Health Professional Shortage Areas and Physician Location Decisions*

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Abstract

To address geographic disparities in access to healthcare, the U.S. government designates primary care Health Professional Shortage Areas (HPSAs), and the Centers for Medicare and Medicaid Services provides 10% bonus payments to physicians billing in these areas. We use administrative data 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 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 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, policymakers 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). One particularly prominent policy aims to improve access to primary care by financially incentivizing physicians to practice in shortage areas. 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% 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.

[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).

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. Notably, 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. The degree to which they respond may also 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 shows 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 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% 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 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 stylized policy exercise, we show that a 20% 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.

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] 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 national program designed to address shortage areas through

[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). Another related paper set in a different context is Huh (2018), who finds that Medicaid expansions can attract dentists to poorer areas.

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

direct monetary payments. Furthermore, in exploiting our data to study how responses vary by career stage, we are able to uncover evidence that early-career PCPs are more responsive to shortage area designations.

Finally, our findings connect to an important discussion in the literature on how payment policies influence the overall capacity of the healthcare system, particularly as it relates to the allocation of human capital to and within the health sector. Existing work shows that Medicare policy can increase investments in medical technology (Finkelstein 2007, Acemoglu and Finkelstein 2008, and Clemens and Gottlieb 2014) as well as physician on-the-job investments in human capital and entrepreneurial capital (Clemens et al. forthcoming), 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 highlight how government payment policy can expand access to healthcare in specific geographies and influence 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

[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 links 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`.

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

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

Medicare Bonus Payments from CMS. The Centers for Medicare and Medicaid Services provides 10% 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

[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.

[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.

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]

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 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 annual dissem-

[10]For example, loan forgiveness and scholarship programs through the National Health Service 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. In addition, most states have loan forgiveness programs for practicing in rural areas (Kulka and McWeeny 2019) which could potentially interact with HPSA designations.

[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.

inations 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 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 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

[12]The MPUP data contain information on practice location; however, the variables in this dataset are not suitable for our analysis. Specifcally, 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 2015. For this reason, we use the NPPES data to define physician location as the primary practice location in December of the year of observation.

[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.

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. We define doctors with specialties of "family practice," "general practice," "internal medicine," "geriatric medicine," or "pediatric medicine" as PCPs. Finally, into our newly-constructed panel we merge data derived from two more sources. First, for information regarding HPSA status, we use the official, CMS-maintained list of ZIP codes that define automatically billed HPSAs. We aggregate this data up to the county level by simply counting the number of HPSA ZIP codes in a county. Second, for more information on county characteristics, we pull variables from the *Area Health Resources File* (AHRF), which contains a wide range of county-level, health-related variables derived from the American 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 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 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 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

[14]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 exclude from our count of early-career PCPs the handful of physicians in the data who are not likely to have yet completed their residencies, we define early-career PCPs as those graduating 5 to 10 years earlier.

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.[15]

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.[16] We also use county-level variables from the AHRF indicating the total number of active physicians per capita and the percent of the population below the federal poverty line to carry out our matching procedure, 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, Figure 1 illustrates exactly this concern. The solid line depicts the average 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, which is not unexpected. In contrast, the dotted line depicts the average count of PCPs for the potential control group that consists of all other counties. Relative time for this comparison group is defined by matching to each HPSA all other counties, and then assigning a placebo designation year to the comparison counties equal to the actual designation year for the HPSA county to which they are matched. The stock of physicians in all other counties is not

[15] 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.

[16] While some counties are only "partially" designated, meaning only some of their zip codes are automatically billed HPSAs, the majority of designated counties in our sample are fully designated. There are 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.

declining in the years before placebo designation, which raises 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.

