The Earnings and Labor Supply of U.S. Physicians*

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Abstract

Is government guiding the invisible hand at the top of the labor market? We use new administrative data to measure physicians' earnings and estimate the influence of healthcare policies on these earnings, physicians' labor supply, and allocation of talent. Combining the administrative registry of U.S. physicians with tax data, Medicare billing records, and survey responses, we find that physicians' annual earnings average \$350,000 and comprise 8.6% of national healthcare spending. Business income comprises one-quarter of earnings and is systematically underreported in survey data. Earnings increase steeply early in the career, and there are major differences across specialties, regions, and firm sizes. The geographic pattern of earnings is unusual compared with other workers. We argue that these patterns reflect policy choices to subsidize demand for physician care, amplified by restrictions on physician entry, especially in certain specialties. Health policy has a major impact on the margin: 25% of physician fee revenue driven by Medicare reimbursements accrues to physicians personally. Physicians earn 8% of public money spent on insurance expansion. These policies in turn affect the type and quantity of medical care physicians supply, retirement timing, and the allocation of talent across specialties.

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The [healthcare] industry is not very good at promoting health, but it excels at promoting wealth among healthcare providers, including some successful private physicians who operate extremely profitable practices.

(Case and Deaton, 2020)

My hand surgeon should have been paid $4.5 billion for fixing my broken wrist, not $1,000.

(Crawford, 2019)

The medical profession has changed substantially since Friedman and Kuznets (1945) emphasized the importance of entry barriers. Health insurance coverage has increased—especially tax-financed coverage—and these insurance contracts regulate physicians' payment rates. What determines a profession's earnings when its output price is regulated yet potential entrants face high barriers? We use a new U.S. tax data linkage to analyze the labor market for physicians, a skilled and highly-regulated occupation. We find that government insurance policies play a central role in shaping physicians' earnings and labor supply. Since doctors are one of the most common occupations among top earners, these policies effectively contribute to overall top income inequality.

Physicians merit detailed study as they comprise a large share of high earners in the United States, so their labor market rewards are central to the economy's valuation and allocation of top talent. The allocation of skill across different activities is key to how a sector or an entire economy functions (Murphy, Shleifer and Vishny, 1991), and the government's pronounced role in the physician labor market may give it unique power to drive talent allocation of these quintessential "human capitalists" (Smith et al., 2019). If reimbursement policies and entry barriers shape physician earnings, they may affect physicians' decisions about labor supply and even specialization. This could influence the value they create for society.

Our empirical work begins with novel descriptive facts essential to understanding physicians' labor markets. While this market is of longstanding academic interest (Friedman and Kuznets, 1945; Feldstein, 1970; Fuchs and Kramer, 1973; Sloan, 1975), the modern literature has been hamstrung by measurement challenges that obscure even basic facts.[1] We document the level and composition of physician earnings, how earnings evolve over time, and the pronounced differences across geography and specialty. We find that physician earnings comprise 8.6 percent of total healthcare spending, but with dramatic variation depending on specialty, region, and type of practice.

By distinguishing the contributions of individual and geographic factors to the variation in physician earnings, we find an unusual geographic pattern: rural areas have positive location effects and there is negative physician-location sorting. That is, smaller markets attract lower-earning physicians but boost their earnings. This differs from lawyers, whose pattern we examine separately, and other workers and industries examined elsewhere. One natural hypothesis for this unusual pattern is the government subsidies that permeate this market. The tremendous demand increase spurred by insurance (Finkelstein, 2007), and centrally set reimbursements for healthcare services, may increase physicians' earnings in rural markets relative to other occupations.

To isolate policy influence from other market characteristics that affect physicians' earnings and labor supply, we use two types of policy variation: changes to insurance coverage and to payment rates per service. In both cases, the government's influence is substantial.

In terms of physician earnings, one quarter of marginal revenue induced by Medicare reimbursement changes accrues to physicians personally. When healthcare reform permanently increased insurance coverage, physicians earned 8% of the resulting public spending.

Do top incomes influence how much and what kind of work people do, or do they purely reflect unearned rents? This is often hard to answer, but our setting enables us to examine

[1] This literature (*e.g.* Baker, 1996; Nicholson and Souleles, 2001; Bhattacharya, 2005; Vaughn et al., 2010; Nicholson and Propper, 2011; Esteves-Sorenson and Snyder, 2012; Chen and Chevalier, 2012; Jagsi et al., 2012; Seabury et al., 2013; Altonji and Zhong, 2021; LoSasso et al., 2020; Gottlieb et al., 2023a) has relied on survey data and faced measurement challenges, such as top-coding and complicated income structures. Our data overcome many (though not all) of these issues and allow us to newly establish basic facts about U.S. physicians' earnings. Appendix A presents a survey on the public's beliefs about physician earnings.

labor supply responses to the same insurance coverage and payment policy changes. Using income tax, Medicare billing, and specialty choice data, we find positive labor supply responses, such as a procedure-level short-run supply elasticity of 0.4. Doctors who are past the lifetime earnings peak delay retirement when they experience positive earnings shocks. We also investigate specialty choice, a particularly powerful margin because it is both sticky and perhaps the most important dimension of labor supply in the long run. We find that specialty choice responds strongly to changes in how government payments remunerate different specialties.

We use our estimates to conclude with three policy analyses. We consider how geographic payment adjustments shape earnings across regions, and how both reimbursement and tax policies shape talent allocation across specialties. We find that Medicare's policies for paying physicians by geographic region can account for about a third of the unusual geographic earnings pattern we observe. Suppose Medicare payments for internal medicine increased to the level of dermatology—a specialty well-known for its high compensation and quality of life—while holding constant the number of available slots and other specialties' earnings. Our estimates imply that this would select for internists with higher test scores by 0.46 standard deviations on average, while nearly doubling the share with top scores. Increased earnings attract physicians with higher test scores to a specialty while displacing those with lower test scores and less choice.[2] This means that policies subsidizing surgery or primary care have the power to attract more top talent into those specialties, potentially changing their quality of care for a generation.[3] We calculate that using taxes to replicate the same

[2]Specialty choice is a complex labor supply margin to examine due to binding entry restrictions for some specialties: physicians cannot simply enter more lucrative specialties at will, even early in their careers. So changing earnings need not affect the number of physicians in a specialty. But payment policy and earnings can nevertheless shape labor supply, and quality of care, by reallocating talent across specialties. To take a concrete example, "not enough new doctors are going into pediatrics," according to Goldman (2024). Yet 99% of pediatric residency slots are filled. But these slots are not all filled in the first round of the match, and the number of U.S. applicants continues to decline, suggesting that the concern may be about applicant quality rather than numbers *per se*.

[3]Doyle et al. (2010) find that residents from a higher-ranked program save more money on inpatient care for sicker patients than for relatively healthy patients. This might suggest that the return to talent increases with patient complexity, but caution is required to apply the Doyle et al. estimates for cross-specialty analysis.

magnitude of talent reallocation would require unrealistic differences in income tax rates across specialties.

Taking these results together with policy's sizable impact on earnings, we conclude that government payment rules play a key role in valuing and shaping the use of one of society's most expensive assets: physicians' human capital. Our results are key to understanding equilibrium in the market for physicians. Our data highlight physicians' success in the U.S. labor market, particularly in some specialties and geographic areas. We show that government policies—and especially public insurance coverage—play important roles in these earnings and in allocating talent within medicine. Put simply, the expansion of government healthcare spending contributes to income inequality at the top of the earnings distribution.

These results suggest a clear agenda for future research. Policy evaluation in this environment must account for the health impacts, and thus social returns, to physician ability in different specialties—currently unknown parameters. We encourage future work to estimate these in order to determine the welfare impact of talent allocation and hence insurance policies.

1 Institutional Background, Data, and Measurement

This section describes the standard sequence of medical training and career progression, important background for our data and measurement choices. We also briefly describe our main data sources and sample definitions; Appendicies B.1 and B.2 provide details.

1.1 Career in Medicine

A career in medicine is competitive and follows a relatively rigid script. Practicing physicians choose specializations early and these are hard to change. Physicians' earnings can be complex and frequently include both wages and business income.

Medicine is a professional degree in the U.S. A high school student who wants to become a physician must first complete an undergraduate degree and then earn a doctor of medicine (MD) degree from one of 158 medical schools. Around 50,000 students apply to U.S. MD-granting medical schools annually and around 45% are admitted (AAMC, 2022). The top-ranked schools are highly competitive; Stanford admits 2.2% of applicants and Harvard reports an average undergraduate grade point average of 3.9. Halfway through their (usually) four years of medical school, students take the first standardized test required for the U.S. medical license, specifically the U.S. Medical Licensing Examination (USMLE) Step 1.

To practice medicine, MD graduates must next complete a *residency* in a specific specialty. Residency slots are allocated through a matching algorithm administered by the National Residency Matching Program (NRMP). Residency programs take several years, but vary substantially in their competitiveness and length.[4] Primary care is typically less competitive and shorter than more specialized programs. After completing residency, physicians can begin to work in private practice, small groups, or larger organizations, or complete further *fellowship* training.

The earnings structure of independently practicing physicians can be classified into three broad models. One extreme is physicians who only earn wage or salary income reported on Form W-2. This is common in larger organizations such as academic medical centers. The second model is a sole proprietorship, generating income that only appears on Schedule C of the physician's personal tax return ("Profit or Loss from Business, Sole Proprietorship"). The third model involves a pass-through entity, usually an S-corporation or a partnership. A medical practice organized as an S-corporation pays physicians a market wage, reported on W-2, plus a share of profits that remain after all practice expenses. The exact legal structure affects the tax liability and the profit-sharing incentives within the practice.

[4] Overall, 5,313 residency programs offered 36,277 positions in 2022. 19,902 U.S. MD graduates applied and 93% received an offer (NRMP, 2022).

1.2 Data Sources

Our primary data sources are the administrative database of individual federal income tax returns, as shared by the Internal Revenue Service with the U.S. Census Bureau, merged with an administrative registry of all healthcare providers in the U.S. We augment these with additional administrative and survey datasets, detailed in Appendix B.1.

Tax Data. We use an extract from federal income tax data containing the universe of individual tax returns for tax years 2005 through 2017. We augment individual returns with third-party information returns, notably Forms W-2 and 1099-SSA. Form W-2 reports wage earnings for each filer in the tax unit (*i.e.* either one taxpayer or a married couple) and includes the Employer Identification Number (EIN) for those physicians who had any W-2 income.[5] We inflation-adjust all monetary values to 2017 dollars using the Consumer Price Index for All Urban Consumers (CPI-U) deflator from the Bureau of Labor Statistics and replace missing records with $0 if the person filed taxes. Tax data also include the state and county of residence.

Physician Registry. We merge tax data with the administrative registry of physicians (the National Plan and Provider Enumeration System, or NPPES) using the Census Bureau's Protected Identification Key (PIK)-based data linkage infrastructure. NPPES lists all physicians and their specialty.[6] We augment this with medical school name from the Centers for Medicare & Medicaid Services (CMS) Doctors and Clinicians file and the school's U.S. News and World Report ranking.

Demographic Data. We obtain date of birth, date of death if applicable, sex, and citizenship status from the Census Bureau's version of the Social Security Administration's Numerical Identification database (Census Numident, as described in, *e.g.*, Bailey et al.,

[5]For ease of exposition, we refer to the EIN tax unit as "firm" throughout.

[6]Physician specialty is defined at varying levels of detail; in this paper, we primarily use Medicare's specialty codes or broader aggregates we define in Appendix B.1.

2020; Polyakova et al., 2021). We infer marital status from the tax filing status.

American Community Survey. Using PIK-based linkages, we add responses from the restricted-use version of the American Community Survey (ACS) for those physicians whose household was surveyed by ACS between 2005 and 2017.[7] This provides self-reported earnings and work hours. We also use ACS to construct a sample of lawyers for comparison.

Medicare Data. We add data on treatments physicians provide to Medicare patients. Since 2012, CMS has released data with the list of services performed, the number of times each service was offered, and additional detail by physician. We add data on Medicare reimbursement rates for each service-year from the CMS Physician Fee Schedule files.

NRMP Data. NRMP reports aggregate statistics from the residency match algorithm. The number of physicians who apply to each specialty, grouped by 10-point intervals of the USMLE score, are reported for six of the years between 2005 and 2016. We use these data, combined with average hourly income, Medicare revenue, and service composition by specialty-year, to estimate a specialty choice model in Section 3.3.

1.3 Income and Retirement Definitions

Physician incomes come through diverse and changing mechanisms. This mishmash of sources makes it particularly challenging to study physician earnings and highlights the advantage of using tax rather than survey data to measure income. We construct four measures of income in the tax data: individual total income; individual total wage income including any pre-tax deferrals to retirement plans or alike;[8] individual total business income; and Adjusted Gross Income (AGI) at the household level. We define retirement as the year

[7] Restricted-use ACS has finer geographic detail, less income top-coding, and a larger sample than the public-use version.

[8] We include deferred contributions into wages and subtract likely deferred account withdrawals from total individual income. The idea is to record earnings in the year they are earned, not consumed.

in which an individual who is older than 40 first receives form 1099-SSA, "Social Security Benefit Statement." Details are in Appendix B.2.

1.4 Sample Definitions

Our main sample is a physician-year panel from 2005 to 2017 for physicians aged 20 to 70. This results in 11.6 million physician-year observations for 965,000 unique physicians in our main sample, 848,000 of whom are observed in the 2017 cross-section (Table 1). In many of our analyses we also use two age-based subsamples: a peak-earnings sample of ages 40 to 55 and high-retirement-risk sample of ages 56 to 70 (350,000 and 287,000 physicians, respectively, in 2017).[9]

2 Sources of Variation in Physician Earnings

2.1 Basic Facts

Table 1 reports summary statistics for the full sample (column 1), the 2017 cross-section (column 2), and two age-based sub-samples of this cross-section (columns 3 and 4). The average physician in 2017 earns \$243,400 in wages and \$350,000 in total individual income. Income is right-skewed; median total individual income is \$265,000. One third of physicians have business income exceeding \$25,000. At the tax unit level, median Adjusted Gross Income is \$325,500 and 24% of physicians are in the top percentile of the national income distribution. Physicians' real earnings grew by 1% annually over the time period we consider (see Appendix Figure E.3). Table 1 reports additional characteristics of physicians and their work environments, including specialty, firm size, work hours, and medical school characteristics. We discuss these aspects in Appendix B.3.

We find that physicians in aggregate earn \$297 billion in pre-tax dollars measured by

[9]All numbers in this paper are rounded according to U.S. Census Bureau disclosure protocols.

total individual income, or 8.6% of total U.S. healthcare spending in 2017.[10] Put differently, out of $10,611 that an average American spent on healthcare in 2017, physicians earned $913. While billing for physicians' clinical services comprises one-fifth of spending, less than half of this amount is physicians' own pay.[11] Subtracting individual income tax payments at a rate of 30% implies that physicians' total after-tax earnings is closer to 6% of total U.S. healthcare spending, or 1% of GDP. This puts an upper bound on the magnitudes at play in policy discussions that suggest lowering healthcare spending by cutting physician pay (*e.g.* Baker, 2017). A common version of these discussions involves comparing U.S. salaries to those in Europe. While U.S. physicians earn more than their European counterparts, their relative positions in the income distribution are similar.[12]

Earnings Variation. Average earnings mask substantial heterogeneity. More than 25% of physicians in 2017 earn above $425,000, and the top 1% of physicians earns above $1.7 million (Figure E.1).[13] Table E.2 asks what share of this variation relates to observable characteristics. We run a series of regressions of physician earnings on covariates. We first include basic demographics—age, sex, race (white or not white), marital status, state (or country) of birth fixed effects, and an indicator for whether the individual was ever a non-U.S. citizen. Age accounts for 14% of the variation. Conditional on age, adding other demographics brings R^2 up to 0.19. Women earn 30% less than men. We then consider the explanatory power of covariates that physicians have (at least some) control over throughout their careers: attending a top-5 medical school, specialty, location (commuting zone), size of practice, and presence of business earnings. Specialty and firm size (statistically) explain

[10]Total healthcare spending in 2017 was $3.4 trillion according to CMS (2019, Table 1).

[11]This distinction is a major limitation of previous studies that use health record or claims data to infer something about physicians' own earnings, such as the gender pay gap (*e.g.* Ganguli et al., 2020).

[12]Chen et al. (2022) report that 10% of physicians are in the top two, and 42% of physicians are in the top five, percentiles of the Swedish income distribution. If U.S. physician earnings changed to match Swedish physicians' positions in the Swedish income distribution, aggregate physician earnings would fall by $90 billion. Reducing U.S. physicians' incomes to the absolute *level* of average physician incomes in Sweden would require lowering the average to $95,000. All else equal, this would reduce earnings by $200 billion, or 5% of aggregate U.S. healthcare expenditures.

[13]Appendix Figures E.1 and E.2 show the full distribution of individual earnings and AGI, respectively.

substantial shares of earnings variation. Physicians who graduate from the very top schools have 12% higher income than others, but this relationship appears to almost entirely reflect different access to specialties. Together, pre-determined demographics and observable career attributes explain up to 37% of the observed variation.

These results highlight two facts that guide our subsequent analyses. First, we see that age, the presence of business income, firm size, and specialty appear to play key roles in (statistically) explaining earnings. We flesh out the specific patterns along these dimensions next. Second, conditional on all observables, almost two-thirds of the variation remains unexplained. We unpack this unexplained variation further in Section 2.3.2, using two-way fixed effects to fully decompose the variation into individual- and market-specific factors.

Age Profile. Figure 1A plots individual total income by five-year age groups in 2017. The earnings profile is very steep. Physicians earn around $60,000 on average in their late twenties, while they are still in training. This escalates rapidly to an average of more than $185,000 in the early thirties, and peaks at around $425,000 at age 50. Work hours begin to fall and the probability of retirement starts rising at age 60 (Figures E.4A and E.4B), but earnings remain close to $270,000 into the late 60s. This age pattern motivates our focus on income during ages 40 to 55 in much of subsequent analyses, as that age interval reflects physicians' maximum earnings period.

Figure 1B shows that the growth in earnings during the highest-earning ages occurs through business income. Average *wages* are almost flat at around $285,000 at ages 40 to 55, while business income (along with the probability of filing Schedule C; see Figure E.4C) grows steadily and accounts for nearly one quarter of earnings at age 50.

Administrative vs. Survey Data. To highlight the differences between survey and administrative data, Figure 1C zooms in on the physicians who responded to the 2017 ACS. For the same individuals, total individual income is recorded as substantially higher in tax data than in the ACS. The difference is especially large at the career peak. During physicians' most productive years, the ACS estimates are about $140,000 lower, or one-third of the administrative data mean. Tax-based earnings grow much more rapidly during the highest-growth ages. The difference between the two measures is driven by the extensive margin underreporting of business income in the survey data (details are in Appendix B.4)—a crucial part of physicians' earnings, as discussed above.

Firm Size. Figure 1D shows the relationship between earnings and firm size among 40- to 55-year-old physicians. We see a pronounced non-monotonicity. Physicians in single-physician EINs have the lowest average earnings of $382,000. Average earnings are highest in firms sizes that correspond to small group practices of 8 to 10 physicians. Moving to larger firms, such as large physician organizations or hospitals, average earnings decline.

Top Earners Among Physicians. Table 2 examines the long right tail of the physician income distribution, showing how top earners differ from average physicians. We focus on physicians age 40 to 55 in 2017. First, as with the general population, the income gradient is steep for these quintessential "human capitalists." The top 1% of physicians averages $4 million in annual earnings, 10 times average annual earnings in the sample and more than twice the average earnings in the top 5%.[14]

Second, business income is crucial for the top earners. 80% of physicians in the top 1% report business income of at least $25,000, compared to 44% in the top half and 35% overall. The share of earnings coming from non-W-2 sources is also substantially higher among top earners: 85% for those in the top percentile, but 6% for an average physician.

Third, top earners are 67% more likely than the average physician to attend top-5 medical schools and 62% less likely to work in primary care. Top earners are 6 times more likely to be neurosurgeons, one of the specialties with most extensive training.

[14] For each cutoff in the table, mean incomes among physicians above the cutoff are very nearly double the value of the cutoff point itself. This suggests the physician income distribution is close to Pareto with a shape parameter of 2 throughout the top half of the physician distribution, as Gottlieb et al. (2023a) assumed previously when relating physician and non-physician top earnings.

Overall, the evidence on top physician earners is consistent with top earners in the economy broadly (Smith et al., 2019). The very top incomes are observed among highly trained physicians who earn business incomes rather than only wages.

2.2 The Importance of Specialty

Earnings by Specialty. Earnings vary substantially across specialties (Table E.5). Primary care physicians (PCPs), the most common specialty category, is also the lowest-earning. Average total individual income among 40- to 55-year-old physicians is $282,300 ($235,300 at median) for PCPs, or 70% of the overall sample mean in this age range in 2017. The highest earners are specialists who perform procedures and surgeons, whose average individual earnings are about twice those of PCPs. The right tail reflects similar variation. The probability of being in the top 1% of households nationally in 2017 ranges from 16% among primary care physicians to 57% among surgeons. While most specialties experienced a rise in earnings over the decade we consider, average earnings fell in radiology and subspecialties of internal medicine.

Correlates of Specialty Income. Higher earnings could make one specialty more attractive than another, or could represent a compensating differential. While Section 3.3 formally evaluates how earnings allocate physicians' talent across specialties, here we present descriptive relationships between earnings and specialty characteristics. These suggest that higher incomes indeed make specialties attractive rather than just compensate for disamenities.

We first examine how earnings differences across specialties relate to two key job amenities: working hours and training length. Figure 2A shows the relationship between total individual income (ages 40 to 55) and weekly working hours (based on ACS responses) at the granular Medicare specialty level using all years of our data (2005–2017). Specialties in which physicians report longer work weeks (such as neurosurgeons and cardiac surgeons, at close to 65 hours) have higher incomes. Ten extra hours per week is associated with $195,000 higher annual income (or around \$375 per hour). Two notable outliers well above the regression line are dermatology (44 hours) and ophthalmology (48 hours). Family practice, internal medicine, and pediatrics are all below the regression line. Figure 2B shows a very strong relationship between average income in specialty and physicians' average length of training.[15] Each extra year of training is associated with \$143,000 in extra annual income.

The challenges of medical training extend beyond the length alone. For instance, new residents matching in 2020 report having conducted two to three times as much research during medical school as their counterparts a decade earlier (Ahmed and Adashi, 2023). Panel C shows that the level of research experience—among those who successfully match in a specialty—is positively related to a specialty's income. Research experience among matched physicians is an equilibrium choice, so it is not clear whether to view it as a measure of ability or the specialty's entry costs.[16]

Panels A, B, and C show clear relationships, but also a fair amount of variation around the regression lines ($R^2 = 0.36$, 0.54, and 0.64 in Panels A–C, respectively). Any specialty above the regression lines must either have compensating differentials for unobserved job characteristics (such as flexibility, time on call, liability risk, or type of work) or be more attractive to potential entrants.

To distinguish between these explanations, we examine labor supply given the bundle of earnings, training, and hours that each specialty offers. Given the presence of entry restrictions, the number of physicians in a specialty is not an appropriate measure of labor supply. Instead, we look at *who* enters each specialty. Residency and fellowship programs generally prefer domestic MD graduates to other applicants. So each specialty's share of entrants from U.S. MD programs is a coarse metric of the specialty's appeal to incoming

[15] Although training is largely standardized within a specialty, there is variation across programs and across individuals. To systematically determine each specialty's *actual* average training length, we develop a method to estimate it empirically using the tax data. Appendix B.2 provides details. Our measure ranges from 3.7 years for family practice to 7.2 years for cardiac surgery.

[16] The theoretical model in our working paper (Gottlieb et al., 2023b, sec. 1) formalizes these two interpretations.

physicians. Figure 2D relates this share to the unexplained part of specialty earnings.[17] We residualize both the share of U.S. MD-trained physicians and specialty mean income with respect to training duration and work hours. We then plot the residualized U.S.-trained share against residualized income (with sample means of each variable added to the residuals). We observe a clear upward slope. Conditional on hours and training, a specialty with $100,000 higher peak earnings tends to have a 7 percentage point higher share of U.S. MD graduates.[18] This suggests that income above the regression lines in Panels A and B is largely an attractive feature of a specialty rather than a compensating differential. Section 3.3 moves beyond this descriptive relationship and estimates a formal model of specialty choice.

2.3 The Importance of Geography

The geographic pattern of physician earnings is striking. Figure 3A shows average earnings for physicians aged 40 to 55 by state. The pattern is unusual: physicians incomes are not highest on the coasts, as they are for lawyers (Panel B) and for the broader economy.

We use place- and person-fixed effects to unpack this pattern. We first use event studies to implement the movers strategy of Finkelstein et al. (2016), Molitor (2018), and others, to determine the causal importance of location. We then delve into a finer decomposition of place- and person-specific factors based on the methods of Card et al. (2021), and describe the characteristics of high-earning locations. The place effects for physician earnings are unique, with negative sorting between people and places.

The findings here and in Section 2.2 suggest that specific healthcare policies, which often focus on particular geographies or specialties, may shape physician labor markets. This motivates us to specifically examine the role of government payments in Sections 3 and 4.

[17] Panels C and D use National Residency Match Program (NRMP) specialty definitions. NRMP data allows us to distinguish between the number of U.S. MD graduating seniors and other applicants who match to a specialty. Non-U.S. MD graduates are primarily graduates of international medical schools, but also include graduates of U.S. DO programs.

[18] A similar pattern emerges when we compare the share of graduates from top 5 medical schools in a specialty to the specialty's average hourly income. We find that surgeons and procedural specialists have nearly twice the share of top 5 graduates and the highest incomes (Figure E.5).

2.3.1 Event Study

Empirical Approach. We use physician movers to ask if location matters for earnings. For each physician i who moves across commuting zones, denote i's origin CZ by c, destination CZ by c', and the difference between average log physician incomes in these CZs by $\Delta\overline{\ln y}_{(c,c')}$.[19] Using data from twelve years around the move, we estimate:

$$\ln y_{it} = \alpha_i + \sum_{\tau \neq -1} \beta_\tau \times \mathbb{1}_\tau \times \Delta\overline{\ln y}_{(c,c')} + \theta_{a(i,t)} + \lambda_\tau + \varepsilon_{it}, \tag{1}$$

where $\ln y_{it}$ denotes physician i's annual log individual income. This is a dynamic, parametric event study specification, which yields coefficients $\hat{\beta}_\tau$ on the income change for each year τ relative to the year prior to the move ($\tau = -1$). Under the standard assumption that shocks ε_{it} are conditionally mean-independent of location causal effects, the post-move coefficients can be interpreted as the share of the geographic income differences due to place rather than person. Although standard, this assumption cannot be taken for granted, so we use the pre-move $\hat{\beta}_\tau$ coefficients to investigate it. We control for physician fixed effects, α_i, physician age effects, $\theta_{a(i,t)}$, and relative time fixed effects, λ_τ.[20]

Results. Figure 4 shows that location drives a large share of earnings. The estimates of $\hat{\beta}_\tau$ show a sharp change in income at the time of the move and no differential trends in income preceding the move. The point estimates suggest that movers' incomes shift by over 50% of the difference between mean incomes in origin and destination. This estimate is even higher within location-by-specialty.[21] Having established that location influences earnings, we next

[19] Our regression sample is all 40-to-55-year-old physicians who changed their commuting zone (CZ) residence exactly once between 2005 and 2017. $\Delta\overline{\ln y}_{(c,c')}$ is computed using data on all 40-to-55-year-old physicians. We use CZs to capture both within- and cross-state variation; unadjusted CZ average incomes are shown in Figure E.6.

