Health Professional Shortage Areas and Physician Location Decisions*

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July 11, 2022

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.

Keywords: Physician Labor Supply, Medicare, Government Health Expenditures
JEL classification: H51, I18, I11

*We thank Jeff Clemens and Julie Cullen, as well as Prashant Bharadwaj, Itzik Fadlon, Todd Gilmer, Joshua Graff Zivin, Gaurav Khanna, Michael Richards, Chad Stecher, participants at the 2019 Western Economics Association International conference, participants at the 2019 National Tax Association conference, and participants at the 2022 American Society of Health Economists conference for helpful conversations and comments. We thank Jean Roth for assistance in accessing the NPPES data, made available at the NBER.

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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.

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 on Medicare services furnished by physicians in HPSAs. Other federal programs also use HPSA designations. The National Health Service Corps 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 various 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 directly due to declines in the number of physicians practicing in a county. Thus comparing a control group of all non-HPSA counties with a treatment group of HPSA counties is unlikely to be a credible approach. Our matching strategy 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 of moving compared to physicians in later stages of their careers, who are more likely to have an already-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

[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).

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 evidence that designations on average affect the total number of PCPs practicing in a county; however, we do find 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 on 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 0.114 physicians per 10,000 residents on average, which roughly amounts to 0.67 physicians per county. This modest increase in early-career PCPs represents a 23% increase off of a small baseline mean, reflecting the fact that the HPSA program is focused 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 further 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, at least to some extent, working. 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.

We thus relate most closely to papers that investigate physician location decisions, especially in the context of physician shortages.[3] A vast literature in the medical 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

[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.

[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.

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 are able to uncover evidence that early-career PCPs are more responsive to shortage area designations.

Finally, our findings connect to an important discussion in the literature on how 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 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.

[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).

[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).

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 it is upon this type of designation that several programs are based. 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 to both determine HPSA eligibility status and to calculate a score intended to quantify the severity of the shortage. The score is primarily determined by an 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

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

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 on 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 as our source of variation designations that result in automatically-billed HPSA ZIP codes.

2.3 National Health Service Corps 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

[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.

eligible to apply for the competitive 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]

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, who then can submit an approval recommendation to the U.S. Department of State, who then can 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 reim-

[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.

bursement 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 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. Rather, we will estimate the impact of geographic primary care HPSA designations on the counts of primary care physicians, under the current policy environment.

A related contextual issue to keep in mind is that our data on HPSA designations come from the CMS bonus payment program. We discuss the data in more detail below, but this means that we are only able to study geographic primary care HPSAs that result in automatic bonus payments. We are not able to study population HPSAs and facility HPSAs with these data. The bonus payment program uses only geographic HPSAs, but the other programs generally make use of population or facility HPSAs as well. In our matched difference-in-differences setup discussed later, we study how becoming a primary care geographic HPSA impacts physician counts. Both treatment and matched control counties may contain population or facility HPSAs that we do not see with our data, which means that we conduct our analysis in an environment where some areas within counties may already be subject to incentives from the non-bonus-payment programs.

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.[11] It is based on CMS administrative claims data for Medicare Part B fee-for-service beneficiaries, and it represents the near-universe of Medicare-billing physicians. Only Medicare-billing doctors who do not bill any HCPCS code at least 10 times in a given year are omitted from the data for that year. Of note, more than 90% of non-pediatric primary care physicians accept Medicare patients (Boccuti et al. 2015). We extract from this dataset the unique physician identification numbers, National Provider Identifiers (NPIs), of Medicare-billing doctors and information regarding their specialty. From 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. 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.[12] Importantly, CMS requires physicians to update their NPPES information, including practice location, within 30 days of a change (CMS 2004). Linking these two 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

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

[12] The MPUP 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.

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

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

"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.[14] We define later-career PCPs as those who graduated more than 10 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, 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.[15] We also use county-level variables from the AHRF indicating the total number of active physicians per capita and

[14] 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.

[15] 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 5.4.

the percent of the population below the federal poverty line to carry out our matching procedure, and we employ three more variables from the AHRF specifying the population, unemployment rate, and median household income of counties as controls. In Section 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 a control group of non-designated counties. A potentially-naive difference-in-differences framework that aims to approximate this ideal would involve the comparison of designated counties (i.e., the treatment group) to counties that are not designated (i.e., the control group). Such a comparison is not without problems though, as counties designated as HPSAs are likely quite different in observable and unobservable ways than counties that are not designated. Indeed, we show below when discussing the analysis sample that physician counts in the potentially-naive control group of counties are trending quite differently from physician counts in HPSA counties, which raises concerns about the validity of a difference-in-differences estimator that uses all non-designated counties as a control group. We then show how our matching procedure solves this problem by selecting a control group of non-designated counties that are more similar to HPSAs.

4.1 Matched County Design

To select our control group, we use a matching procedure based on Deryugina et al. (2018), who study the long-run effects of Hurricane Katrina. We match to each treated county three control counties, and we assign the matched controls a placebo designation year equal to the actual designation year of their corresponding treated county.

To select the three control counties for each treated county, we use as our set of matching variables $\mathbf{X}_{ct}$ three variables defined 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 (pulled 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 of the counties that are never designated as HPSAs in our sample period. To compute the closeness between a treatment county c^* and a control county c, we sum the squared difference between counties of each variable $x_{ct} \in \mathbf{X}_{ct}$ (normalized by that variable's standard deviation in the pool of counties σ_{x_t}) across both years in the baseline period 2010–2011.[16] 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 whom they are matched.

We show that our results are robust to changing the matching procedure in Section 5.4. 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

The treatment group consists of 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. The matching method described above generates a control group from the sample of counties that are never designated as HPSAs between 2012 and 2017. Three counties are matched to each treatment county to serve as controls, and counties are allowed to be matched to more than one treatment county;

[16]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.

the resulting analysis sample thus includes 651 control counties, 470 of which are unique.[17]

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, a 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.

