equation (3). Standard errors are clustered at the county level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Robustness of Medium-Run Estimates to Regression Specification and Sample Selection Criteria

	Early-Career PCPs (1)	Early-Career Ranked PCPs (2)	Early-Career Unranked PCPs (3)	Mid-Career PCPs (4)	Late-Career PCPs (5)
A. Leading Specification	0.111*	0.099***	0.003	0.011	0.021
	(0.058)	(0.036)	(0.034)	(0.122)	(0.081)
B. Winsorize Less	0.114	0.117**	0.003	0.010	0.039
	(0.070)	(0.052)	(0.039)	(0.130)	(0.090)
C. Winsorize More	0.102**	0.075**	-0.000	-0.017	0.014
	(0.050)	(0.029)	(0.030)	(0.112)	(0.076)
D. No Winsorizing	0.111	0.117**	-0.005	0.015	0.034
	(0.071)	(0.056)	(0.042)	(0.140)	(0.092)
E. Add Control Variables	0.114**	0.100***	0.007	0.019	0.021
	(0.057)	(0.036)	(0.034)	(0.121)	(0.079)
F. County x Desig. Event Fixed Effects	0.071	0.076**	-0.007	-0.127	-0.017
	(0.050)	(0.031)	(0.030)	(0.087)	(0.060)
G. Control Variables and Fixed Effects	0.074	0.078**	-0.005	-0.115	-0.024
	(0.050)	(0.031)	(0.029)	(0.087)	(0.060)
H. Only Fully Designated Counties	0.092	0.104**	-0.020	-0.087	-0.054
	(0.071)	(0.045)	(0.042)	(0.145)	(0.094)
I. Less Matched Control Counties	0.103*	0.103***	-0.011	0.006	0.052
	(0.062)	(0.039)	(0.037)	(0.133)	(0.086)
J. Different State Control Counties	0.112*	0.109***	0.001	0.054	-0.015
	(0.058)	(0.035)	(0.035)	(0.120)	(0.083)
K. Exclude 2017 Designations	0.143**	0.107***	0.018	0.052	0.038
	(0.056)	(0.035)	(0.034)	(0.111)	(0.080)
L. Exclude 2010/2011 Designations	0.125*	0.113***	0.002	0.010	0.032
	(0.065)	(0.042)	(0.039)	(0.135)	(0.090)

Notes: This table presents estimates of δ^{MR} from estimating equation (4) for the main outcomes as we vary the regression specification and sample selection criteria. Row A reproduces our leading 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 adds control variables and year fixed effects to the regression. Row F replaces the treatment variable indicator with county-by-designation-event specific fixed effects. Row G adds control variables, year fixed effects, and county-by-designation-event specific fixed effects. Row H studies only HPSA counties that are 100% designated, meaning that the entire county is an automatically billed HPSA, whereas it excludes counties that are "partially" designated, meaning counties that have only some of their ZIP codes as automatically-billed HPSAs. Row I matches only two control counties to each treatment county, rather than three. Row J studies an analysis sample where the control counties cannot be located in the same state as the treatment county to which they are matched. Row K excludes counties designated in 2017. Row L excludes counties that were previously designated in either 2010 or 2011. Standard errors are clustered at the county level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

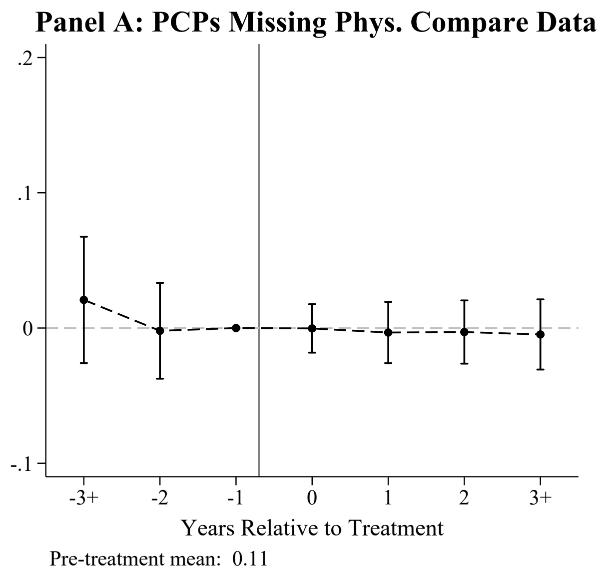
Table 6: Robustness of Medium-Run Estimates To Match Variables

	Leading Specification (1)	No Physician Trends (2)	No Physician Counts (3)	Only Other Area Attributes (4)
Early-Career PCPs	0.111*	0.092*	0.094	0.110*
	(0.058)	(0.055)	(0.060)	(0.058)
Early-Career Ranked PCPs	0.099***	0.093***	0.084**	0.084**
	(0.036)	(0.034)	(0.037)	(0.037)
Early-Career Unranked PCPs	0.003	-0.008	0.008	0.013
	(0.034)	(0.033)	(0.036)	(0.034)
Mid-Career PCPs	0.011	-0.050	-0.038	-0.024
	(0.122)	(0.117)	(0.118)	(0.119)
Late-Career PCPs	0.021	-0.018	-0.018	-0.032
	(0.081)	(0.081)	(0.081)	(0.083)
Match Variables				
Physician Count	✓	✓	×	×
Percent Change in Physician Count	✓	×	✓	×
Poverty Rate	✓	✓	✓	✓
Geographic Region	✓	✓	✓	✓
Median Household Income	×	×	×	✓
Population	×	×	×	✓

Notes: This table presents estimates of δ^{MR} from estimating equation (4) for the main outcomes as we vary our matching strategy. Column (1) reproduces the leading estimates, where we match on geography as well as physician counts, trends in physician counts, and poverty rates defined during 2010 and 2011, a baseline time period that occurs before our analysis time horizon begins. Column (2) relaxes the leading matching strategy by not matching on physician trends. Column (3) relaxes the leading matching strategy by not matching on physician counts. Column (4) does not match on either physician counts or physician trends, but instead matches only on geography, poverty rate, median household income, and population, again defined during the baseline time period, which are area characteristics that are less-directly-linked to our main physician outcome variables. Standard errors are clustered at the county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