[20] Calendar year fixed effects are implicitly included as a linear combination of the other fixed effects.

[21] Figure E.7 shows analogous event study graphs for four subsamples of physicians: specialists, PCPs, and physicians who graduated from ranked and from unranked medical schools. For each sample, we construct the income difference using physicians in the same category. Looking within specialty leads to coefficients meaningfully larger than the overall average, with the point estimates closer to 0.75. This suggests that the key driver of variation is the interaction of specialty and location, and specialty earnings have different

examine the patterns of these locations' effects and how physicians sort across them.

2.3.2 Place vs. Physician Factors: Variance Decomposition

Empirical Approach. To decompose place (c) and person (i) contributions to individual earnings, we use a two-way fixed effects model. Year t earnings are:

$$\ln y_{it} = \alpha_i + \psi_{c(i,t)} + \theta_{a(i,t)} + \lambda_\tau + \varepsilon_{it}, \tag{2}$$

in which α_i is the individual component, ψ_c is the location (commuting zone) component, and ε_{it} is a person-time residual assumed to be mean-independent. Some specifications include fixed effects for age, $\theta_{a(i,t)}$, and for time relative to the year of move, λ_τ. Moves must be independent of the shocks ε_{it}, and the lack of pre-trends in Section 2.3.1 supports this assumption. Limited mobility bias could plague a naive variance decomposition, so we implement the Andrews et al. (2008) homoskedastic correction, and the Kline et al. (2020) heteroskedastic correction, along with a direct fixed effects estimation (Abowd et al., 1999).[22]

Results. Figure 5A shows the key results. The first three bars show the estimated variance of location effects, $\text{Var}(\psi_c)$, using standard fixed effects estimation, the homoskedastic correction, and the heteroskedastic correction. The next three bars report estimates of how physicians sort across space, $2\text{Cov}(\alpha_i, \psi_c)$, for the same three methods. All three show pronounced negative sorting. The magnitude of sorting is substantial relative to that of the location effects themselves; the ratio of covariance to variance is between 0.6 and 0.8 (Table E.6). Column (4) shows that the result is stable when adding time-varying controls.

Panel B presents analogous estimates for lawyers, another highly-educated occupation,

geographic patterns.

[22] Limited mobility bias is a term for the estimation error that can emerge in two-way fixed effects estimation (Andrews et al., 2008). In our context, if too few physicians move across regions, identifying separate effects of physician and location becomes difficult. This tends to bias downwards the covariance between worker and firm effects. Under the assumption of homoskedastic, independent errors, Andrews et al. (2008) derive the exact analytic expression for the bias and the bias-corrected estimator of the variance components. The Kline et al. (2020) heteroskedastic correction uses a leave-one out variance component estimator. We use the Bonhomme et al. (2023) `PyTwoWay` package to implement all estimators.

but with a differently structured labor market. We again initially find a negative covariance when using the standard fixed effects estimator, but the limited mobility bias corrections reverse the sign for lawyers.[23] The magnitude of the corrected covariance is in the same ballpark as for physicians, but with the opposite sign. This demonstrates that our data and procedures do yield the expected positive sorting, consistent with Card et al. (2021) and the broader literature on worker-firm matching—when the pattern exists. Physicians' pattern is unique.[24]

Importance of Sorting for Geographic Patterns. What does this sorting mean for the overall pattern of earnings across space? We follow Card et al. (2021) and address this question by aggregating equation (2) to the CZ level:

$$\overline{\ln y_c} = \overline{\alpha}_c + \psi_c + \beta \overline{X}_c. \tag{3}$$

This decomposes area-level average log earnings among physicians $\overline{\ln y_c}$[25] into a location effect ψ_c, the *average* person effect among physicians in the location $\overline{\alpha}_c$, and the part predicted by observable characteristics of the physicians.[26] The variance decomposition of (3) reveals what share of variation in areas' average incomes come from the places themselves, the

[23]Comparing the lawyer and physician samples, the latter is an order of magnitude larger because we identify physicians with administrative data and lawyers from the ACS sample (see Appendix B.1). The trace of the matrix governing the TWFE bias is an order of magnitude larger for lawyers, which is why the corrections have such an impact for them but little for physicians.

[24]Two further analyses provide evidence that this negative covariance is not an artifact of limited mobility bias. First, we conduct a simple split sample estimation and obtain results extremely similar to those reported here. Second, we estimate a parametric alternative to two-way fixed effects: we use a linear regression to adjust raw earnings for various individual-level *observables*, but *not* individual fixed effects, and estimate each CZ's fixed effect conditional on these observables. We then correlate these CZ effects with the individual effects from (2). As we add covariates, the correlation of individual fixed effects with the conditional CZ effects becomes increasingly negative, trending towards the pattern in Figure 5C.

[25]Equation (3) computes average incomes and average person effects on the sample of moving physicians. Area-level average log earnings among moving and non-moving physicians are highly correlated, as are the implied average person effects.

[26]The estimates of $\hat{\beta}\overline{X}_c$ reveal the share of earnings variation that comes from worker composition, along *observable* dimensions, leaving the average of physician fixed effects α_c to capture the unobservable part. Including or excluding age fixed effects and relative time fixed effects has little impact on the estimates of location variance and sorting (Table E.7).

composition of workers, and sorting of those workers across locations.

Figure 5D shows the results by relating the estimates of $\overline{\alpha}_c$ and ψ_c in a binned scatterplot. The sorting remains negative. The last column of Table E.6 reports the magnitude. The relative magnitude of the sorting effect increases to around 1.2 times the variance of location effects, compared with around two-thirds of the location variance when estimated at the individual level. To benchmark the magnitude, the covariance of CZ-by-industry effects with person effects in Card et al. (2021) explains 1.8 times the magnitude of the CZ-industry effects.[27] The relative magnitude of our sorting is 1.2 times that of location effects, but with the opposite sign.

Firm Fixed Effects. While intimately linked with the physician's location, the firm at which a physician works may have its own influence on earnings. To explore this, we estimate a firm-worker two-way fixed effects decomposition using a model analogous to (2). The results, shown in Table E.6 Panel C and Figure E.8 Panel A, are broadly consistent with the location-physician decomposition. Both the raw and bias-corrected covariances between the physician's individual and firm effects are negative. This negative worker-firm covariance reinforces the uniqueness of physicians' labor markets.[28]

2.3.3 Correlates of Place Fixed Effects

We explore the economics of these places by projecting the place fixed effects on observable characteristics.[29] Figure 6 shows a series of correlations between location characteristics and our estimated place fixed effect, and between the same characteristics and the location's

[27]Card et al. (2021) consider CZ-by-industry while we consider location effects for one occupation. Our data differ from the LEHD Card et al. (2021) use in that we include the self-employed and non-wage income.

[28]Notably, the negative relationship between firm and worker effects disappears when we condition on location fixed effects (Figure E.8B), suggesting that the physician-firm relationship likely reflects the firms' geographic location.

[29]We use estimates for commuting zone fixed effects based on (2) with the full set of controls, but without the limited mobility bias corrections that—as we have shown above—do not change the baseline sorting pattern among physicians. The TWFE analysis in equation (2) also yields fixed effects for each individual physician. Their patterns are similar to the raw physician descriptives discussed in Section 2, so we do not present them further.

mean log earnings. Measures of the location's general economic strength tend to be uncorrelated, or have a slight positive relationship, with the location's raw physician earnings. This pattern holds whether measuring economic strength with income, education, real estate prices, or population size. Life expectancy is slightly negatively correlated with both earnings measures, though the movers-based treatment effect on mortality (from Finkelstein et al., 2021) is statistically unrelated.

In contrast, the physician earnings fixed effects in each location have a markedly different relationship with regional characteristics. The fixed effect is strongly negatively correlated with local income.[30] This pattern holds up whether comparing physician earnings to local average income, prices, or other economic characteristics.

A few economic forces could generate this pattern. First, physicians could be fundamentally more productive in low-income places, though this contradicts empirical evidence on agglomeration in healthcare (Dingel et al., 2023). Second, physicians may face less competition in smaller and lower-income markets and thus be able to charge higher markups to self-paying and privately insured patients. But the magnitude of this force is probably insufficient to explain all of the earnings differences we observe.[31] Third, the income gradient may reflect compensating differentials for skilled workers' preferences to live in higher-income locales. But it is not clear why this would be true for only physicians and not lawyers. Finally, government's major role in the healthcare market, and the complex political economy of this role, could cause outcomes to differ from other industries. Federal and state governments

[30] As Table E.8 shows, we find a positive, but noisy relationship between the fixed effects and area income for lawyers; we cannot reject a zero. The somewhat noisier results for lawyers are not surprising, since we have a smaller sample, as we don't have the universal registry of lawyers as we do for physicians. Table E.8 reports regression coefficients and standard errors for all regressions shown in Figure 6.

[31] Clemens and Gottlieb (2017, Fig. 2) report 20 percent higher private payments in the most concentrated markets compared with the least. Dunn and Shapiro (2014) find that a 10 percent increase in physician market concentration increases prices by 1 percent. In hospital pricing, Cooper et al. (2019) report 12 percent higher prices in monopoly markets than those with at least four competitors. The pattern we observe is also not driven by CZs with very few physicians. Indeed, the negative correlation between CZ FE and median household income is stronger among CZs with more than 10 physicians. Further, CZ FE are only weakly negatively correlated with the number of physicians in each CZ in our data, while we would have expected a pronounced negative correlation driven by small markets if the main underlying mechanism were market power (Bresnahan and Reiss, 1991). In short, these estimates imply that lack of competition is insufficient to drive the scale of differences we estimate in earnings across commuting zones.

purchase medical services on behalf of lower-income and rural residents, increasing these consumers' effective purchasing power for healthcare relative to other goods or services. Section 3 measures this influence and Section 4.1 asks how it affects the geographic earnings gradient.

3 Policy Influence on Earnings and Labor Supply

We use multiple empirical strategies to investigate the government's influence on physicians' earnings and labor supply. We first use short-run price changes, which occur as Medicare adjusts its reimbursements for each procedure. We then use the persistent demand shock resulting from health insurance coverage expansion under the Affordable Care Act (ACA) to study medium-run labor supply decisions, such as retirement. Finally, we quantify how much Medicare reimbursement adjustments affect specialty choice—physicians' key long-run labor supply decision.

3.1 Transient Price Shocks and Short-Run Supply Responses

We use physician payment adjustments in the $900 billion-per-year Medicare program to estimate short-run elasticities of income and labor supply. Medicare reimburses physicians' professional services based on a fee schedule that defines a "Relative Value Unit" (RVU) for each medical service. The number of RVUs is supposed to reflect the time, skill, and effort required to perform a service. It changes over time due to periodic reviews, which reflect political factors.[32] We use changes in the RVUs assigned to each service to estimate how much Medicare payments influence physicians' contemporaneous incomes and labor supply.

[32]We rely on three facts about this system. First, this RVU metric is meant to reflect the differences in the time and effort it requires to provide different services. As a result, RVUs vary across time and geography, but not across individual physicians. Second, Medicare's RVU Update Committee (RUC) regularly reviews how many RVUs are assigned to each service. The reviews can be triggered by changes in the service, by Medicare's request, or based on a pre-determined five-year review cycle. Third, the timing of when a particular code, or even codes of which specialty, is reviewed is uncertain. Chan and Dickstein (2019) explain the uncertainty in which specialties will be able to propose code reviews at any given RUC meeting. Appendix C.1 provides more details about the institutional setting and our empirical approach.

Physicians may shift their service mix as relative prices change. Given the broad set of price changes Medicare implements each year, we analyze supply responses at the procedure code level. To study physician-level response margins, such as earnings, retirement, and total procedure supply, we measure each physician's exposure to each year's reimbursement shock using differences in physicians' service bundles. While each billing code's update is applicable nationally, physicians perform different bundles of services. So physicians are differentially exposed to each year's set of RVU changes. We use the logic of simulated instruments to construct physician-year exposure to Medicare price changes.

For each physician i, we first compute the average number of times each service k was performed across all years of our utilization data (2012 to 2017), denoted $\overline{q_{i,k}}$.[33] This is a time-invariant quantity measure, which we multiply by the time-varying number of RVUs that Medicare assigns to service k and add them up by physician-year. The result is a series of annual price shocks for each physician, purged of any behavioral response. Mathematically, the composite price for physician i who performs a set K of services in year t is:

$$P_{i,t} = \sum_{k \in K} \overline{q_{i,k}} \times RVU_{k,t}. \tag{4}$$

We label this the *Medicare price instrument*. Figure E.9 shows the distribution of annual shocks to this instrument, $\Delta \ln P_{i,t} = \ln P_{i,t} - \ln P_{i,t-1}$.

We estimate the following empirical relationship to determine how log income, $\ln Y_{i,t}$, responds to the log Medicare price instrument:

$$\ln Y_{i,t} = \alpha_i + \beta \ln P_{i,t} + \theta_{a(i,t)} + \eta_{t,s(i)} + \varepsilon_{i,t}. \tag{5}$$

We are interested in β, the reduced form elasticity of income Y with respect to the Medicare price instrument.[34] We control for physician fixed effects, α_i, physician age fixed effects,

[33] Results are similar if we instead construct the weights $\overline{q_{i,k}}$ based on the quantity in only the first year a physician-procedure pair is observed.

[34] To the extent that changes in Medicare fee schedule can trigger changes in private insurers' reimburse-

$\theta_{a(i,t)}$, and year-by-specialty fixed effects, $\eta_{t,s(i)}$. The key coefficient β is thus identified from variation in the composition of services that each individual physician performs.[35] We run our analysis separately for 40–55- and 56–70-year-old physicians; the former group is prime working age physicians, so it excludes trainees with fixed incomes and minimizes the mechanical decline in income due to retirements. The latter group is closer to retirement, allowing us to measure that labor supply margin.

To study Medicare's impact on the supply of medical care, we replace the dependent variable with the log number of RVUs physician i bills in year t, denoted $\ln Q_{i,t}$.[36] Since each procedure's RVU weight enters into both this total and into our instrument $P_{i,t}$, we expect a mechanical coefficient of 1 in the absence of any behavioral response. With total RVUs as an outcome, the difference between the coefficient and 1 yields the supply elasticity. A coefficient below 1 indicates income-targeting behavior, while a coefficient above 1 indicates a positive supply elasticity.

To obtain the elasticity of income to Medicare billing, we estimate an IV setup treating the log Medicare price, $\ln P_{i,t}$, as an instrument for the log total RVUs billed, $\ln Q_{i,t}$, with log income, $\ln Y_{i,t}$, as the dependent variable. To quantify any response via the retirement margin of labor supply, we treat income as the endogenous variable and retirement as the outcome. We estimate both of these IV specifications using two-stage least squares.

To estimate more granular labor supply responses, we count the number of times a physician bills for each code in each year, $q_{i,k,t}$. We directly measure how much a change in the code's own RVU weight, $RVU_{k,t}$, affects this measure of supply:

$$\ln q_{i,k,t} = \alpha_i + \beta RVU_{k,t} + \theta_{a(i,t)} + \eta_{t,s(i)} + \varphi_{\kappa(k)} + \varepsilon_{i,t} \tag{6}$$

ment rates, as in Clemens and Gottlieb (2017), our reduced form estimate will capture both the direct and indirect effects of Medicare's reimbursement on physician earnings and labor supply.

[35] To account for the large variability in Medicare billing volumes across providers, we use the average Medicare revenue each physician collected in 2012–2017 as regression weights. We cluster standard errors by Medicare specialty.

[36] This can be interpreted as the number of services a physician provides, weighted by value. It is formally defined in Appendix C.1, where we also present the estimating equations. We also estimate a version that counts the number of unique procedures each physician bills, regardless of value.

where φ_κ is a set of procedure fixed effects.[37] To measure impacts on the number of patients treated and the care provided per patient, we also estimate a version in which the dependent variable is the number of unique patients per procedure.

Results

Table 3 and Appendix Figure E.10 report the results. A 10% increase in Medicare payment rate leads to a 2.4% increase in professional earnings of 40- to 55-year-old physicians, i.e. a reduced-form elasticity of 0.24 between earnings and Medicare prices. A substantial component of this change is physicians' behavioral response; a 10% increase in the payment rate leads physicians to bill 4.4% more RVUs (column 2).[38] 2SLS estimates that divide the reduced form elasticity between earnings and prices by the first stage imply that the extra 10% that physicians bill Medicare increases their income by 1.7% (column 6). This intensive margin labor supply response is a composition of performing 3.9% more unique procedures (column 3), and shifting to relatively higher-paid procedures.[39] The procedure-level analysis directly shows that a 10% increase in a given procedure's price leads physicians to supply 3.8% more of this procedure. Nearly the full effect (3.4% out of 3.8%) is driven by performing this procedure on additional patients rather than providing the procedure more frequently for the same number of patients.

Intensive margin responses are broadly similar among 55-to-70-year-old physicians. For this group, we also find a response on the extensive margin. The IV estimate shows that a 10% increase in professional earnings driven by changes in the reimbursement rates leads to a 0.5 percentage point decline in the probability of retirement that year.

To interpret the magnitude, we convert our earnings estimates into a pass-through—how much do physicians' earnings increase when the government pays one more dollar? Our

[37]The subscript κ is distinct from k because the fixed effects are by procedure (HCPCS code), while the unit of observation is at the code-by-place of service level.

[38]Recall that we must subtract 1 from the coefficient of 1.437 to get the supply elasticity.

[39]The elasticity for total RVUs is higher than for the number of procedures. This means the RVUs per procedure must be increasing as the Medicare payment rate increases.

direct estimates imply that physicians earn $62 of each $100 in additional *Medicare* spending. Accounting for Medicare's spillover into private insurance spending (Clemens and Gottlieb, 2017; Clemens et al., 2017), we get a lower pass-through of $25 for each $100 of *any* insurance spending.[40] Under either interpretation, pass-through is quite large. Our estimates differ from the modest level of rent-sharing with workers found in response to many other shocks (Card et al., 2018), but are similar to rent-sharing with higher skilled workers who benefit, for example, from patent rents (Kline et al., 2019).

Our results indicate that these marginal earnings have real consequences: paying physicians more increases care provision, as more patients receive better-compensated treatments and physicians delay retirement. We do not observe health outcomes so cannot assess the net social benefits of this marginal spending. The labor supply elasticity we estimate of 0.4 is lower than in Clemens and Gottlieb (2014) or Cabral et al. (2021), but is similar to other estimates of compensated wage elasticities (Nicholson and Propper, 2011).

3.2 Persistent Demand Shocks and Medium-Run Supply Responses

We next study the impact of a persistent demand shock on physician earnings and behavior. The Affordable Care Act increased the insured share of the non-elderly population, and the increase has persisted for a decade. This could be a sufficiently large and persistent shock to affect physicians' longer-term decisions such as retirement and employment structure.

The ACA increased insurance coverage through two main mechanisms. First, 37 states expanded Medicaid eligibility. Second, governments created health insurance Marketplaces selling subsidized individual insurance plans. The ACA became law in 2010, but most of the insurance expansions were implemented in 2014 and 2015. We analyze these expansions in the 24 states where the full package of key ACA reforms took place roughly simultaneously—those that expanded Medicaid in 2014 or early in 2015, coinciding with the rollout of Marketplaces in 2014.[41]

[40] Appendix C.1 details these calculations.

[41] Appendix C.2 provides more details on our definitions and sources.

Our identification relies on variation in the magnitude of potential insurance coverage expansions in each county. There is more scope for insurance coverage to increase in counties that had a higher share of uninsured population prior to the law's implementation. The share of uninsured under-65 population in 2013, on the eve of ACA expansions, varied from under 10% in some counties of Minnesota to over 30% in some counties of Nevada (Figure E.11). Let $U_{c,2013}$ denote the share of the under-65 population uninsured in county c in year $t = 2013$. We estimate the reduced form impact of insurance expansions on physician-level outcomes $Y_{i(c),t}$. We use our physician-year panel covering four years post-expansion to run:

$$Y_{i,t} = \sum_{t=2005, t\neq 2010}^{2017} \beta_t \times \mathbb{1}_t \times U_{c(i),2013} + \delta_t + \mu_{c(i)} + \theta_{a(i,t)} + \epsilon_{i,t}. \tag{7}$$

We include calendar year fixed effects, δ_t, county fixed effects, μ_c, and age fixed effects, $\theta_{a(i,t)}$. The coefficients $\hat{\beta}_t$ on year fixed effects interacted with our time-invariant measure of exposure, $U_{c(i),2013}$, should be interpreted as relative to 2010, the year in which ACA passed. Estimated on a physician-year panel, this specification accounts for differences in the number of physicians affected in each county and flexibly controls for differences in the age composition of physicians. We cluster standard errors at the county level.

To interpret the coefficients $\hat{\beta}_t$ as measuring how much insurance coverage affected outcome $Y_{i,t}$, we need the identifying assumption that changes in potential outcomes absent the ACA rollout would have been independent of the uninsurance rate among the non-elderly population in 2013, conditional on covariates. While this parallel trends assumption is not directly testable, the event study specification in (7) allows us to assess whether counties with different uninsurance rates in 2013 followed parallel trends in outcomes prior to ACA passage in 2010. Expectations of future demand may be important for persistent outcomes like retirement or firm structure. These choices may respond to anticipated changes in insurance coverage, and thus to the ACA's passage, rather than realized insurance coverage. In contrast, income is likely to change only once expansions take place and demand increases.

In practice, ACA expansions led only a subset of previously uninsured people to obtain insurance. To capture the relationship between $Y_{i,t}$ and the insured population in a county, we estimate a first-stage event study to see how insurance coverage $I_{c(i),t}$ in county c in year t changed as a function of the share uninsured in 2013. The specification is nearly identical to (7), but with $I_{c(i),t}$ as the outcome and normalizing to 2013, the year prior to implementation. We also estimate a 2SLS specification, with the rate of insurance in the under-65 population as the endogenous variable and the uninsured share in 2013 as an instrument. To facilitate this, we collapse the differential time path of treatment effect into the pre- and post-implementation periods.[42] To interpret this estimate, we assume the baseline uninsured share only affects outcomes through its effects on insurance.

We estimate the effect of insurance expansions on (log) total individual income and the likelihood of generating extra income through self-employment (as measured by filing Schedule SE) among physicians in their peak earning years (ages 40 to 55). For the population at a higher risk of retirement we measure the effect of ACA expansion on the probability of retirement.[43]

Results

Figure 7 plots coefficients $\hat{\beta}_t$ for the first stage and the reduced form for individual income and retirement. Table 4 reports the first stage, reduced form, and 2SLS coefficients for all outcomes. The first stage estimate (Column 1 of Table 4) shows that counties with a ten percentage point higher pre-ACA uninsurance rate saw a 4.97 percentage point higher rate of insurance coverage in the post-implementation years, with no noteworthy changes in insurance between 2010 and 2013.

Figure 7B and columns (2) and (5) of Table 4 show that earnings among physicians aged

[42]Because of the potential for anticipation effects in long-run decisions, we report two sets of 2SLS specifications: one in Table 4 that defines the pre-period to include all years before insurance expansions began (all years before 2014), and another in Table E.9 that drops the intervening years between the ACA becoming law and its implementation (2011 to 2013). Results are similar.

[43]As in Section 3.1, we consider ages 56 to 70 to be at a higher risk of retirement. To capture all physicians who turn 56 during our event study time window, the regression is estimated on all physicians age 44 to 70.

40 to 55 grew faster in these more affected areas. We estimate that a ten percentage point higher baseline uninsurance rate leads to 3.9% higher individual earnings in the fourth year post-expansion, or 2.2% on average across post-implementation years. Scaling this income effect by the first stage suggests that a ten percentage point increase in insurance coverage (a 12% increase over the baseline average of 85% in 2013) increases physician income by 4.9% across post-implementation years. The elasticity of physicians' earnings to the rate of insurance coverage in the under-65 population is thus 0.41.

Columns (3) and (6) of Table 4 shed some light on how physicians may achieve these changes. The probability that a physician files Schedule SE (self-employment income above $400) increases by 3 percentage points for each 10 percentage point increase in insurance. This proxies for the extensive margin of self-employment and may capture increasing opportunities to generate side income.

Turning to labor supply, columns (4) and (7) of Table 4 report that a 10 percentage point higher insurance rate leads to a 0.85 percentage point decline in retirement probability after the implementation of ACA expansions. Figure 7C shows that this effect emerges after the law is signed rather than after implementation, which we would expect if physicians delay retirement in anticipation of a demand increase. This evidence suggests that the substitution effect dominates the income effect over the time horizon we consider. Converting the post-implementation estimate to an elasticity, a 12% increase in the rate of insurance coverage leads to 4.9% higher income and 9.1% less retirement, for a medium-run elasticity of retirement to income of -1.8.[44] This suggests a larger behavioral change in response to a more permanent change in income than we found in response to short-run fluctuations in reimbursement rates.

We use our estimates to ask what share of insurance spending on marginally insured patients goes to physicians—a key issue for the political economy of health insurance.[45]

[44]A 2SLS specification that drops the pre-ACA-implementation years implies 4.1% higher income and 9.8% less retirement, for a medium-run elasticity of -2.4 (Table E.9).

[45]The analogous question among hospitals is well-studied (Garthwaite et al., 2018).

Based on our pooled estimates, 8% of the $110 billion annual spending (CBO, 2016, Table 3) on the ACA insurance expansion accrued to physicians.[46] Since physicians' baseline earnings as a share of medical spending is 8.6%, their gain from expansions was nearly proportional to their baseline expenditure share.

3.3 Price Shocks and Long-Run Supply Responses: Specialty Choice

Beyond the immediate labor supply responses found in Sections 3.1 and 3.2, the effect of government policies on earnings may be even more important if these policies shape talent allocation over the long run. Figure 2 shows suggestive patterns: in the cross-section, higher-earning specialties tend to attract physicians with more qualifications along measurable dimensions. We now use variation in Medicare reimbursement policies to identify the elasticities of specialty choice to Medicare reimbursement rates and to income. As physicians can choose from multiple specialties, we analyze these decisions with a discrete choice model.

An important consideration for the model is that specialty choice is regulated through constraints on residency slots. In practice this means that only physicians sufficiently attractive to residency programs have free choice of specialty, while less-desirable applicants may be rationed out of the most lucrative specialties.[47] Rather than imposing *ex ante* restrictions on the choice set, we estimate the full model at different points in the distribution of talent, which we proxy with USMLE scores.[48] We use six years of aggregate NRMP data on the number of physicians who apply to each specialty, reported by bins of USMLE scores. We posit that our estimates for the highest-scoring applicants reflect true preferences, while

[46]Policy reports suggest that ACA expansion resulted in approximately 5.9 percentage points more people insured among non-elderly in total (Tolbert et al., 2020); the uninsurance rate went down from 16.8% in 2013 to 10.9% in 2015. Applying our 2SLS estimate, this expansion led to a 2.9% increase in physician incomes, or around $10,000 per physician (2.9% of $350,000). In aggregate for 848,000 physicians in our cross-section, this means $8.7 billion of extra spending, or 8% of the $110 billion in annual spending.

[47]Our working paper (Gottlieb et al., 2023b, sec. 1) offers a formal model of this.

[48]The USMLE exam has historically played an important role in the residency match. In 2022, numerical scores were replaced with pass/fail grading (USMLE, 2021). USMLE test scores are of course not the only determinant of physicians' freedom to choose a specialty, but do strongly predict applicant success (NRMP, 2014). This talent measure need not be the same as a physician's clinical skill, although other work suggests that clinical skill is correlated with traditional ranking measures (e.g. Doyle et al., 2010)

other physicians' choices reflect a combination of preferences and choice set rationing.

