Health Professional Shortage Areas and Physician Location Decisions*

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Abstract

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

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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 and debates the relative importance of supply side factors versus demand side factors in causing this phenomenon, a closely-related fact has captured the interest of researchers and policymakers alike: some areas have significantly fewer doctors per capita than other areas. Individuals living in these so-called "shortage areas" may face higher costs of obtaining medical treatment and may be less likely to seek preventive care.

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

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

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

group of HPSA counties is unlikely to be a credible approach. Our matching strategy, which uses variables defined over a baseline time period that capture information directly relevant for official shortage area designations, addresses this concern by selecting counties similar to HPSAs to serve as controls.

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

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

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

Our findings have direct implications for policy. The 10 percent bonus payment attached to HPSAs is provided to all PCPs billing services to Medicare, but the majority of these are later-career doctors, who we find to be generally unresponsive. A more effective and cost-efficient way to increase physician counts in underserved areas may be to target a higher percentage bonus payment at the subset of physicians we find to be responsive. For instance,

using a simple and stylized policy exercise, we show that a 20 percent bonus payment offered to PCPs who relocate to a HPSA in the first 10 years of their career may induce even more movement of early-career physicians than the current program while substantially reducing overall payments to inframarginal doctors who would practice in a HPSA under either regime.

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

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

Finally, our findings connect to an important discussion in the literature on how payment policies influence the overall capacity of the healthcare system, particularly as it relates to

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

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

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

the allocation of human capital to and within the health sector. Existing work shows that Medicare policy can increase investments in medical technology (Finkelstein 2007, Acemoglu and Finkelstein 2008, and Clemens and Gottlieb 2014) as well as physician on-the-job investments (Clemens et al. 2018), and other papers highlight an important role for financial incentives in shaping the decision to become a doctor (Chen et al. 2020 and Gottlieb et al. 2020).[5] In finding that the HPSA program brings physicians to designated counties, we present evidence of a government payment policy expanding access to healthcare in specific geographies and influencing the distribution of health-sector human capital across space.[6]

The rest of this paper is organized as follows. Section 2 provides an exposition of the policy environment. Section 3 describes the data sources and highlights how we construct our dataset. Section 4 lays out our matched difference-in-differences framework. Section 5 presents our results. Section 6 discusses policy implications. We conclude in Section 7.

2 Policy Environment

Overview of Health Professional Shortage Areas. The Health Resources and Services Administration (HRSA), which is an agency of the United States Department of Health and Human Services, strives to "improve health outcomes and address health disparities through access to quality services, a skilled health workforce, and innovative, high-value programs."[7] In order to bring federal resources to people in need, HRSA creates shortage designations. Health Professional Shortage Areas (HPSAs) are one type of shortage designation, and it is this particular type on which CMS bases their Medicare bonus payment program.[8] HPSA designations can be made for three disciplines (primary care, mental health, and dental health) at three different levels (geographic area, population group, and facilities). Because primary care physicians (PCPs) play such a central role in the provision of healthcare in the United States, and because the CMS Medicare incentive payment program that we study in this paper does not apply to population group or facility shortage designations, we re-

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

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

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

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

strict our attention to HPSAs designated for the primary care discipline at the geographic level. Unless otherwise specified, hereafter we use the more general terms, "HPSAs" and "designations," to refer to this specific type of shortage designation.

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

Medicare Bonus Payments from CMS. The Centers for Medicare and Medicaid Services provide 10 percent bonus payments on Medicare services furnished by physicians in primary care geographic HPSAs designated by December 31 of the previous year. Bonuses are paid quarterly and are generated automatically when physicians provide services in a CMS-maintained list of HPSA ZIP codes, which consists of ZIP codes that fall entirely within a designated HPSA (e.g., all ZIP codes completely contained in a county that is 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. The 10% bonus payment program produces the major incentive for locating in HPSAs and applies to all physicians in HPSAs; though for some groups of doctors, other related programs may interact with designations to create additional incentives.[10]

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

[10] A variety of smaller federal incentive programs aim to bring physician and non-physician healthcare

3 Data

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

3.1 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 2012–2017.[11] It is based on CMS administrative claims data for Medicare Part B fee-for-service beneficiaries, and it represents the near-universe of Medicare billing physicians. Only Medicare-billing doctors who do not bill any HCPCS code at least 10 times in a given year are omitted from the data for that year. Of note, more than 90% of non-pediatric primary care physicians accept Medicare patients (Boccuti et al., 2015). We extract from this dataset the unique physician identification numbers, National Provider Identifiers (NPIs), of Medicare-billing doctors and information regarding their specialty. From annual disseminations of a second physician-level dataset, the *National Plan and Provider Enumeration System* (NPPES), we extract information on the primary practice location for the Medicare-billing physicians.[12] Linking these two datasets yields panel data for Medicare-

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

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

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

billing physicians spanning the years 2012 to 2017, with information on physician specialty and practice location.

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

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

After linking together the three physician-level data sources, we aggregate the data up to the county level. That is, we create a county-level dataset with counts of primary care Medicare-billing physicians spanning the years 2012 to 2017.[14] Finally, into our newly-constructed panel we merge data derived from two more sources. First, for information regarding HPSA status, we use the official, CMS-maintained list of ZIP codes that define

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

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

automatically billed HPSAs. We aggregate this data up to the county level by simply counting the number of HPSA ZIP codes in a county. Second, for more information on county characteristics, we pull variables from the *Area Health Resources File* (AHRF), which contains a wide range of county-level, health-related variables derived from the American Medical Association Masterfile and county-level demographic and economic variables derived from the American Community Survey. Linking together all of the data sources, we create a county panel containing information on population demographics, economic conditions, HPSA designations, and the stock of Medicare-billing primary care physicians.