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 covariates (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 are our main outcome variables.[18]

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

[18]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.

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, and then assigning a placebo designation year to the comparison counties equal to the actual designation year for the HPSA county to which they are matched. The stock of physicians in all other counties is not trending 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, are trending in parallel with HPSAs over the pre-period.[19]

4.3 Difference-in-Differences Framework

To analyze the effect of designations, we use a standard difference-in-differences framework. Specifically, to document the dynamic impacts, we estimate

$$y_{ct} = \alpha + \beta treat_c + \sum_{\tau \neq -1} \gamma_\tau I_\tau + \sum_{\tau \neq -1} \delta_\tau (treat_c \times I_\tau) + Z_{ct}\theta + \varepsilon_{ct}, \tag{2}$$

where y_{ct} is an outcome for county c in year t (e.g., the number of Medicare-billing PCPs per 10,000 county residents at baseline), $treat_c$ is an indicator that equals one for counties receiving a designation over our sample period, the I_τ's are indicators for years relative to (actual or placebo) designation, Z_{ct} is a vector of controls, and the δ_τ's are the parameters of interest, which capture the average difference in y between the treatment and control groups relative to the omitted year.[20] In our leading specification, we include county-level

[19]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 be biasing our estimates, it would need to be the case that amenities change differentially across treatment and control right around the timing of designations. For some reference, Appendix Figure A.2 analyzes the evolution of the control variables around HPSA designations, using the dynamic difference-in-differences specification described below, and shows that the control variables are not changing differentially around designations.

[20]Recall that a control county can be matched to multiple treatment counties; in these cases, we give each instance of the control county a separate index value of c.

controls for median household income, current population, current population squared, and the unemployment rate. 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 completely standard difference-in-differences equation,

$$y_{ct} = \alpha + \beta treat_c + \gamma post_{ct} + \delta(treat_c \times post_{ct}) + Z_{ct}\theta + \varepsilon_{ct}, \tag{3}$$

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

$$\begin{aligned} y_{ct} = \alpha + \beta treat_c + \gamma^{SR} post_{ct}^{SR} + \gamma^{MR} post_{ct}^{MR} \\ + \delta^{SR}(treat_c \times post_{ct}^{SR}) + \delta^{MR}(treat_c \times post_{ct}^{MR}) + Z_{ct}\theta + \varepsilon_{ct}, \end{aligned} \tag{4}$$

where $post_{ct}^{SR}$ is a (post-period short-run) indicator that equals one if for county c year t is in the year of the designation, and $post_{ct}^{MR}$ is a (post-period medium-run) indicator that equals one if for county c year t is after the immediate year of designation. 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.

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 and C highlight how the total counts mask substantial response heterogeneity. Both panels illustrate how the parallel trends assumption for the counts of physicians by career stages does not seem to hold when comparing HPSA counties to all non-HPSA counties, but that the counts in the matched controls do move in parallel with the counts in the HPSA counties before designation. Panel B then shows that, after designation, average counts of early-career PCPs in HPSAs increase relative to the matched control group, and panel C shows that, even after designation, the average counts of later-career PCPs in HPSAs seem to track the counts in the control group. Because the majority of doctors are not early-career doctors, the pattern for later-career doctors drives the total counts, but these initial graphs emphasize the importance of analyzing physician counts 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 and contain other ZIP codes that are not HPSAs. To thus help with interpreting the reduced form results that follow, Figure 3 graphically presents a sort of 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. The graph shows a mechanical increase in HPSA exposure around the time of designations as we have defined them. The fact that the average percent of HPSA ZIP codes for treatment counties elevates to roughly 75%, instead of 100%, reflects the presence of some partially designated counties. The fact that this percentage remains roughly constant over time means that treatment intensity (i.e. the fraction of designated ZIP codes within a treatment county) does not seem to fluctuate much on average, which should be kept in mind when interpreting the patterns of the dynamic treatment effects.

Figure 4 presents results from estimating equation (2) separately for early-career and later-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, we winsorize outcomes at the 95th percentile to reduce the influence of outliers, and we include county-level controls for household income (flexibly, using indicators for median household income bins of $5,000), current population, current population squared, and the unemployment rate. 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 before the year of designation, which lends support to the parallel trends assumption. After designation, we see a relatively quick rise in the stock of these physicians practicing in HPSAs relative to non-HPSAs. The point estimate in year 0 is slightly elevated, whereas each of the point estimates on the indicators for the later post periods are positive and very similar to one another. The pattern of the dynamic estimates is consistent with a brief transition period over which the stock of doctors increases before stabilizing at the new level. In contrast, panel B shows no evidence of responses from later-career physicians. None of the point estimates are statistically distinguishable from zero, and the graph shows no discernible pattern or trend.

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 estimate, which quantifies 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 0.114 early-career PCPs per 10,000 (s.e. 0.0570). 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 0.67 more doctors per county, on average. The second column reports the pooled estimate, which is based on the entire post period and includes the transition year as seen in the dynamics, thus resulting in a slightly smaller point estimate. The point estimates for later-career physicians are not statistically distinguishable from zero. They are also much smaller in magnitude than those for early-career physicians, and the mean before designation is larger. Finally, for completeness, panel B reports estimates for the total counts of PCPs. As later-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 up 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 responses over the entire period.[21]

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 0.100 early-career ranked PCPs per

[21] 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 how the null results for unranked physicians are not driven by those reporting a medical school of "Other."

10,000 population on average following HPSA designation, which corresponds to about 0.59 physicians in the average treated county, a 40% increase off of a small mean. Point estimates for early-career unranked PCPs are much smaller and indistinguishable from zero. Panel B shows no evidence that ranked or unranked later-career PCPs 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 ranked medical schools.

5.3 Impact on Physicians in Other Specialties

Our 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 primary care specialty specifically, the CMS HPSA Physician Bonus Program does not limit bonus payments to primary care physicians. To thus provide a more comprehensive analysis, here we consider physicians in other specialties.