Consider a new physician i in USMLE test score bin a entering the residency match in year t. Physician i chooses one specialty s out of a set of nine specialty categories. The doctor's choice maximizes utility, which depends on specialty characteristics as observed for prime age physicians (age 40 to 55) working in year t:[49]

$$u_{ist} = \alpha_a P_{st} + \beta \phi_{st} + A_s + \delta_a + \xi_{ast} + \varepsilon_{ist}. \tag{8}$$

P_{st} is the specialty-year Medicare price instrument, as defined as in equation (4) but now at the specialty-year level, divided by the specialty's average number of hours worked per year. We compute P_{st} using data on 40-to-55-year-old physicians working in specialty s in year t. α_a captures the marginal utility of income and is allowed to vary across test score bins a. Specialty fixed effects A_s capture differences in preferences for time-invariant specialty-specific amenities, while δ_a normalizes utility across score bins. The vector $\phi_{st(i)}$ denotes time-varying features of specialty s that we can measure.[50] ξ_{ast} denotes time-varying characteristics that are not observed and could vary across score bins. Finally, ε_{is} is the idiosyncratic part of individual i's utility for specialty s. We assume that this unobserved part of utility is independently and identically distributed with a type I extreme value distribution, which gives us a logit discrete choice model specification.

We estimate the model using the Berry (1994) log-shares transformation. We use data for all USMLE score bins, allowing the main coefficient of interest, α_a, to vary by score.

[49]Because we are thus comparing one cohort's choices with a different cohort earnings, equilibrium changes in a specialty's ability do not bias our estimates—a concern that would emerge if earnings and ability were measured among the same cohort.

[50]These characteristics are the female share in the specialty, standard deviation of hourly income in the specialty, and average firm size of physicians in the specialty, all measured among 40- to 55-year-old physicians in specialty s year t. Our specification is consistent with the literature on occupational choice that has found that beliefs about financial returns matter for choices, but that nonpecuniary features also play an important role (Arcidiacono et al., 2020; Altonji et al., 2016). It is also consistent with evidence in Wasserman (2022) that gender-specific preferences influence specialty choice.

With observations at the specialty-year-score bin level, we estimate:

$$\ln \pi_{ast} - \ln \pi_{a0t} = \alpha_{base} P_{st} + \sum_{a \leq 190}^{a > 260} \alpha_a \cdot \mathbb{1}_a \cdot P_{st} + A_s + \delta_a + \beta \phi_{st} + \xi_{ast}. \tag{9}$$

Here, $\ln \pi_{ast}$ denotes the log share of graduates in score bin a in cohort t who applied to specialty s, with $s = 0$ denoting the specialty we treat as the outside option (family medicine).[51] P_{st} is interacted with test score bin fixed effects, $\mathbb{1}_a$, with the lowest bin omitted. All other right-hand side variables are defined as in equation (8). We normalize all right-hand side variables by subtracting the variable's contemporaneous value for family medicine, the outside option.

A common identification concern in discrete choice models is that the unobserved characteristics (ξ_{ast}) that affect choices could be correlated with prices. In our main specification, variation in Medicare RVU rates P_{st} is not an equilibrium object, helping to alleviate this concern. By instead constructing P_{st} based on policy changes, as described in Section 3.1, we exploit variation more likely to be independent of ξ_{ast}.

We estimate three variants of this model. First, equation (9) as specified above is a reduced-form specification that directly measures how variation in government policy affects specialty choices. We also estimate an OLS specification in which we replace P_{st} with M_{st}, the average hourly earnings computed using data on 40-to-55-year-old physicians working in specialty s in year t. This yields the *income* elasticity of specialty choice, as opposed to the *Medicare reimbursement* elasticity estimated in the reduced form. But it is based on equilibrium earnings, not policy variation, so the estimates could be biased. We thus estimate a 2SLS specification in which the Medicare price variable P_{st} instruments for earnings M_{st}.[52]

[51] We observe zero applicants in the data for fewer than 2% of score bin × specialty × cohort combinations. This creates the common issue in discrete choice models (Dubé et al., 2021; Gandhi et al., 2023) of choice options with zero shares; in our case the incidence of such observations is very low. We add one non-matching applicant to those specialty-by-score bin-by-year observations in which NRMP reports zero non-matching applicants. Alternative approaches, including the exclusion of zero share observations, result in very similar estimates.

[52] The analysis of the relationship between specialty-level earnings and the Medicare price instrument here is conceptually analogous to the analysis in Section 3.1. Since the 2SLS version of equation (9) has an

The coefficients $\hat{\alpha}_a$ on P_{st} or M_{st} are our primary coefficients of interest in all specifications. They measure the responsiveness of specialty choice to the specialty's financial return, and importantly how this responsiveness changes across the score distribution. We expect $\hat{\alpha}_a$ for high USMLE scores to reflect the true marginal utility of income, measuring how graduates trade off financial and non-financial amenities of specialties. As we move down in the score distribution, $\hat{\alpha}_a$ should shift, reflecting the shadow price of the entry constraint. At the bottom of the distribution, the coefficient $\hat{\alpha}_a$ on the lowest score groups could even reverse sign—not because lower-scoring graduates have different preferences, but because their choice is limited to the slots remaining after higher-scoring candidates choose specialties.

Results. Table 5 reports OLS, reduced form, and 2SLS estimates of equation (9)'s α_a parameters. The estimates for high-scoring physicians suggest that Medicare reimbursement has a direct effect on specialty choices among graduates who are likely unconstrained in their choices. Indeed, our full set of estimates (Appendix Table E.11) suggests that high-ability physicians prefer the amenities offered by family medicine and various procedural specialties appear to have relative disamenities.[53] Yet higher government reimbursements reallocate higher-ability physicians away from family medicine to procedural specialties.

Moving across the columns, we see that $\hat{\alpha}_a$ for students with lower USMLE scores become smaller and even turns negative for the lowest score bins. This is consistent with an equilibrium in which higher payments attract new physicians to a specialty, while the scarcity of residency slots screens out lower-scoring physicians to relatively lower-paying specialties.

interaction between M_{st} and each score bin, we use multiple instruments and first-stage regressions created by interacting P_{st} with score bin fixed effects. As all of these first-stage regressions are analogous, we report one of them in Appendix Table E.11. The first-stage coefficients are close to Medicare's true payment rate per RVU.

[53]The specialty choice elasticities to earnings implied by our results are in a similar range to earlier studies that account for rationed entry into the highest-paid specialties (Nicholson, 2002; Bhattacharya, 2005). Consistent with studies that do not account for entry barriers and find much lower elasticities (Nicholson and Propper, 2011), we get much lower elasticity estimates as we move down the USMLE score distribution where MD graduates likely have less choice. Full elasticity matrices for each regression specification are reported in Tables E.12, E.13, and E.14.

4 Can Government Shape Earnings Variation?

We have documented dramatic variation in physician earnings by specialty and location, and that insurance policies drive these earnings and physician labor supply. We now put these facts together to understand the role of government policy in driving the patterns from Section 2. We consider the impacts of reimbursement policy on the geographic pattern of earnings, how talent is distributed across specialties, and how insurance policy compares with the effects of tax policy.

4.1 Medicare's Contribution to Geographic Earnings Variation

We first connect government's influence on physician incomes to the unusual geographic pattern of physician earnings: higher-earning physicians being in lower-earning areas. We focus on reimbursement rates in Medicare—one of the main policy instruments—and use our estimates of how government policies shape physician incomes from Section 3. Medicare adjusts its rates for local input costs, but the adjustment is incomplete, resulting in effective subsidies to rural areas (GAO, 2022).[54] We create a measure of how incomplete the adjustment is in each CZ. For this, we compute the difference (in logs) between the Medicare Geographic Adjustment Factor (GAF) for physician care—a factor that multiplies Medicare reimbursement rates—and the local price index computed by Diamond and Moretti (2021) which we take as a more accurate measure of differences in costs across space.[55] We call this difference the "adjusted local Medicare reimbursement." As we would expect if Medicare effectively subsidizes rural areas, this difference is strongly (negatively) related to local average earnings (Figure E.12). We then relate the adjusted local Medicare reimbursement to the CZ fixed effects for physician earnings estimated in Section 2.3.

[54]Other government policies specifically intended to subsidize rural healthcare include Critical Access Hospitals, Rural Health Clinics, and provider subsidies in Health Professional Shortage Areas such as the National Health Service Corps. These programs include features such as payments for providers in rural areas and student loan forgiveness for physicians (and other healthcare workers) who commit to work in medically underserved areas.

[55]We thank Rebecca Diamond for sharing these data.

Figure 8 shows the relationship in a binned scatterplot, along with the corresponding regression line. We see a sharp positive relationship, with an elasticity of 0.723, or about thrice the causal estimate of 0.236 from Section 3.1. The causal estimate implies that 10% higher Medicare payments increase earnings by 2.4%. In the cross-section, physicians in areas with a 10% higher adjusted local Medicare reimbursement earn 7.2% more. This suggests Medicare's premium for physicians in low-income areas can explain a sizable share, though not all, of the unusual geographic pattern of physician earnings.

4.2 Allocation of Talent Across Specialties

We use the specialty choice model to illuminate the policy debate surrounding the "shortage" of primary care physicians and the role of entry restrictions in physician labor markets (Glied et al., 2009). Policy discussions often consider increasing primary care physicians' incomes, either through reimbursements, bonuses, or loan forgiveness. Our specialty choice estimates from Section 3.3 allow us to compute how physicians' specialty choices would respond to a change in Medicare reimbursement. Given the importance of physician test scores in these decisions, and thus in our model, we focus on how policy would affect the distribution of physicians (by test score) in each specialty.[56]

Specifically, consider an increase in internal medicine hourly Medicare reimbursements to dermatologists' level, i.e. a 2.3-fold increase in internists' hourly RVU production. Figure 9 shows the counterfactual distribution of internists' test scores in this regime.[57] The share of new internists with USMLE scores above 250 increases by 12.5 percentage points, displacing some lower-scoring entrants. The average USMLE score in internal medicine increases by 8

[56]We use our reduced form model specification here, as public insurance reimbursement levels are central to discussion of primary care policy. We present analogous estimates for a counterfactual that directly changes hourly incomes rather than reimbursements across specialties in Appendix Figure E.13. We return to the income-based counterfactual in Section 4.3 where we compare the reimbursement instrument to tax policy.

[57]We compute the counterfactual specialty shares within each score group by changing the level of the reimbursement P_{st} in the utility function (equation 8). As our model has no unobserved heterogeneity in preferences, all share functions are standard closed-form logit choice probabilities. We use the resulting counterfactual shares of specialty within each score group to recompute the share of each score group within a specialty.

points, or 0.46 standard deviations.

Our results imply that increasing internists' reimbursements to the level of highly-paid specialists makes internal medicine more attractive. But, given entry constraints, this change attracts *more talented* rather than *more* physicians to internal medicine. Higher-scoring physicians reallocate to the specialty that becomes financially more attractive and away from other specialties. The take-away is that under the existing structure of physician labor markets, government policy plays the central role in the allocation of talent across specialties.

We frame this counterfactual as an increase in primary care physicians' incomes. Since the model considers only *relative* incomes, the results would be the same if we instead reduced specialists' earnings. This distinction—*i.e.* the absolute level of earnings—may affect the choice to enter medicine in the first place. While our analysis abstracts from this decision, Appendix D uses our data to speculate on implications for this extensive occupational choice margin.

4.3 Magnitude: Is Health Policy More Powerful than Tax Changes?

Another way to interpret the magnitude of our estimates is to compare the power of health care policy to affect top incomes with that of tax policy—the domain that commands most policy attention in discussions of top income inequality.

While tax rate changes can affect the full income distribution (Scheuer and Slemrod, 2019) most estimates of the elasticity of taxable income rely on partial equilibrium approaches. This is appropriate for our setting, since we obtain partial equilibrium estimates, as our empirical strategies use comparisons across physicians, specialties, or locations. We treat the supply response of physician care from Table 3 (measured in RVUs) as analogous to an elasticity of taxable income, denoted ϵ.[58] Given a starting tax rate τ_0, we can then find the tax rate τ_1 that would generate any specific increase of Δy in log physician earnings using

[58]Our estimated supply elasticity of 0.44 is similar to to the Gruber and Saez (2002) estimate of $\epsilon = 0.57$.

the formula:[59]

$$\tau_1 = 1 - \exp\left(\frac{\Delta y}{\epsilon} + \ln(1-\tau_0)\right). \tag{10}$$

Table 6 shows the tax changes that would be needed to generate income changes of the same magnitude as the changes induced by the policies we study. The tax changes needed would be dramatic. To move top incomes by 5%, about as much as the ACA expansion changed physicians' earnings, would require tax changes larger than those generated by the Tax Cut and Jobs Act of 2017, which lowered the top federal income tax rate from 39.6 to 37 percent; by the ACA, which increased the Medicare payroll tax on high earners by 0.9 percentage points; by the American Taxpayer Relief Act of 2012, which increased the top rate for high earning households from 35 to 39.6 percent; and by the Economic Growth and Tax Relief Reconciliation Act of 2001, which lowered the top rate from 39.6 to 35 percent. (See Panel A.) While the ranges of tax rates in most of the table are well beyond the available empirical evidence, this only strengthens our point: payment policy has a dramatic effect on physicians' top earnings relative to marginal tax rates.

The longer-run considerations are even more profound. If physicians exhibit Rosen (1981)-style superstar effects, then Scheuer and Werning (2017) show that the relevant elasticity of taxable income increases.[60] Intuitively, if more productive workers sort to more productive firms, sorting and effort terms compound to increase the tax-policy-relevant elasticity. To account for this possibility, Table 6 Panel B includes calculations with a higher elasticity of $\epsilon = 1$.

Over the longer run, taxes could also affect specialty choice just like the reimbursement changes estimated above. The logic of Rothschild and Scheuer (2016) implies that tax policy could have a role to play in correcting talent allocation externalities when some specialties

[59] Equation (10) follows immediately from the definition of the elasticity of taxable income by solving for τ_1.

[60] Gottlieb et al. (2023a) emphasize a different mechanism for superstar physicians, based on matching with consumers rather than firms. In this framework, the Scheuer and Werning (2017) logic would go through if physician effort enables them to treat higher-income patients, rather than a larger number of patients.

have a higher social return relative to earnings than others. They take regulatory constraints as given and argue that, subject to those constraints, externality-correcting taxes should be adjusted to account for imperfect targeting of rent-seeking.

To account for this potential role of taxes in reallocating talent, the remainder of Table 6 consider larger income gaps: those between internal medicine and average specialist income (Panel C), or between dermatology and average specialists (Panel D). If physicians respond to net-of-tax earnings, reducing internal medicine's tax rate to 22% would generate a similar talent reallocation as increasing internists' incomes to the specialist average. Alternatively, tax rates of 60% for dermatologists, compared with 37% for everyone else, would imply a similar talent reallocation as reducing dermatology income to that of the average specialist. The existing progressive income tax schedule already operates in this manner to some extent, but the scale of tax rate differences needed to counteract earnings differences across specialties illustrates the power of government reimbursement policies for talent allocation.[61]

5 Conclusion

This paper uses a new administrative data linkage to describe and understand U.S. physicians' earnings. Physicians are the most common occupation in the top percentile of the income distribution and are at the core of the $4 trillion healthcare economy—half of which is government-financed. We find that physicians earn $350,000 on average, and 8.6% of U.S. healthcare spending in aggregate. The age-earnings profile is steep, reflecting the extensive human capital investments required to enter a career in medicine. Earnings vary widely across specialties and geographic areas; we show that regulations are key drivers of these differences and thus top income inequality. The combination of government payment rules and binding entry restrictions profoundly impact earnings and thus play a key role in

[61] These exercises use differences in income rather than reimbursement rates across specialties; see footnote 56. The $\epsilon = 1$ case is more natural here, following the Scheuer and Werning (2017) argument about accounting for talent reallocation. With a lower elasticity, the range of tax differences necessary to achieve similar differences would be even larger. This highlights the importance of determining the appropriate elasticity when analyzing extensive-margin behavioral responses.

valuing and allocating one of society's most expensive assets: physicians' human capital.

Our results teach how policy drives the most consequential long-run outcomes in this labor market and provide a clear agenda for future research. To analyze the long-run welfare impacts of healthcare policies, including those we investigate, we need evidence on the distribution of health impacts and thus social returns to physician ability in different specialties. We do not speculate on the magnitude of such returns in this paper, but our results show that quantifying the health impacts of ability is an essential direction for future work and is key to formulating payment policies.

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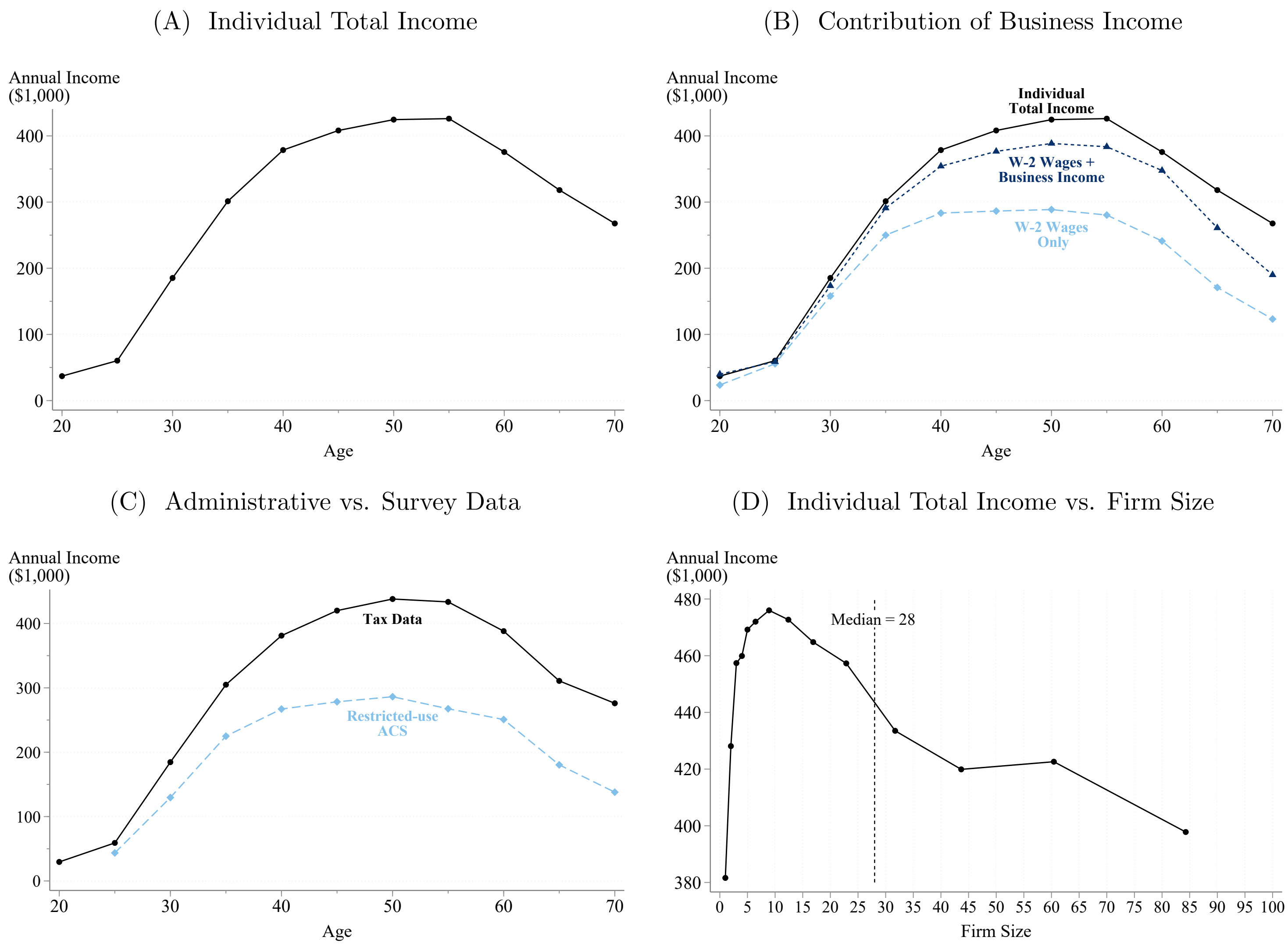
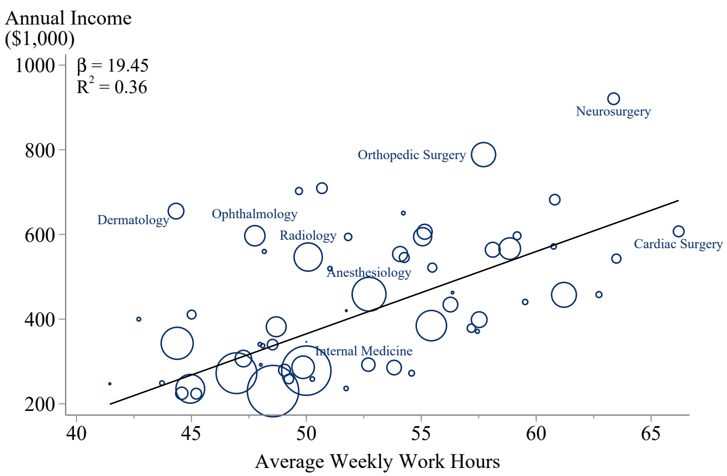
Figure 1: **Physician Earnings over the Lifecycle and by Firm Size**



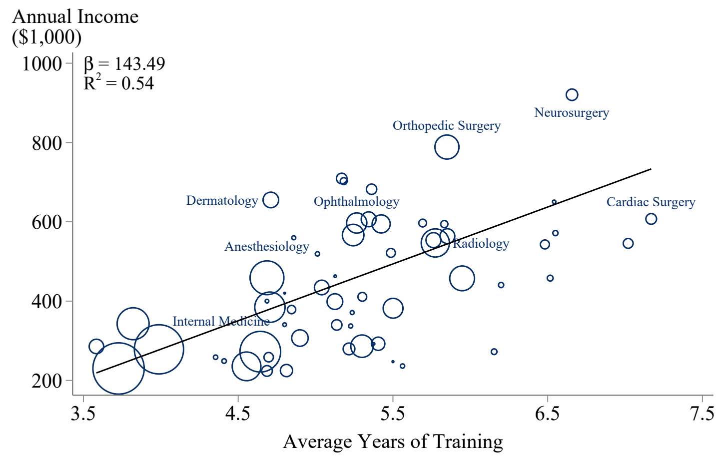
Notes: The figure plots mean individual total income in our 2017 sample of physicians by 5-year age intervals (Panels A–C) and by firm size (Panel D). Business income in Panel B is defined as the household's Total Money Income net of wages, taxable dividends, taxable interest, Social Security, partially observed profit and loss from Schedule E, and distributions from pre-tax deferral accounts. ACS total individual income in Panel C is defined as the sum of individual wage and self-employment income of the index individual plus self-employment income of the spouse. Panel D is restricted to physicians age 40 to 55 and firms with fewer than 100 physicians; the horizontal axis shows ventiles of the physician-level distribution of firm size. The term "firm" refers to the tax unit, measured as the EIN on Form W-2. Appendix B.2 provides measurement details. Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

Figure 2: **Correlates of Specialty Income**

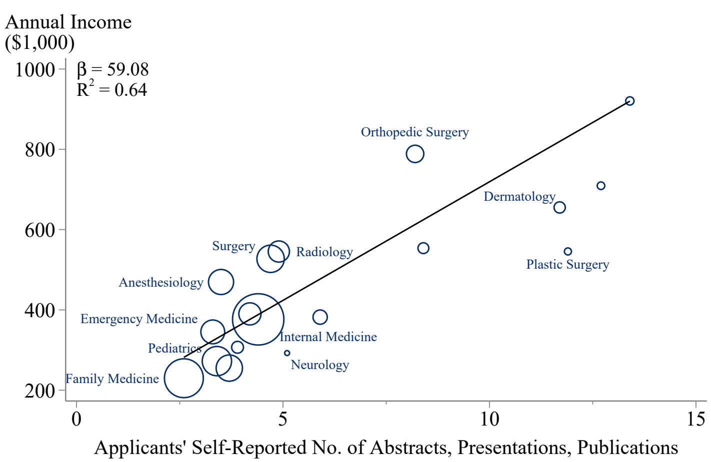
(A) Income vs. Hours of Work



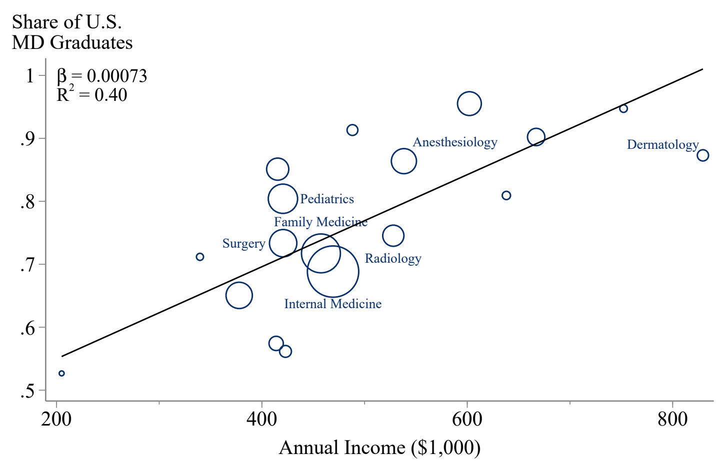
(B) Income vs. Length of Training



(C) Income vs. Applicants' Research Experience



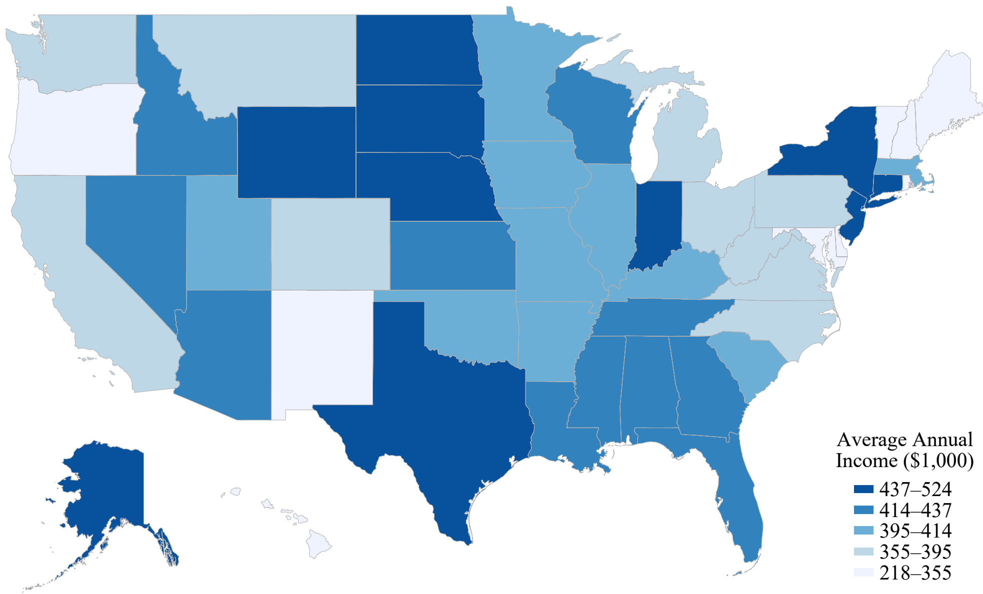
(D) U.S. Degree vs. Income | Hours, Training