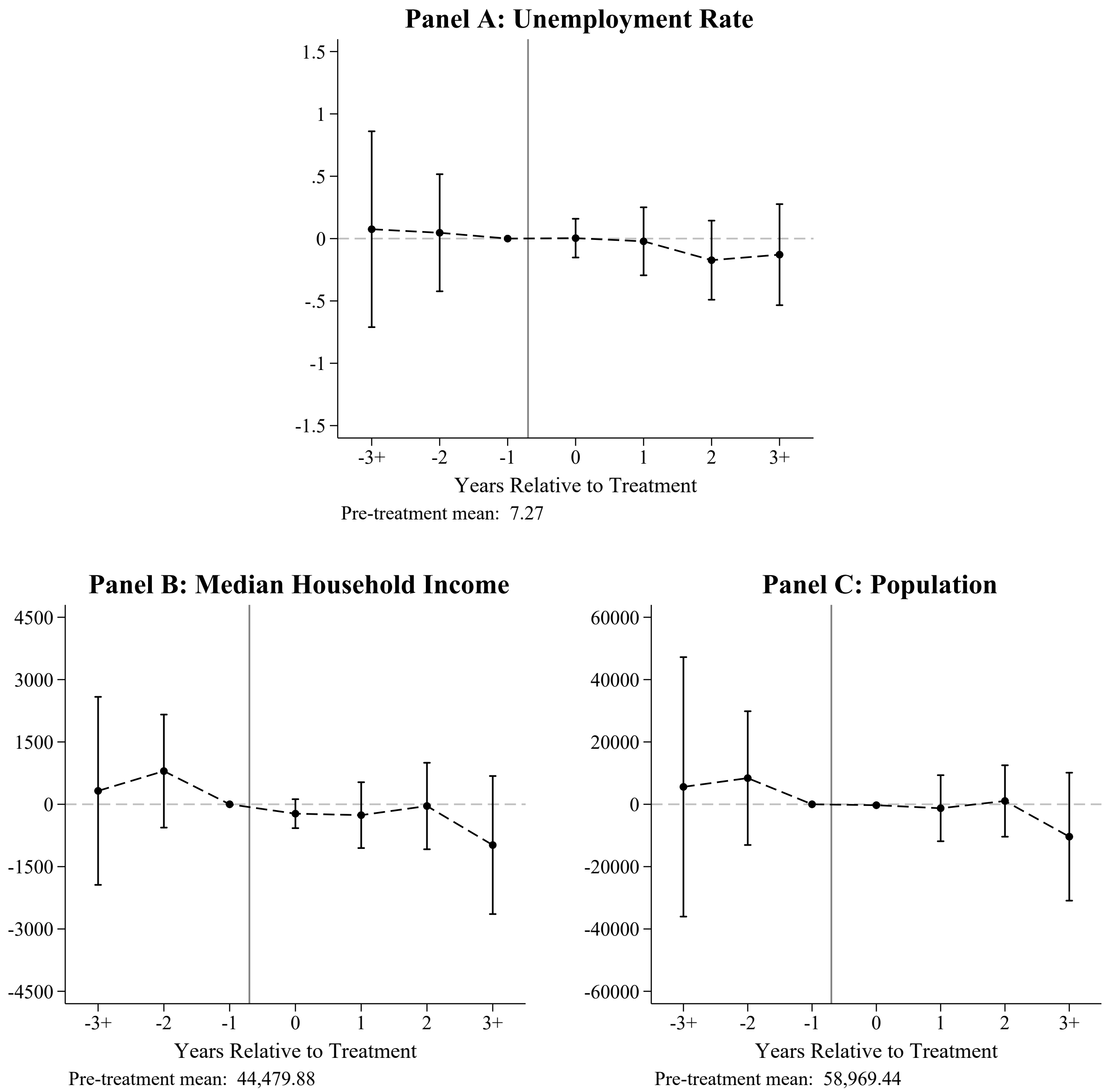
Appendix A   Additional Figures and Tables

Figure A.1: Analyzing Primary Care Physicians Missing Data Relative to HPSA Designation



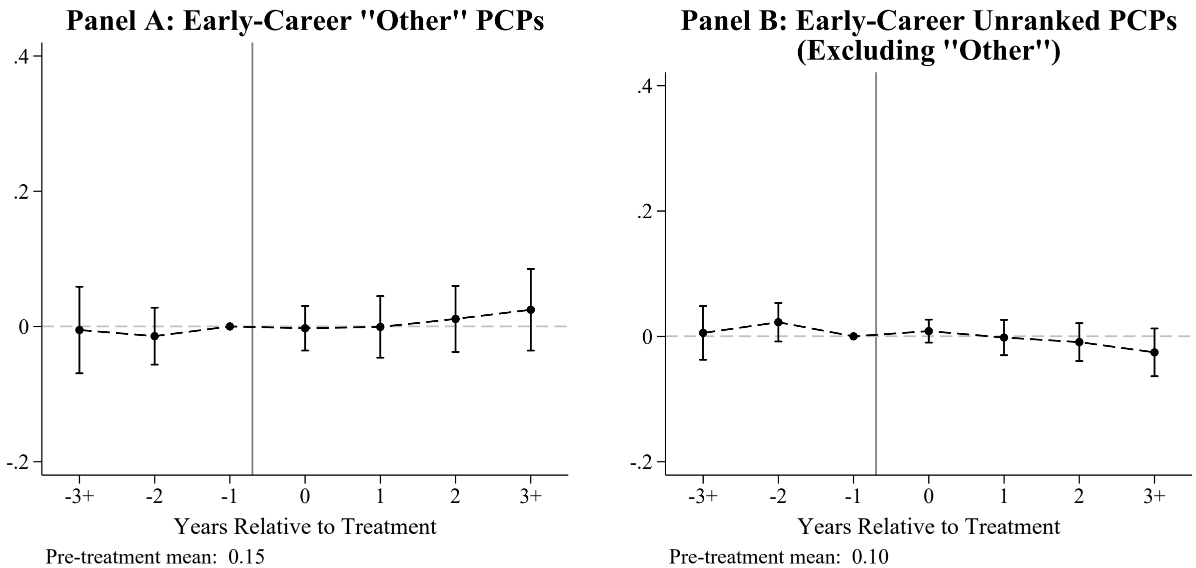
Notes: This graph plots the dynamic impact of HPSA designation on counts of primary care physicians (PCPs) for whom we are missing data on graduation year or medical school per 10,000 population. The graph plots point estimates of the δ_τ's and their 95% confidence intervals from estimating equation (2). Standard errors are clustered at the county level.