3.2 Key Variables

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

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

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

be ranked if it is any one of the 95 schools receiving an official ranking.[16]

We use several additional variables in our matched difference-in-differences design. In particular, we define our treatment variables based on whether or not a county contains at least one automatically-billed designated HPSA ZIP code.[17] We also use county-level variables from the AHRF indicating the total number of active physicians per capita and the percent of the population below the federal poverty line to carry out our matching procedure, and we employ three more variables from the AHRF specifying the population, unemployment rate, and median household income of counties as controls. In Section 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 physician location decisions. An ideal experiment would randomly assign HPSA designations to some counties and track the counts of physicians in these counties compared to a control group of non-designated counties. A potentially-naive difference-in-differences framework that aims to approximate this ideal would involve the comparison of designated counties (i.e., the treatment group), in which 10% bonus payments are made to Medicare-billing PCPs, to counties that are not designated (i.e., the control group), in which there are no 10% bonus payments for Medicare-billing PCPs. Such a comparison is not without problems, as counties designated as HPSAs are likely very different in observable and unobservable ways than counties that are not designated.

Indeed, Figure 1 illustrates exactly this concern. The solid line depicts the average count of PCPs in HPSAs, where time on the x-axis is relative to designation year. The stock of physicians in HPSA counties tends to fall leading up to the designation year, which is not unexpected. In contrast, the dotted line depicts the average count of PCPs for the potential control group that consists of all other counties. Relative time for this comparison group is defined by matching to each HPSA all other counties, and then assigning a placebo designation year to the comparison counties equal to the actual designation year for the

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

[17] While some counties are only "partially" HPSA-designated, meaning only some of its zip codes are on the CMS list of automatically billed HPSAs, the majority of HPSA-designated counties in our sample are fully designated. There are 79 (36.4%) partially designated counties in our analysis data. Of those, 20% are at least 50% designated. We assess the robustness of our results to the exclusion of partially designated counties in Section 5.2.

HPSA county to which they are matched. The stock of physicians in all other counties is not falling in the years before placebo designation, which would raise concerns about the validity of a straightforward difference-in-differences estimator.

For these reasons, we use a matched difference-in-differences approach to select a control group of non-designated counties that are more similar to HPSAs. In Section 4.1, we detail our procedure for selecting the control group and discuss our analysis sample. In Section 4.2, we describe the specifics of how we implement our matched difference-in-differences design.

4.1 Matched County Design

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

To select the three control counties for each treated county, we use as our set of matching variables $\mathbf{X}_{ct}$ three variables defined at a baseline: number of active physicians per capita, annual percentage change in active physicians per capita, and percent of the population below the federal poverty line. We use these variables (pulled from the AHRF) from 2010 and 2011, which corresponds to two or three years before any of the earliest designations that we study. HRSA uses both the stock of physicians and the poverty rate to determine the score of proposed HPSAs, and designations are largely due to declines in physician counts; therefore, we view these variables as a reasonable and natural benchmark set on which to match.

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

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

pool of counties σ_{x_t}) across both years in the baseline period 2010–2011.[19] That is,

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

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

We probe the robustness of our results to changing different aspects of the matching procedure in Section 5.2. Specifically, we vary the combination of baseline variables used to construct the match, and we vary the number of control counties matched to each treatment county.

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

Table 1 presents summary statistics for descriptive variables, for the treatment and control groups separately. The statistics come from the year preceding (actual or placebo) designation. The table shows that HPSAs generally look similar to control counties in terms of descriptive observables, although they are less populous and have slightly fewer physicians. Figure 1 makes it clear that the matched sample improves upon the non-matched sample in terms of assessing the validity of a difference-in-differences estimator through examination of

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

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

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

parallel pre-trends. The dashed line plots the average counts of PCPs in our control group constructed using the matching procedure. The group experiences a decline in the stock of PCPs before placebo designation year similar to that in HPSAs, which allows us to more confidently use the evolution of PCP counts in the control group as a counterfactual for the evolution of PCP counts in the treatment group.

4.2 Implementation

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

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

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

The identifying assumption asserts that, in the absence of HPSA designations, the stock of Medicare-billing PCPs in treated counties would have evolved in parallel with that in control counties. Analyzing the estimated δ_τ's from equation (2) provides an assessment on the validity of the design; specifically, we test whether the δ_τ's for $\tau < 0$ are different from zero, which would indicate the presence of pre-trends and might raise concerns regarding our difference-in-differences approach. Encouragingly, we consistently find no evidence of pre-trends that might invalidate the design.

Estimating the fully dynamic specification permits an evaluation of the key parallel trends assumption, but it also shows how the stock of doctors evolves over time; that is, results from estimating equation (2) shed light on how immediate or delayed, as well as how persistent or temporary, any physician responses to designations might be. After assessing the dynamic

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

impact of HPSA designations, to better quantify the magnitudes of the mean treatment effect, we estimate the usual difference-in-differences estimating equation:

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

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

Finally, while estimating equation (3) pools all pre-period years together and all post-period years together in order to quantify the overall effect, we employ one related additional specification. Guided by the graphical analysis of the dynamic impact, we split the post-designation period into two: a short-run period and a medium-run period. Specifically, we estimate

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

where $post_{ct}^{SR}$ is a (post-period short-run) indicator that equals one if for county c year t is in the year of the designation, and $post_{ct}^{MR}$ is a (post-period medium-run) indicator that equals one if for county c year t is after the immediate year of designation. Estimating equation (4) allows us to split up the post period and quantify short-run and medium-run effects, captured by δ^{SR} and δ^{MR} respectively. We often highlight the medium run estimates, which capture the impact on counts of doctors practicing in a county after allowing for the stock to evolve over a brief transition period.

5 Results

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

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

5.1 Main Results

Figure 2 presents the results of estimating equation (2) for early-career and later-career PCPs.[24] The estimates for each parameter δ_τ are plotted along with 95% confidence intervals. These point estimates allow us to assess the validity of the identifying assumption and examine dynamic impacts.