4.1 Matched County Design

Matching Procedure. 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 use these variables (pulled from the AHRF) from 2010 and 2011, which corresponds 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.[17] That is,

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

In addition to the variables included in the closeness measure, we match on region, given that the existing literature indicates that geography influences physician residential choices (Burfield et al. 1986, Chen et al. 2010). Specifically, we stipulate that a designated county can only be matched to control counties that are in its geographic region. (We define four distinct

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

regions corresponding roughly to the Northeast, South, Midwest, and West.) 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 probe the robustness of our results to changing different aspects of the matching procedure in Section 5.2. Specifically, we vary the combination of 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.[18]

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. Figure 1 builds on this assessment of our matching procedure with a graphical examination. As discussed above, we see that counts of PCPs for the unamtched sample of non-HPSA counties is not trending in parallel with HPSAs before designation. However, after matching, we have a group of control counties that, while still different in levels over the pre-period, are trending in parallel. This allows us to more confidently use the evolution of PCP counts in the matched control group as a counterfactual for the evolution of PCP counts in the treatment group.

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

4.2 Implementation

To 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 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.[19]

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 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

[19] 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.

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

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

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 year 2011 and winsorized at the 95th percentile; we include county-level controls for household income (using indicators for median household income bins of $5,000), 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.[20] 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,

[20]The corresponding graphs of raw means for these outcomes can be found in Appendix Figure A.1.

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; 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 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 mean before designation is larger. At face value, the standard pooled difference-in-differences estimate for this outcome would represent a 0.13% increase in later-career doctor counts.

These results are consistent with PCPs in later career stages facing higher barriers to relocating. The cost of leaving behind an already-established practice 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, may 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

ranked or unranked medical school.

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 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 in Table 4. We find no statistical 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. 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 our leading 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 are 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 A.1 reports point estimates for the medium run effects as well as overall pooled estimates. The first column reproduces our leading 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 A.2 reports results from changing the number of control counties matched to each treatment county. Point estimates are broadly stable, though those for later-career PCPs appear more sensitive. We also vary the variables used in our matching procedure. One may be potentially concerned about matching on both physician counts and physician trends while also defining the control group to be counties who are never designated over our time period. Reassuringly, we find results similar to our leading estimates when we adjust our matching procedure accordingly, which we report in Appendix Table A.3. Specifically, our results are not sensitive to matching on only one of either physician counts or trends in physician counts (columns (2) and (3)). Moreover, our results are mostly stable when we incorporate trends in poverty rates into our matching procedure, whether that be in place of trends in physician counts (columns (4) and (5)) or in place of physician counts in levels (column (6)). Overall, point estimates are generally similar in magnitude across strategies, although the estimates for early-career PCPs are not always statistically significant. The results on early-career ranked PCPs are the most robust.

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 an 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. 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. First, 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 potentially correlated with HPSA designations and possible interactions between private insurance payments and HPSA status may contribute to the total incentives associated with designations. For instance, most states maintain loan forgiveness programs for practicing in rural areas (Kulka and McWeeny 2019), some of which may use criteria related to HPSA designations. Moreover, if private insurers follow the lead of Medicare (Clemens and Gottlieb 2017, Clemens et al. 2017) and offer bonus payments for providing services in HPSAs, then the financial incentives for locating in a HPSA could be even stronger than the bonus payments paid by CMS would imply. Second, we focus only on the costs and effects of the program for PCPs, even though all physicians practicing in HPSAs receive bonus payments. Third, 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. Overall, our aim is to conduct a simple yet informative exercise that highlights the policy implications of our main findings.

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 $2,269,000 per additional PCP per 10,000 in the average HPSA-designated county.[21]

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 $285,500 per additional PCP per 10,000. This amounts to nearly an eight-fold decrease in costs per PCP.[22]

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

Some areas have significantly fewer physicians per capita than other areas. Policymakers 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 program uses bonus payments to incentivize physicians to locate in designated shortage areas.

[21] The figure of $2,269,000 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 the MPUP data omit line items for services provided by an NPI to 10 or fewer beneficiaries in a given year, so all cost figures slightly understate true totals.

[22] This analysis assumes no effect of HPSA designation on later-career PCPs, but 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 designation 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 additional PCP per 10,000 under the 10% bonus payment program would be $630,300, still greater than the $285,500 under our proposed targeted 20% bonus payment program.