Figure A.2: Analyzing Control Variables as Outcome Variables



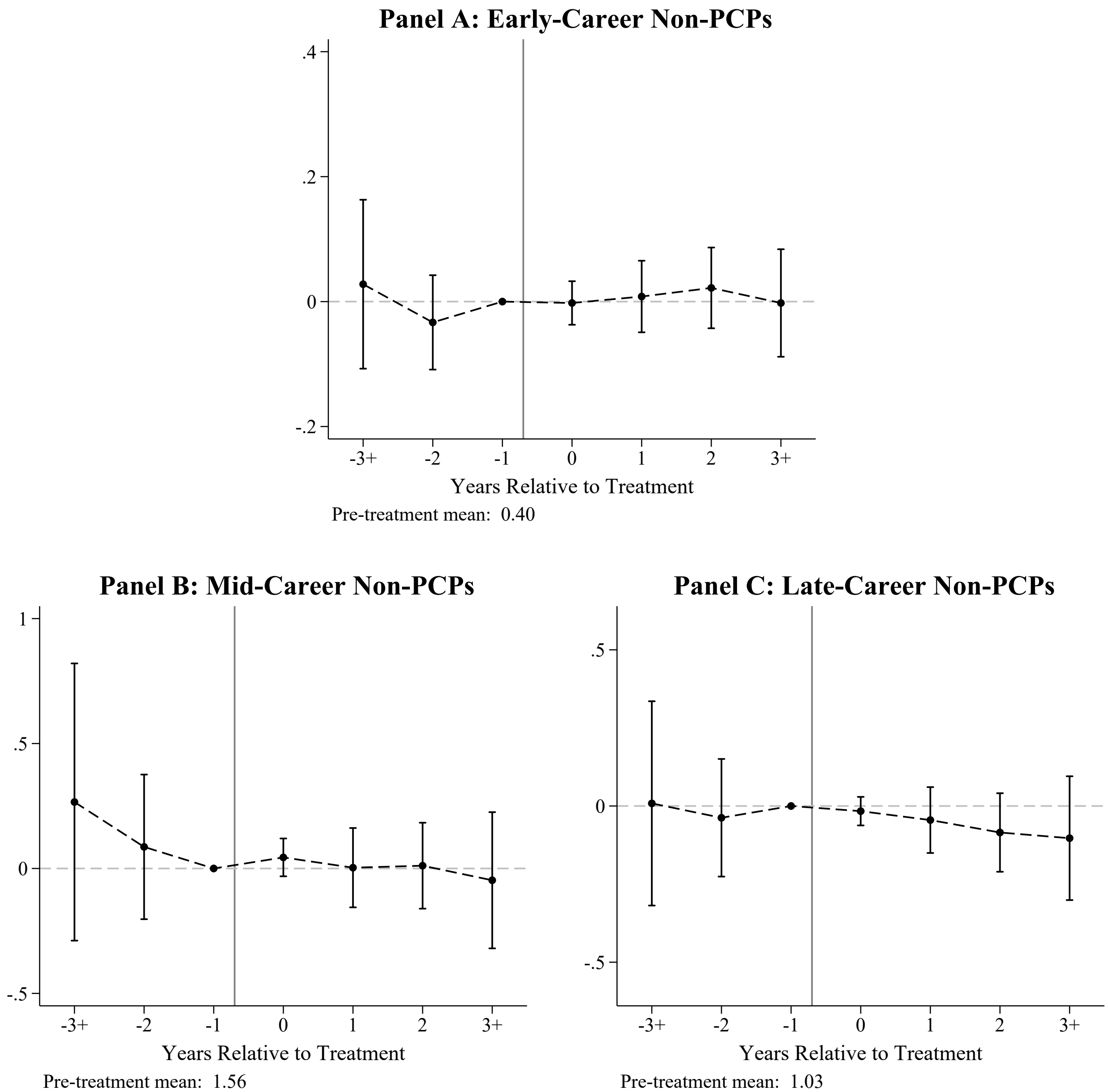
Notes: These graphs plot the dynamic impact of HPSA designations on the control variables. The graphs plot point estimates of the δ_τ's and their 95% confidence intervals from estimating equation (2). Standard errors are clustered at the county level.

Figure A.3: Analyzing Separate Counts of "Other" and Unranked Primary Care Physicians



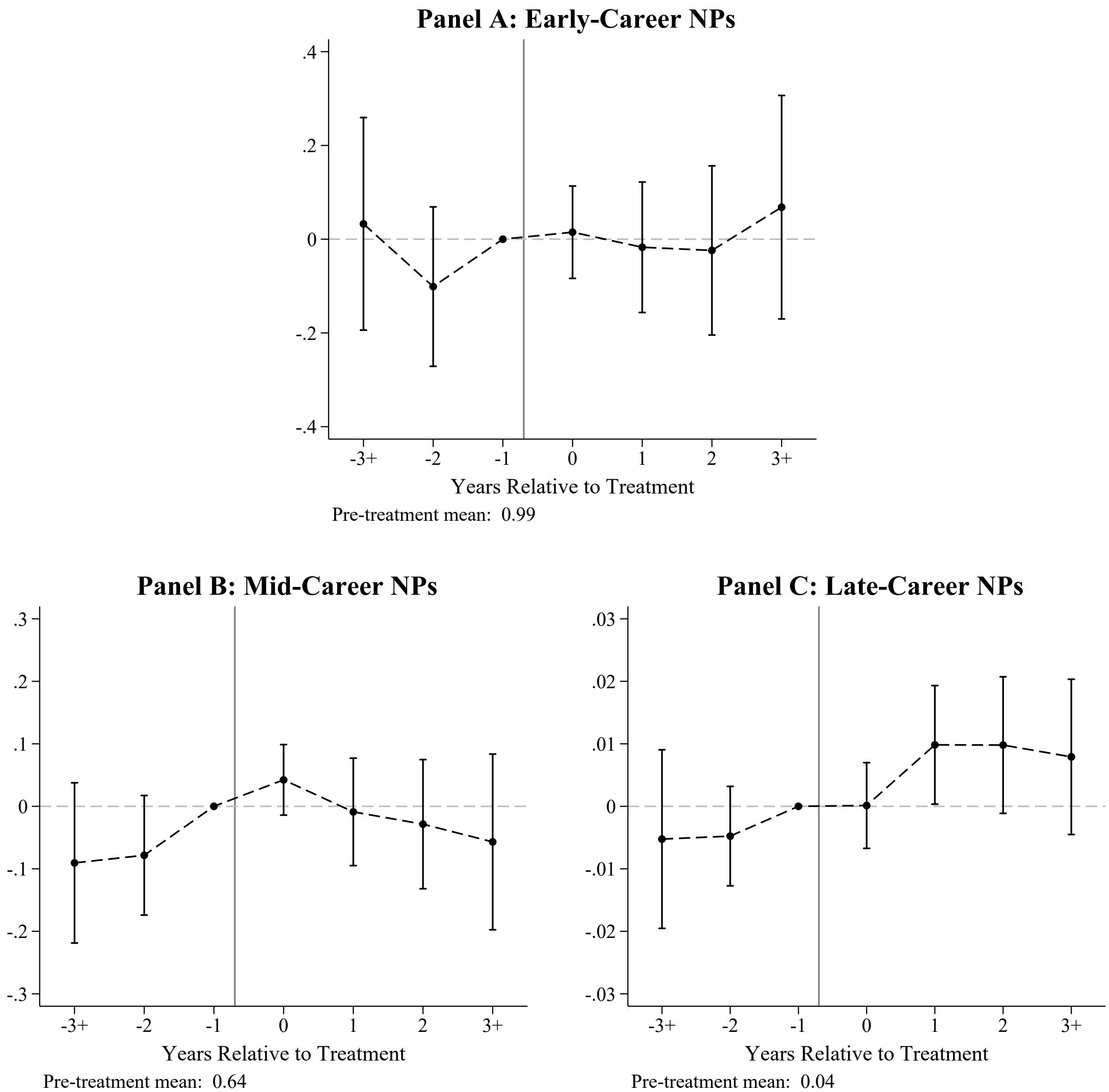
Notes: These graphs plot the dynamic impact of HPSA designation on counts of early-career primary care physicians (PCPs) per 10,000 population, where we further break down the counts of physicians who did not report attending a medical school that is ranked. Specifically, panel A analyzes counts of early-career PCPs who reported attending a named medical school that is not on the list of ranked medical schools. Panel B analyzes counts of early-career PCPs who reported attending a medical school of "Other," who we classify in our leading analysis as unranked. The graphs plot point estimates of the δ_τ's and their 95% confidence intervals from estimating equation (2). Standard errors are clustered at the county level.