The left-hand-side graph presents estimates of the impact of HPSA designation on counts of early-career doctors. The point estimates for δ_τ where $\tau < 0$ are not statistically different from zero and do not appear to be trending in any direction before the year of designation, which lends support to the parallel trends assumption. After designation, we see a relatively quick rise in the stock of these physicians practicing in HPSAS relative to non-HPSAs. The point estimate in year 0 is slightly elevated, whereas each of the point estimates on the indicators for the later post periods are positive and very similar to one another. The pattern of the dynamic estimates is consistent with a brief transition period over which the stock of doctors increases in response to the reform before stabilizing at the new level; this pattern also motivates a particular focus on the medium run estimates, which will quantify the effect of the policy on the stock of doctors after this brief transition period. Results from estimating equations (3) and (4) to quantify magnitudes are reported in Table 2. Column (1) summarizes the responses of early-career doctors. Panel A shows a statistically significant average medium-run increase of 0.114 early-career doctors per 10,000 (s.e. 0.0570). This estimate corresponds to an increase of about 23% when compared to the baseline mean of 0.49 in the period before designation, and given that the average population of a treated county in our sample is around 59,000, it translates to approximately 0.67 more doctors per county on average. Panel B reports the average treatment effect for the entire post period, which includes the transition year as seen in the dynamics, thus resulting in a slightly smaller point estimate.

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

[24]The corresponding graphs of raw means for these outcomes can be found in Appendix Figure A.1. As defined in Section 3.2, early-career PCPs are those who graduated 5 to 10 years ago.

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

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

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

Lastly, to provide a gauge for the overall impact of designations, we present estimates

on the per capita stock of all Medicare-billing PCPs. Figure 4 shows no evidence that designations have an impact on total PCP counts. This is not surprising, as the majority of PCPs are later-career PCPs, whom we have found to be unresponsive to HPSA status. We quantify corresponding magnitudes in Table 4. Columns (2) and (3) report separate estimates for the total stocks of ranked and unranked PCPs, both of which are statistically indistinguishable from zero.

5.2 Robustness and Specification Checks

We assess the robustness of our results along several dimensions. For simplicity, we focus on treatment effects from estimating equation (3) and medium run effects from estimating equation (4), for each of our main outcome variables: early-career PCPs; early-career PCPs from ranked schools; early-career PCPs from unranked schools; and later-career PCPs.

First, we probe the sensitivity of our results to various regression specifications. Table 5 displays results for the medium run effects, and Table 6 displays results for the mean treatment effects over all post-designation years. Each table is constructed as follows. Row A reproduces the baseline estimates. Rows B through D vary the approach to censoring the data for outliers. Rows E and F assess the sensitivity to inclusion of control variables. Overall, across both tables, we see that our results are not too sensitive to the choice of winsorization; point estimates are similar if we winsorize more stringently, winsorize less stringently, or do not winsorize at all, though we tend to experience precision gains when winsorizing more of the data. Further, results appear robust to both omitting all of the control variables as well as adding additional controls (year and state fixed effects).

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

Third, we vary our matching strategy. Appendix Table B.2 reports results from altering the number of control counties that we match to each treatment county. Point estimates are broadly stable, though those for later-career PCPs appear more sensitive. Appendix Table

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

6 Policy Discussion

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

To illustrate this, we walk through a simple policy analysis that compares the estimated cost effectiveness of the 10% bonus payment program to that of a hypothetical alternative program that offers larger bonus payments to only early-career PCPs. This exercise requires some caveats, as we make a handful of simplifying assumptions. Importantly, we assume that the entirety of the effect of HPSA designation on the stock of early-career PCPs stems from the bonus payments. However, other programs connected to HPSA designations as well as potential interactions between private insurance payments and HPSA status may contribute to the total incentives associated with designations.[26] We also focus just on the costs and

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

[26]The 10% bonus payment is a salient and major incentive that impacts all doctors in HPSAs, and our estimates come from studying designations defined using CMS data on automatically-billed HPSAs. To the extent that official HPSA designations interact with other various government programs related to

effects of the program for PCPs, even though all physicians practicing in HPSAs receive the bonus payments. We make back-of-the-envelope calculations that take our point estimates at face value and assume that effects scale linearly with the size of the bonus payments. Our aim is to conduct a simple yet informative exercise that draws from our main findings to highlight policy implications.

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

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

shortage areas though, there could be additional incentives for locating in a HPSA. For instance, most states maintain loan forgiveness programs for practicing in rural areas, some of which may use criteria related to official HPSA designations. (See Kulka and McWeeny (2019) for a more detailed discussion of state loan forgiveness programs.) Additionally, to the extent that private insurance companies follow the lead of Medicare (Clemens and Gottlieb 2017, Clemens et al. 2017) and offer bonus payments for providing services in shortage areas, the direct financial incentives for locating in a HPSA could be even greater.

[27]The figure of $2,268,600 per year for 1 additional PCP per 10,000 comes from dividing the average annual bonus payment at the county level ($226,900) by the average increase in early-career PCPs attributed to HPSA designation (about 0.1 PCPs per 10,000). Note that the MPUP dataset omits line items for services provided by an NPI to 10 or fewer beneficiaries in a given year, so all cost figures slightly understate the true totals.

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

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

7 Conclusion

This paper studies how physician location decisions respond to 10 percent Medicare bonus payments for practicing in "shortage areas." We find that while the majority of primary care physicians do not appear to respond to the policy, an important subset of doctors do respond. Designated counties, on average, experience an increase in the stock of early-career physicians that amounts to roughly 23% and corresponds to about 0.67 more doctors per county. Results indicate that this increase occurs rather quickly, is stable over time, and is driven by increases in counts of PCPs who attended ranked medical schools.