Specifically, we study the effects of HPSA designations on counts of 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. A lack of response by non-PCPs 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. However, 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, e.g., needing to be close to a hospital.

5.4 Robustness and Specification Checks

We assess the robustness of our results along several dimensions. For simplicity, we focus on the four main outcome variables: early-career PCPs, early-career PCPs from ranked schools, early-career PCPs from unranked schools, and later-career PCPs.

First, we assess the robustness of our estimates to various specification and sample selection choices and report results for the medium-run estimates in Table 4.[22] Row A reproduces the leading estimates for ease of comparison. The next 4 rows change the regression specification. Rows B through D vary the approach to censoring the data for outliers. Point

[22] Appendix Table A.3 presents corresponding results for the pooled estimates.

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. Row E drops the control variables from the regression, which does not meaningfully change the results.

Row F addresses partial county designations. It includes 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 similar for this sample, although the standard errors are larger, likely due to decreased sample size.

Rows G and H speak to potential issues related to overstating magnitudes. 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 physicians might locate, 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 G though, we study a smaller sample by only matching two control counties to each treatment county. This 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 H, 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.

Second, we assess the robustness of our estimates to the variables on which we match and report results for the medium-run estimates in Table 5.[23] 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

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

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 do not appear too 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. We find results similar to our main estimates. For a visual assessment, Appendix Figure A.5 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 0.67 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 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 already-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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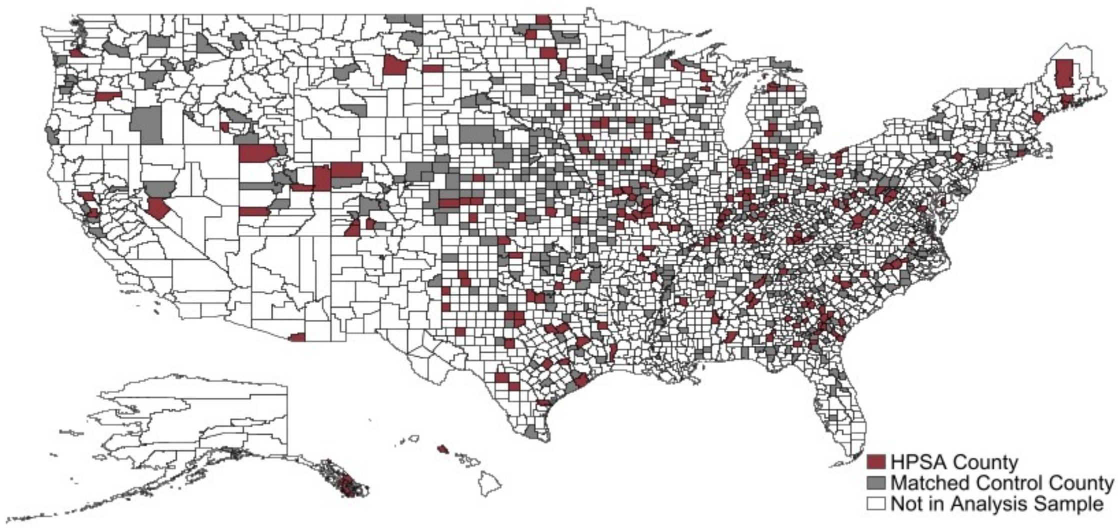
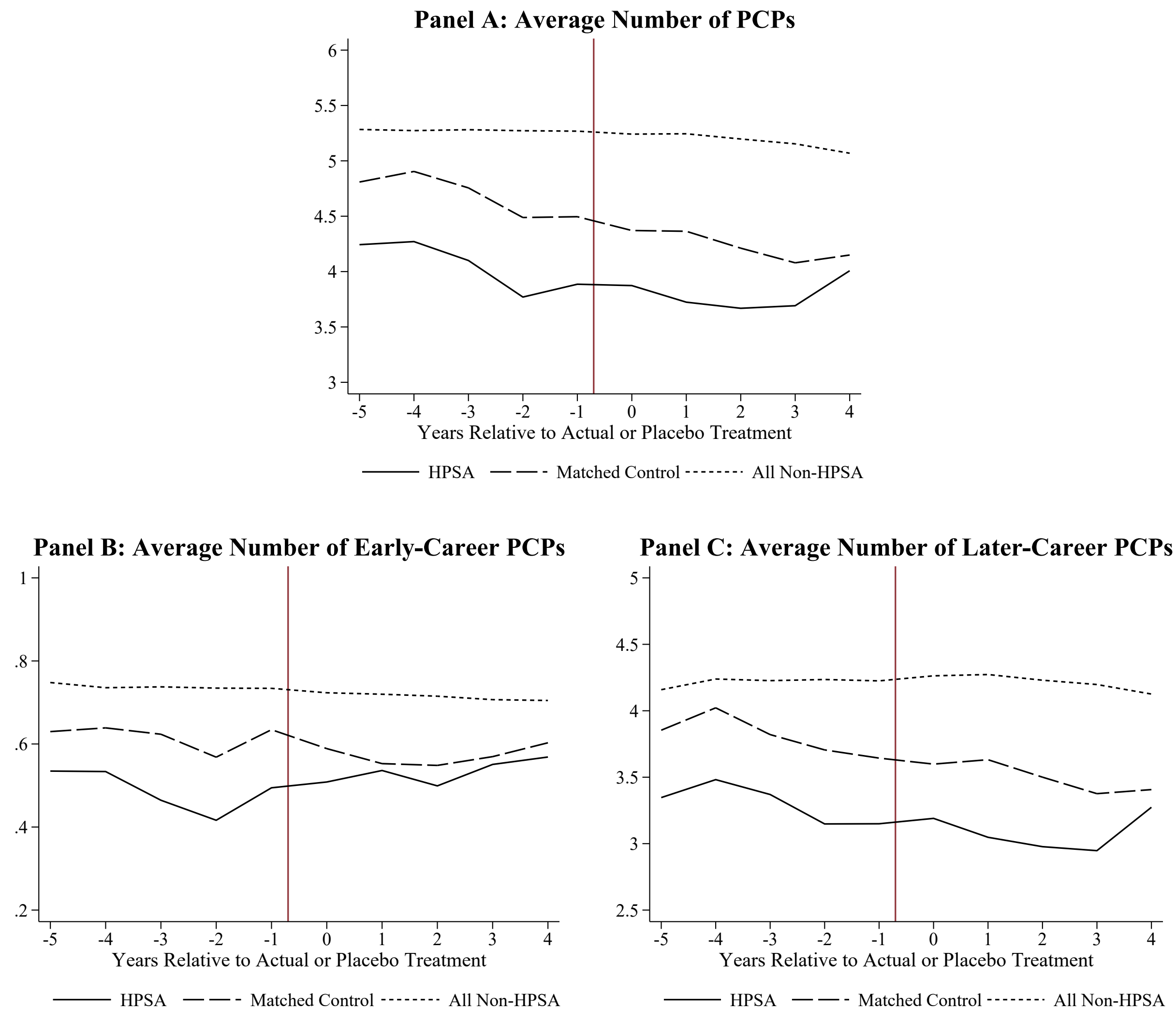
Figure 1: Geographic Variation in HPSA Designation Status