Figure A.4: Impact of HPSA Designation on Counts of Physicians in Other Specialties by Career Stage



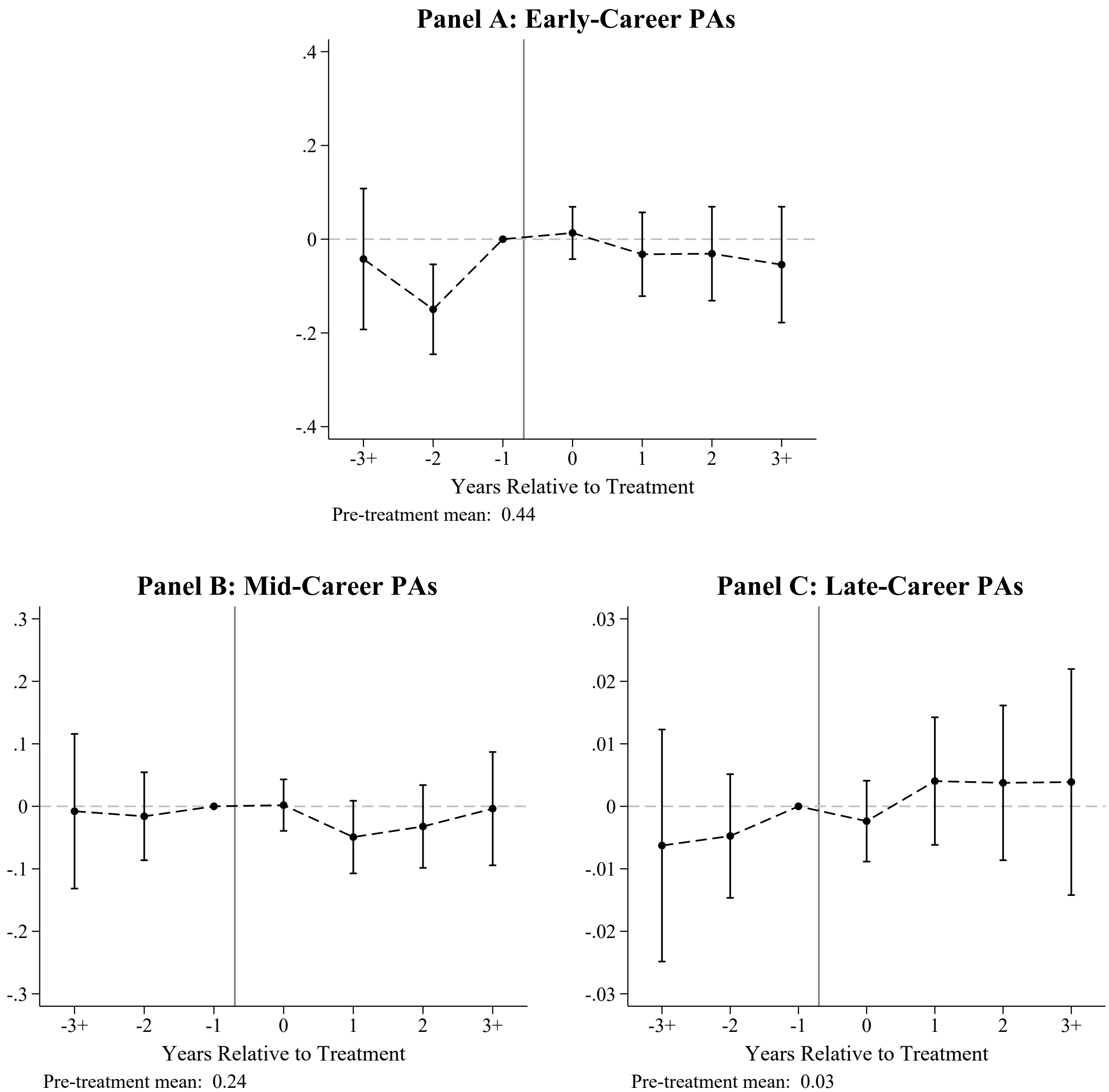
Notes: These graphs plot the dynamic impact of HPSA designation on counts of physicians in specialties other than primary care per 10,000 population by career stage. The graphs plot point estimates of the δ_τ's and their 95% confidence intervals from estimating equation (2). Standard errors are clustered at the county level.

Figure A.5: Impact of HPSA Designation on Counts of Nurse Practitioners



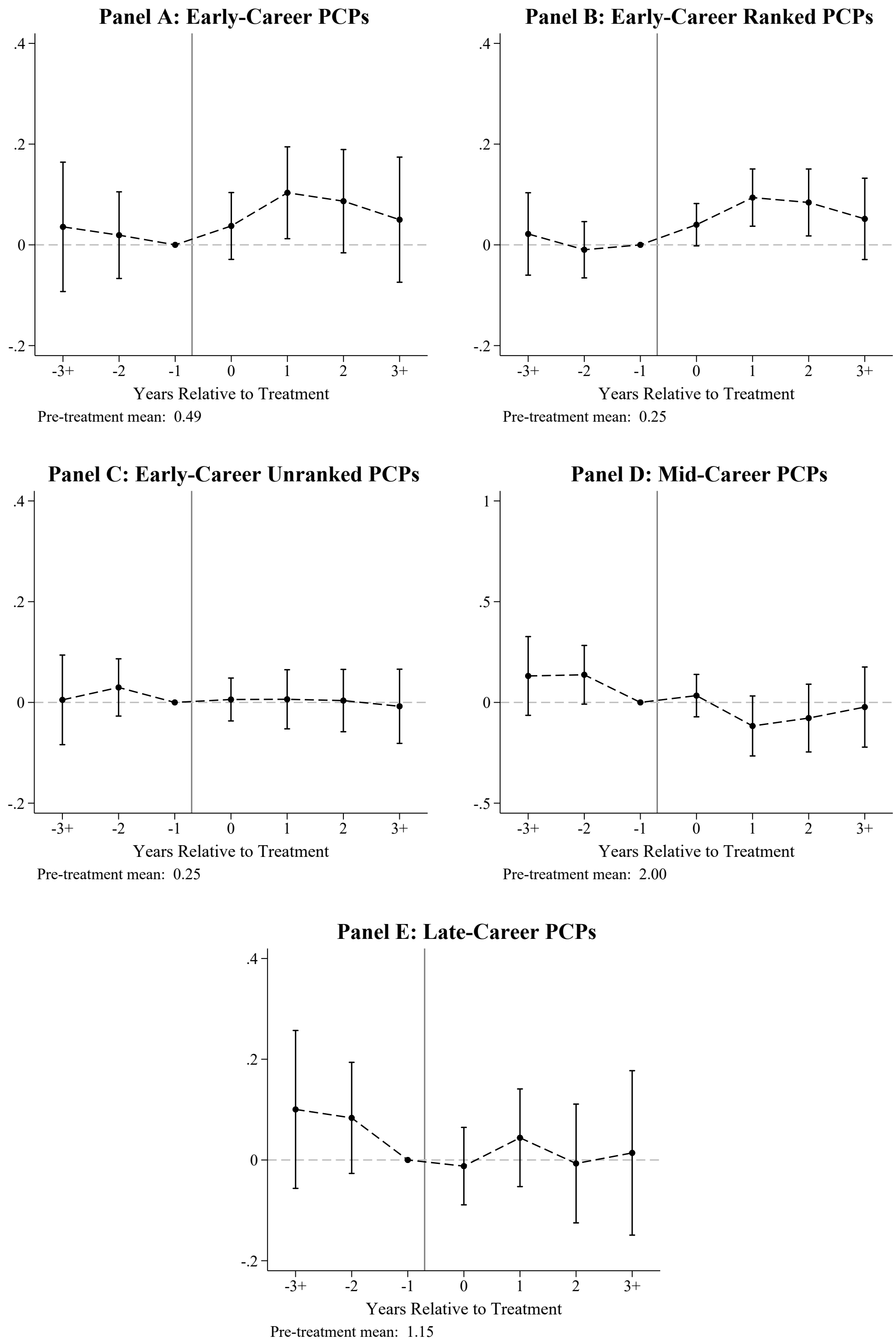
Notes: These graphs plot the dynamic impact of HPSA designation on counts of nurse practitioners per 10,000 population by career stage. The graphs plot point estimates of the δ_τ's and their 95% confidence intervals from estimating equation (2). Standard errors are clustered at the county level.