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

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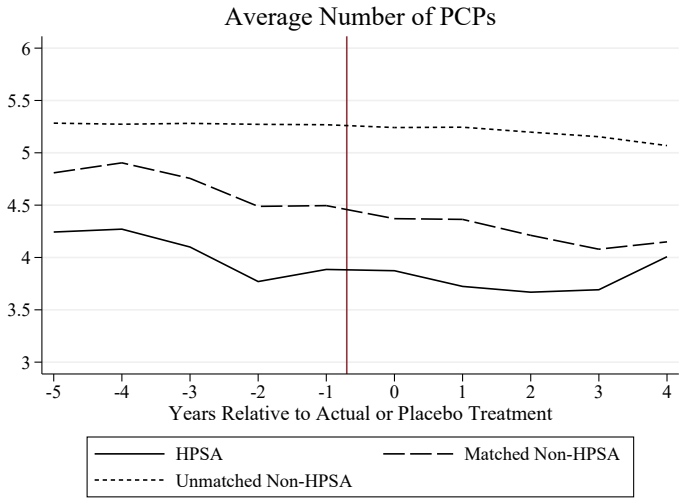
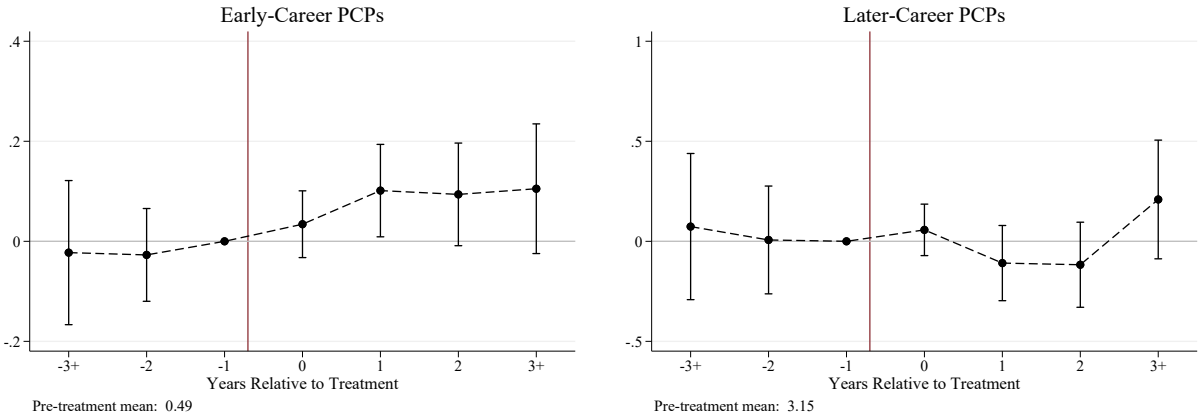
Figure 1: Average Number of PCPs for HPSA and Non-HPSA Counties



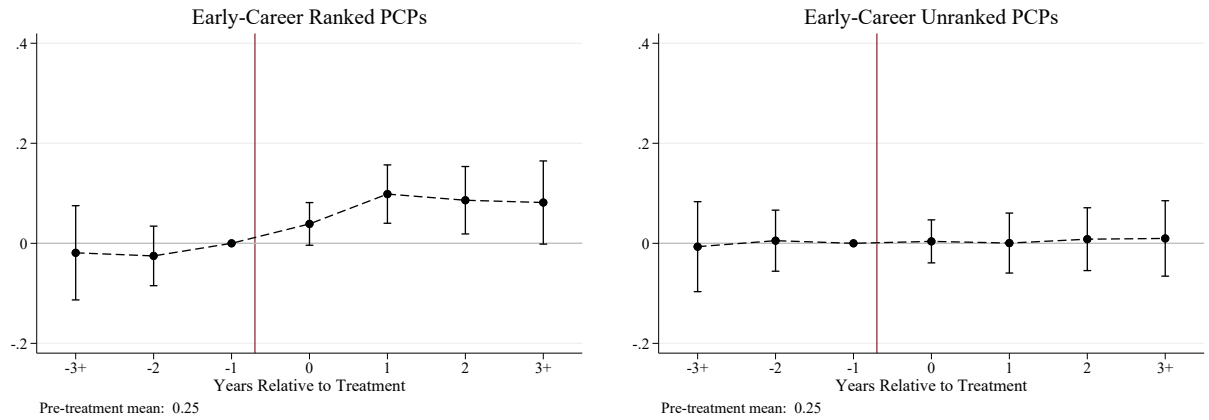
Notes: This graph plots the average number of PCPs per 10,000 population for treatment HPSA counties and potential non-HPSA control counties around actual or placebo designation year. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The matched control sample consists of the non-HPSA counties that are matched to HPSA counties using the method described in Section 4. The unmatched control sample consists of all counties that are never designated as a HPSA during 2012–2017, assigned as controls to and given placebo designation years from all counties in the treatment sample. Three control counties are matched to each treatment county, resulting in 217 treatment counties, 651 matched control counties (470 of which are unique), and 1,606 unmatched control counties. The x-axis shows the years relative to actual or placebo HPSA designation.

Figure 2: Impact of HPSA Designation on PCP Counts by Career Stage



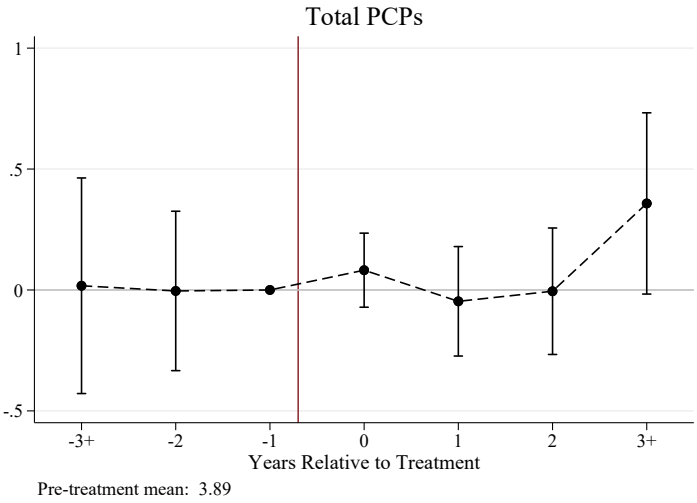
Notes: These graphs plot the point estimates of the δ_τ's and their 95% confidence intervals from estimating equation (2), where the outcome y_{ct} is the stock of PCPs in the indicated career stage per 10,000 population in a county. Early-career PCPs are those graduating 5-10 years earlier and later-career PCPs are those graduating more than 10 years earlier. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The control sample consists of all counties that are never designated as a HPSA during 2012–2017 and are matched to the treatment counties using the matching procedure described in Section 4.1. Three control counties are matched to each treatment county, resulting in 217 treatment counties and 651 control counties. 470 of the 651 control counties are unique, as control counties can be matched to multiple treatment counties. The x-axis shows the years relative to HPSA designation. Controls for unemployment rate, median household income, and population at the county-year level are included in each regression.