We study the causal effects of HPSA designations on counts of primary care physicians using data from the Centers for Medicare and Medicaid Services and a matched difference-in-differences framework. We find that the HPSA program induces early-career physicians to practice in shortage areas, resulting in an average increase of 0.67 physicians per county, and we find that this increase is driven by physicians who attended higher-quality medical schools. However, we find that the program is not an effective incentive for physicians in later career stages, although the vast majority of bonus payments are paid to later-career physicians. Our results are consistent with the idea that physicians in later career stages face higher costs of relocating—which could be due to difficulties associated with moving an already-established practice—whereas early-career physicians, who are perhaps less likely to have formed their own practice, may find it easier to relocate for an increase in remuneration.

Our findings can inform policy. Our results suggest that the effectiveness of shortage area programs could be improved at a reduced cost by targeting payments at those most likely to respond to them, namely early-career physicians. More broadly, our findings show how geographic-specific government payment policies can be used as a tool to influence the allocation of primary care physicians—who constitute a critical component of healthcare systems—across space.

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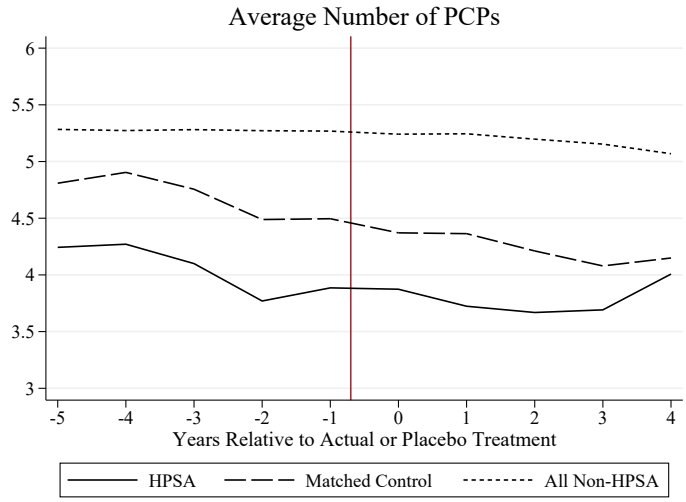
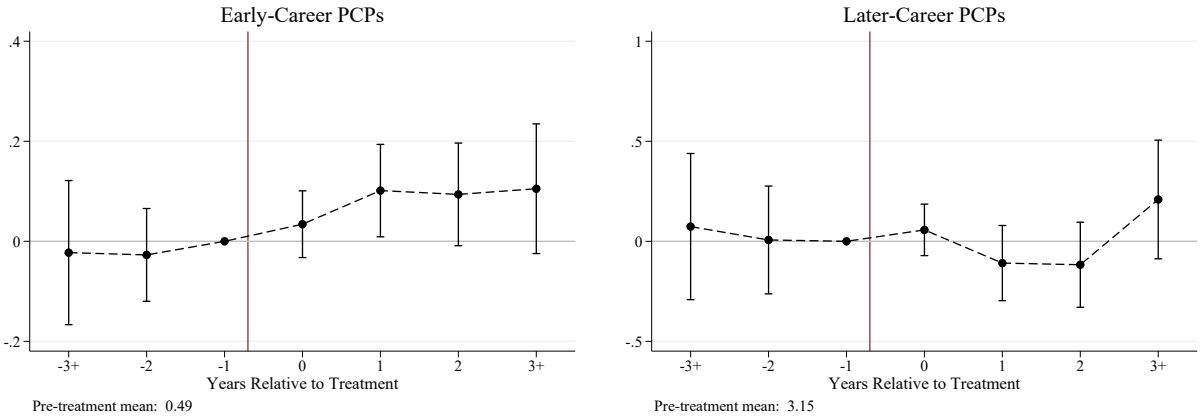
Figure 1: Average Number of PCPs for HPSA and Non-HPSA Counties