Figure A.6: Impact of HPSA Designation on Counts of Physician Assistants



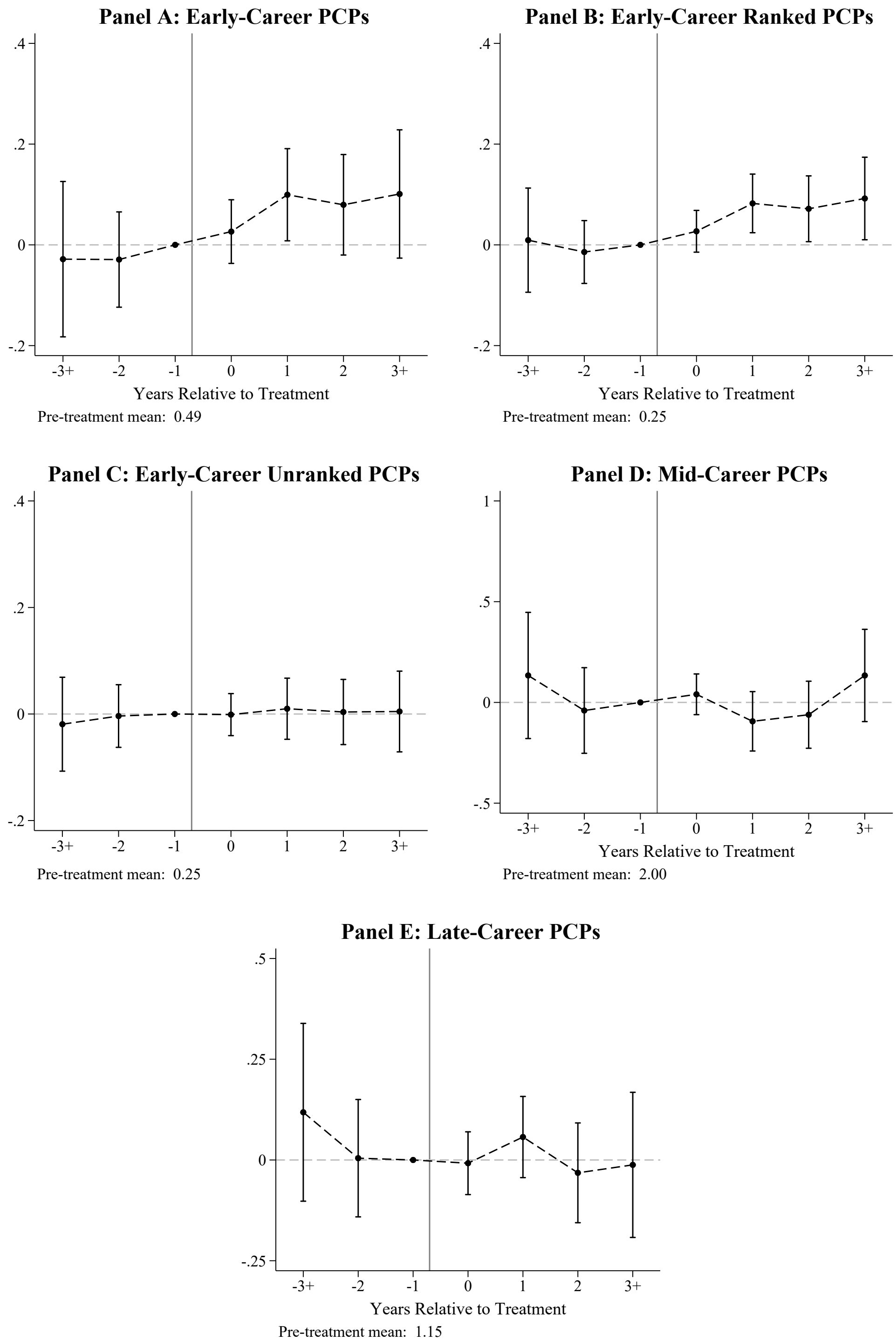
Notes: These graphs plot the dynamic impact of HPSA designation on counts of physician assistants per 10,000 population by career stage. The graphs plot point estimates of the δ_τ's and their 95% confidence intervals from estimating equation (2). Standard errors are clustered at the county level.

Figure A.7: Robustness to Specification: Dynamic Difference-in-Differences Graphs Using County-by-Designation-Event Specific Fixed Effects