Figure 3: Impact of Designation on Early-Career PCP Counts by Medical School Rank



Notes: These graphs plot the point estimates of the δ_τ's and their 95% confidence intervals from estimating equation (2), where the outcome y_{ct} is the stock of early-career PCPs who attended ranked or unranked medical schools per 10,000 population in a county. Early-career PCPs are those graduating 5-10 years earlier. The 95 schools included in the 2018 U.S. News Primary Care medical school rankings are defined as ranked, and all other medical schools are defined as unranked. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The control sample consists of all counties that are never designated as a HPSA during 2012–2017 and are matched to the treatment counties using the matching procedure described in Section 4.1. Three control counties are matched to each treatment county, resulting in 217 treatment counties and 651 control counties. 470 of the 651 control counties are unique, as control counties can be matched to multiple treatment counties. The x-axis shows the years relative to HPSA designation. Controls for unemployment rate, median household income, and population at the county-year level are included in each regression.

Figure 4: Impact of HPSA Designation on Total PCP Counts



Notes: This graph plots the point estimates of the δ_τ's and their 95% confidence intervals from estimating equation (2), where the outcome y_{ct} is the stock of PCPs per 10,000 population in a county. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The control sample consists of all counties that are never designated as a HPSA during 2012–2017 and are matched to the treatment counties using the matching procedure described in Section 4.1. Three control counties are matched to each treatment county, resulting in 217 treatment counties and 651 control counties. 470 of the 651 control counties are unique, as control counties can be matched to multiple treatment counties. The x-axis shows the years relative to HPSA designation. Controls for unemployment rate, median household income, and population at the county-year level are included in each regression.

Table 1: Summary Statistics for Descriptive Variables

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

Notes: This table presents summary statistics for the analysis sample. Statistics are presented separately for the treatment group and the control group. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The control sample consists of all counties that are never designated as a HPSA during 2012–2017 and are matched to the treatment counties using the matching procedure described in Section 4.1. Three control counties are matched to each treatment county, resulting in 217 treatment counties and 651 control counties. 470 of the 651 control counties are unique, as control counties can be matched to multiple treatment counties. Data for each variable in the table is obtained for each county from the Area Health Resources File in the year before treatment for treatment counties and the year before the assigned treatment year for control counties. Physicians Per 10k (and its percentage change) and Percent Persons in Poverty are the variables used in the matching procedure to determine the closeness of eligible control counties to treatment counties.

Table 2: Impact of HPSA Designation on PCP Counts by Career Stage

	(1) Early-Career PCPs	(2) Later-Career PCPs
Panel A. Split Post-Period		
$treat_c \times post_{ct}^{SR}$	0.0476	0.0349
	(0.0431)	(0.0947)
$treat_c \times post_{ct}^{MR}$	0.114**	-0.00913
	(0.0570)	(0.146)
Panel B. Pooled Post-Period		
$treat_c \times post_{ct}$	0.0968*	0.00400
	(0.0509)	(0.128)
Dep. Mean	0.49	3.15
Clusters	687	687
Observations	5208	5208

Notes: This table presents the point estimates of δ^{SR} and δ^{MR} from estimating equation (4) in Panel A, and the point estimate of δ from estimating equation (3) in Panel B, where the outcome y_{ct} is the stock of PCPs in the indicated career stage per 10,000 population in a county. Early-career PCPs are those graduating 5-10 years earlier and later-career PCPs are those graduating more than 10 years earlier. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The control sample consists of all counties that are never designated as a HPSA during 2012–2017 and are matched to the treatment counties using the matching procedure described in Section 4.1. Three control counties are matched to each treatment county, resulting in 217 treatment counties and 651 control counties. 470 of the 651 control counties are unique, as control counties can be matched to multiple treatment counties. Controls for unemployment rate, median household income, and population at the county-year level are included in each regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Impact of Designation on Early-Career PCPs by Medical School Rank

	(1) Early-Career Ranked PCPs	(2) Early-Career Unranked PCPs
Panel A. Split Post-Period		
$treat_c \times post_{ct}^{SR}$	0.0507*	0.00446
	(0.0278)	(0.0264)
$treat_c \times post_{ct}^{MR}$	0.100***	0.00694
	(0.0361)	(0.0335)
Panel B. Pooled Post-Period		
$treat_c \times post_{ct}$	0.0873***	0.00625
	(0.0323)	(0.0299)
Dep. Mean	0.25	0.25
Clusters	687	687
Observations	5208	5208

Notes: This table presents the point estimates of δ^{SR} and δ^{MR} from estimating equation (4) in Panel A, and the point estimate of δ from estimating equation (3) in Panel B, where the outcome y_{ct} is the stock of early-career PCPs who attended ranked or unranked medical schools per 10,000 population in a county. Early-career PCPs are those graduating 5-10 years earlier. The 95 schools included in the 2018 U.S. News Primary Care medical school rankings are defined as ranked, and all other medical schools are defined as unranked. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The control sample consists of all counties that are never designated as a HPSA during 2012–2017 and are matched to the treatment counties using the matching procedure described in Section 4.1. Three control counties are matched to each treatment county, resulting in 217 treatment counties and 651 control counties. 470 of the 651 control counties are unique, as control counties can be matched to multiple treatment counties. Controls for unemployment rate, median household income, and population at the county-year level are included in each regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Impact of HPSA Designation on PCPs by Medical School Rank

	(1) Total PCPs	(2) Ranked PCPs	(3) Unranked PCPs
Panel A. Split Post-Period			
$treat_c \times post_{ct}^{SR}$	0.0786	0.0381	0.0322
	(0.115)	(0.0917)	(0.0803)
$treat_c \times post_{ct}^{MR}$	0.121	0.163	-0.0106
	(0.180)	(0.136)	(0.118)
Panel B. Pooled Post-Period			
$treat_c \times post_{ct}$	0.111	0.131	0.000776
	(0.157)	(0.120)	(0.105)
Dep. Mean	3.89	1.89	1.88
Clusters	687	687	687
Observations	5208	5208	5208

Notes: This table presents the point estimates of δ^{SR} and δ^{MR} from estimating equation (4) in Panel A, and the point estimate of δ from estimating equation (3) in Panel B. The outcome y_{ct} is the stock of PCPs per 10,000 population in a county in column 1, and this outcome is split up into PCPs who attended ranked or unranked medical schools in columns 2 and 3. The 95 schools included in the 2018 U.S. News Primary Care medical school rankings are defined as ranked, and all other medical schools are defined as unranked. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The control sample consists of all counties that are never designated as a HPSA during 2012–2017 and are matched to the treatment counties using the matching procedure described in Section 4.1. Controls for unemployment rate, median household income, and population at the county-year level are included in each regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Robustness of Medium-Run Estimates to Alternative Regression Specifications