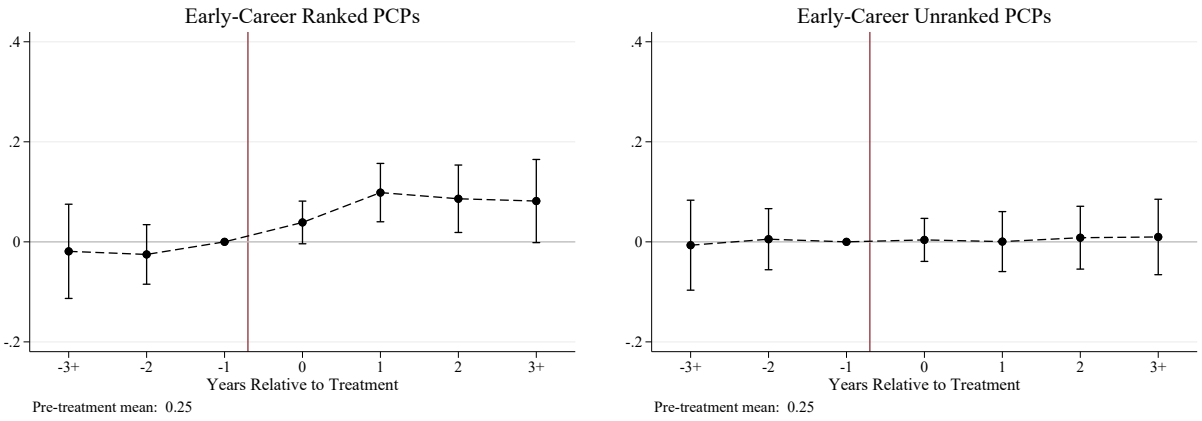
Notes: This graph plots the average number of PCPs per 10,000 population around actual or placebo designation year for treatment HPSA counties, for unmatched potential controls, and for matched controls. The treatment sample consists of all counties that become designated as a primary care HPSA in some year 2013–2017. 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. The matched control sample consists of the non-HPSA counties that are matched to HPSA counties using the method described in Section 4.

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



Notes: These graphs plot the dynamic impact of HPSA designation on 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.

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



Notes: These graphs plot the dynamic impact of HPSA designation on early-career 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.

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

	HPSAs	Matched Control
	Mean	Mean
	(1)	(2)
Panel A. Main Outcome Variables (County Panel)		
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)
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 main outcome variables, which come from our constructed county panel dataset. Panel B presents means for other variables, including variables used in our match as well as covariates, which come from the Area Health Resource 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 PCP Counts by Career Stage

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

Notes: This table presents difference-in-differences estimates of the impact of HPSA designation on PCP counts per 10,000 population by career stage. Panel A presents the point estimates of δ^{SR} and δ^{MR} from estimating equation (4). Panel B presents the point estimate of δ from estimating equation (3). 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 HPSA Designation on Early-Career PCP Counts by Medical School Rank

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

Notes: This table presents difference-in-differences estimates of the impact of HPSA designation on early-career PCP counts per 10,000 population by rank of medical school attended. Panel A presents the point estimates of δ^{SR} and δ^{MR} from estimating equation (4). Panel B presents the point estimate of δ from estimating equation (3). 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 Total PCP Counts by Medical School Rank

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

Notes: This table presents difference-in-differences estimates of the impact of HPSA designation on total PCP counts per 10,000 population by rank of medical school attended. Panel A presents the point estimates of δ^{SR} and δ^{MR} from estimating equation (4). Panel B presents the point estimate of δ from estimating equation (3). 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

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

Notes: This table presents point estimates of δ^{MR} from estimating equation (4) for the main outcomes as we vary the regression specification. 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 controls from the regression. Row F adds year and state fixed effects to the regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