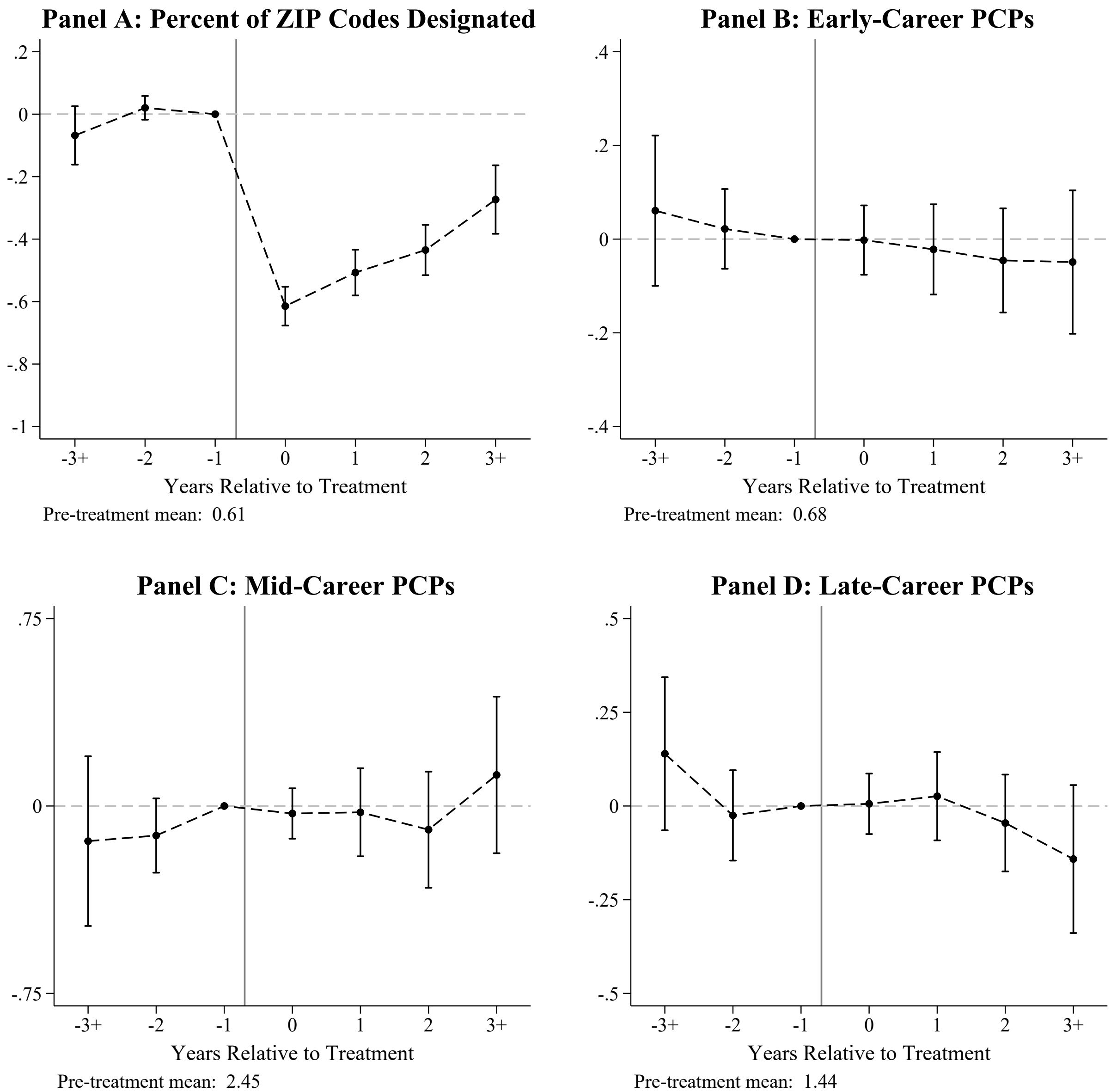
Notes: These graphs plot the dynamic impact of HPSA designation on each of our main outcomes, when using an alternative regression specification that replaces the treatment group indicator with county-by-designation-event specific fixed effects.

Figure A.8: Robustness to Match Variables: Dynamic Difference-in-Differences Graphs After Matching on Only Other Area Attributes



Notes: These graphs plot the dynamic impact of HPSA designation on each of our main outcomes, after using an alternative matching strategy that matches only on area characteristics that are less-directly-linked to our main physician outcome variables.

Figure A.9: Impact of HPSA De-Designation on Counts of Primary Care Physicians by Career Stage



Notes: These graphs plot the dynamic impact of HPSA de-designation on primary care physician (PCP) counts per 10,000 population by career stage. The graphs plot point estimates for the δ_τs and their 95% confidence intervals from estimating equation (2). Standard errors are clustered at the county level.

Table A.1: Summary Statistics for Always-Designated Counties

	Always-Designated Counties	
	Mean (1)	Std. Dev. (2)
Panel A: Outcome Variables (County Panel)		
Total PCPs Per 10,000	3.27	2.75
Early-Career PCPs Per 10,000	0.43	0.64
Early-Career Ranked PCPs Per 10,000	0.16	0.37
Early-Career Unranked PCPs Per 10,000	0.27	0.46
Mid-Career PCPs Per 10,000	1.64	1.72
Mid-Career Ranked PCPs Per 10,000	0.72	1.28
Mid-Career Unranked PCPs Per 10,000	0.93	1.00
Late-Career PCPs Per 10,000	1.04	1.29
Late-Career Ranked PCPs Per 10,000	0.50	1.09
Late-Career Unranked PCPs Per 10,000	0.54	0.72
Panel B: Other Variables (AHRF)		
Total Physicians Per 10,000	8.94	10.61
Percent Persons in Poverty	18.07	7.25
Population	98,471	472,703
Median Household Income	45,320	11,078
Unemployment Rate	6.60	2.70
Number of Counties	971	

Notes: This table presents summary statistics during 2014 for the sample of counties that are always designated throughout the time horizon of our data. The table reports means and standard deviations. Panel A presents statistics for our outcome variables, which come from our constructed county panel dataset. Panel B presents statistics for other variables, including variables used in our match as well as covariates, which come from the Area Health Resources File (AHRF). We note the presence of a few outlier counties, in terms of population, in this sample of always-designated counties. For instance, the sample includes Los Angeles County in California, which contains the city of Los Angeles, Cook County in Illinois, which contains the city of Chicago, Harris County in Texas, which contains the city of Houston, and Maricopa County in Arizona, which contains the city of Phoenix.

Table A.2: **Impact of HPSA Designation on Counts of Physicians in Other Specialties**

	Medium-Run Estimate (1)	Pooled Estimate (2)	Dependent Mean (3)
Panel A: Non-PCP Counts by Career Stage			
Early-Career Non-PCPs	0.009	0.006	0.40
	(0.048)	(0.042)	
Mid-Career Non-PCPs	-0.113	-0.098	1.56
	(0.192)	(0.171)	
Late-Career Non-PCPs	-0.072	-0.056	1.03
	(0.124)	(0.109)	
Panel B: Total Non-PCP Counts			
Total Non-PCPs	-0.243	-0.215	3.27
	(0.364)	(0.323)	
Clusters	687	687	
Observations	5,208	5,208	

Notes: This table presents difference-in-differences estimates of the impact of HPSA designation on counts of physicians in specialties other than primary care per 10,000 population. Panel A presents estimates for counts by career stage. Panel B presents estimates for total counts. Column (1) reports our preferred medium-run estimates; that is, it reports estimates of δ^{MR} from equation (4). Column (2) reports pooled estimates; that is, it reports estimates of δ from equation (3). Standard errors are clustered at the county level.