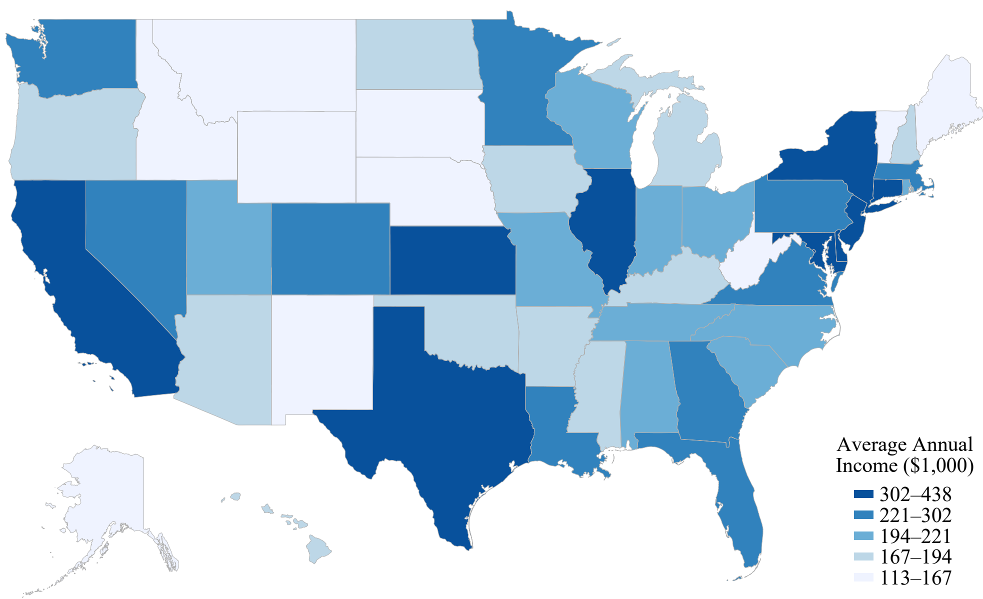
Notes: This figure shows relationships between specialty earnings and specialty characteristics. Specialty earnings are measured as mean individual total income among 40-to-55-year-old physicians in our full panel 2005–2017 in a Medicare Specialty (Panels A and B) or NRMP specialty (Panels C and D). We plot specialty earnings against the average number of hours worked among physicians aged 40 to 55 in 2005–2017 (Panel A), the average imputed years of training (Panel B), and the average number of abstracts, presentations, and publications that MD students report having completed on their residency application, as provided by NRMP (Panel C). Panel D plots the specialty's share of physicians with a U.S. degree against average earnings, conditional on the number of work hours and years of training. Years of training is imputed from tax data as described in Appendix B.2. Circle sizes in the graphs are proportional to the number of individuals in each specialty in our baseline sample in 2017. The line of best fit is estimated as a weighted bivariate OLS on specialty-level data. Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

Figure 3: **Geographic Variation in Earnings**

(A) Physicians

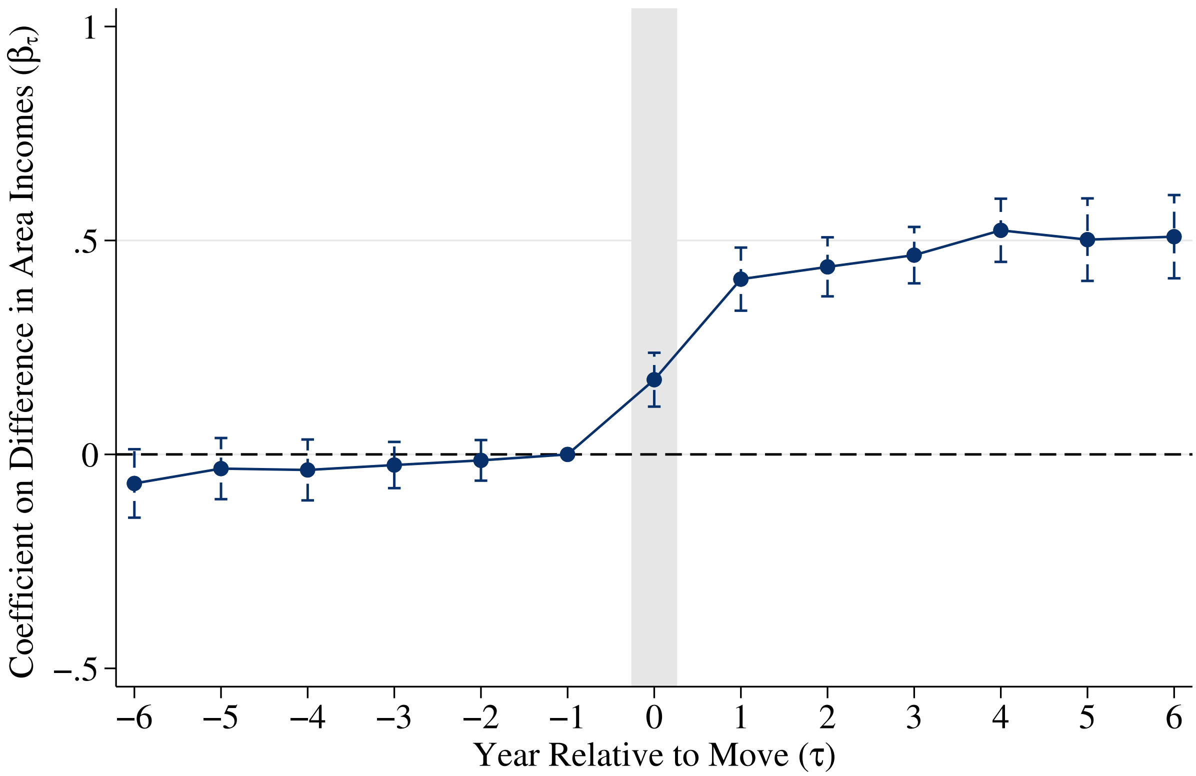


(B) Lawyers



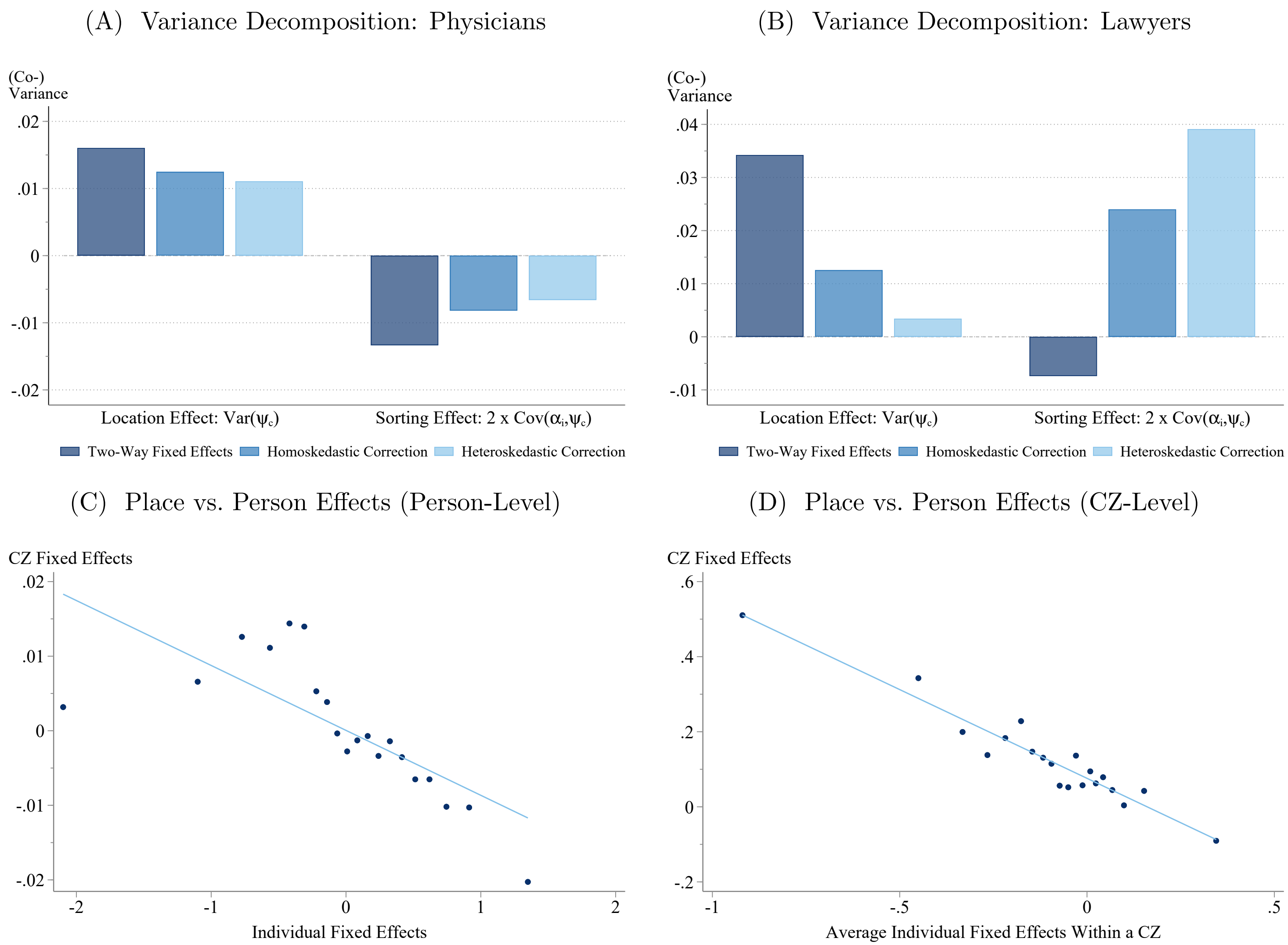
Notes: This figure plots mean individual total income among 40 to 55 year old physicians (Panel A) and lawyers (Panel B) in 2017 by state. Income is measured using individual tax return data and is defined as the sum of individual total wage income and the household AGI net of all wage earnings and taxable retirement distributions (for those aged 60 or older), but gross of tax-exempt interest and Social Security payments. Physicians and lawyers are defined as described in Section 1 and Appendix B.1. Appendix B.2 provides more details on income measurement. Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

Figure 4: **Event Study: Physician Movers**



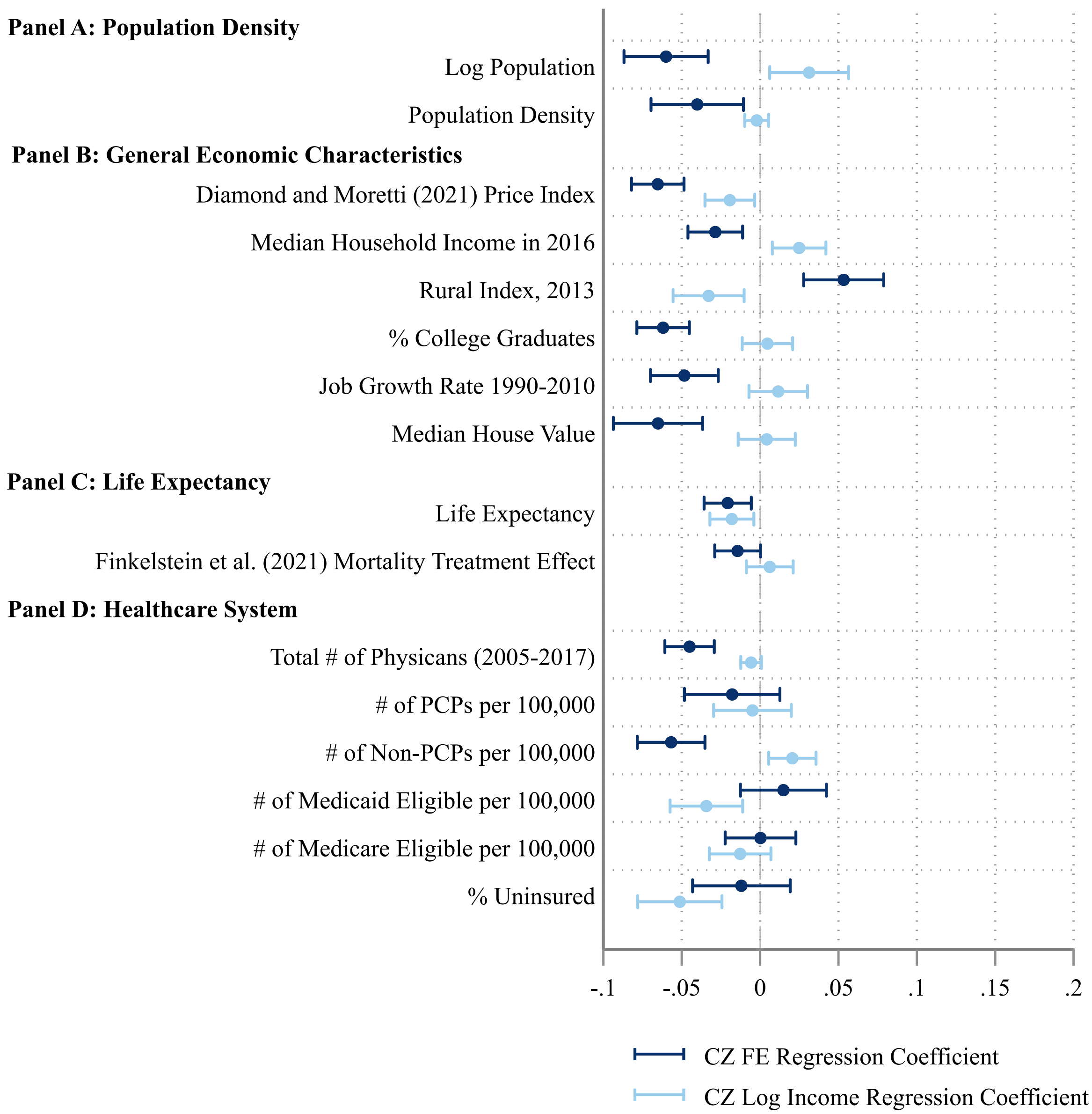
Notes: This figure shows coefficient estimates on the difference between mean individual total income between origin and destination commuting zones ($\Delta\overline{\ln y}_{(j,j')}$) from equation (1). The coefficient is normalized to 0 in the year prior to the move ($\tau = -1$). The dashed lines indicate 95% confidence intervals. The outcome is log individual total income. The independent variables include $\Delta\overline{\ln y}_{(j,j')}$ interacted with relative year fixed effects, physician fixed effects, and age fixed effects. A physician is defined as a mover and is included in the sample if they changed their commuting zone once between years 2005 to 2017, and were aged 40 to 55 during that change. Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024, CBDRB-FY24-0456.

Figure 5: **Place vs. Physician Contributions to Earnings**



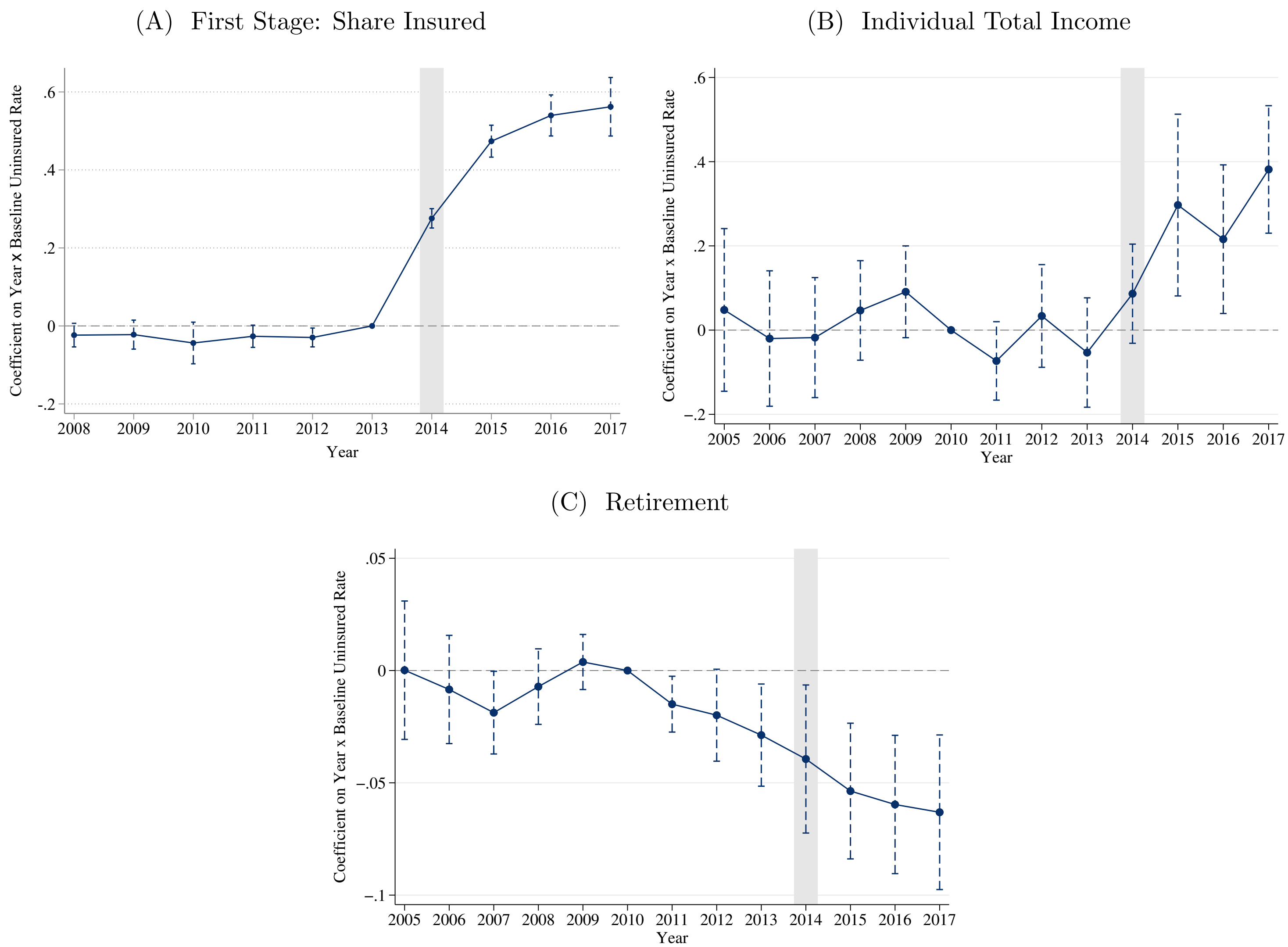
Notes: This figure shows elements of a variance decomposition of individual total income among 40–55-year-old physicians (Panel A) and lawyers (Panel B) in the sample of movers (see definition in Figure 4.) Estimates in bars labeled "Two-Way Fixed Effects" are based on equation (2). The outcome is log individual total income. The importance of location effects is computed as the variance of estimated CZ fixed effects, $\text{Var}(\psi_c)$. The effect of sorting of people to locations, $2\text{Cov}(\alpha_i, \psi_c)$, is computed as twice the covariance of individual and CZ fixed effect estimates. The bars labeled homoskedastic and heteroskedastic correction report the corrected variance and covariance terms based on Andrews et al. (2008) and Kline et al. (2020), respectively, implemented following Bonhomme et al. (2023). Panels C and D show binned scatterplots relating place effects and person effects based on estimation of equation (2) in the sample of movers. Panel C reports the average CZ fixed effect within each ventile of individual fixed effects distribution. In Panel D we first collapse the data to the CZ level by averaging individual fixed effects within a CZ as in Card et al. (2021). The line of best fit is based on a bivariate OLS regression using underlying data points. Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

Figure 6: **Correlates of Place Effects**



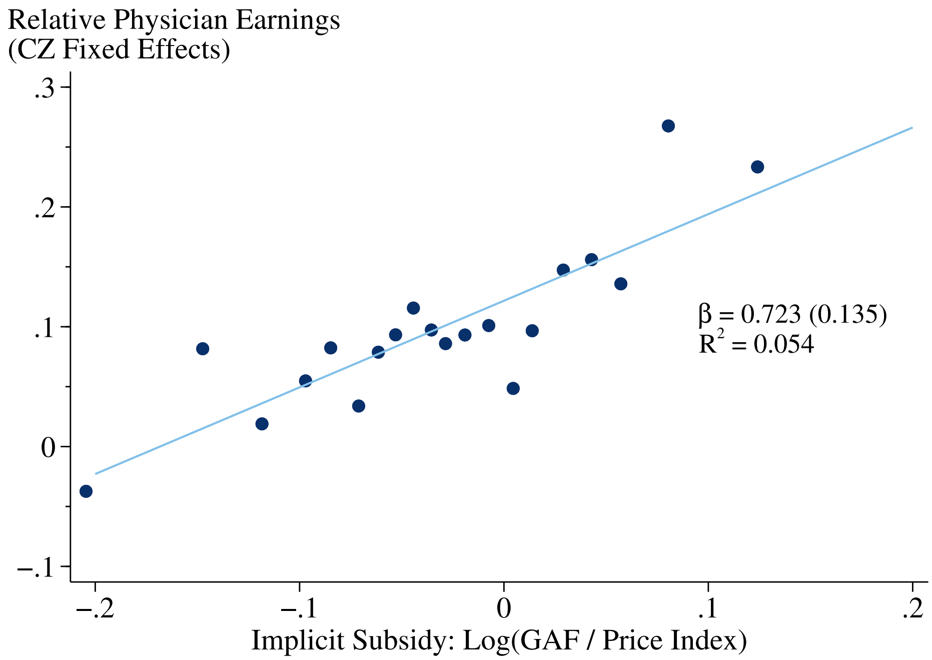
Notes: This figure plots the results of bivariate OLS regressions of raw average individual total income in a commuting zone (light blue colors), as well as of place treatment effect on earnings (dark blue), on z-scores of the indicated place characteristics. Place treatment effects on earnings are CZ fixed effects from estimating equation (2) in the sample of movers (see definition in Figure 4). Raw mean income is computed in the same sample. CZ-level characteristics are as reported in Chetty et al. (2014), Finkelstein et al. (2021), and Diamond and Moretti (2021). Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

Figure 7: **Effect of ACA Insurance Expansion**



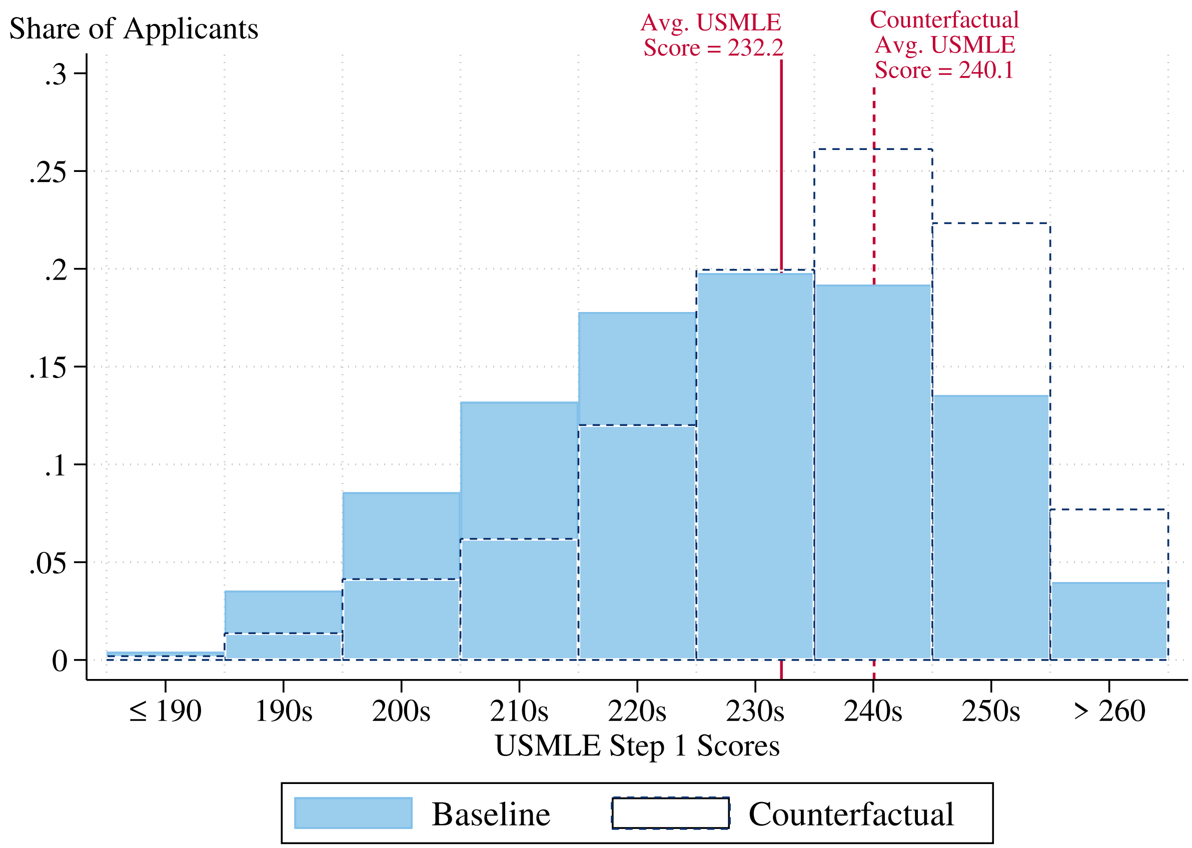
Notes: This figure shows event study estimates of the effects of ACA insurance expansions on insurance rates (Panel A), log individual total income of physicians aged 40–55 (Panel B), and the probability of retirement (defined as receiving Form 1099-SSA) among 44–70-year-old physicians (Panel C). Independent variables include county fixed effects, age fixed effects, year fixed effects, and year fixed effects interacted with the share of under 65 population that was uninsured in 2013. The sample includes counties in states that had ACA expansions in 2014 and 2015 as detailed in Appendix C.2. The regression specification is in equation (7). Error bars represent 95% confidence intervals, with standard errors clustered at the county level. Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024, CBDRB-FY24-0456.

Figure 8: **Location Effects vs. Implicit Subsidies**



Notes: This figure plots the relationship between the causal geographic component of physician earnings—our CZ fixed effects estimated in Section 2.3—and an implicit geographic subsidy for physician services. The subsidy is calculated as the difference (in logs) between local input costs, measured using a local price index from Diamond and Moretti (2021), and the degree to which Medicare adjusts for those costs, measured using the Medicare Geographic Adjustment Factor (GAF) for physician care. The GAF is a factor that multiplies Medicare reimbursement rates; when this adjustment overestimates local production costs, rural areas are effectively subsidized (GAO, 2022). The figure is a binned scatterplot, where R^2 and the line of best fit are from a bivariate OLS regression on the underlying data points. Disclosure Review Board approval CBDRB-FY24-0456.

Figure 9: **Increase Internal Medicine Medicare Payments to Dermatology Level**



Notes: This figure reports the results of a counterfactual in which we set the mean hourly RVUs in internal medicine to equal the mean hourly RVUs currently observed in dermatology. Mean hourly RVUs, P_{st}, is constructed by aggregating P_{it} (see Appendix C.1) up to the specialty level. Counterfactual choices are predicted using the estimates of the specialty choice model in equation (9). We first compute predicted choices within each USMLE score group and then re-normalize the data to plot the share of each USMLE score group within one specialty—internal medicine. Disclosure Review Board Approval CBDRB-FY24-0456.