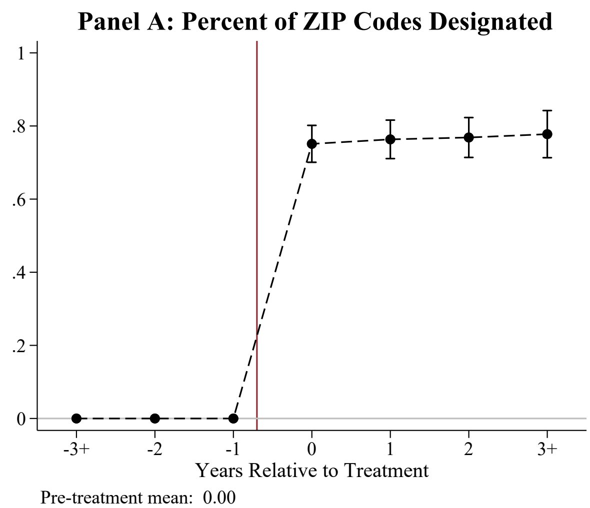
Notes: This map illustrates the geographic variation in Health Professional Shortage Area (HPSA) designation status that we study. The maroon counties are our treatment group. They are counties that become designated as a geographic primary care HPSA in some year between 2013 and 2017, where we define 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



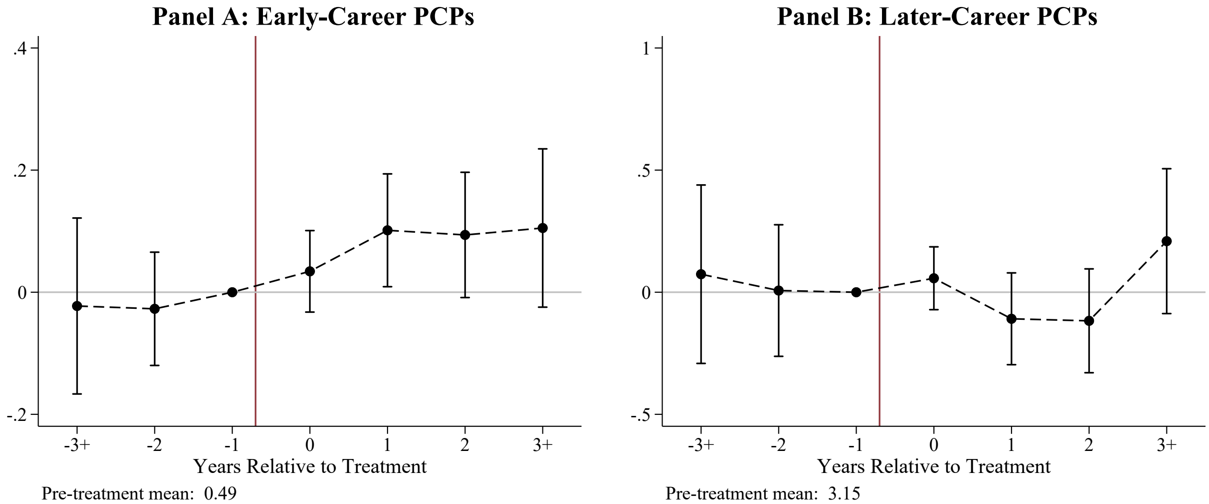
Notes: These graphs plot the average number of primary care physicians (PCPs) per 10,000 population around actual or placebo designation year for treatment HPSA counties, for unmatched potential controls, and for matched controls. The treatment sample consists of all counties that become designated as 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. Panel A plots the average total number of PCPs, panel B plots the average number of early-career PCPs, and panel C plots the average number of later-career PCPs.