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

Table A.3: Impact of HPSA Designation on Counts of Nurse Practitioners and Physician Assistants

	Medium-Run Estimate (1)	Pooled Estimate (2)	Dependent Mean (3)
Panel A: NP and PA Counts by Career Stage			
Early-Career NPs	0.030	0.030	0.99
	(0.100)	(0.086)	
Early-Career PAs	0.010	0.024	0.44
	(0.063)	(0.055)	
Mid-Career NPs	0.013	0.033	0.64
	(0.062)	(0.053)	
Mid-Career PAs	-0.020	-0.013	0.24
	(0.046)	(0.041)	
Late-Career NPs	0.012*	0.009*	0.04
	(0.006)	(0.006)	
Late-Career PAs	0.007	0.005	0.03
	(0.008)	(0.006)	
Panel B: Total NP and PA Counts			
Total NPs	0.036	0.063	1.74
	(0.141)	(0.121)	
Total PAs	-0.060	-0.036	0.92
	(0.120)	(0.106)	
Clusters	687	687	
Observations	5,208	5,208	

Notes: This table presents difference-in-differences estimates of the impact of HPSA designation on counts of nurse practitioners (NPs) and physician assistants (PAs) per 10,000 population. Panel A presents estimates for counts by career stage. Panel B presents estimates for total counts. Column (1) reports our preferred medium-run estimates; that is, it reports estimates of δ^{MR} from equation (4). Column (2) reports pooled estimates; that is, it reports estimates of δ from equation (3). Standard errors are clustered at the county level.

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

Table A.4: Robustness of Pooled Estimates to Regression Specification and Sample Selection Criteria

	Early-Career PCPs (1)	Early-Career Ranked PCPs (2)	Early-Career Unranked PCPs (3)	Mid-Career PCPs (4)	Late-Career PCPs (5)
A. Leading Specification	0.095*	0.087***	0.003	0.021	0.007
	(0.052)	(0.032)	(0.031)	(0.108)	(0.073)
B. Winsorize Less	0.096	0.100**	0.002	0.026	0.025
	(0.062)	(0.047)	(0.035)	(0.115)	(0.081)
C. Winsorize More	0.086*	0.064**	0.001	-0.008	0.002
	(0.044)	(0.026)	(0.026)	(0.100)	(0.068)
D. No Winsorizing	0.098	0.104**	-0.006	0.038	0.024
	(0.064)	(0.051)	(0.038)	(0.124)	(0.082)
E. Add Control Variables	0.097*	0.087***	0.006	0.029	0.006
	(0.051)	(0.032)	(0.030)	(0.108)	(0.071)
F. County x Desig. Event Fixed Effects	0.056	0.064**	-0.005	-0.095	-0.029
	(0.042)	(0.026)	(0.026)	(0.073)	(0.052)
G. Control Variables and Fixed Effects	0.059	0.065**	-0.004	-0.084	-0.034
	(0.042)	(0.026)	(0.026)	(0.073)	(0.051)
H. Only Fully Designated Counties	0.072	0.089**	-0.019	-0.075	-0.065
	(0.065)	(0.041)	(0.038)	(0.132)	(0.084)
I. Less Matched Control Counties	0.084	0.088**	-0.011	0.014	0.035
	(0.055)	(0.035)	(0.033)	(0.118)	(0.077)
J. Different State Control Counties	0.094*	0.094***	0.001	0.055	-0.020
	(0.052)	(0.031)	(0.031)	(0.107)	(0.074)
K. Excluding 2017 Designations	0.130**	0.101***	0.015	0.052	0.029
	(0.051)	(0.031)	(0.031)	(0.100)	(0.073)
L. Exclude 2010/2011 Designations	0.112*	0.100***	0.006	0.010	0.021
	(0.058)	(0.037)	(0.034)	(0.120)	(0.080)

Notes: This table presents estimates of δ from estimating equation (3) for the main outcomes as we vary the regression specification and sample selection criteria. Row A reproduces our leading 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 adds control variables and year fixed effects to the regression. Row F replaces the treatment variable indicator with county-by-designation-event specific fixed effects. Row G adds control variables, year fixed effects, and county-by-designation-event specific fixed effects. Row H studies only HPSA counties that are 100% designated, meaning that the entire county is an automatically billed HPSA, whereas it excludes counties that are "partially" designated, meaning counties that have only some of their ZIP codes as automatically-billed HPSAs. Row I matches only two control counties to each treatment county, rather than three. Row J studies an analysis sample where the control counties cannot be located in the same state as the treatment county to which they are matched. Row K excludes counties designated in 2017. Row L excludes counties that were previously designated in either 2010 or 2011. Standard errors are clustered at the county level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.5: Robustness of Pooled Estimates To Match Variables

	Leading Specification (1)	No Physician Trends (2)	No Physician Counts (3)	Only Other Area Attributes (4)
Early-Career PCPs	0.095*	0.077	0.075	0.092*
	(0.052)	(0.048)	(0.054)	(0.052)
Early-Career Ranked PCPs	0.087***	0.0793***	0.070**	0.069**
	(0.032)	(0.031)	(0.033)	(0.033)
Early-Career Unranked PCPs	0.003	-0.005	0.005	0.011
	(0.031)	(0.029)	(0.032)	(0.030)
Mid-Career PCPs	0.021	-0.039	-0.022	-0.015
	(0.108)	(0.103)	(0.105)	(0.106)
Late-Career PCPs	0.007	0.007	-0.027	-0.035
	(0.073)	(0.073)	(0.073)	(0.075)
Match Variables				
Physician Count	✓	✓	×	×
Percent Change in Physician Count	✓	×	✓	×
Poverty Rate	✓	✓	✓	✓
Geographic Region	✓	✓	✓	✓
Median Household Income	×	×	×	✓
Population	×	×	×	✓

Notes: This table presents estimates of δ from estimating equation (3) for the main outcomes as we vary our matching strategy. Column (1) reproduces the leading estimates, where we match on geography as well as physician counts, trends in physician counts, and poverty rates defined during 2010 and 2011, a baseline time period that occurs before our analysis time horizon begins. Column (2) relaxes the leading matching strategy by not matching on physician trends. Column (3) relaxes the leading matching strategy by not matching on physician counts. Column (4) does not match on either physician counts or physician trends, but instead matches only on geography, poverty rate, median household income, and population, again defined during the baseline time period, which are area characteristics that are less-directly-linked to our main physician outcome variables. Standard errors are clustered at the county level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix B Data Details

This data appendix provides more details about each of the data sources that we use, our data cleaning steps, and how we use the data to create our county panel dataset and arrive at our analysis sample.

B.1 Data Sources and Initial Data Cleaning Steps

B.1.1 Medicare Provider Utilization and Payment Data: Physician and Other Supplier

These data are publicly available and provided by CMS. They contain records of Medicare services billed by healthcare professionals for Medicare Part B fee-for-service beneficiaries. Observations in the data are uniquely defined by (1) a National Provider Identifier, (2) a Healthcare Common Procedure Coding System code, and (3) a place of service, which is either facility or non-facility. We use data from 2012 to 2017. We carry out a few data cleaning steps to take the data to the physician-year level. First, we keep only observations of healthcare professionals that have either an M.D. or a D.O. credential. To do this, we keep all providers with either (i) a credential that contains an "M" with a subsequent "D," or (ii) a credential that contains a "D" with a subsequent "O," and then we manually sort through the remaining providers, dropping observations of those that are not physicians. Next, we keep only one observation for each physician in each year, as we do not use information on billings. Finally, we create an indicator variable for being a primary care physician, which we define as having a specialty of "family practice," "general practice," "internal medicine," "geriatric medicine," or "pediatric medicine." After these data cleaning steps, we are left with a panel dataset of physicians that includes information on credentials and specialties and spans the years 2012 to 2017. This panel dataset will form the base of our doctor data.