Table 1: **Summary Statistics**

	Years:	2005-2017	2017		
		(1)	(2)	(3)	(4)
	Ages:	All	All	40 to 55	56 to 70
Number of Person-Years		11,600,000	848,000	350,000	287,000
Number of Unique Individuals		965,000	848,000	350,000	287,000
Demographics					
Age	Mean	45.3	49.3	47.3	62.6
	Median	45.0	49.0	47.0	62.0
	Std. Dev.	12.7	11.6	4.6	4.2
Female		0.36	0.38	0.40	0.26
Non-U.S.-Born		0.19	0.22	0.27	0.12
Married		0.77	0.80	0.83	0.82
Share Observed in ACS		0.20	0.20	0.20	0.22
Income					
Individual Total Wage (2017 $)	Mean	201,600	243,400	286,200	224,900
	Median	155,700	209,400	247,700	188,700
	Std. Dev.	945,300	283,000	260,300	359,400
Individual Total Income (2017 $)	Mean	290,800	350,000	404,500	367,500
	Median	210,700	265,000	308,600	267,500
	Std. Dev.	3,589,000	1,192,000	711,400	1,578,000
AGI (2017 $)	Mean	359,200	429,500	502,400	435,100
	Median	264,700	325,500	384,300	314,800
	Std. Dev.	859,200	1,266,000	827,500	1,652,000
Business Income > $25K		0.29	0.32	0.35	0.38
Households in Top 1% of AGI		0.22	0.24	0.31	0.24
Career Choices and Characteristics					
Firm Size (Number of Physicians)	Mean	1,101	1,472	1,536	1,480
	Median	52.0	84.6	75.0	20.0
	Std. Dev.	3,677	4,855	5,025	5,312
Weekly Working Hours (ACS)	Mean	50.5	49.5	49.5	47.6
	Median	50.0	50.0	50.0	50.0
	Std. Dev.	16.4	15.2	14.5	15.4
Retired (Based on 1099-SSA)		0.05	0.07	0.01	0.19
Name of Medical School Observed		0.52	0.53	0.56	0.55
Graduated from Ranked Medical School		0.48	0.48	0.48	0.50
Graduated from Top-5 Medical School		0.06	0.06	0.06	0.06
Share in Specialty Category					
Primary Care		0.43	0.44	0.42	0.43
Medicine Subspecialty		0.12	0.12	0.13	0.13
Hospital-Based		0.11	0.11	0.11	0.09
Surgery		0.09	0.09	0.09	0.09
Procedural Specialties		0.06	0.06	0.06	0.06
Anesthesiology		0.06	0.06	0.06	0.06
Radiology		0.05	0.05	0.05	0.05
OB-GYN		0.05	0.05	0.05	0.05
Neurology		0.03	0.03	0.03	0.03

Notes: This table reports summary statistics for the main samples used in our analysis. Column (1) includes years 2005–2017 and physicians aged 20 to 70. Columns (2) to (4) report summary statistics for the 2017 cross-section, overall, and by age subgroups. The sample in column (1) is constructed by merging the 2017 vintage of the National Plan and Provider Enumeration System (NPPES) file that includes National Provider Identifiers of all physicians in the U.S. with the universe of individual income tax return data. Section 1 and Appendix B.2 provide details on data sources and measurement of each variable. Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

Table 2: **Characteristics of Top Earning Physicians**

		Top X% of Physicians by Income					
		1%	5%	10%	25%	50%	All
Number of Unique Individuals		3,500	17,500	35,000	87,500	175,000	350,000
Income and Labor Supply							
Individual Total Income ($1,000)	Mean	4,051	1,817	1,319	871	626	405
	Median	2,739	1,280	960	652	473	309
	Cutoff	1,937	960	719	473	309	-
Wage Income ($1,000)	Mean	897	737	654	524	417	286
AGI ($1,000)	Mean	4,465	1,993	1,448	964	708	502
Business Income ($1,000)	Mean	1,313	588	394	228	147	87
	Share > $25K	0.80	0.72	0.65	0.53	0.44	0.35
Median Share of Income from Business		0.28	0.20	0.13	0.05	0.02	0.00
Median Share of Income from Non-Labor		0.85	0.51	0.31	0.14	0.08	0.06
Median Share of Income from Labor		0.15	0.49	0.69	0.86	0.92	0.94
Mean Weekly Hours Worked		48	54	54	54	53	50
Retired (Based on 1099-SSA)		0.002	0.001	0.001	0.001	0.001	0.004
Mean Firm Size		354	449	493	699	1,091	1,536
Specialties and MD Training							
Graduated from Top-5 MD Program		0.10	0.08	0.07	0.07	0.06	0.06
Cardiology Share		0.02	0.04	0.05	0.05	0.03	0.02
Neurosurgery Share		0.06	0.05	0.04	0.02	0.01	0.01
General Surgery Share		0.03	0.03	0.04	0.05	0.05	0.03
Primary Care Share		0.16	0.12	0.12	0.15	0.24	0.42
Family Practice Share		0.03	0.03	0.03	0.04	0.07	0.14
Demographics							
Mean Age		48	48	48	48	48	47
Female		0.24	0.18	0.18	0.20	0.27	0.40
Non-U.S.-Born		0.22	0.23	0.23	0.23	0.25	0.27
Married		0.92	0.91	0.91	0.89	0.87	0.83
Share in New York and New Jersey		0.20	0.14	0.12	0.10	0.10	0.10
Share in California		0.11	0.09	0.09	0.10	0.12	0.12
Share in Florida		0.10	0.08	0.08	0.07	0.06	0.07
Share in Texas		0.11	0.11	0.10	0.09	0.08	0.08
Share in Arizona		0.03	0.03	0.02	0.02	0.02	0.02

Notes: This table reports selected summary statistics for the sample of age 40–55 physicians in 2017 (sample in column (3) of Table 1), by selected percentiles of the individual total income distribution (as specified in column titles). Variables are as defined in Table 1. Section 1 and Appendix B.2 provide more details on data sources and measurement of each variable. Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

Table 3: **RVU Regression Table**

	NPI-Level			Procedure-level		2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:	Log Income	Log Total RVUs Billed	Log Number of Unique Procedures	Log Number of Unique Patients	Log Total RVUs Billed	Log Income	Retired
Panel A: Physicians Age 40-55							
Log Medicare Price Instrument ($\ln P_{i,t}$)	0.236	1.437	0.395				
	(0.035)	(0.109)	(0.039)				
Log RVUs per Procedure ($\ln RVU_{k,t}$)				0.344	1.382		
				(0.050)	(0.057)		
Log Total RVUs Billed ($\ln Q_{i,t}$)						0.167	
						(0.028)	
Log Income							-0.001
							(0.003)
Mean of Dependent Variable (2010-13)	13.13	8.75	2.99	3.88	4.82	13.13	0.00
Std. Dev. of Dependent Variable (2010-13)	0.84	1.13	0.79	1.12	1.70	0.84	0.06
Mean of Independent Variable	8.94	8.95	8.94	0.63	0.63	8.75	13.13
Std. Dev. of Independent Variable	1.02	1.00	1.02	1.18	1.18	1.12	0.84
Number of Observations	1,357,000	1,354,000	1,373,000	16,900,000	16,900,000	1,338,000	1,357,000
Panel B: Physicians Age 56-70							
Log Medicare Price Instrument ($\ln P_{i,t}$)	0.340	1.402	0.370				
	(0.042)	(0.107)	(0.044)				
Log RVUs per Procedure ($\ln RVU_{k,t}$)				0.394	1.441		
				(0.058)	(0.070)		
Log Total RVUs Billed ($\ln Q_{i,t}$)						0.246	
						(0.023)	
Log Income							-0.054
							(0.028)
Mean of Dependent Variable (2010-13)	13.02	8.68	2.96	3.90	4.78	13.02	0.10
Std. Dev. of Dependent Variable (2010-13)	0.88	1.07	0.78	1.13	1.69	0.88	0.30
Mean of Independent Variable	8.86	8.87	8.86	0.54	0.54	8.68	13.02
Std. Dev. of Independent Variable	0.96	0.94	0.96	1.13	1.13	1.06	0.88
Number of Observations	897,000	907,000	920,000	10,800,000	10,800,000	884,000	897,000

Notes: This table reports coefficients and standard errors from estimating equation (5) for each outcome variable as indicated in column names, and each age group, as indicated in panel names. Independent variables are the log Relative Value Units (RVU) rate, age fixed effects, and Medicare specialty-by-year fixed effects. For physician-level regressions, the log Medicare price ($\ln P_{i,j}$) faced by the physician is computed as a weighted average of procedure-level RVU rates for a fixed vector of services. 2SLS specifications regress the outcome variable of interest on the log total number of RVUs billed (RVU rate for each service multiplied by the number of times a service is performed), instrumented by $\ln P_{i,j}$. This is defined in equation (4), with the fixed vector of services defined as the average number of times each service (a combination of HCPCS procedure code and facility or non-facility place of service) was performed by a physician between years 2012 and 2017. Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

Table 4: **ACA Regression Table**

	First Stage	Reduced Form			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:	Share Insured	Log Income	Share with Schedule SE	Share Retired	Log Income	Share with Schedule SE	Share Retired
Share Uninsured in 2013 ($U_{c,2013}$)							
× Years 2010 – 2013	0.013	-0.056	0.078	-0.016			
	(0.011)	(0.048)	(0.037)	(0.014)			
× Year $\geq$ 2014	0.497	0.221	0.194	-0.049			
	(0.039)	(0.066)	(0.044)	(0.020)			
Share Insured ($I_{c,t}$)					0.495	0.322	-0.085
					(0.114)	(0.068)	(0.024)
Mean of Dependent Variable	0.851	12.470	0.469	0.088	12.470	0.456	0.093
Std. Dev. of Dependent Variable	0.047	0.924	0.499	0.283	0.911	0.498	0.290
Mean of Independent Variable	0.147	0.147	0.147	0.148	0.851	0.851	0.849
Std. Dev. of Independent Variable	0.044	0.044	0.044	0.044	0.047	0.047	0.047
Number of Observations	1,777,000	2,250,000	2,200,000	3,241,000	1,742,000	1,702,000	2,592,000
Physician Age Range	40-55	40-55	40-55	44-70	40-55	40-55	44-70

Notes: The table displays parametric difference-in-differences estimates of the effects of the ACA insurance expansions on the outcomes indicated in column names. The regression specification is as in equation (7), except that we collapse the time dimension into three periods: before the ACA passage (2010 and earlier); post-ACA passage and pre-implementation period (2011-2013); and post-implementation period (2014-2017). Age range restrictions are specified in the last row of the table. Independent variables include the three time intervals interacted with the fraction of population that was uninsured in a county in 2013, as well as county, age, and calendar year fixed effects. Standard errors are clustered at the county level. The sample is restricted to physicians who resided in states that expanded Medicaid in 2014 or 2015. Appendix C.2 provides the full list of states. Column (1) reports the first stage, where the outcome variable is the share of individuals under 65 who are insured in a county. Columns (2) to (4) report reduced-form estimates. Columns (5) to (7) report the results of corresponding 2SLS specifications that treat the rate of insurance in the under-65 population as the endogenous variable of interest and the rate of uninsured population in 2013 as an instrument. The 2SLS specification treats all years pre-implementation as the pre-period. Table E.9 reports the same specifications, but dropping the post-ACA passage and pre-implementation years (2011-2013). Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024, CBDRB-FY24-0456.

Table 5: **Specialty Choice Model**

Ability Group (a) :	USMLE Step 1 Scores								
	> 260	251-260	241-250	231-240	221-230	211-220	201-210	191-200	≤ 190
Panel A: Reduced Form									
Coefficient on Hourly RVUs	0.516	0.444	0.361	0.250	0.117	0.004	0.012	-0.046	Reference
$\times$ Ability Group Dummy (α_a)	(0.052)	(0.046)	(0.046)	(0.042)	(0.047)	(0.049)	(0.046)	(0.050)	-
Total Marginal Effect for	0.287	0.214	0.132	0.020	-0.113	-0.226	-0.218	-0.275	-0.229
Ability Group a ($\alpha_{\text{base}} + \alpha_a$)	(0.062)	(0.055)	(0.052)	(0.050)	(0.058)	(0.055)	(0.054)	(0.056)	(0.060)
Panel B: OLS									
Coefficient on Hourly Income	0.024	0.021	0.018	0.014	0.009	0.004	0.003	-0.001	Reference
$\times$ Ability Group Dummy (α_a)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	-
Total Marginal Effect for	0.016	0.013	0.010	0.006	0.001	-0.003	-0.005	-0.008	-0.007
Ability Group a ($\alpha_{\text{base}} + \alpha_a$)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Panel C: 2SLS									
Coefficient on Hourly Income	0.020	0.017	0.014	0.010	0.005	0.000	0.000	-0.002	Reference
$\times$ Ability Group Dummy (α_a)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	-
Total Marginal Effect for	0.008	0.005	0.002	-0.002	-0.007	-0.012	-0.011	-0.014	-0.012
Ability Group a ($\alpha_{\text{base}} + \alpha_a$)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)

Notes: This table reports selected coefficients from estimating the discrete choice model specified in Section 3.3, equation (9). Panel A reports the reduced form estimates that use mean hourly RVUs in specialty s in year t, denoted P_{st}, and its interactions as the main independent variables. Panel B reports OLS estimates that use mean hourly income in specialty s in year t, M_{st}, and its interactions as the main independent variables. In Panel C we report the results of a 2SLS specification in which P_{st} instruments for M_{st}. The coefficients reported in different columns are estimates from one pooled regression, in which the main effect of mean hourly RVUs or mean hourly income is interacted with dummies for USMLE score groups. We report both the estimated interactions, as well as the full marginal effect for each score group. Standard errors on the marginal effects are calculated using the delta method. See Section 3.3 for more discussion of the interpretation. Table E.11 reports the full set of estimates for these specifications, including the first stage for Panel C. Tables E.12—E.14 report own and cross-income elasticities of specialty choice probability computed based on the three model specifications. Disclosure Review Board approval CBDRB-FY24-0456.

Table 6: **Predicted Effect of Tax Changes on Income**

Elasticity	Income Growth	Original Tax Rate	New Tax Rate
ϵ	Δy	τ_0	τ_1
Panel A: Baseline Empirical Elasticity			
0.44	0.05	37%	29%
0.44	-0.05	37%	44%
Panel B: Augmented Elasticity per Scheuer and Werning (2017)			
1.00	0.05	37%	34%
1.00	-0.05	37%	40%
Panel C: Internal Medicine vs. Average Specialist			
1.00	0.22	37%	22%
1.00	-0.22	37%	49%
Panel D: Average Specialist vs. Dermatology			
1.00	0.45	37%	1%
1.00	-0.45	37%	60%

Notes: The table uses equation (10) to calculate the top income tax rates needed to move average log earnings by different amounts (Δy). $\Delta y = 0.05$ is roughly the change in physician incomes caused by the ACA expansion (Table 3). $\Delta y = 0.22$ is the difference between internal medicine and average specialist income, and $\Delta y = 0.45$ is the difference between dermatology and average specialist income. The elasticity of $\epsilon = 0.44$ is obtained from Table 3, column (2), and is similar to elasticities of taxable income in the literature. The elasticity of $\epsilon = 1$ is included because more productive physicians sorting into more productive firms could increase the elasticity of taxable income (Scheuer and Werning, 2017).

Online Appendix to: The Earnings and Labor Supply of U.S. Physicians

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December 2024

A NORC AmeriSpeak Survey

Summary

The broader public is not well-informed about physicians' incomes, as we observed in a nationally-representative survey of 1,071 respondents conducted via both internet and telephone in June 2021.[62] Respondents had dispersed views about physician earnings: 19% believed that the average physician earned above $300,000; another 33% believed the number was $200,000 to $300,000, leaving nearly half of the respondents believing that physicians earned under $200,000. 36% considered physicians overpaid, while 50% said they were paid "the right amount" and 11% chose underpaid.

Respondents broadly understood that physicians have substantially higher earnings than nurses, with 8% stating that average nurse earnings exceed $125,000, 31% answering $75,000 to $125,000, and 40% reporting that nurses earn $50,000 to $75,000. The BLS reports average registered nurse earnings of $89,010,[63] though it is not clear whether respondents were thinking of registered nurses or also including other categories such as licensed practical nurses. In general, respondents tend to underreport pay for both types of health care workers, perhaps reflecting earnings growth in healthcare over time, leaving the general public with an outdated view of earnings in the sector.

Methodological Notes

Our survey questions were added to NORC's AmeriSpeak Omnibus survey, conducted monthly using a sampling frame that captures 97% of the U.S. population. The survey was conducted among adults 18 and over from all 50 states plus D.C. from June 10 to 14, 2021. 1,036 responded via the internet and 35 via telephone. Among other questions, we asked:

- "What do you think is the average annual income of people in each of the following jobs?"
  - Answers were reported for doctors, nurses, and other selected occupations.
  - The response grid included the following options: less than $25,000; $25,000 to $50,000; $50,001 to $75,000; $75,001 to $125,000; $125,001 to $200,000; $200,001 to $300,000; or more than $300,000.

[62] This survey was conducted jointly by the University of Chicago Harris School of Public Policy and The Associated Press-NORC Center for Public Affairs Research with funding from NORC at the University of Chicago.

[63] https://www.bls.gov/oes/current/oes291141.htm

  - For doctors, 3% of respondents skipped this question and 1% answered "don't know."
  - For nurses, 4% skipped and 1% did not know.

- "Thinking about the different types of health care professionals, would you say each of the following is overpaid, underpaid[64] or gets paid the right amount?"
  - Answers were reported for doctors, nurses, and other selected occupations.
  - The response grid included the following options: very overpaid, somewhat overpaid, the right amount, somewhat underpaid, very underpaid.
  - For doctors, 3% of respondents skipped this question and 1% answered "don't know."
  - For nurses, 3% skipped and 1% did not know.

This survey was deemed exempt by the NORC Institutional Review Board.

[64] The order of these two options was chosen randomly.

B Data and Measurement Appendix

B.1 Data Sources

Tax Data

Tax data available to us contain the universe of filers, but a limited number of variables. From Form 1040, we observe the tax unit's filing status, adjusted gross income (AGI), taxable dividend and interest amounts, social security income, as well as indicators for filing schedules C, S, and SE. In addition, we observe wage income on Form W-2 and receipt of Social Security benefits on Form 1099-SSA, which are both information returns filed by third parties.

We follow the Chetty et al. (2014) approach for harmonizing raw Form 1040, 1099-SSA, and W-2 data. In case of multiple W-2s from different employers, we add earnings across all W-2s and consider the EIN with the largest amount of earnings to be the primary EIN. We use address information on Form 1040 to assign a commuting zone to the individual. If no address is available on Form 1040, we use information returns, and if those are not available either, we rely on other survey and administrative sources of the Census Bureau to determine an individual's address.

Physician Registry

Individuals and organizations that provide healthcare services in the U.S. must use their unique 10-digit National Provider Identifier (NPI) to identify themselves throughout the healthcare system, including in submitting claims for payment. These NPIs are recorded in the National Plan and Provider Enumeration System (NPPES) file maintained by the Centers for Medicare and Medicaid Services (CMS). We define an individual as a physician if we observe them in the April 2018 vintage of NPPES and if their associated primary provider taxonomy code starts with 20 ("physicians"). The data also includes all NPIs deactivated prior to April 2018, such as would occur due a physician's retirement or death. We merge tax data with this physician list using the Census Bureau's Protected Identification Key (PIK)-based data linkage infrastructure, which Wagner and Layne (2014) describe in detail.

Although the brief discussion in Section 1.2 does not describe every path a physician can take, such as obtaining the Doctor of Osteopathic Medicine (DO) degree or an MD abroad, all physicians who practice in the U.S. and have an NPI are included in our income data.

Specialty Taxonomy

The NPPES file provides a granular provider taxonomy code for each physician.[65] We crosswalk these codes to a more aggregated specialty classifications: 60 Medicare Specialty Codes.[66] We then create a crosswalk of Medicare Specialty Codes to nine aggregate *specialty categories*, defined in Table E.1. We also crosswalk Medicare Specialty Codes to the specialty

[65]Provider taxonomy codes and their description can be found at `https://taxonomy.nucc.org`).

[66]The crosswalk is available from `http://www.cms.gov/Medicare/Provider-Enrollment-and-Certification/MedicareProviderSupEnroll/Downloads/TaxonomyCrosswalk.pdf`.

taxonomy used by the National Resident Matching Program, which we use in Sections 2.2 and 3.3.

Medical School

Medical school name and graduation year comes from the Doctors and Clinicians National Downloadable File, available from CMS for years 2014 to 2018. We add information on the U.S. News and World Report medical school ranking for years 2005 to 2018, collected from online sources. The report ranks 50 schools each year. Across years 2005-2018, 58 unique medical schools were ranked. We define a school to be top-5 if it was ranked among the top-5 schools in at least one year between 2005 and 2017.

American Community Survey

The ACS surveys repeated cross-sections of approximately 1% of the U.S. population per year. ACS full implementation happened in 2005. Prior to 2005, sample sizes were much smaller. We retain the following self-reported ACS variables: wages, indicator for being self-employed and self-employment income, spousal income, and the number of hours per week and the number of weeks per year an individual reports working.

We consider an ACS respondent to be a lawyer if they have an occupational code for a legal profession. This includes lawyers, judges, magistrates, judicial law clerks, and other judicial workers. Similarly to physicians, we use PIKs to merge this list of lawyers with their 2005–2017 tax returns.

Medicare Data

Since 2012, CMS has released the Physician and Other Supplier Public Use File of the Physician Medicare Provider Utilization and Payment Data (MPUPD) for each provider who treated fee-for-service Medicare patients. The file is publicly available on cms.gov.[67] The data exclude procedure codes that a physician provides to Medicare patients ten or fewer times in a given year. Subject to these restrictions, the file reports the list of services performed, the number of times each service was offered, the place of service, the number of unique patients for each service, and Medicare payment. The term "service" here refers to a Healthcare Common Procedure Coding System (HCPCS) code, treated as distinct when performed in a facility and in a non-facility setting.

B.2 Measurement

Defining Income

We observe individual wage earnings (including pre-tax deferrals) and household AGI directly in tax data.

Measuring individual total income and business income is more challenging for two reasons. First, our tax data do not fully record the amount of business or self-employment

[67] https://data.cms.gov/provider-summary-by-type-of-service/medicare-physician-other-practitioners/medicare-physician-other-practitioners-by-provider-and-service.

income on Schedules E and C. Second, non-wage income on Form 1040 is reported at the tax unit rather than individual level. We follow Bell et al. (2019), who use similar data to study incomes of inventors for whom non-wage earnings may also play an important role. As in Bell et al. (2019), we define total individual income as the sum of individual total wage income and the household AGI net of all wage earnings and taxable retirement distributions (for those aged 60 or older), but gross of tax-exempt interest and Social Security payments. For non-filers, we only use individual wage earnings as a measure of total individual income. The idea is that AGI net of wages and retirement cash flows captures current business income as well as financial returns and capital gains on previous earnings. For those physicians who file joint returns with a spouse, this object technically captures business and financial income of both spouses. We examined various approaches to approximating the income attributable to the index individual of interest (versus the spouse). The results are not qualitatively sensitive to the approach we use, so we focus on the measure that attributes all of imputed business income to the index physician for simplicity. We make one exception. If a physician is filing jointly with a spouse and the spouse is *also* a physician in our data, then we attribute 50% of the implied business and financial earnings of the household to each spouse.

We define individual total business income as the Total Money Income (TMI) of the household net of wages, taxable dividends, taxable interest, social security, partially observed profit and loss from Schedule E, and distributions from pre-tax deferral accounts irrespective of age. For physicians married filing jointly with a physician spouse, we take 50% of this amount. TMI is a measure of income used by the U.S. Census Bureau and is pre-computed in the tax data extract available to us. Its main advantage for the purpose of inferring business income is that TMI excludes capital gains. The U.S. Census Bureau defines TMI as income received on a regular basis (exclusive of certain money receipts such as capital gains) before payments for personal income taxes, social security, union dues, Medicare deductions, etc.

Following the literature on income inequality, we use the tax unit's AGI when characterizing physicians' position in the national income distribution.

We construct the self-reported analogues of all income objects using ACS income variables.

Measuring the length of training

We use the tax data to construct estimates of average training length by specialty by measuring the number of years for which physician incomes are fixed after medical school completion before they discontinuously jump. There is relatively little variation in physicians' earnings during residency. While earnings may increase somewhat as resident progress into fellowships, earnings reliably increase dramatically when physicians start their first post-training jobs. These two facts about physicians' early-career income levels and changes allow us to use panel income data to estimate the average duration of training by specialty.

For this exercise, we use all physicians in our data who were between 20 and 28 years old (inclusive) in 2005 and have W-2 wage income information available every year from 2005 through 2017. Since residencies begin halfway through the year, we can identify new residents as those who earn about half the typical resident's wage income in year t (assuming they do not have meaningful wage income while in their last semester of medical school) and then see their wage income increase to a typical resident's income in year $t+1$. We identify a person as starting their residency when we observe year t wage income between \$15,000 and \$35,000 (roughly half the wage income range in which we observe a large share of the mass in the distribution of physicians at typical residency ages) followed by an increase in wage income of at least 30% (constructed as the change in income between the first and second year divided by the average income over the two years). We use a percent change requirement rather than specifying the level of income for the second year to allow for some variation in salaries across programs, plus the possibility that residents might have wage income from other sources. We identify a person as completing their training in the first year that they experience another 30% increase in their wage income from the prior year, and that year's income is at least \$80,000. Variations on these parameters produce similar results. We take the mean of the resulting person-level estimates of residency duration by each level of residency taxonomy that we use throughout the paper.

Measuring hourly earnings

We construct hourly earnings in specialty s in year t by dividing the average annual earnings in specialty s in year t among 40-to-55-year-old physicians by 52 times the average of weekly hours worked as reported in the ACS by 40-to-55-year-old physicians in specialty s in year t.

Measuring tuition costs

We calculate the average tuition cost for a medical education—which we define as the tuition and fees for both an undergraduate and graduate degree—from a variety of sources. These include undergraduate tuition from the National Center for Education Statistics and medical school tuition from the Association of American Medical Colleges (AAMC) surveys. These datasets report both public and private school tuition. Using data for 2016-2017 academic year, we compute (i) average tuition and fees for attending a public (in-state) college and a public medical (or law) school, and (ii) average tuition and fees for attending a private college and a private medical (or law) school. For a medical career in public universities this yields: \$32,351 per year times four years for medical school and \$9,003 per year times four years for college, for a total of \$165,416. For a medical career via private universities, we get \$53,850 per year for four years of medical school and \$30,139 per year for four years of college, for a total tuition and fees cost of \$335,956. In Appendix D, we use a simple average of this range as the measure of tuition.

The underlying sources are:

- American Association of Medical Colleges, Tuition and Student Fees Report, October 2018 (https://www.aamc.org/data/tuitionandstudentfees/)
- National Center for Education Statistics, 2018 Digest of Education Statistics, Table 330.10. This reports average undergraduate tuition and fees and room and board rates charged for full-time students in degree-granting postsecondary institutions. (https://nces.ed.gov/programs/digest/d17/tables/dt17_330.10.asp).
- American Bar Association, Tuition Fee Expenses, ABA 509 required disclosures (http://abarequireddisclosures.org/Disclosure509.aspx)

Our estimate of \$250,688 in aggregate tuition costs for a medical degree is consistent with reports of debt levels among medical students. For example, Harvard Medical School reports that the average graduating debt in 2022 at HMS was \$108,382, compared to the national averages of \$179,679 at public medical schools and \$187,229 at private medical schools.[68] Our estimates use older data, but are not weighted by the share attending private versus public schools, and include tuition and fees overall rather than only portions that resulted in debt.

Present discounted value of earnings

We use the panel structure of our data to estimate the present discounted value (PDV) of income earned over a physician's (or a lawyer's) career for analysis in Appendix D. The data allow us to incorporate variability across individuals and over time, accounting for actual income dynamics over the career. We start by grouping observations with physicians (lawyers) of the same age. To minimize noise, we pool data from all years 2005 to 2017 and adjust income observed in different calendar years for inflation. For each age cohort, we divide individuals into thirteen income bins: top 1% of income within each age cohort, next 4%, next 5%, each of the bottom nine deciles, and zero income. We estimate empirical transition probabilities between income bins from age a to age $a+1$. In practice, to improve precision, we use individuals within a five-year age window centered on each age; that is, to calculate transition probabilities between ages 50 and 51, we actually use people who had age a between 48 and 52 in any year t between 2005 and 2016. We link these respondents to their incomes at age $a+1$ in year $t+1$, and use the transition probabilities from a to $a+1$ to estimate the transition probabilities between 50 and 51. We estimate one-year transition probabilities across income bins for each year of age beginning at age 20 and ending at age 70. We use the empirical distribution of income levels at the starting age and age-specific transition probabilities to simulate 50,000 careers for physicians and lawyers, which gives us the distribution of income paths in each occupation. We discount the value of these incomes back to age 20 using a discount factor of 0.97.