Figure 3: Exposure to HSA Designation over Time



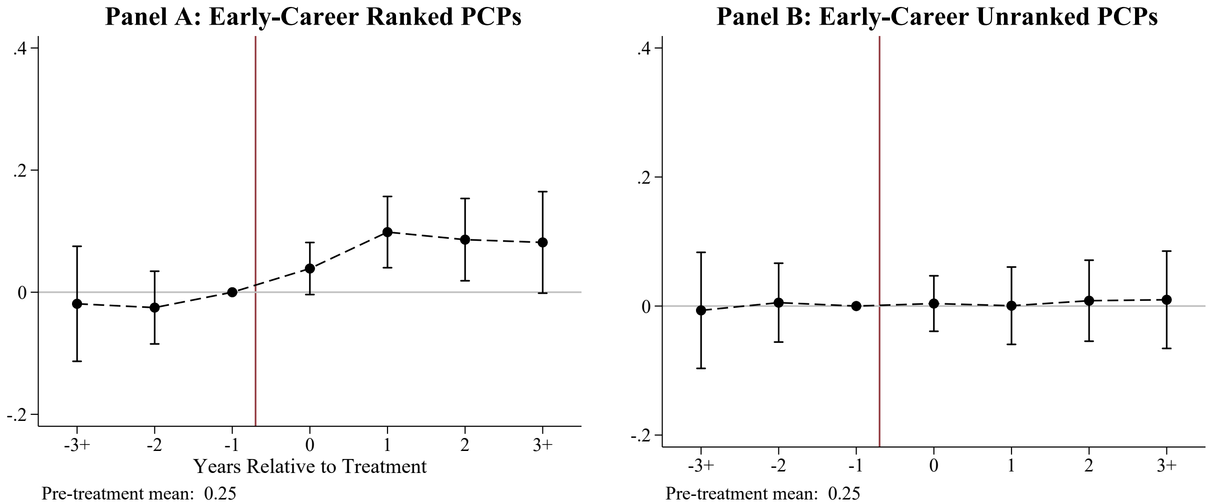
Notes: This graph illustrates how the fraction of county ZIP codes designated as an automatically-billed primary care geographic HPSA evolve around the timing of the designations that we study. Most of the treatment counties that we study (65%) are fully designated, meaning that 100% of the ZIP codes within the county have been designated; however, some are only partially designated. The graphs plot 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, without additional control variables and with standard errors clustered at the county level. The figure shows that, on average, after our treatment definition of designation, approximately 75% 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). Controls for unemployment rate, median household income, and population at the county-year level are included in each regression. 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). Controls for unemployment rate, median household income, and population at the county-year level are included in each regression. 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
	(0.168)	(0.128)
Early-Career PCPs Per 10,000	0.49**	0.63
	(0.049)	(0.042)
Early-Career Ranked PCPs Per 10,000	0.25	0.31
	(0.038)	(0.034)
Early-Career Unranked PCPs Per 10,000	0.25*	0.32
	(0.031)	(0.027)
Later-Career PCPs Per 10,000	3.15***	3.64
	(0.129)	(0.100)
Later-Career Ranked PCPs Per 10,000	1.56**	1.86
	(0.103)	(0.084)
Later-Career Unranked PCPs Per 10,000	1.59*	1.78
	(0.085)	(0.071)
Panel B: Other Variables (AHRF)		
Total Physicians Per 10,000	9.95	10.40
	(0.776)	(0.451)
Percent Persons in Poverty	17.27	17.42
	(0.450)	(0.421)
Population	58,969	67,569
	(9,967)	(8,372)
Median Household Income	44,480	44,161
	(692)	(531)
Unemployment Rate	7.23	6.86
	(0.207)	(0.161)
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 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	0.114**	0.0968*	0.49
	(0.0570)	(0.0509)	
Later-Career PCPs	-0.0092	0.0040	3.15
	(0.146)	(0.128)	
Panel B: Total PCP Counts			
Total PCPs	0.121	0.111	3.89
	(0.180)	(0.157)	
Clusters	687	687	687
Observations	5208	5208	5208

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). Controls for unemployment rate, median household income, and population at the county-year level are included in each regression. 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	0.100***	0.0873***	0.25
	(0.0361)	(0.0323)	
Early-Career Unranked PCPs	0.0069	0.0063	0.25
	(0.0335)	(0.0299)	
Panel B: Later-Career PCP Counts			
Later-Career Ranked PCPs	0.0474	0.0349	1.56
	(0.118)	(0.103)	
Later-Career Unranked PCPs	-0.0457	-0.0270	1.59
	(0.103)	(0.0914)	
Clusters	687	687	687
Observations	5208	5208	5208

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 later-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). Controls for unemployment rate, median household income, and population at the county-year level are included in each regression. Standard errors are clustered at the county level.

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

Table 4: 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)	Later-Career PCPs (4)
A. Leading Specification	0.114**	0.100***	0.0069	-0.0092
	(0.0570)	(0.0361)	(0.0335)	(0.146)
B. Winsorize Less	0.115*	0.116**	0.0059	0.0485
	(0.0691)	(0.0529)	(0.0380)	(0.161)
C. Winsorize More	0.107**	0.0772***	0.0047	-0.0502
	(0.0490)	(0.0293)	(0.0288)	(0.136)
D. No Winsorizing	0.113	0.116**	-0.0027	0.0476
	(0.0712)	(0.0570)	(0.0418)	(0.168)
E. No Control Variables	0.111*	0.0988***	0.0025	-0.0134
	(0.0578)	(0.0360)	(0.0344)	(0.150)
F. Only Fully Designated	0.101	0.105**	-0.0099	-0.184
	(0.0721)	(0.0454)	(0.0425)	(0.168)
G. Less Matched Controls	0.107*	0.106***	-0.00563	0.0464
	(0.0613)	(0.0392)	(0.0359)	(0.160)
H. Different State Controls	0.114**	0.109***	0.00511	0.0171
	(0.0572)	(0.0353)	(0.0342)	(0.147)

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 drops control variables from the regression. Row F 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 G matches only two control counties to each treatment county, rather than three. Row H studies an analysis sample where the control counties cannot be located in the same state as the treatment county to which they are matched. Standard errors are clustered at the county level.