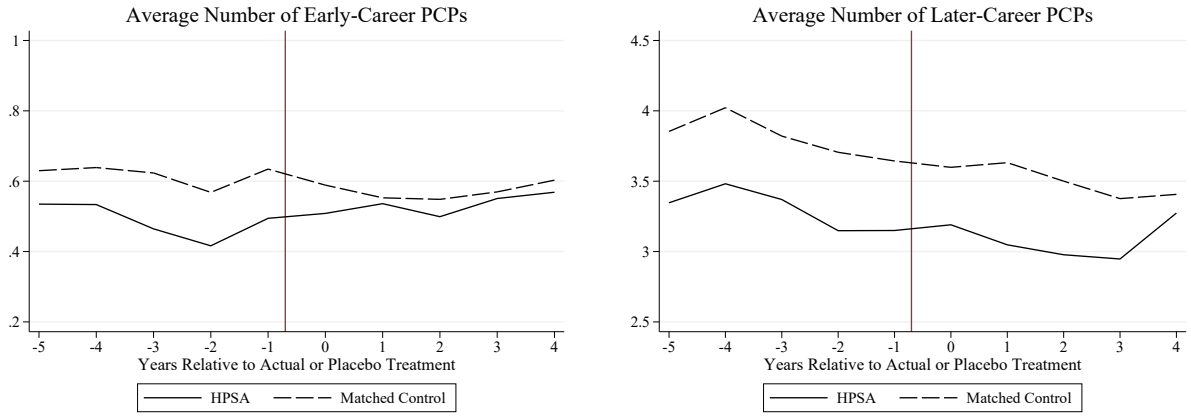
Table 6: Robustness of Pooled Estimates to Alternative Regression Specifications

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

Notes: This table presents point estimates of δ from estimating equation (3) for the main outcomes as we vary the regression specification. 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 controls from the regression. Row F adds year and state fixed effects to the regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

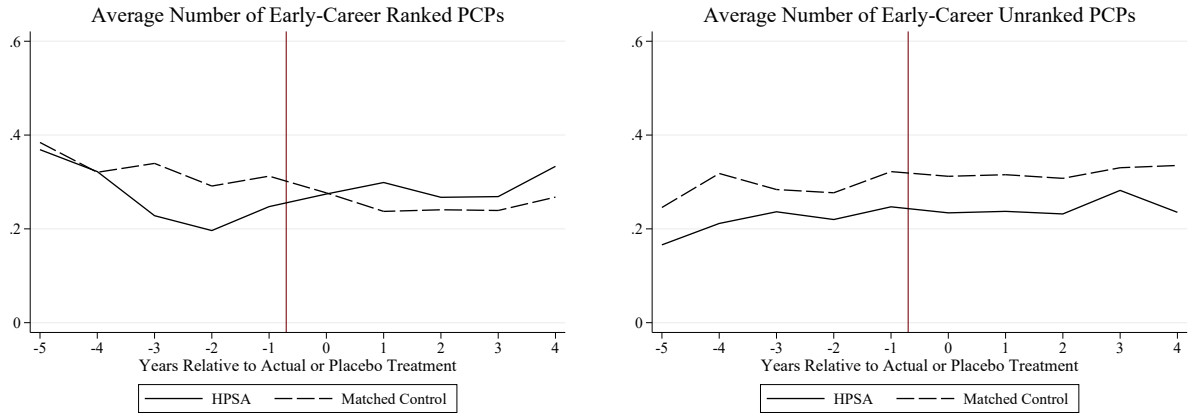
Appendix A Additional Figures and Tables