B.3 Additional Descriptive Patterns

Table 1 shows that the average physician in 2017 is 49 years old, 38% of physicians are women, 22% were not U.S. citizens at birth (record of ever being an "alien" in Census Numident), and 80% are married. Older cohorts of physicians are substantially less likely to be female or not U.S. citizens at birth. The most common specialty category in all samples is primary care, accounting for a bit more than 40% of physicians.

In Table 2 we observe that top-earning physicians work in smaller firms, are a year older than the sample average, and work similar hours as the average physician, but more than physicians in the top half of the distribution. They are 1.5 to 2 times more likely to live in New York or New Jersey, Florida, Arizona, or Texas. Only 24% of top earners are women, as compared to 40% in the full sample. Top earners are also 5 percentage points less likely than the median physician to have had any immigration history and are ten percentage

[68] https://meded.hms.harvard.edu/admissions-at-a-glance

points more likely to be married. 7% of all physicians in 2017 were retired according to our measure, with almost all of these individuals in the 56 to 70 age sample, for a retirement rate of 19% in that sample.

Figures E.3A and E.3B plot the time series of real earnings and of the share of physician households in top 1% of the national income distribution. We plot raw and regression-adjusted values to capture the evolution of mean real income for a comparable physician over time. We regression-adjust the time series for age fixed effects, sex, state of residence fixed effects, and Medicare Specialty Code fixed effects. The rate of real income growth among physicians from 2005 to 2017 is around 1% annually—or half of the inflation-adjusted growth rate in per capita national healthcare expenditures over the same time period.

The median physician in 2017 works 50 hours per week in a firm with 85 physicians. Firm size is very skewed, as employers vary from single-person practices to large hospitals. Firm size increased substantially over time, with a median firm having 52 physicians in the full panel and only 20 physicians among older cohorts. The share of single-person firms fell from 26% to 20% over our time period; see Figure E.3C.[69] This pattern is not driven by a change in the share of physicians not having any W-2 earnings (who would hence would have a missing EIN) as we see in Figure E.3D.

Figure E.4 shows lifecycle patterns of labor supply. Physician peak work hours are in their early late 20s and early 30s, consistent with the time in residency. Hours start declining after age 55. Physicians start retiring at age 65, with a significant jump in retirement rate between 69 and 70.

[69]Since our measure of firm relies on the EIN in W-2 records, we only observe those single-physician practices that are either structured as S-corporations that pay the physician some portion of income with a W-2. If the physician is only practicing as a solo proprietor, there will be no W-2. We could also be capturing as single physician firm a practice where one physician is an owner and has no W-2 income, while another physician works as a W-2 employee.

B.4 Comparison of Tax and Survey Data

We use 2017 American Community Survey (ACS) to compare physicians' self-reported income to income measures constructed from the administrative tax data. In the ACS, we define individual total income as the sum of individual wage and self-employment income of the index individual plus self-employment income of the spouse (or 50% of the latter if the spouse is also a physician according to NPPES). ACS defines self-employment income to include both farm or non-farm self-employment income. The non-farm self-employment income includes all sources of business income that we also capture in the tax data, including one's own business, professional enterprise, or partnership income. We start with our baseline sample of 848,000 physicians in 2017 cross-section and restrict it to 14,000 individuals who are also observed in 2017 ACS (Table E.3). Among these individuals, 11,500 also report being a physician in ACS, while 2,500 report other occupations. We observe a large difference in average individual total income between the tax data and ACS for both subsamples.

In the sample of 11,500 individuals who are physicians in NPPES and also report being physicians in ACS, average individual total income in the tax data is $365,400, while ACS income is $258,100—a more than $100,000 (29%) difference in annual income between the tax and survey data. We zoom onto this sample to examine this large discrepancy in average tax-based and survey-based income of physicians. Table E.4 separates average individual total income into wage income and business income. Average wages, conditional on reporting strictly positive wages, differ by $32,100. The number of individuals reporting positive wages is similar. This difference implies that wage reporting is quite accurate in ACS data, as the difference of $32,100 is close to allowed pre-tax contributions that we added in our measure of wages in the tax data. It is reasonable to assume that in survey questions, individuals report their wages after pre-tax retirement contributions.

Columns (3)-(6) of Table E.4 report average business income, for the full sample, and conditional on business income being strictly positive. We use self-employment income as the measure of business income in ACS. Business income is $58,000 lower in ACS data in the full sample. The difference shrinks to $41,000 when we condition on business income having to be positive. In relative terms, average business income in the tax data goes from being 3.7 to being 1.4 times average business income in ACS data when we move from the full subsample to conditioning on business income being strictly positive. Only 19% of individuals report positive business income in ACS, compared to 60% in the tax data. Overall, we find that about a third of the total $100,000 difference in income between tax and survey data is attributable to differences in wage reporting that likely stems from the difference in attribution of pre-tax deductions in survey responses. More than 85% of the remaining difference is due to differences in business income reporting (primarily on the extensive margin), and the remainder are other types of income that we capture in the tax data, but not in ACS data.

C Details of Empirical Methods and Further Results

C.1 Changes in Medicare Reimbursement

RVU Example and Definitions. In 2017, a standard office visit was worth 2.06 RVUs, while inserting a cardiac stent (code 92928) was worth 17.24 RVUs (CMS, 2017). Each service's RVUs are adjusted across geographies using geographic practice cost indices and converted to dollars using a "conversion factor"—$35.89 per RVU in 2017.

Since Medicare has separate RVU allocations for many codes depending on whether they are performed in a *facility* (such as hospital) or *non-facility* (such as physician's office) setting, we treat "service" throughout as a pair of billing code (Healthcare Common Procedure Coding System, which differentiates across 13,000 unique codes) and place of service (facility or non-facility).

IV Framework for Earnings. Our instrumental variable setup builds on equation (5) in the text, repeated here for convenience:

$$\ln Y_{i,t} = \alpha_i + \beta \ln P_{i,t} + \theta_{a(i,t)} + \eta_{t,s(i)} + \varepsilon_{i,t}. \tag{C.1}$$

where $\ln Y_{i,t}$ denotes log income, $P_{i,t}$ is the Medicare price instrument driven by RVU changes, α_i are physician fixed effects, $\theta_{a(i,t)}$ are age fixed effects, and $\eta_{t,s(i)}$ are year-by-specialty fixed effects. The instrument is defined by equation (4), repeated here for convenience:

$$P_{i,t} = \sum_{k \in K} \overline{q_{i,k}} \times RVU_{k,t}. \tag{C.2}$$

We use $Q_{i,t}$ to denote the total number of RVUs a physician bills in year t, formally:

$$Q_{i,t} = \sum_{k \in K} q_{i,k,t} \times RVU_{k,t}. \tag{C.3}$$

The difference from equation (C.2) is that $q_{i,k,t}$ denotes the actual number of times physician i provides service k in year t, rather than the average number of times physician i provides service k across all years. Thus $Q_{i,t}$ incorporates endogenous supply responses in $q_{i,k,t}$ and changes in RVUs, while $P_{i,t}$ only reflects the latter.

We estimate the following two-stage least squares (2SLS) model:

First stage: Total RVUs billed

$$\ln Q_{i,t} = \pi \ln P_{i,t} + \alpha_i + \theta_{a(i,t)} + \eta_{t,s(i)} + u_{i,t} \tag{C.4}$$

Second stage: Income

$$\ln Y_{i,t} = \beta \widehat{\ln Q_{i,t}} + \alpha_i + \theta_{a(i,t)} + \eta_{t,s(i)} + \varepsilon_{i,t} \tag{C.5}$$

The first-stage regression reveals changes in billing that include both the mechanical impact of RVU changes along with the supply responses. A coefficient of $\pi = 1$ must be interpreted as no supply response; the supply elasticity is $\pi - 1$.

IV Framework for Retirement Decision. We also estimate a supply response of retirement to earnings. To do this, we treat income as the endogenous variable in the following 2SLS setup:

$$\begin{aligned} &\text{First stage: Income} \\ &\qquad \ln Y_{i,t} = \pi \ln P_{i,t} + \alpha_i + \theta_{a(i,t)} + \eta_{t,s(i)} + \varepsilon_{i,t} \qquad \text{(C.6)} \\ &\text{Second stage: Retirement} \\ &\qquad R_{i,t} = \beta \ln Y_{i,t} + \alpha_i + \theta_{a(i,t)} + \eta_{t,s(i)} + \varepsilon_{i,t} \qquad \text{(C.7)} \end{aligned}$$

where $R_{i,t}$ is a dummy for the retirement decision.

Baseline Pass-Through Calculation. Suppose a physician's Medicare reimbursements were to increase by 1%, as measured by our Medicare price instrument $P_{i,t}$. Our reduced-form estimate of the elasticity of earnings to the Medicare price instrument is 0.236 (column 1 of Table 3), so a 1% increase in reimbursements would increase earnings by 0.236% or \$955 for the mean physician (=0.236% × \$404,500, which is the mean income in the age 40 to 55 sample). The mean physician in our sample provides 4,079 RVUs (mean of $P_{i,t}$), and Medicare's Conversion Factor was \$37.89 at the beginning of our sample, for total Medicare billing of \$154,553. The extra spending from this hypothetical 1% reimbursement increase would thus be \$1,545, for a pass-through of 62% (=\$955/\$1,545).

Accounting for Private Insurance Spillovers. Clemens and Gottlieb (2017) find that a \$1 increase in Medicare reimbursements increases private insurance reimbursements by \$1.16, or 83% of the baseline private/public payment difference (a factor of 1.39 in their data). Private insurance is 1.7 times as large as Medicare (CMS, 2019), so 83% as large a response in a market 1.7 times the scale of Medicare implies 1.4 times the extra spending, or \$2,192. The total increase in Medicare-attributable spending is then \$3,737, implying a pass-through of 25%.

C.2 ACA Insurance Expansions

Based on Medicaid expansion dates (listed below in parentheses) obtained from the Kaiser Family Foundation we include the following states in our analysis: AZ (1/2014), AR (1/2014), CA (1/2014), CO (1/2014), CT (1/2014), HI (1/2014), IL (1/2014), IN (2/2015), IA (1/2014), KY (1/2014), MD (1/2014), MI (4/2014), MN (1/2014), NV (2/2014), NH (8/2014), NJ (1/2014), NM (1/2014), ND (1/2014), OH (1/2014), OR (1/2014), PA (1/2015), RI (1/2014), WA (1/2014), WV (1/2014). Following the literature on Medicaid expansion (Ghosh et al., 2019; Miller and Wherry, 2019; Miller et al., 2021; McInerney et al., 2020), we exclude DE, DC, MA, NY, and VT from our analysis, as ACA insurance expansions in these states either took place earlier than 2014 or were not binding in practice, as the states had more generous coverage rules already prior to the ACA.

County-level insurance rates are from the Census Bureau Small Area Health Insurance Estimates (SAHIE) data at `https://www.census.gov/data/datasets/time-series/demo/sahie/estimates-acs.html`.

D Extensive Margin Choice: Medicine vs. Law

We use our data to compare physicians earnings to the next-most-common high-earning career in the U.S.: lawyers. This calculation provides loose guidance about how much scope there realistically is for policy to reduce physicians' incomes before alternative career options would dominate financially. Law is also a profession with high human capital investments, expensive specialized training, and licensure requirements. Yet barriers to entry are lower. Anecdotally, there is no shortage of law school spots, no analogue to limited residency slots, and no shortage of lawyers (Murphy et al., 1991). So it seems plausible that most people who become a physician could have become a lawyer, and lawyers' income provides a useful measure of outside options available to potential physicians.

Physicians and lawyers have very different lifecycle earnings patterns, so simple comparisons of mean earnings between working physicians and working lawyers would be misleading. We use the panel dimension of our data to estimate the distribution of total career earnings, reported in Table E.15. Appendix B.2 describes our method of computing the present discounted value of earnings. With 3% annual discounting, we estimate that physicians' average PDV of earnings at age 20 is \$10.1 million (equivalent to a \$386,000 annuity payment). The analogous estimate for lawyers is \$7.1 million (equivalent to a \$274,300 annuity payment). Notably, our estimates of annual earnings and resulting PDVs for both physicians and lawyers are 2 to 5 times higher than in Altonji and Zhong (2021), consistent with the substantial underreporting of non-wage earnings in survey data documented in Section 2. Against these discounted earnings we must count the cost of undergraduate and professional training, which we estimate to be \$250,500 for physicians and \$187,000 for lawyers, each corresponding to 2.5% of average lifetime earnings. (Appendix B.2 provides the sources for this estimate.) Once we account for difference in tuition, an average physician earns 42% more over their lifetime than an average lawyer.[70]

We next consider differences in working hours. We include a premium for hours beyond a 40-hour work week, since labor supply slopes up and the skilled labor market offers a premium for working long hours (Goldin, 2014). If physicians and lawyers had the same base hourly income, physicians would earn 12% more based purely on the difference in hours. This leaves a 30 percentage point difference in earnings attributable to forces beyond hours worked. For the lowest-paid specialty, primary care, we estimate the average lifetime earnings at \$6.5 million. The total cost of tuition with debt is around 5% of these lifetime earnings. This implies that an average PCP earns \$0.6 million less than an average lawyer and, with interest,

[70] To make the calculation as conservative as possible, we can also consider borrowing costs. It is not obvious that these should matter—after all, future debt payments should be discounted. But, for argument's sake, suppose students have to pay a risk premium entirely due to financial market frictions and their pure rate of time preference is zero. Medical students might borrow an extra \$115,000 relative to lawyers to cover the additional year of schooling (tuition of \$63,000 and approximately \$50,000 for living expenses) (Stanford, 2020). Suppose students borrow this at an average interest rate of 6.6% for 10 years (Bhole, 2017). This results in total (undiscounted) debt payment of around \$160,000 over 10 years. Assuming a 40% marginal tax rate, but ignoring any beneficial tax treatment of student loans, physicians would need to earn \$267,000 in undiscounted income to repay this extra loan. Under this extremely conservative calculation, the extra debt constitutes 9% of the extra \$3 million in discounted income that an average physician earns relative to an average lawyer.

pays about 1.5 percentage points more of lifetime earnings for their training.[71] Another way to see this is that lawyers would be very close to the regression line in Figure 2A; physicians have outside options that offer a similar hours-earnings tradeoff.

Overall, the evidence on relative incomes along with our estimates of meaningful labor supply responses suggest that policies aiming to cut physician earnings across the board could encounter serious problems. Section 3 shows the government has the power to make these sorts of changes. But income cuts may push lower-paid primary care physicians further below realistic outside options available to them within the U.S.

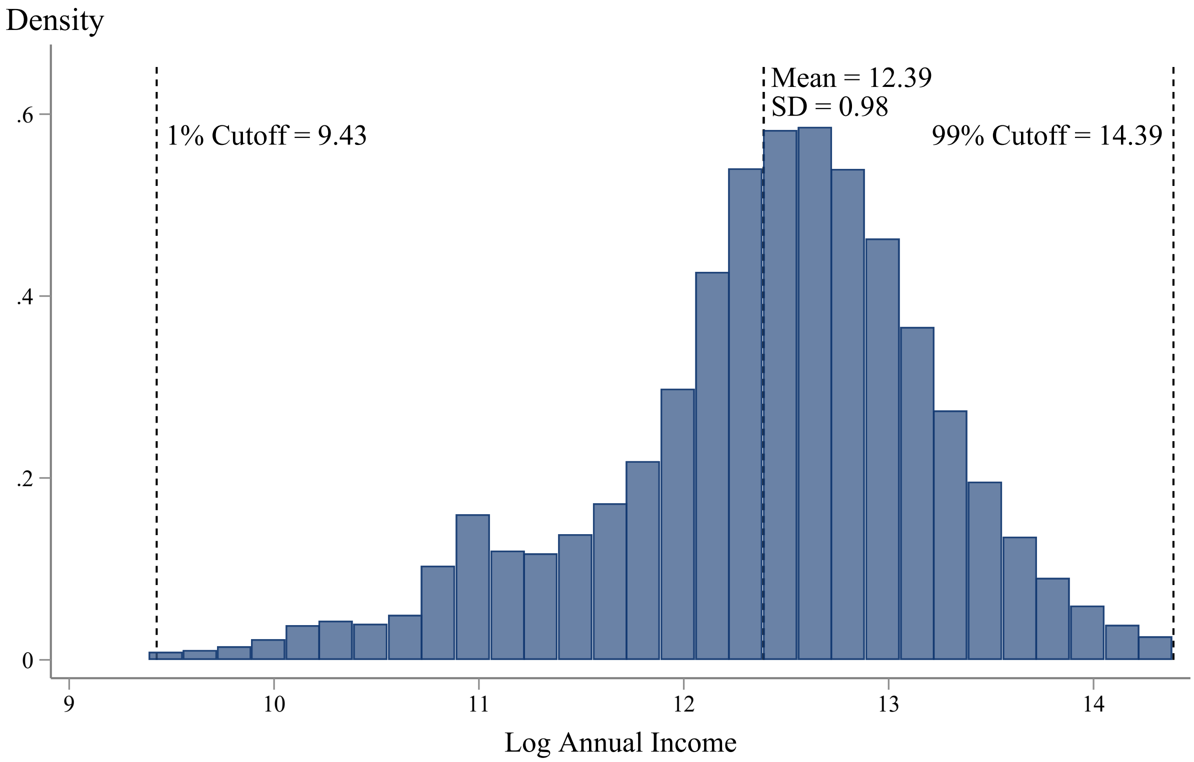
These results also highlight that the comparison of physician earnings in the U.S. to physician earnings in other OECD countries is not necessarily helpful for domestic policy debates. These comparisons miss the point that U.S. physicians could alternatively have been other high-skilled professionals in the U.S., who also command high incomes. Indeed, while U.S. physicians clearly earn more than their counterparts in other countries in absolute terms, their position in their respective national income distribution is not necessarily as different (Fadlon et al., 2020; Chen et al., 2022; Ketel et al., 2016).[72]

[71] Note that, even for PCPs, average tuition accounts for a modest share of earnings. This casts doubt on the importance of efforts to reduce or eliminate tuition for medical education (Supiano, 2018) as a way of reallocating talent towards PCPs.

[72] Using Swedish administrative earning records, Chen et al. (2022) found that 10% of physicians are in the top two and 42% of physicians are in the top five percentiles of the Swedish income distribution, thus resembling the U.S. in relative terms.

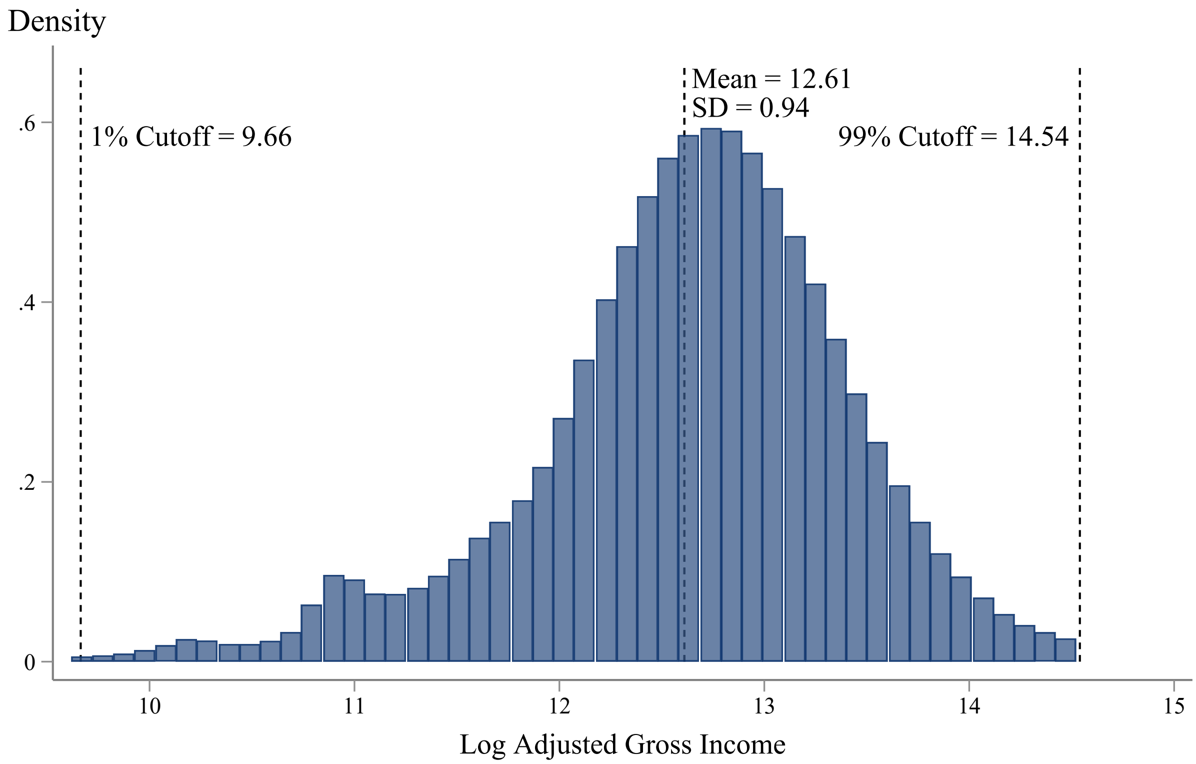
E Appendix Tables and Figures

Figure E.1: **Distribution of Physician Income**



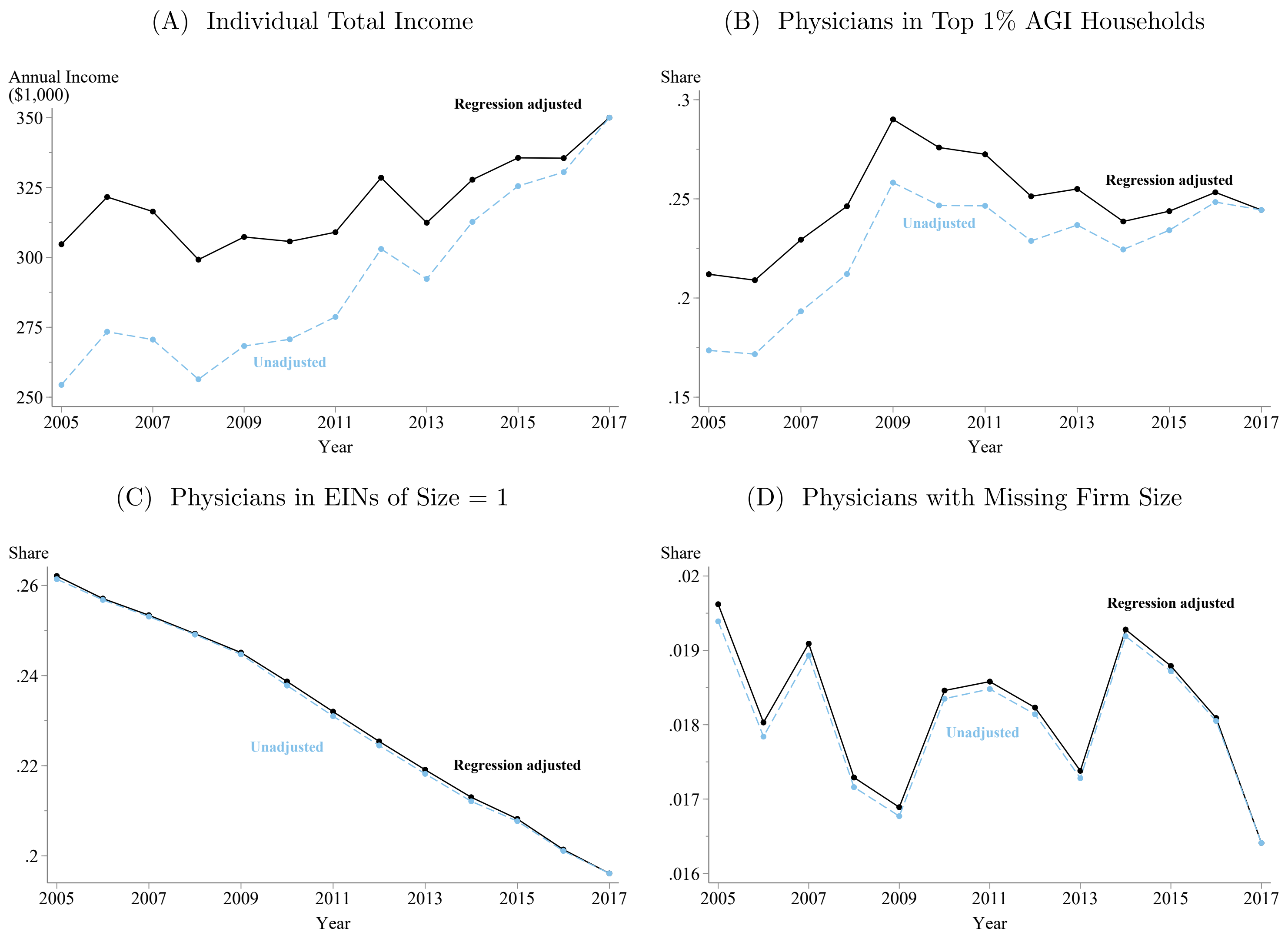
Notes: This figure plots the distribution of log individual total income among 20- to 70-year-old U.S. physicians in year 2017. The sample includes all physicians who were listed in the 2017 vintage of the National Plan and Provider Enumeration System (NPPES) for whom a record was observed in the universe of 2017 U.S. individual income tax return data. Individual total income is measured using individual tax returns data and is defined as the sum of individual total wage income and the household AGI net of all wage earnings and taxable retirement distributions (for those aged 60 or older), but gross of tax-exempt interest and Social Security payments. Section 1 and Appendix B.2 provide measurement details. Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

Figure E.2: **Distribution of Physician Adjusted Gross Income**



Notes: This figure plots the distribution of log Adjusted Gross Income (AGI) among 20- to 70-year-old U.S. physicians in year 2017. The sample includes all physicians who were listed in the 2017 vintage of the National Plan and Provider Enumeration System (NPPES) for whom a record was observed in the universe of 2017 U.S. individual income tax return data. AGI is directly reported in the individual tax data and is a household-level measure of income. Appendix B.2 provides details of all income measures. Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

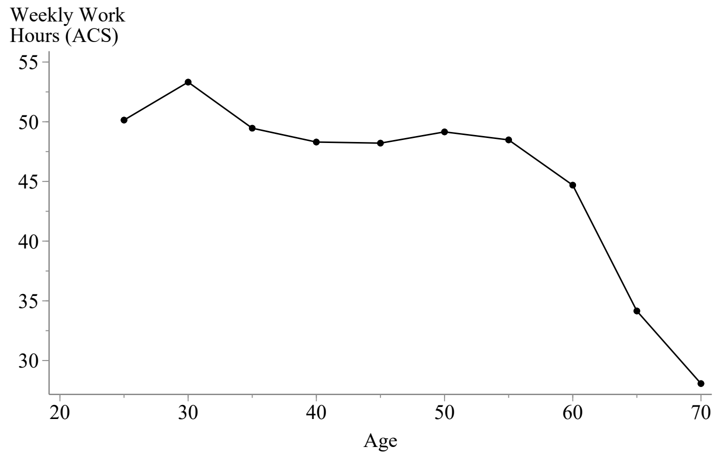
Figure E.3: **Time Series of Earnings and Firm Size**