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

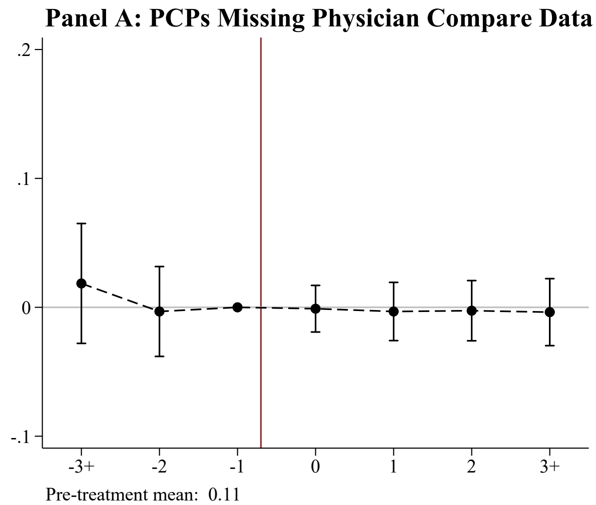
Table 5: Robustness of Medium-Run Estimates To Match Variables

	Leading Specification (1)	No Physician Trends (2)	No Physician Counts (3)	Only Other Area Attributes (4)
Early-Career PCPs	0.114**	0.0961*	0.0996*	0.109*
	(0.0570)	(0.0537)	(0.0597)	(0.0573)
Early-Career Ranked PCPs	0.100***	0.0943***	0.0862**	0.0851**
	(0.0361)	(0.0344)	(0.0375)	(0.0375)
Early-Career Unranked PCPs	0.0069	-0.0047	0.0116	0.0106
	(0.0335)	(0.0321)	(0.0355)	(0.0331)
Later-Career PCPs	-0.0092	-0.054	-0.0761	-0.0820
	(0.146)	(0.142)	(0.146)	(0.148)
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. Controls for current unemployment rate, median household income, and population at the county-year level are included in each regression. Standard errors are clustered at the county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

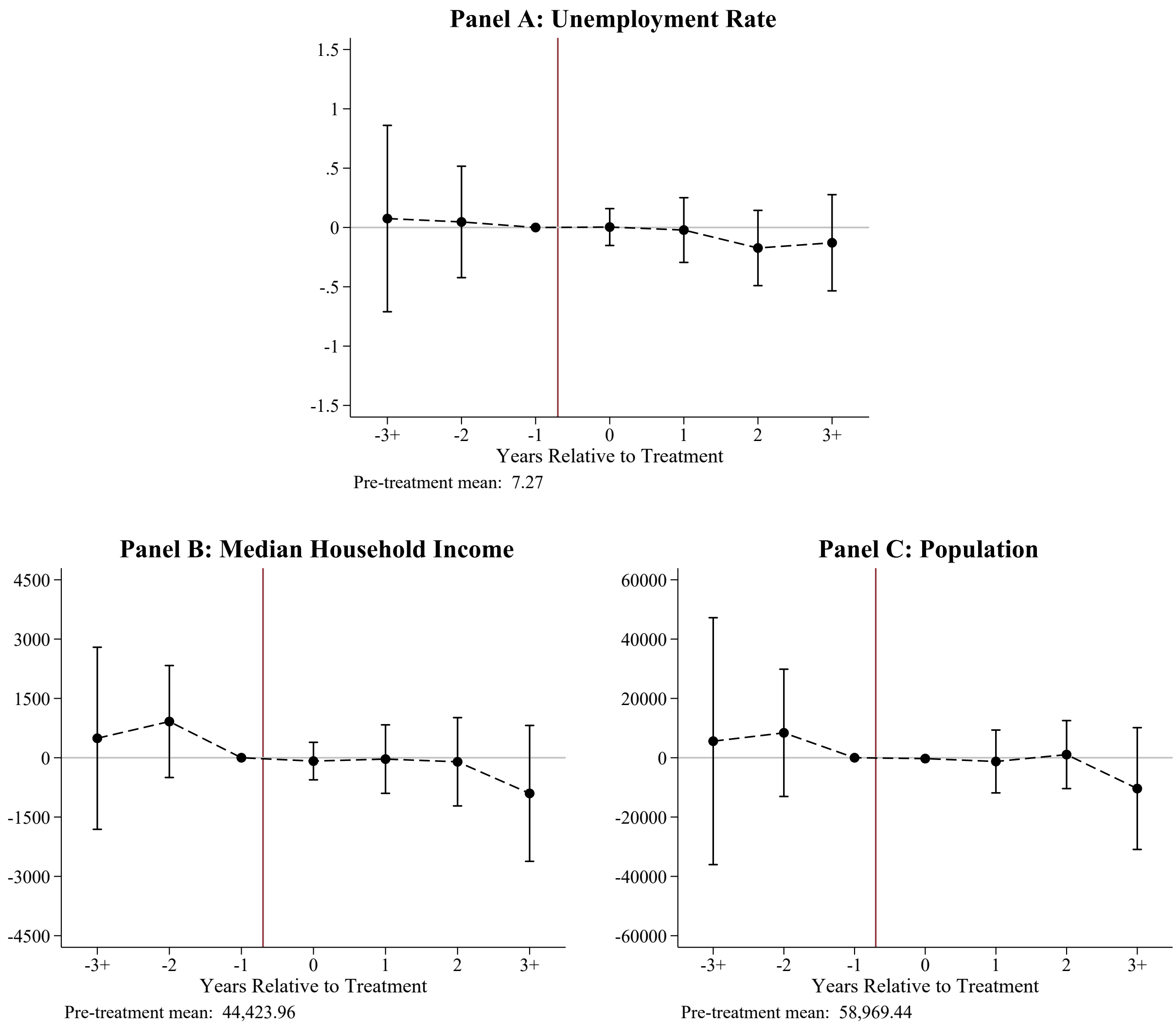
Appendix A   Additional Figures and Tables