	(1) Early-Career PCPs	(2) Early-Career Ranked PCPs	(3) Early-Career Unranked PCPs	Later-Career PCPs
A. Baseline	0.114**	0.100***	0.00694	-0.00913
	(0.0431)	(0.0361)	(0.0335)	(0.146)
B. Winsor. 99	0.115*	0.116**	0.00594	0.0485
	(0.0691)	(0.0529)	(0.0380)	(0.161)
C. Winsor. 90	0.107*	0.0772***	0.00470	-0.0501
	(0.0490)	(0.0293)	(0.0288)	(0.136)
D. No Censoring	0.113	0.116**	-0.00269	0.0477
	(0.0712)	(0.0570)	(0.0418)	(0.168)
E. No Controls	0.111*	0.0988***	0.00251	-0.0134
	(0.0578)	(0.0360)	(0.0344)	(0.150)
F. More Controls	0.109*	0.0979***	0.00547	-0.0422
	(0.0553)	(0.0341)	(0.0327)	(0.144)
Clusters	687	687	687	687
Observations	5208	5208	5208	5208

Notes: This table presents point estimates of δ^{MR} from estimating equation (4) for the main outcomes as we vary the regression specification. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The control sample consists of all counties that are never designated as a HPSA during 2012–2017 and are matched to the treatment counties using the matching procedure described in Section 4.1. Three control counties are matched to each treatment county, resulting in 217 treatment counties and 651 control counties. 470 of the 651 control counties are unique, as control counties can be matched to multiple treatment counties. Row A reproduces our baseline estimates. Row B winsorizes outcome variables at the 99th percentile. Row C winsorizes outcome variables at the 90th percentile. Row D does not winsorize outcome variables. Row E drops controls from the regression. Row F adds year and state fixed effects to the regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

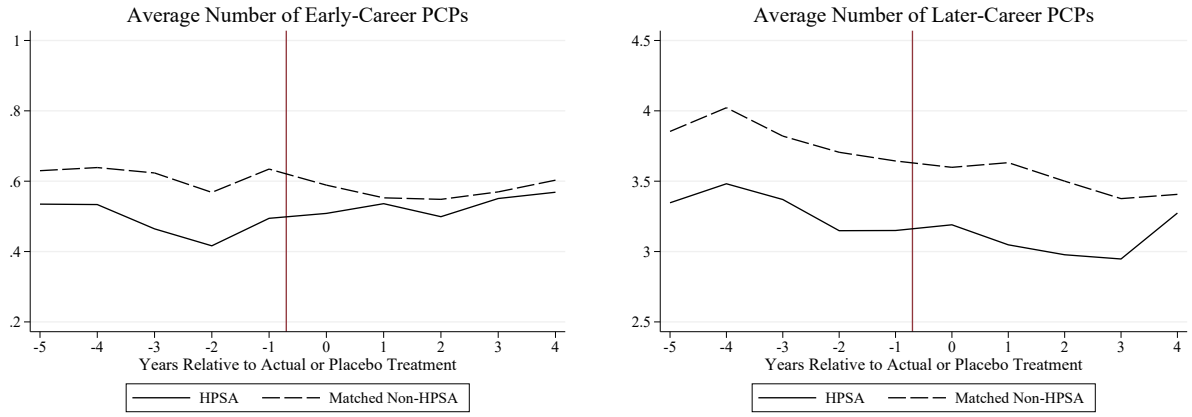
Table 6: Robustness of Pooled Estimates to Alternative Regression Specifications

	(1) Early-Career PCPs	(2) Early-Career Ranked PCPs	(3) Early-Career Unranked PCPs	Later-Career PCPs
A. Baseline	0.0968*	0.0873***	0.00625	0.0349
	(0.0509)	(0.0323)	(0.0299)	(0.0947)
B. Winsor. 99	0.0969	0.0987**	0.00434	0.0604
	(0.0616)	(0.0478)	(0.0341)	(0.141)
C. Winsor. 90	0.0902**	0.0659**	0.00483	-0.0368
	(0.0436)	(0.0261)	(0.0256)	(0.119)
D. No Censoring	0.0989	0.103**	-0.00409	0.0605
	(0.0641)	(0.0521)	(0.0374)	(0.147)
E. No Controls	0.0946*	0.0865***	0.00270	0.0000441
	(0.0515)	(0.0322)	(0.0307)	(0.131)
F. More Controls	0.0924*	0.0851***	0.00522	-0.0233
	(0.0495)	(0.0306)	(0.0291)	(0.125)
Clusters	687	687	687	687
Observations	5208	5208	5208	5208

Notes: This table presents point estimates of δ from estimating equation (3) for the main outcomes as we vary the regression specification. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The control sample consists of all counties that are never designated as a HPSA during 2012–2017 and are matched to the treatment counties using the matching procedure described in Section 4.1. Three control counties are matched to each treatment county, resulting in 217 treatment counties and 651 control counties. 470 of the 651 control counties are unique, as control counties can be matched to multiple treatment counties. Row A reproduces our baseline estimates. Row B winsorizes outcome variables at the 99th percentile. Row C winsorizes outcome variables at the 90th percentile. Row D does not winsorize outcome variables. Row E drops controls from the regression. Row F adds year and state fixed effects to the regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