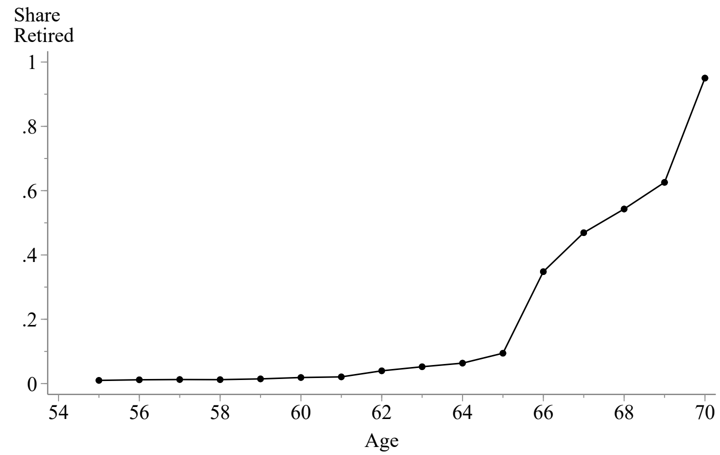
Notes: This figure plots the time evolution of mean individual total income (Panel A), share of physicians in households that are in the top 1% of the national income distribution (Panel B), share of physicians in firms (EIN) with only one physician (Panel C), and share of physicians with no W-2 filing and hence no EIN (Panel D). All panels include our full sample—years 2005 to 2017 and all ages from 20 to 70. Each panel plots the raw time series of means or shares, as well as the regression-adjusted time series. Regression-adjustment equalizes the composition of age, sex, Medicare specialties, and states across time to 2017 levels. We plot the raw means for the same sample as the regression-adjusted sample which requires us to observe age, sex, Medicare specialty, and state. See Appendix B.2 for a more detailed discussion of how we measure total individual income and firm size. Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

Figure E.4: **Physician Labor Supply over the Lifecycle**

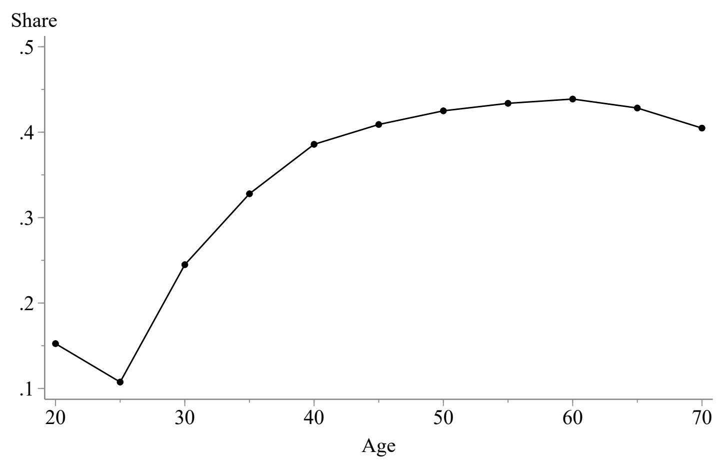
(A) Weekly Work Hours



(B) Retirement

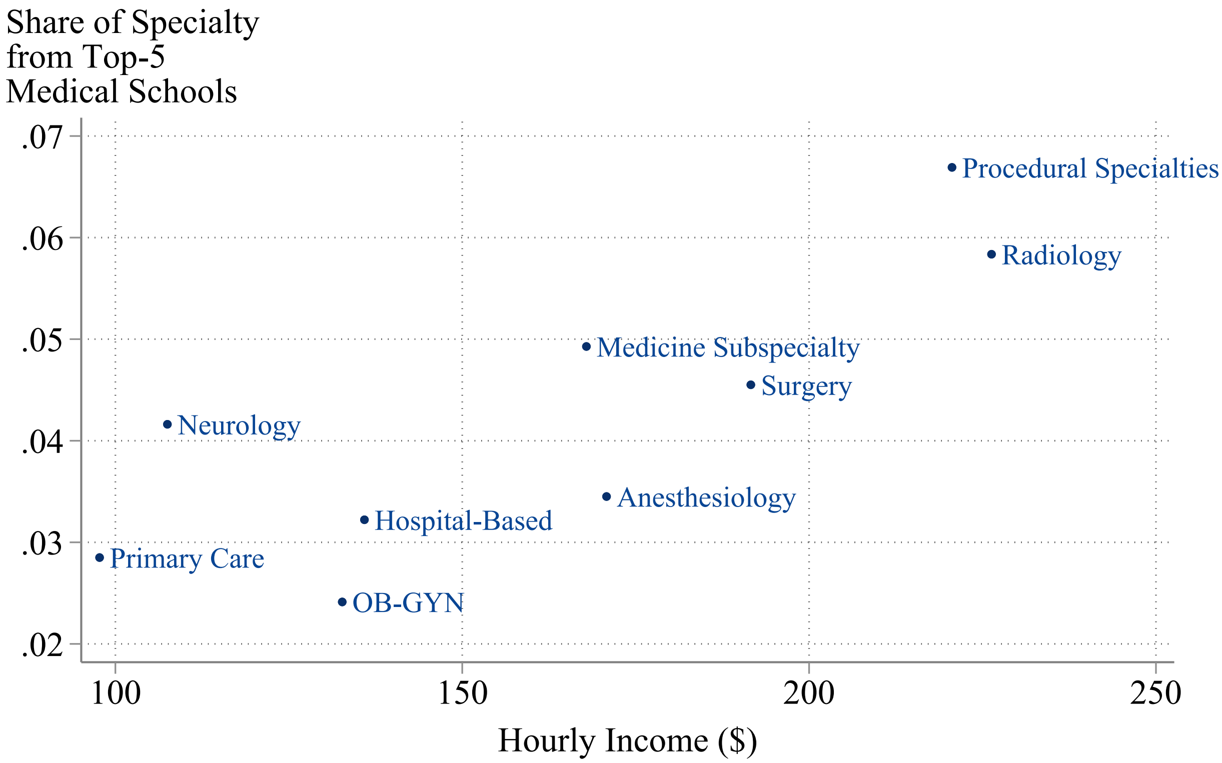


(C) Filed Schedule C



Notes: The figure plots mean weekly hours of work (Panel A), the share of physicians who are retired (Panel B), and the share of physicians filing Schedule C (Panel C) in our 2017 sample of physicians, by 5-year age intervals. Weekly work hours are measured from the subsample of physicians who are observed in ACS data. Retirement is defined as receiving Form 1099-SSA. Filing of Schedule C is directly observed in the tax data. Appendix B.2 provides more measurement details. Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

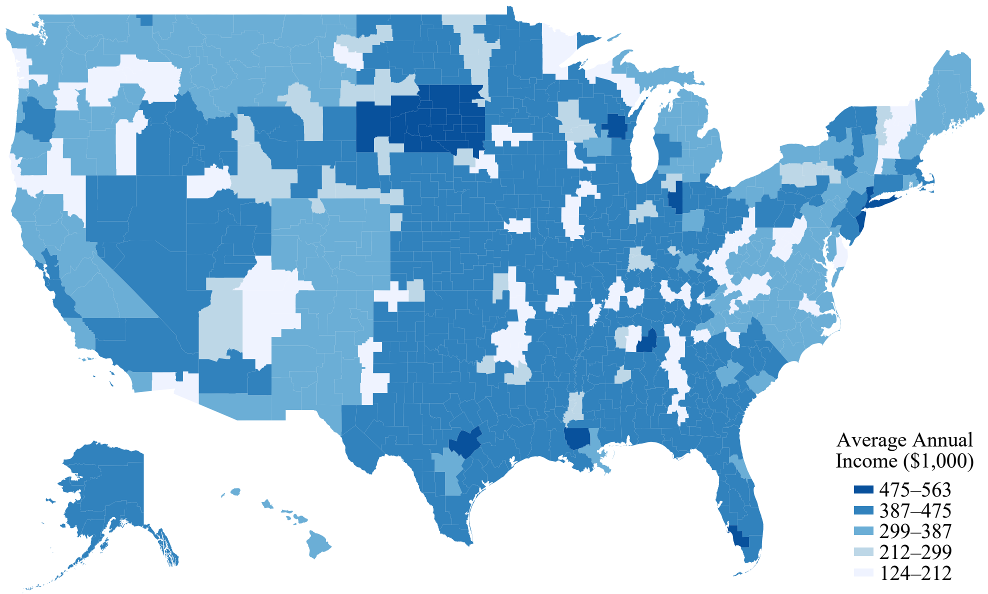
Figure E.5: **Medical School Rank vs. Specialty Income**



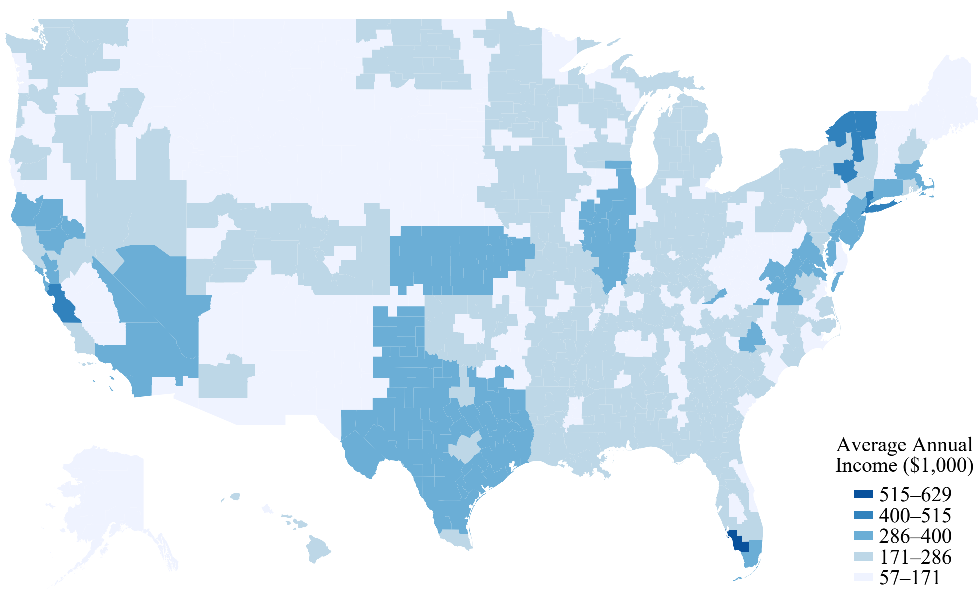
Notes: This figure plots the relationship between mean hourly income in a specialty category and the share of physicians in that specialty that graduated from top-5 MD programs as ranked by the U.S. News and World Report. Mean hourly income is computed as the ratio of mean individual total income among 40–55-year-old physicians in years 2005–2017 in a specialty category to the mean weekly work hours, multiplied by 52 reported, by physicians in the same sample who are also observed in ACS data. The share of top-5 MD graduates is computed on the full sample of physicians for whom we observe the medical school name. "Top-5" is defined as a school that had a rank 1 to 5, inclusive, in one or more year of the U.S. News and World Reports from 2005 to 2018. Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

Figure E.6: **Geographic Variation in Earnings**

(A) Physicians

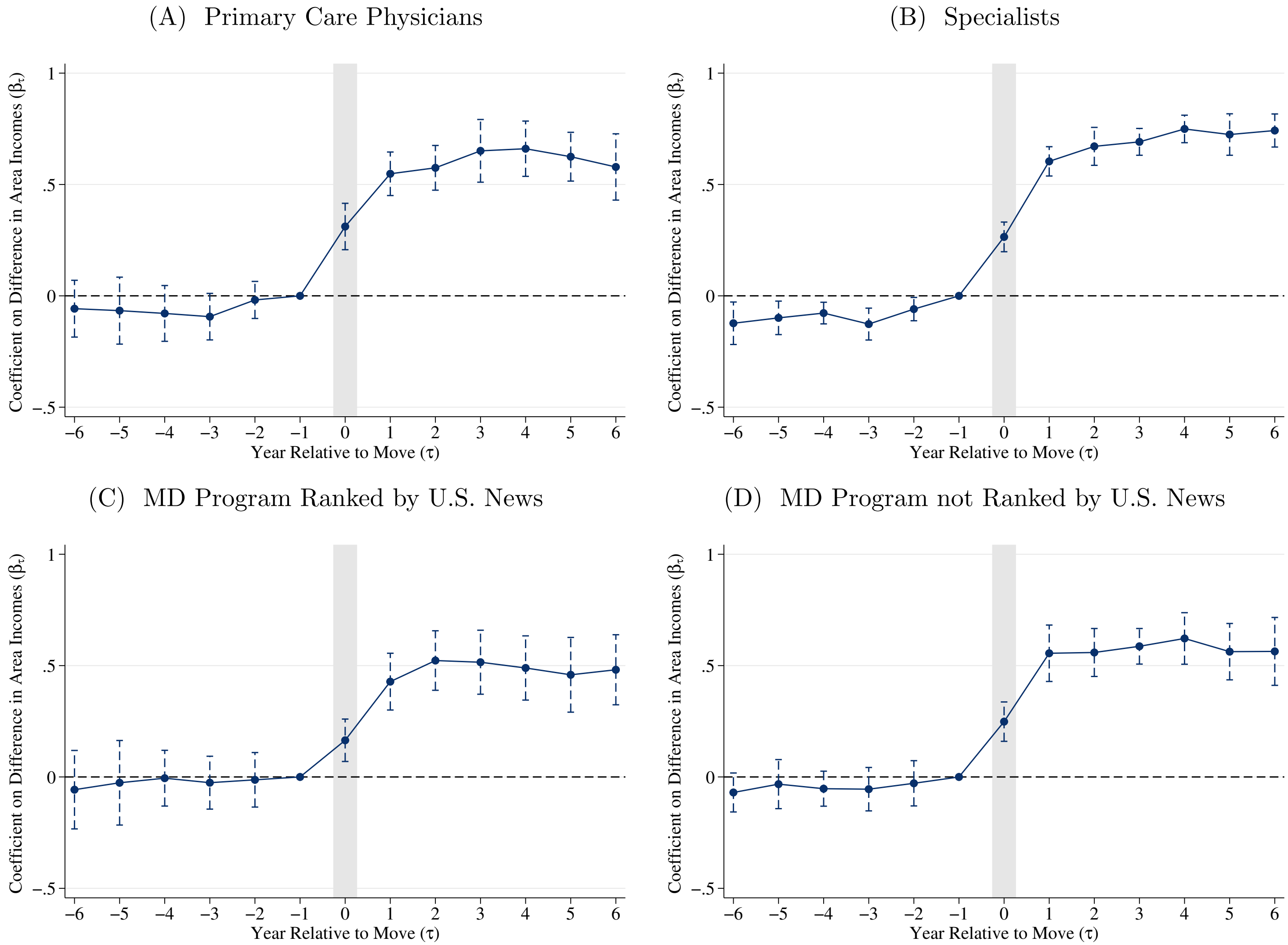


(B) Lawyers



Notes: This figure plots mean individual total income among 40- to 55-year-old physicians (Panel A) and lawyers (Panel B) in year 2017 by Commuting Zone (CZ). Mean income in the 122 largest commuting zones was computed directly. Mean income in remaining commuting zones was computed as an adjusted mean state-level income (state-levels means are shown in Figure 3), weighted by CZ population shares when CZs cross state boundaries. Adjusted mean state-level income excludes 122 CZs that are reported separately using population weights. Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

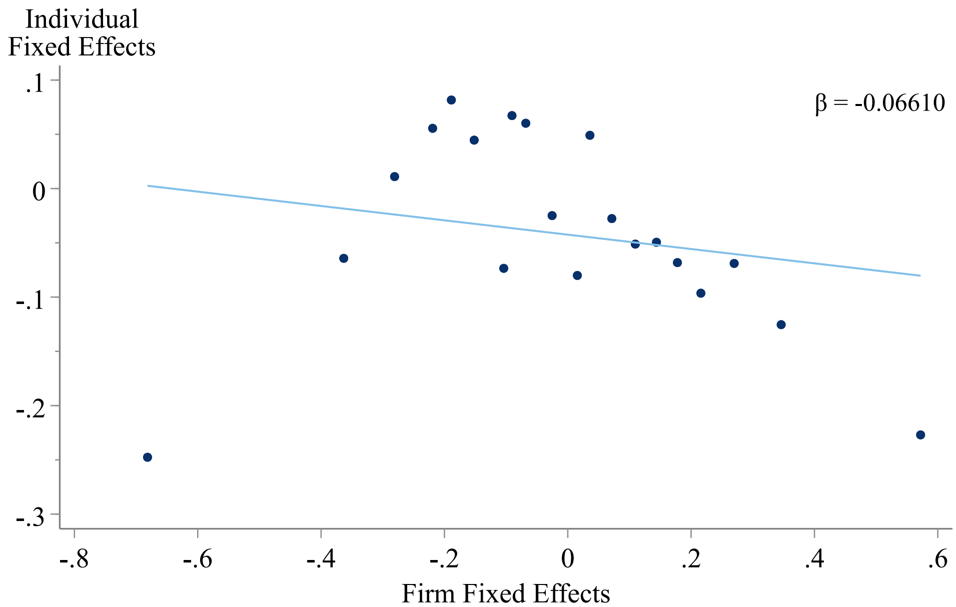
Figure E.7: **Event Study: Subsamples of Physician Movers**



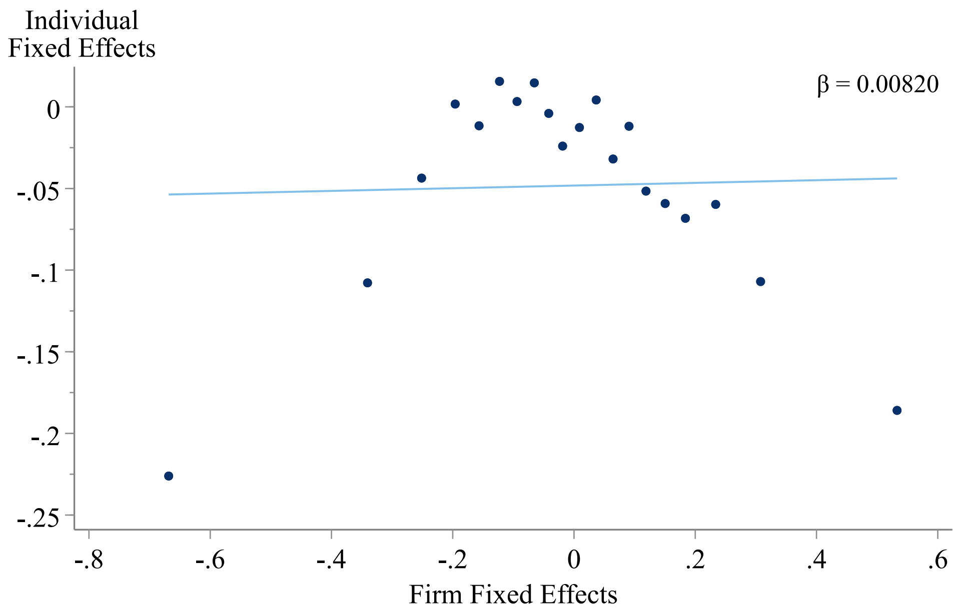
Notes: This figure shows coefficient estimates on the difference between mean physician individual total income in the origin and destination commuting zones ($\Delta\overline{\ln y}_{(j,k)}$) from equation (1) for four subsamples of physician-movers as indicated in panel titles. The coefficient is normalized to 0 in the year prior to the move ($\tau = -1$). The dashed lines mark the 95% confidence intervals. The outcome variable is log individual total income. The independent variables include $\Delta\overline{\ln y}_{(j,k)}$ interacted with physician fixed effects, relative year fixed effects, and age fixed effects. A physician is considered to be a mover if they changed their commuting zone once between years 2005 to 2017, and were age 40 to 55 during that change. Disclosure Review Board approval CBDRB-FY24-0456.

Figure E.8: **Firm and Individual Fixed Effects**

(A) Unconditional

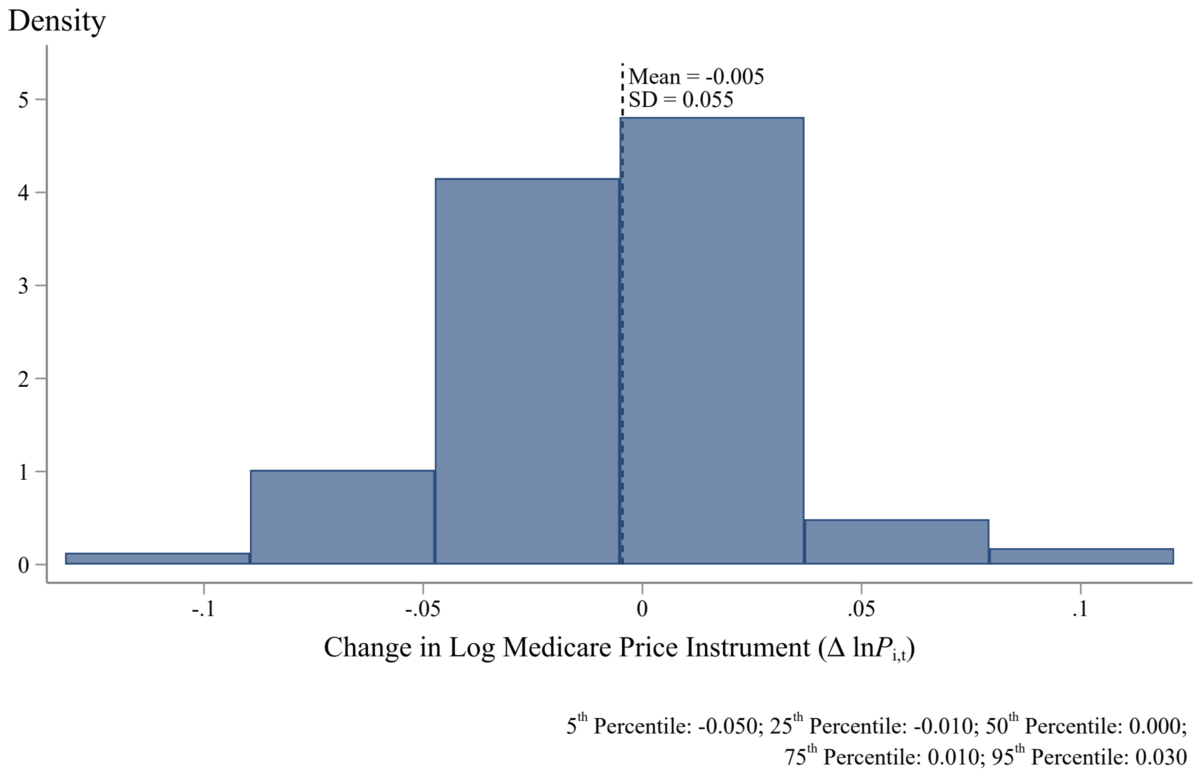


(B) Within Commuting Zone



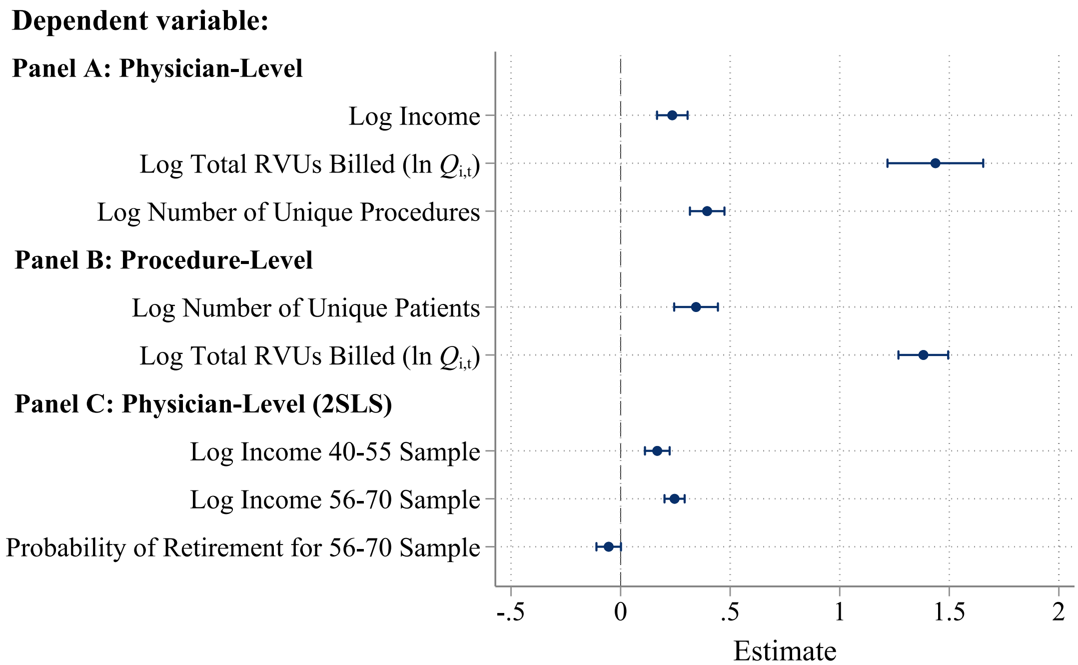
Notes: This figure plots the relationship between firm effects and person effects based on estimation of the firm analogue of equation (2) in the sample of physicians who switched firms (defined as an EIN) once in the full panel and were age 40 to 55 when they did so. The analysis is restricted to firms with 15 or more physicians. The outcome variable is log individual total income. The independent variables include physician, firm, relative year, and age fixed effects. Panel A is a binned scatterplot that plots the average individual fixed effect within each ventile of the firm fixed effects distribution. In Panel B we residualize the x-axis and the y-axis on commuting zone fixed effects as in Dauth et al. (2022). The line of best fit is a bivariate OLS regression on the underlying data points. Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

Figure E.9: **Distribution of RVU Changes**



Notes: This figure reports the distribution of one year changes in $\ln P_{i,t}$—the log of the total number of RVUs for a fixed vector of services by physician i in year t as computed in equation (4). The sample includes all physicians in our baseline sample who were also observed in 2012 to 2017 Physician and Other Supplier Public Use File of the Physician Medicare Provider Utilization and Payment Data (MPUPD). The fixed vector of services is defined as the average number of times each service (defined as a combination of HCPCS procedure code and facility or non- facility place of service designation) was performed by a physician between years 2012 and 2017. Each service in this time-invariant vector is multiplied by the year-specific RVU rate for this service. The resulting total number of RVUs per physician can vary from year to year only if Medicare changes how many RVUs are assigned to a service. Section 3.1 describes further the institutional details of Medicare billing. Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

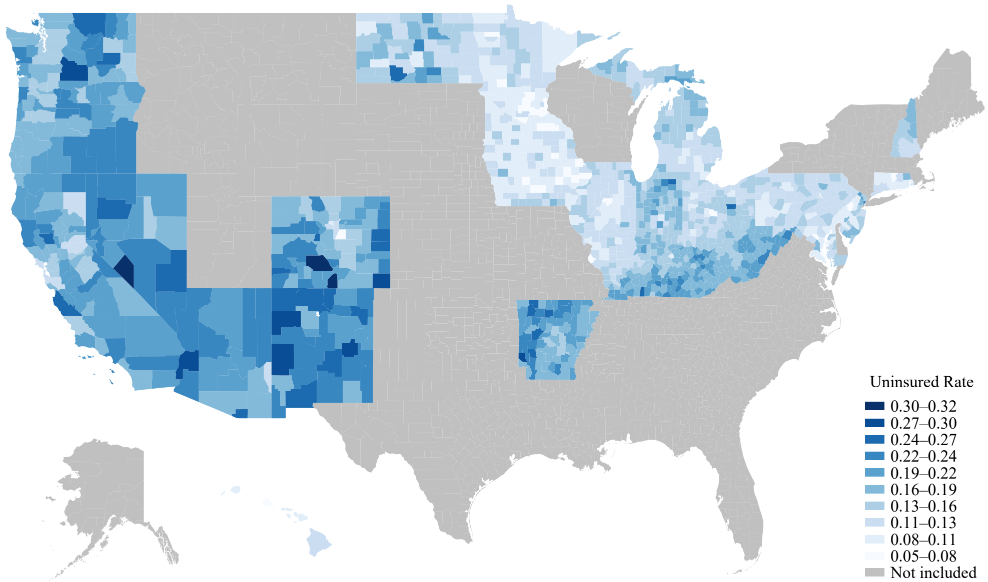
Figure E.10: **Effects of Changes in Medicare RVUs**