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



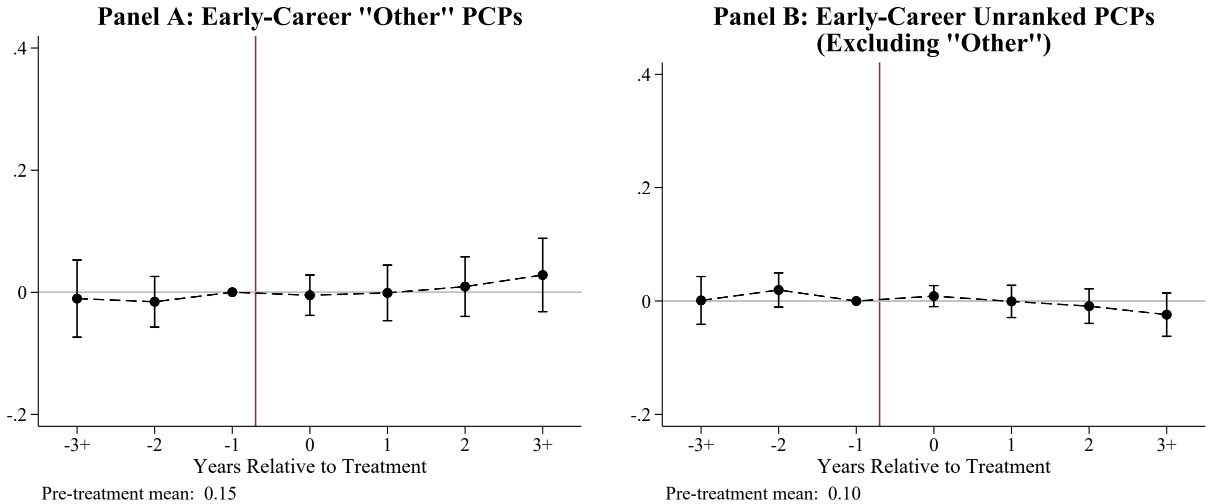
Notes: This graph plots the dynamic impact of HPSA designation on counts of primary care physicians (PCPs) for whom we are missing data on graduation year or medical school per 10,000 population. The graph plots point estimates of the δ_τ's and their 95% confidence intervals from estimating equation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

Figure A.2: Analyzing Control Variables as Outcome Variables



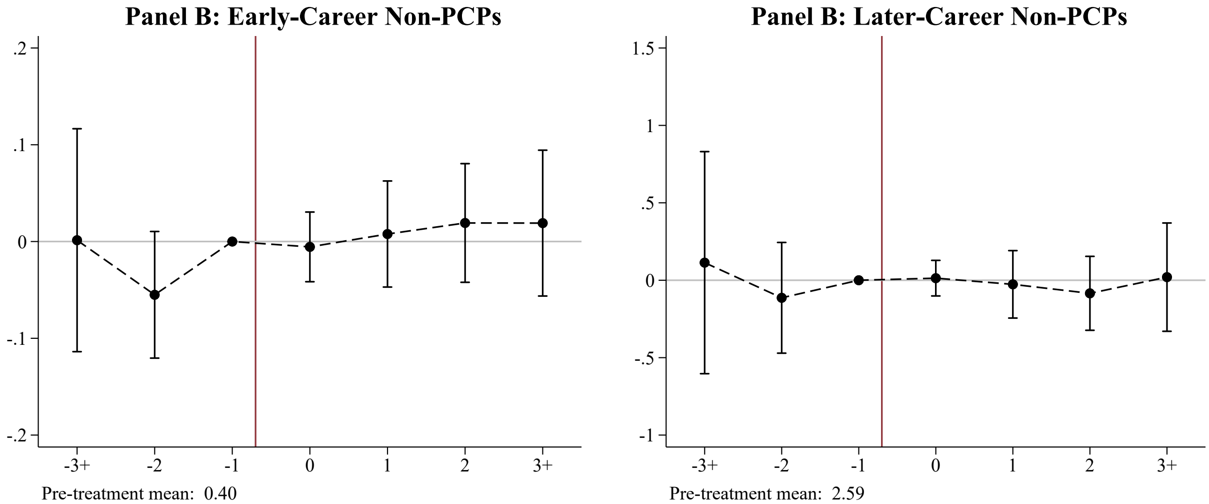
Notes: These graphs plot the dynamic impact of HPSA designations on the control variables. The graphs plot point estimates of the δ_τ's and their 95% confidence intervals from estimating equation (2). Since the control variables are being used as outcome variables, there are no controls in the regressions. Standard errors are clustered at the county level.