B.1.2 National Plan and Provider Enumeration Systems

These data are publicly available, and processed versions are available at the NBER. The data contain unique identifiers for healthcare providers. We use the December disseminations of the data from each calendar year between 2014 and 2017. From these disseminations, we keep only the physician identifiers and the business practice location variables, which we use to define primary practice location in each year. We are left with a panel dataset of providers that includes information on practice location and spans the years 2012 to 2017.

B.1.3 Physician Compare

These data are publicly available and provided by CMS. The data are snapshots in time of physicians currently billing Medicare. The data only became available after 2014, so we do not have data on physicians who only billed Medicare before 2014, as described in the text. We make use of all available archived data from 2014 onward though, to attempt to fill in information for as many physicians as possible. Specifically, we use Physician Compare snaphots made available by CMS from September 2014, December 2014, April 2015, July

2015, October 2015, November 2015, April 2016, July 2016, October 2016, December 2016, April 2017, July 2017, October 2017, and August 2018.

We clean these data in two broad steps. First, we use the snapshots to create a single Physician Compare dataset. Specifically, we append the snapshots together, but keep only one observation for each physician. Using all of the available snapshots allows us to extract medical school and graduation year for every physician that appears in at least one snapshot. Sometimes the same physician reports different medical schools or different graduation years in different snapshots. When this occurs, we update the values to be those provided by the physician in the most recent snapshot available, which leads to a consistent and time-invariant definition of the variables. Second, after creating one Physician Compare dataset, we define the variables to be used in our analysis. Specifically, we replace graduation year and medical school names with missing values for those who do not report information and for a handful of observations with graduation years that are likely erroneous values. Then, to define medical school ranking, we manually code up the rankings of reported medical schools based on the 2018 U.S. News & World Report rankings of medical schools for primary care. As noted in the text, a substantial number of physicians report "Other" for their medical school. We do not assign the "Other" schools a rank, and thus in our leading analysis we classify physicians who report "Other" as "unranked" primary care physicians, meaning that they did not report having attended a ranked medical school. (In the robustness section, we analyze separately counts of physicians who report "Other" and counts of physicians who report the name of a medical school that is not on the list of ranked schools.) After these two data-cleaning steps, we are left with a provider-level dataset with information on medical school ranking and graduation year.

B.1.4 Area Health Resources File

These data are released annually by the Bureau of Health Workforce and are publicly available from the Health Resources and Services Administration website. We use the *County Area Health Resources File (AHRF) 2017-2018 Release*, and we extract county-level information from 2010 to 2017. We carry out the following data cleaning steps to arrive at the AHRF dataset that we use in our analysis. First, we create a variable for the total number of active physicians in each county by summing the total number of active M.D.s and the total number of active D.O.s. Next, we use information on population, median household income, the unemployment rate, and the poverty rate. Household income and the poverty rate are missing for all observations in 2017, so we replace these missing values with values based on a linear extrapolation using data on the variables from 2010 to 2016. We are left with a panel of counties from 2010 to 2017. We use the data from 2010 and 2011 to facilitate our matching procedure, and we use the data from 2012 to 2017 to include control variables in our regressions.

B.1.5 CMS Primary Care HPSA ZIP Code Data

These data are publicly available and provided by CMS. They contain the list of ZIP codes that are automatically-billed primary care geographic HPSAs. We use the lists that correspond to each year from 2012 to 2017.

B.1.6 HUD USPS ZIP Code Crosswalks

The ZIP-code-to-county crosswalks that we use are publicly available and provided by the U.S. Department of Housing and Urban Development at this website: `https://www.huduser.gov/portal/datasets/usps_crosswalk.html`. We use the crosswalks from quarter 4 of each calendar year from 2012 to 2017. Sometimes ZIP codes are linked to multiple counties. In these instances, we use the accompanying information on residential ratio, i.e. the fraction of each ZIP code's residential addresses that are in each county, to link each ZIP code to the one county that has the greatest fraction of that ZIP code's residential addresses.

B.2 Constructing Our County Panel

After cleaning the raw data, we merge together the various datasets to create a county panel. Then we use the county panel to define a treatment group of HPSA-designated counties and a matched control group of similar non-HPSA-designated counties.

To create our county panel, we carry out four steps. First, we start with the *Medicare Provider Utilization and Payment Data: Physician and Other Supplier* panel dataset and merge in other doctor information. We merge in information on practice location for these physicians from the *National Plan and Provider Enumeration Systems* data, and we merge in information on graduation year and medical school attendance from the *Physician Compare* data. We then create indicator variables for our main outcomes. That is, we create indicator variables for (i) early-career primary care physicians, defined as primary care physicians who graduated between 5 and 10 years ago, (ii) later-career primary care physicians, defined as primary care physicians who gradated more than 10 year ago, (iii) "ranked" primary care physicians, defined as primary care physicians who reported attending one of the 95 schools receiving an official 2018 ranking in the U.S. News & World Report list of the best medical schools for primary care, and "unranked" physicians, who did not report attending one of those 95 schools. We then interact these indicators to create indicators for ranked and unranked early- and later-career primary care physicians.

Second, we merge HPSA information into our physician data and assign a county to each physician in the data. We start by merging in the primary care HPSA designation status using the list of HPSA ZIP codes for each year. We then merge in information on the county using the ZIP-code-to-county crosswalks for each year. We drop the 0.016% of physician-year observations that are not linked to any county.

Third, we aggregate the data to the county level. We aggregate doctor outcomes by simply counting up the number of primary care physicians with values of one for each relevant indicator variable. We aggregate the HPSA ZIP code information by simply counting up the number of ZIP codes within each county that are automatically-billed geographic primary care HPSAs.

Fourth and finally, we merge in county-level data from the *Area Health Resources File*. We are left with a panel dataset of counties that spans the years 2012 to 2017 and contains information on population, median household income, the unemployment rate, the percent of the population below the federal poverty line, various counts of primary care physicians that constitute our outcome variables, and the number of ZIP codes within each county that are primary care geographic HPSAs.

B.3 Defining the Analysis Sample

To arrive at our analysis sample, we start with the county panel, identify counties that will form our "treatment" group, and then match similar counties to those counties to form our "control" group. We define the treatment group as counties that are designated over our analysis time horizon. Specifically, a county is in the treatment group if we observe the county move from having 0 HPSA ZIP codes to having at least 1 HPSA ZIP code at some point between 2013 and 2017. We exclude counties that we see as designated in 2012, as we do not have outcome data for any of their pre-treatment periods. We also exclude counties that are designated throughout our entire analysis time horizon, as we cannot define a designation year for those counties. We then consider the remaining counties, which are never designated in any year between 2012 and 2017, as our pool of potential controls. To each treatment county, we assign three control counties from the pool of potential controls, using the matching method described in Section 4.