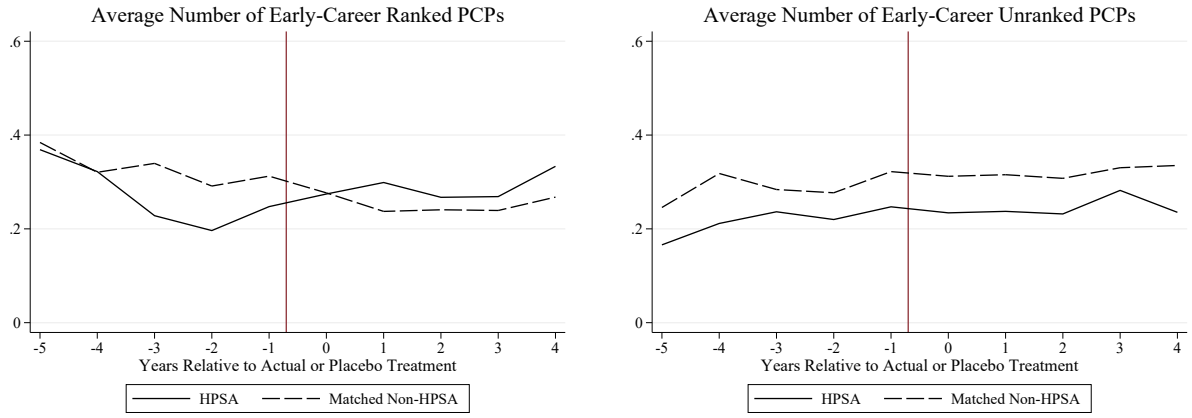
Appendix A Additional Figures and Tables

Figure A.1: Average PCP Counts by Career Stage



Notes: These graphs plot the average number of PCPs in the indicated career stage per 10,000 population in a county in the sample of treatment HPSA counties and the non-HPSA control counties around actual or placebo treatment. Early-career PCPs are those graduating 5-10 years earlier and later-career PCPs are those graduating more than 10 years earlier. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The control sample consists of all counties that are never designated as a HPSA during 2012–2017 and are matched to the treatment counties using the matching procedure described in Section 4.1. Three control counties are matched to each treatment county, resulting in 217 treatment counties and 651 control counties. 470 of the 651 control counties are unique, as control counties can be matched to multiple treatment counties. The x-axis shows the years relative to HPSA designation.

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



Notes: These graphs plot the average number of early-career PCPs who attended ranked or unranked medical schools per 10,000 population in a county in the sample of treatment HPSA counties and the non-HPSA control counties around actual or placebo treatment. Early-career PCPs are those graduating 5-10 years earlier. The 95 schools included in the 2018 U.S. News Primary Care medical school rankings are defined as ranked, and all other medical schools are defined as unranked. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The control sample consists of all counties that are never designated as a HPSA during 2012–2017 and are matched to the treatment counties using the matching procedure described in Section 4.1. Three control counties are matched to each treatment county, resulting in 217 treatment counties and 651 control counties. 470 of the 651 control counties are unique, as control counties can be matched to multiple treatment counties. The x-axis shows the years relative to HPSA designation.

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

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

Notes: This table presents the δ_τ point estimates from estimating equation (2), where the outcome y_{ct} is the stock of PCPs in the indicated career stage per 10,000 population in a county. Early-career PCPs are those graduating 5–10 years earlier and later-career PCPs are those graduating more than 10 years earlier. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The control sample consists of all counties that are never designated as a HPSA during 2012–2017 and are matched to the treatment counties using the matching procedure described in Section 4.1. Three control counties are matched to each treatment county, resulting in 217 treatment counties and 651 control counties. 470 of the 651 control counties are unique, as control counties can be matched to multiple treatment counties. Controls for unemployment rate, median household income, and population at the county-year level are included in each regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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

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

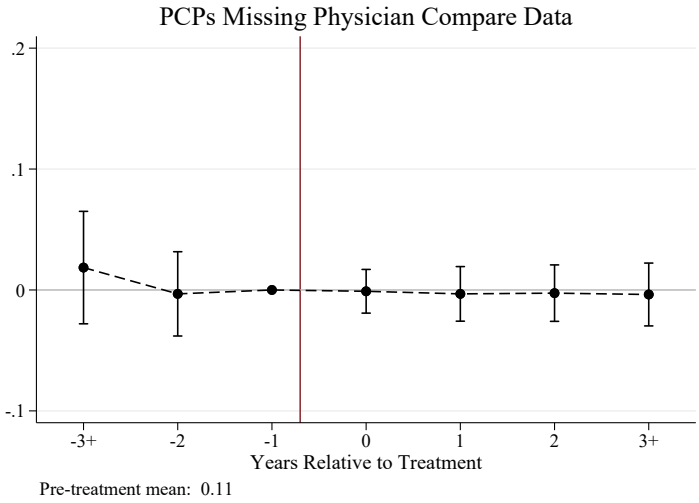
Notes: This table presents the δ_τ point estimates from estimating equation (2), where the outcome y_{ct} is the stock of early-career PCPs who attended ranked or unranked medical schools per 10,000 population in a county. Early-career PCPs are those graduating 5–10 years earlier. The 95 schools included in the 2018 U.S. News Primary Care medical school rankings are defined as ranked, and all other medical schools are defined as unranked. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The control sample consists of all counties that are never designated as a HPSA during 2012-2017 and are matched to the treatment counties using the matching procedure described in Section 4.1. Three control counties are matched to each treatment county, resulting in 217 treatment counties and 651 control counties. 470 of the 651 control counties are unique, as control counties can be matched to multiple treatment counties. Controls for unemployment rate, median household income, and population at the county-year level are included in each regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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

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

Notes: This table presents the δ_τ point estimates from estimating equation (2), where the outcome y_{ct} is the stock of PCPs per 10,000 population in a county in column 1, and this outcome is split up into PCPs who attended ranked or unranked medical schools in columns 2 and 3. The 95 schools included in the 2018 U.S. News Primary Care medical school rankings are defined as ranked, and all other medical schools are defined as unranked. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013-2017. The control sample consists of all counties that are never designated as a HPSA during 2012-2017 and are matched to the treatment counties using the matching procedure described in Section 4.1. Three control counties are matched to each treatment county, resulting in 217 treatment counties and 651 control counties. 470 of the 651 control counties are unique, as control counties can be matched to multiple treatment counties. Controls for unemployment rate, median household income, and population at the county-year level are included in each regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure A.3: PCP Missing Data Relative to Designation



Notes: This graph plots the point estimates of the δ_τ's and their 95% confidence intervals from estimating equation (2), where the outcome y_{ct} is the stock of PCPs per 10,000 population in a county that are missing data on graduation year or medical school from the Physician Compare dataset. Almost all PCPs missing data on one of these variables are also missing data on the other variable. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. The control sample consists of all counties that are never designated as a HPSA during 2012–2017 and are matched to the treatment counties using the matching procedure described in Section 4.1. Three control counties are matched to each treatment county, resulting in a sample size of 217 treatment counties and 651 control counties. 470 of the 651 control counties are unique, as control counties can be matched to multiple treatment counties. The x-axis shows the years relative to HPSA designation. Controls for unemployment rate, median household income, and population at the county-year level are included in the regression.