Figure A.1: Average PCP Counts by Career Stage



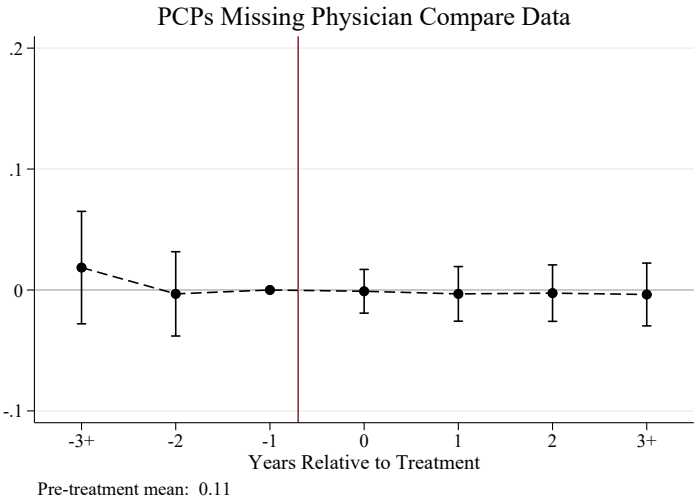
Notes: These graphs plot the average number of PCPs in the indicated career stage per 10,000 population in a county for treatment HPSA counties and for the matched control counties around actual or placebo treatment.

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 for treatment HPSA counties and for the matched control counties around actual or placebo treatment.

Figure A.3: PCP Missing Data Relative to HPSA Designation



Notes: This graphs plots the dynamic impact of HPSA designation on counts of 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.

Table A.1: Robustness of Estimates to Partially Designated County Inclusion

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

Notes: This table presents medium run estimates and pooled estimates 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%, 50%, and 100% 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 A.2: Robustness of Estimates to Number of Matched Control Counties

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

Notes: This table presents medium run estimates and pooled estimates 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 A.3: Robustness of Estimates to Match Variables

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Medium Run Estimates						
Early-Career PCPs	0.114**	0.0961*	0.0996*	0.0716	0.0690	0.0827
	(0.0570)	(0.0537)	(0.0597)	(0.0540)	(0.0503)	(0.0588)
Early-Career Ranked PCPs	0.100***	0.0943***	0.0862**	0.0676*	0.0857***	0.0936**
	(0.0361)	(0.0344)	(0.0375)	(0.0352)	(0.0319)	(0.0364)
Early-Career Unranked PCPs	0.0069	-0.0047	0.0116	-0.0000	-0.0208	-0.0172
	(0.0335)	(0.0321)	(0.0355)	(0.0327)	(0.0326)	(0.0342)
Later-Career PCPs	-0.0091	-0.0539	-0.0761	-0.0101	-0.103	0.0092
	(0.146)	(0.142)	(0.146)	(0.139)	(0.137)	(0.146)
Panel B. Pooled Estimates						
Early-Career PCPs	0.0968*	0.0797*	0.0793	0.0641	0.0629	0.0715
	(0.0509)	(0.0478)	(0.0534)	(0.0480)	(0.0449)	(0.0529)
Early-Career Ranked PCPs	0.0873***	0.0802***	0.0719**	0.0600*	0.0735**	0.0815**
	(0.0323)	(0.0307)	(0.0336)	(0.0312)	(0.0287)	(0.0328)
Early-Career Unranked PCPs	0.0063	-0.0031	0.0083	0.0014	-0.0146	-0.0142
	(0.0299)	(0.0284)	(0.0314)	(0.0289)	(0.0285)	(0.0303)
Later-Career PCPs	0.0040	-0.0417	-0.0569	-0.0121	-0.0842	0.0119
	(0.128)	(0.124)	(0.128)	(0.122)	(0.120)	(0.128)
Match Variables						
Physician Count	✓	✓	×	✓	✓	×
Percent Change in Physician Count	✓	×	✓	×	×	✓
Poverty Rate	✓	✓	✓	✓	×	✓
Percent Change in Poverty Rate	×	×	×	✓	✓	✓
Geographic Region	✓	✓	✓	✓	✓	✓

Notes: This table presents medium run estimates and pooled estimates for the main outcomes as we vary the variables used in the matching procedure. Column (1) reproduces our leading estimates. 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) matches on trends in poverty rates rather than trends in physician counts. Column (5) matches on physician counts and trends in poverty rates. Column (6) matches on trends in physician counts, poverty rates, and trends in poverty rates. Controls for unemployment rate, median household income, and population at the county-year level are included in each regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$