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



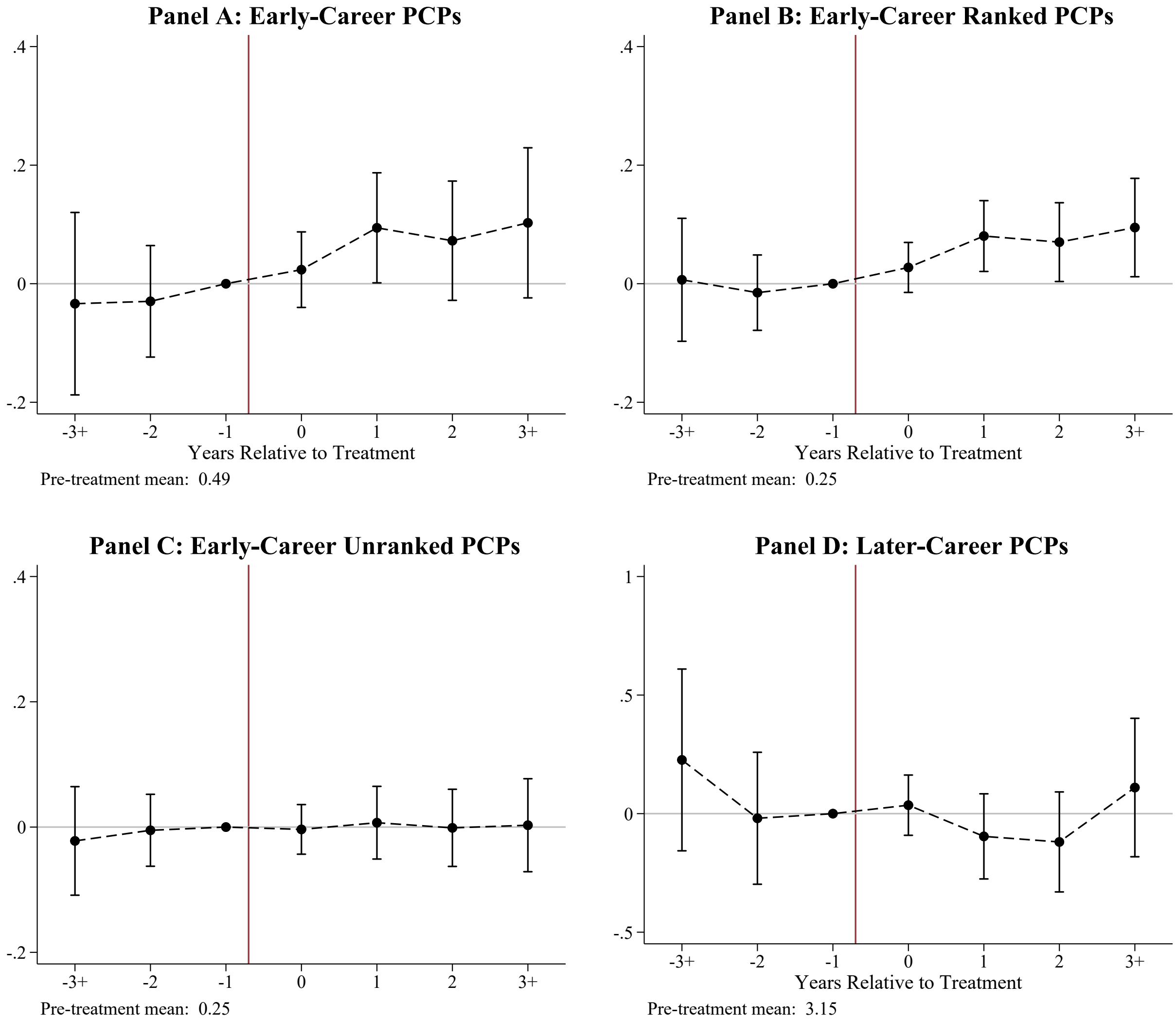
Notes: These graphs plot the dynamic impact of HPSA designation on counts of early-career primary care physicians (PCPs) per 10,000 population, where we further break down the counts of physicians who did not report attending a medical school that is ranked. Specifically, panel A analyzes counts of early-career 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). Controls for unemployment rate, median household income, and population at the county-year level are included in each regression. 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). Controls for unemployment rate, median household income, and population at the county-year level are included in each regression. Standard errors are clustered at the county level.

Figure A.5: 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. Specifically, each graph plots point estimates of the δ_τ's and their 95% confidence intervals from estimating equation (2), using an analysis sample of counties created by matching on geography, poverty rate, median household income, and population, defined over the baseline period of 2010 and 2011. Controls for current unemployment rate, median household income, and population at the county-year level are included in each regression. 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
Later-Career PCPs Per 10,000	2.68	2.34
Later-Career Ranked PCPs Per 10,000	1.21	1.80
Later-Career Unranked PCPs Per 10,000	1.47	1.34
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	0.0292	0.0237	0.40
	(0.0391)	(0.0345)	
Later-Career Non-PCPs	-0.0293	-0.0196	2.59
	(0.247)	(0.220)	
Panel B: Total Non-PCP Counts			
Total Non-PCPs	-0.0267	-0.0248	3.27
	(0.291)	(0.260)	
Clusters	687	687	687
Observations	5208	5208	5208

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). Controls for unemployment rate, median household income, and population at the county-year level are included in each regression. Standard errors are clustered at the county level.

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

Table A.3: 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)	Later-Career PCPs (4)
A. Leading Specification	0.0968*	0.0873***	0.0063	0.0040
	(0.0509)	(0.0323)	(0.0299)	(0.128)
B. Winsorize Less	0.0969	0.0987**	0.0043	0.0604
	(0.0616)	(0.0478)	(0.0341)	(0.141)
C. Winsorize More	0.0902**	0.0659**	0.0048	-0.0368
	(0.0436)	(0.0261)	(0.0256)	(0.119)
D. No Winsorizing	0.0989	0.103**	-0.0041	0.0605
	(0.0641)	(0.0521)	(0.0374)	(0.147)
E. No Control Variables	0.0946*	0.0865***	0.0027	0.0000
	(0.0515)	(0.0322)	(0.0307)	(0.131)
F. Only Fully Designated	0.0779	0.0893**	-0.0110	-0.161
	(0.0655)	(0.0415)	(0.0379)	(0.148)
G. Less Matched Controls	0.0870	0.0901**	-0.0059	0.0512
	(0.0547)	(0.0352)	(0.0320)	(0.141)
H. Different State Controls	0.0956*	0.0945***	0.0041	0.0278
	(0.0512)	(0.0317)	(0.0305)	(0.129)

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 drops control variables from the regression. Row F 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 G matches only two control counties to each treatment county, rather than three. Row H studies an analysis sample where the control counties cannot be located in the same state as the treatment county to which they are matched. 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 Match Variables

	Leading Specification (1)	No Physician Trends (2)	No Physician Counts (3)	Only Other Area Attributes (4)
Early-Career PCPs	0.0968*	0.0797*	0.0793	0910*
	(0.0509)	(0.0478)	(0.0534)	(0.0511)
Early-Career Ranked PCPs	0.0873***	0.0802***	0.0719**	0.0706**
	(0.0323)	(0.0307)	(0.0336)	(0.0337)
Early-Career Unranked PCPs	0.0063	-0.0031	0.0083	0.0089
	(0.0299)	(0.0284)	(0.0314)	(0.0290)
Later-Career PCPs	0.0040	-0.0417	-0.0569	-0.0658
	(0.128)	(0.124)	(0.128)	(0.130)
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. Controls for current unemployment rate, median household income, and population at the county-year level are included in each regression. 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 pull 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.