Appendix B Additional Robustness Checks

Table B.1: Robustness to Partially Designated County Inclusion

	(1) HPSA > 0%	(2) HPSA > 10%	(3) HPSA > 50%	(4) HPSA = 100%
Panel A. Medium Run Estimates				
Early-Career PCPs	0.114**	0.0946	0.112	0.101
	(0.0570)	(0.0600)	(0.0688)	(0.0721)
Early-Career Ranked PCPs	0.100***	0.102***	0.110**	0.105**
	(0.0361)	(0.0386)	(0.0439)	(0.0454)
Early-Career Unranked PCPs	0.00694	-0.0114	-0.0108	-0.00990
	(0.0335)	(0.0349)	(0.0399)	(0.0425)
Later-Career PCPs	-0.00913	-0.0561	-0.108	-0.184
	(0.146)	(0.151)	(0.161)	(0.168)
Panel B. Pooled Estimates				
Early-Career PCPs	0.0968*	0.0789	0.0893	0.0779
	(0.0509)	(0.0537)	(0.0624)	(0.0655)
Early-Career Ranked PCPs	0.0873***	0.0902***	0.0947**	0.0893**
	(0.0323)	(0.0347)	(0.0400)	(0.0415)
Early-Career Unranked PCPs	0.00625	-0.0118	-0.0114	-0.0110
	(0.0299)	(0.0312)	(0.0356)	(0.0379)
Later-Career PCPs	0.00400	-0.0444	-0.0937	-0.161
	(0.128)	(0.132)	(0.142)	(0.148)
Obs.	5,208	4,728	3,696	3,312

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

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

	(1) $n_{control} = 1$	(2) $n_{control} = 2$	(3) $n_{control} = 3$	(4) $n_{control} = 4$	(5) $n_{control} = 5$
Panel A. Medium Run Estimates					
Early-Career PCPs	0.0932	0.107*	0.114**	0.0995*	0.0986*
	(0.0688)	(0.0613)	(0.0570)	(0.0560)	(0.0533)
Early-Career Ranked PCPs	0.101**	0.106***	0.100***	0.0946***	0.0925***
	(0.0415)	(0.0392)	(0.0361)	(0.0350)	(0.0336)
Early-Career Unranked PCPs	-0.00993	-0.00563	0.00694	0.00131	0.00276
	(0.0409)	(0.0359)	(0.0335)	(0.0333)	(0.0323)
Later-Career PCPs	0.118	0.0464	-0.00913	-0.0220	-0.0586
	(0.187)	(0.160)	(0.146)	(0.139)	(0.135)
Panel B. Pooled Estimates					
Early-Career PCPs	0.0768	0.0870	0.0968*	0.0839*	0.0832*
	(0.0614)	(0.0547)	(0.0509)	(0.0500)	(0.0477)
Early-Career Ranked PCPs	0.0858**	0.0901**	0.0873***	0.0833***	0.0809***
	(0.0371)	(0.0352)	(0.0323)	(0.0313)	(0.0301)
Early-Career Unranked PCPs	-0.00751	-0.00590	0.00625	0.000261	0.00142
	(0.0369)	(0.0320)	(0.0299)	(0.0296)	(0.0287)
Later-Career PCPs	0.0932	0.0512	0.00400	-0.00494	-0.0418
	(0.165)	(0.141)	(0.128)	(0.122)	(0.118)

Notes: This table presents the point estimates of δ^{MR} from estimating equation (4) in Panel A and the point estimates of δ from estimating equation (3) in Panel B, for the main outcome variables as we vary the number of controls matched to each treatment county. Column (3) reproduces our preferred matching procedure, in which we match 3 controls to each treatment county. Controls for unemployment rate, median household income, and population at the county-year level are included in each regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.3: Robustness to Match Variables

	(1)	(2)	(3)	(4)	(5)
Panel A. Medium Run Estimates					
Early-Career PCPs	0.114**	0.0961*	0.0996*	0.0556	0.0690
	(0.0570)	(0.0537)	(0.0597)	(0.0600)	(0.0503)
Early-Career Ranked PCPs	0.100***	0.0943***	0.0862**	0.0809**	0.0857***
	(0.0361)	(0.0344)	(0.0375)	(0.0373)	(0.0319)
Early-Career Unranked PCPs	0.00694	-0.00467	0.0116	-0.0249	-0.0208
	(0.0335)	(0.0321)	(0.0355)	(0.0352)	(0.0326)
Later-Career PCPs	-0.00913	-0.0539	-0.0761	-0.122	-0.103
	(0.146)	(0.142)	(0.146)	(0.146)	(0.137)
Panel B. Pooled Estimates					
Early-Career PCPs	0.0968*	0.0797*	0.0793	0.0441	0.0629
	(0.0509)	(0.0478)	(0.0534)	(0.0537)	(0.0449)
Early-Career Ranked PCPs	0.0873***	0.0802***	0.0719**	0.0724**	0.0735**
	(0.0323)	(0.0307)	(0.0336)	(0.0335)	(0.0287)
Early-Career Unranked PCPs	0.00625	-0.00308	0.00831	-0.0272	-0.0146
	(0.0299)	(0.0284)	(0.0314)	(0.0310)	(0.0285)
Later-Career PCPs	0.00400	-0.0417	-0.0569	-0.0953	-0.0842
	(0.128)	(0.124)	(0.128)	(0.127)	(0.120)
Match Variables:					
# Physicians	✓	✓	✗	✗	✓
%Δ Physicians	✓	✗	✓	✗	✗
Poverty Rate	✓	✓	✓	✓	✗
%Δ Poverty Rate	✗	✗	✗	✗	✓
Geographic Region	✓	✓	✓	✓	✓

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