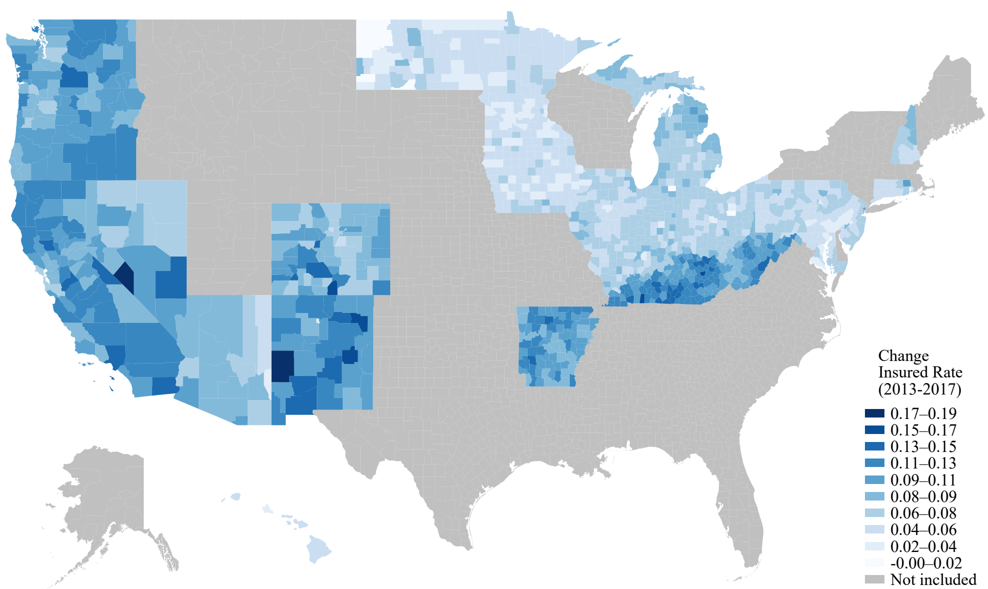
Notes: This figure reports the coefficients and 95% confidence intervals from estimating equation 5 for each outcome variable as indicated on the vertical axis. Each coefficient can be interpreted as an elasticity (or arc-elasticity). For physician-level regressions in Panel A, the main independent variable is $\ln P_{i,t}$, or log Relative Value Units (RVUs) for a fixed vector of services. In Panel B, the main independent variable is the time-varying RVU rate for a serve. Specifications in Panel C regress the outcome variable of interest on the log number of RVUs for performed services instrumented by $\ln P_{i,t}$. Regressions in all panels also include age fixed effects, and Medicare specialty-by-year fixed effects. As described in Section 3.1, the fixed vector of services is defined as the average number of times each service (defined as a combination of HCPCS procedure code and facility or non-facility place of service designation) was performed by a physician between years 2012 and 2017. Each service in this time-invariant vector is multiplied by the year-specific RVU rate for this service as shown in equation (4). The resulting total total number of RVUs per physician can vary from year to year only if Medicare changes how many RVUs are assigned to a service. Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

Figure E.11: **Geographic Variation in Share Uninsured**

(A) Share of Population Under 65 Uninsured in 2013

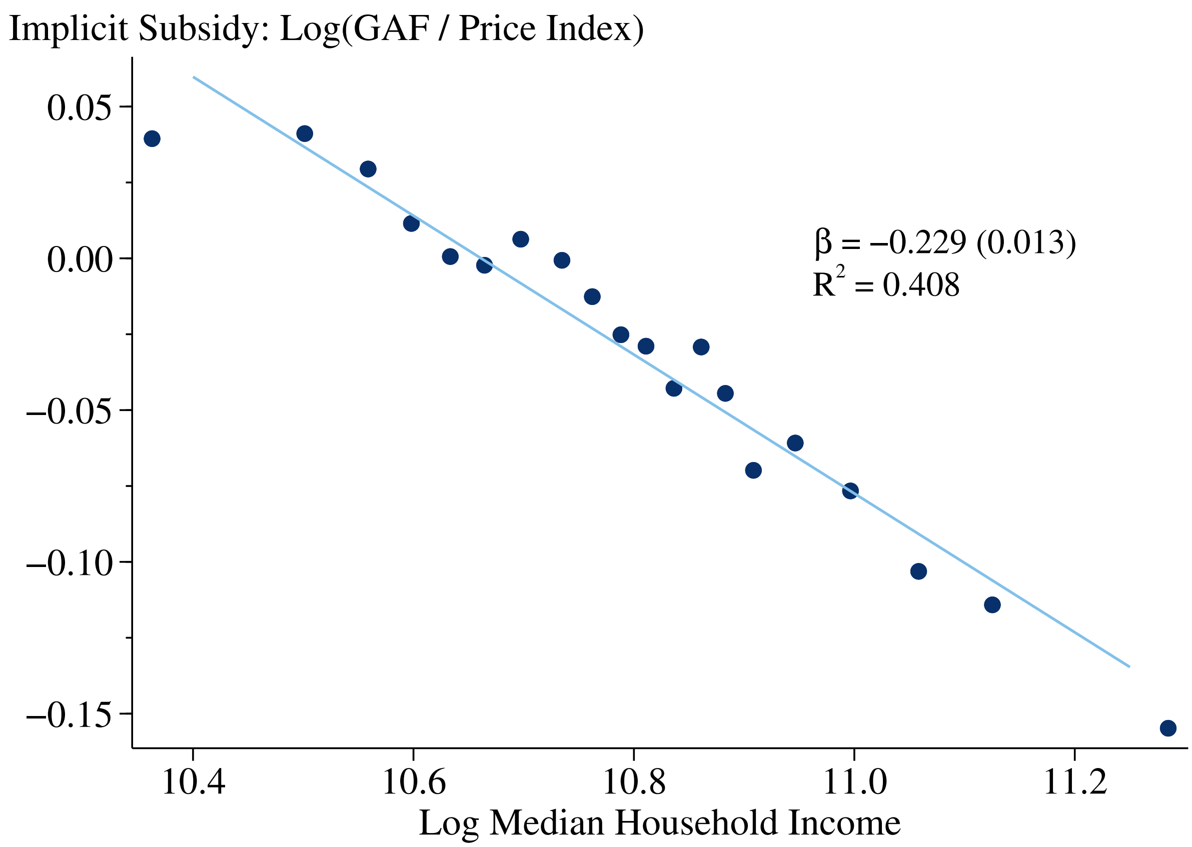


(B) Change in Share of Population Under 65 Insured from 2013-2017



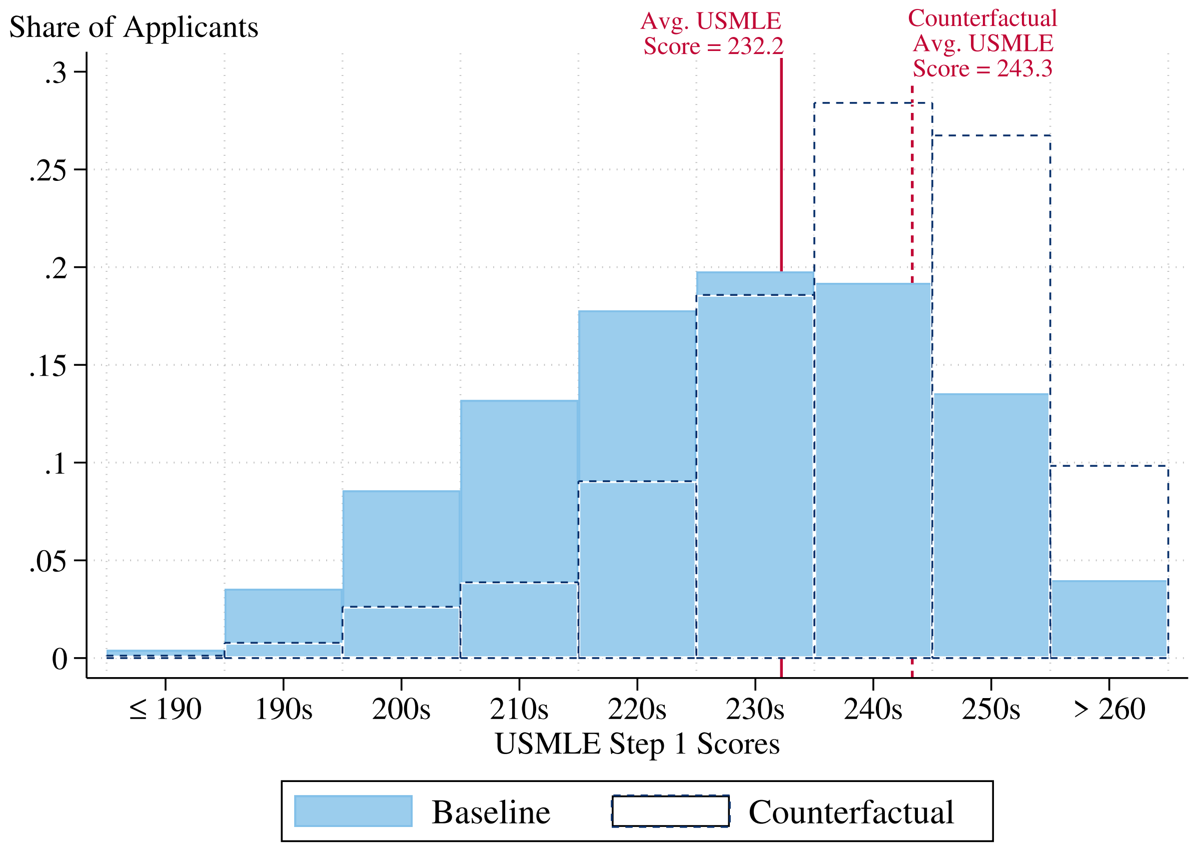
Notes: The figure shows the proportion of the population under 65 years old without health insurance in 2013 (Panel A), and the change in that proportion from 2013 to 2017 (Panel B, shown as change in the rate of insured) for counties that are included in our analyses of the effects of the ACA's expansion in Section 3.2. These counties are located in states that expanded Medicaid in 2014 and 2015, *i.e.* simultaneous with the rollout of ACA Individual Health Insurance Marketplaces. Appendix C.2 provides the list of states and expansion dates. Rate of insurance data is based on U.S. Census Bureau's Small Area Health Insurance Estimates. Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

Figure E.12: **Implicit Subsidy vs. Log Median Household Income**



Notes: This figure plots the relationship between our measure of the implicit geographic subsidy for physician services and CZ level log median household income in 2016. Median household income is as reported in Chetty et al. (2014). The implicit subsidy is calculated as the difference (in logs) between local input costs, measured using a local price index from Diamond and Moretti (2021), and the degree to which Medicare adjusts for those costs, measured using the Medicare Geographic Adjustment Factor (GAF) for physician care. The GAF is a factor that multiplies Medicare reimbursement rates; when this adjustment overestimates local production costs, rural areas are effectively subsidized (GAO, 2022). The figure is a binned scatterplot, where R^2 and the line of best fit are from a bivariate OLS regression on the underlying data points. The regression estimates are reported in Table E.10. Disclosure Review Board approval CBDRB-FY24-0456.

Figure E.13: **Increase Internal Medicine Income to Dermatology Level (2SLS)**



Notes: This figure reports the results of a counterfactual analysis in which we set the mean hourly income in internal medicine to equal the mean hourly income in dermatology. Counterfactual choices are predicted using the 2SLS version of the specialty choice model in equation (9). We first compute predicted choices within each USMLE score group and then re-normalize the data to plot the share of each USMLE score group within one specialty—internal medicine. Disclosure Review Board Approval CBDRB-FY24-0456.

Table E.1: **Definition of Specialty Categories**

Specialty Category	Medicare Specialty Code	Medicare Specialty Description
1	**Primary Care**	
	1	General Practice
	8	Family Practice
	11	Internal Medicine
	17	Hospice and Palliative Care
	23	Sports Medicine
	26	Psychiatry
	37	Pediatric Medicine
	38	Geriatric Medicine
	72	Pain Management
	79	Addiction Medicine
	84	Preventive Medicine
	C0	Sleep Medicine
2	**Medicine Subspecialty**	
	3	Allergy/Immunology
	6	Cardiovascular Disease (Cardiology)
	10	Gastroenterology
	21	Clinical Cardiac Electrophysiology
	29	Pulmonary Disease
	39	Nephrology
	44	Infectious Disease
	46	Endocrinology
	66	Rheumatology
	81	Critical Care (Intensivists)
	82	Hematology
	83	Hematology-Oncology
	90	Medical Oncology
	91	Surgical Oncology
	C3	Interventional Cardiology
	C7	Advanced Heart Failure and Transplant Cardiology
	Undefined	Genetics
	Undefined	Hypertension Specialist
	Undefined	Phlebology
3	**Obstetrics & Gynecology**	
	16	Obstetrics & Gynecology
	98	Gynecological Oncology

(... ... continued on next page...)

Specialty Category	Medicare Specialty Code	Medicare Specialty Description
(...	...	...continued from previous page)
4	**Surgery**	
	2	General Surgery
	14	Neurosurgery
	20	Orthopedic Surgery
	24	Plastic and Reconstructive Surgery
	28	Colorectal Surgery (Proctology)
	33	Thoracic Surgery
	40	Hand Surgery
	76	Peripheral Vascular Disease
	78	Cardiac Surgery
	85	Maxillofacial Surgery
5	**Procedural Specialties**	
	4	Otolaryngology
	7	Dermatology
	18	Ophthalmology
	34	Urology
6	**Hospital-Based**	
	22	Pathology
	25	Physical Medicine and Rehabilitation
	93	Emergency Medicine
	C6	Hospitalist
	Undefined	Pharmacology, Back Office
7	**Anesthesiology**	
	5	Anesthesiology
	9	Interventional Pain Management
8	**Radiology**	
	30	Diagnostic Radiology
	36	Nuclear Medicine
	92	Radiation Oncology
	94	Interventional Radiology
9	**Neurology**	
	12	Osteopathic Manipulative Medicine
	13	Neurology
	86	Neuropsychiatry
	Undefined	Electrodiagnostic Medicine

Notes: Mapping from Medicare Specialty Codes defined by Centers for Medicare and Medicaid Services to nine aggregate specialty categories. The mapping was constructed by the authors.

Table E.2: **Descriptive Variation in Earnings**

	Dependent Variable: Log Individual Total Income										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Female		-0.35								-0.22	-0.22
		(0.002)								(0.002)	(0.002)
Married		0.24								0.16	0.14
		(0.003)								(0.002)	(0.003)
Non-U.S.-Born		-0.05								-0.03	-0.00
		(0.004)								(0.003)	(0.006)
White		0.04								0.03	0.04
		(0.003)								(0.002)	(0.003)
Business Inc. > $25K						0.43			0.51	0.47	0.38
						(0.002)			(0.002)	(0.002)	(0.002)
Graduated from Top-5 Medical School							0.11	0.02			0.04
							(0.005)	(0.005)			(0.004)
N	829,000	807,000	829,000	829,000	817,000	829,000	441,000	441,000	817,000	795,000	427,000
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Medicare Specialty Fixed Effects	No	No	Yes	No	No	No	No	Yes	Yes	Yes	Yes
Commuting Zone Fixed Effects.	No	No	No	Yes	No	No	No	No	Yes	Yes	Yes
Firm Size Fixed Effects	No	No	No	No	Yes	No	No	No	Yes	Yes	Yes
Birth State Fixed Effects	No	Yes	No	No	No	No	No	No	No	Yes	Yes
R^2	0.14	0.19	0.22	0.14	0.19	0.18	0.07	0.20	0.34	0.37	0.34

Notes: This table reports coefficient estimates and R^2 from cross-sectional OLS regressions of 2017 log individual total income of physicians (age 20 to 70) on their individual-level observables. The sample size differs from that in column (2) of Table 1 because we exclude individuals with zero or negative individual total income. For each regression we report only selected point estimates. All columns include age fixed effects. Column (2) includes all demographic variables: indicators for being female, married, and non-U.S. born (alien history), and White. Column (3) shows the explanatory power of Medicare Specialty fixed effects. Column (4) shows the explanatory power of commuting zone (CZ) fixed effects. Column (5) shows the explanatory power of firm size, which is discretized into size 1, 2, 3, 4, 5, 6 to 25, 26 to 45, 46 to 100, 101 to 400, and greater than 401. Column (6) separately includes an indicator for having more than $25,000 in business income as defined in Appendix B.2. Column (7) includes an indicator for having graduates from one of top-5 MD programs according to U.S. News and World Report, while column (8) adds Medicare specialty fixed effects to this specification. Column (9) includes all career choice variables jointly: Medicare specialty fixed effects, discretized firm size fixed effects, CZ fixed effects and indicator for having more than $25,000 in business income. Column (10) includes all variables except top-5 MD indicator that is only available for a subsample as shown in column (11). Section 1 and Appendix B.2 provide details on data sources and measurement of each variable. Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024, CBDRB-FY24-0456

Table E.3: **Comparison of Tax and ACS Data**

		Mean Individual Total Income	
Sample	N	Tax Data	ACS Data
2017 Tax Sample	848,000	$350,000	N/A
2017 Tax Sample ∩ 2017 ACS	14,000	$363,500	$234,700
2017 Tax Sample ∩ 2017 ACS ∩ report being a physician in 2017 ACS	11,500	$365,400	$258,100
2017 Tax Sample ∩ 2017 ACS ∩ do not report being a physician in 2017 ACS	2,500	$354,500	$128,600

Notes: The table compares average individual total income computed in tax data to the analogues of this income measure computed from self-reported income variables in ACS. The samples are as defined in the table, starting with our full sample in 2017 (sample in column 2 of Table 1). Individual total income in the tax data is defined as the sum of W-2 wages (including deferred contribution) and the residual of AGI net of household wages and social security income. ACS income is the sum of wages, self-employment income of the index physician, and self-employment income of the spouse as reported in the survey. We discount non-wage incomes of index physicians whose spouse is also a physician by 50%. Appendix B.2 provides more details on the definition of all income measures. Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

Table E.4: **Comparison of Tax and ACS Data by Income Type**

			Business Income			
	Wage | Wage > 0		Unconditional		| Business Inc. > 0	
	(1) Tax	(2) ACS	(3) Tax	(4) ACS	(5) Tax	(6) ACS
Mean Income	$289,400	$257,300	$79,470	$21,480	$153,700	$112,900
N	10,000	10,500	11,500	11,500	6,800	2,200

Notes: The table compares average wage and business income computed in tax data and ACS data among physicians in our baseline sample, who also appear in 2017 ACS and report being a physician in 2017 ACS ($N = 11,500$). Columns (1) and (2) report mean wages among those physicians who had strictly positive wages. Columns (3) and (4) report mean business income. Column (5) and (6) report mean business income, conditional on having strictly positive business income. Appendix B.2 provides more details on the definition of all income measures. Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

Table E.5: **Summary Statistics by Specialty Category**

		Individual Total Income (2017 $)		Wage Income (2017 $)		Adjusted Gross Income (2017 $)		Share in Top 1%	
		(1) 2005	(2) 2017	(3) 2005	(4) 2017	(5) 2005	(6) 2017	(7) 2005	(8) 2017
Anesthesiology	Mean	452,000	463,300	334,900	335,800	517,100	544,100	0.42	0.38
	Median	403,700	407,400	339,200	350,200	442,000	453,700	-	-
Hospital-Based	Mean	315,600	362,200	229,500	252,900	375,200	444,900	0.19	0.24
	Median	267,400	314,100	217,900	254,800	302,900	367,200	-	-
Medicine Subspecialty	Mean	525,600	488,500	355,400	361,000	602,200	593,800	0.45	0.47
	Median	379,400	399,700	273,200	312,000	446,500	491,600	-	-
Neurology	Mean	277,600	310,700	179,600	226,200	351,900	406,200	0.16	0.20
	Median	216,400	262,900	169,200	217,100	266,300	330,600	-	-
OB-GYN	Mean	379,600	412,100	259,400	291,100	458,500	536,700	0.31	0.34
	Median	311,700	333,900	246,200	278,300	366,600	413,800	-	-
Primary Care	Mean	249,200	282,300	156,400	201,200	302,700	381,900	0.11	0.16
	Median	193,600	235,300	155,000	198,000	235,800	298,900	-	-
Procedural Specialties	Mean	562,500	635,700	337,800	378,400	647,900	763,200	0.50	0.56
	Median	422,000	470,100	281,500	327,500	489,300	564,200	-	-
Radiology	Mean	609,400	561,600	451,000	402,300	681,900	657,300	0.64	0.55
	Median	535,000	481,300	438,600	400,500	585,300	545,800	-	-
Surgery	Mean	579,500	658,000	402,800	477,100	631,000	730,400	0.50	0.57
	Median	451,600	522,400	345,400	419,700	493,000	582,400	-	-

Notes: This table reports mean and median of physician individual total income, wage income, AGI, and share in the top 1% of the national income distribution, by specialty category. We include physicians aged 40 to 55 in years 2005 and 2017. Specialty categories are aggregated from Medicare specialties as defined in Table E.1. All dollar-denominated values are inflation-adjusted to 2017 dollars. Section 1 and Appendix B.2 provide more details on data sources and measurement of each variable. Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

Table E.6: **Decomposition of Variation in Earnings**

	No Covariates			Two-Way Fixed Effects With Covariates	
	Two-Way Fixed Effects (1)	Homoskedastic Bias Correction (2)	Heteroskedastic Bias Correction (3)	Individual-Level (4)	CZ-Level / Firm-Level (5)
Panel A: Physicans					
Location Effect: $\text{Var}(\psi_c)$	0.016	0.012	0.011	0.014	0.071
Sorting Effect: $2 \times \text{Cov}(\alpha_i, \psi_c)$	-0.013	-0.008	-0.007	-0.011	-0.086
Panel B: Lawyers					
Location Effect: $\text{Var}(\psi_c)$	0.034	0.013	0.003	0.041	0.325
Sorting Effect: $2 \times \text{Cov}(\alpha_i, \psi_c)$	-0.007	0.024	0.039	-0.015	-0.453
Panel C: Firms					
Location Effect: $\text{Var}(\psi_c)$	0.088	0.073	0.073	0.070	0.184
Sorting Effect: $2 \times \text{Cov}(\alpha_i, \psi_c)$	-0.047	-0.025	-0.025	-0.025	-0.076

Notes: This table reports elements of variance decomposition of individual total income among 40-to-55-year-old physicians (Panels A and C) and lawyers (Panel B) in the sample of movers (see definition in Figure 4.) Estimates are based on equation (2). The outcome variable is log individual total income. The independent variables include physician, commuting zone (Panel A and B) or firm (Panel C), as well as relative year and age fixed effects (in columns 4 and 5 only). The variation in location effects, $\text{Var}(\psi_c)$, is computed as the variance of estimated CZ fixed effects. The effect of sorting of people to locations, $\text{Cov}(\alpha_i, \psi_c)$, is computed as the covariance of individual and CZ fixed effect estimates. Column (1) reports the result of a two-way fixed effect decomposition in equation (2) with no covariates. Columns (2) and (3) report homoskedastic and heteroskedastic corrections of the same specifications based on Andrews et al. (2008) and Kline et al. (2020), respectively, as implemented in Bonhomme et al. (2023). Column (4) reports the results based on estimating equation (2) with a full set of covariates. Column (5) aggregates person-level fixed effects to CZ means before computing the variance decomposition terms, following Card et al. (2021). Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

Table E.7: **Decomposition of Variation in Earnings in Subsamples**

	All Physicians						
	Without Covariates	With Covariates	Primary Care Physicians	Specialists	Graduates of Ranked MD Program	Graduates of Non-Ranked MD Program	Lawyers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Person-Level Variance Decomposition							
Location Effect: $\text{Var}(\psi_c)$	0.016	0.014	0.018	0.020	0.017	0.022	0.041
Sorting Effect: $2 \times \text{Cov}(\alpha_i, \psi_c)$	-0.014	-0.011	-0.019	-0.013	-0.017	-0.020	-0.015
Panel B: CZ-Level Variance Decomposition							
Location Effect: $\text{Var}(\psi_c)$	0.067	0.071	0.081	0.157	0.121	0.075	0.325
Sorting Effect: $2 \times \text{Cov}(\alpha_i, \psi_c)$	-0.083	-0.086	-0.092	-0.225	-0.132	-0.095	-0.453

Notes: This table reports elements of variance decomposition of total income among 40-to-55-year-old physicians, overall (in columns 1 and 2) and by subsamples as indicated in column names (columns 3 to 7). Estimates are based on equation (2). The outcome variable is log individual total income. The independent variables include physician, commuting zone, as well as relative year and age fixed effects (except for column 1). The variation in location effects, $\text{Var}(\psi_c)$, is computed as the variance of estimated CZ fixed effects. The effect of sorting of people to locations, $\text{Cov}(\alpha_i, \psi_c)$, is computed as the covariance of individual and CZ fixed effect estimates. Panel A decomposes variation in individual income. Panel B decomposes variation across CZs—we aggregate person-level fixed effects to CZ means before computing the variance decomposition terms, following Card et al. (2021). Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

Table E.8: **Correlates of Place Effects**

	(1) CZ Log Income	(2) CZ Fixed Effect
Log Population	0.031	-0.060
	(0.013)	(0.014)
Population Density	-0.002	-0.040
	(0.004)	(0.015)
Diamond and Moretti (2021) Price Index	-0.019	-0.065
	(0.008)	(0.009)
Median Household Income in 2016 (vs. Physician Income FE)	0.025	-0.029
	(0.009)	(0.009)
Median Household Income in 2016 (vs. Lawyer Income FE)	0.131	0.024
	(0.026)	(0.022)
Rural Index, 2013	-0.033	0.053
	(0.012)	(0.013)
Share College Graduates	0.005	-0.062
	(0.008)	(0.009)
Job Growth Rate 1990-2010	0.012	-0.048
	(0.010)	(0.011)
Median House Value	0.004	-0.065
	(0.009)	(0.015)
Life Expectancy	-0.018	-0.021
	(0.007)	(0.008)
Finkelstein et al. (2021) Mortality Treatment Effect	0.006	-0.014
	(0.008)	(0.007)
Total Number of Physicans (2005-2017)	-0.006	-0.045
	(0.003)	(0.008)
Number of PCPs per 100,000	-0.005	-0.018
	(0.013)	(0.016)
Number of Non-PCPs per 100,000	0.021	-0.057
	(0.008)	(0.011)
Number of Medicaid Eligible per 100,000	-0.034	0.015
	(0.012)	(0.014)
Number of Medicare Eligible per 100,000	-0.013	0.000
	(0.010)	(0.011)
Share Uninsured	-0.051	-0.012
	(0.014)	(0.016)

Notes: This table reports the results of bivariate OLS regressions of raw average individual total income in a commuting zone (column 1), as well as of place treatment effect on earnings (column 2), on z-scores of the place characteristics indicated in rows. Place treatment effects on earnings are CZ fixed effects from the estimation of equation (2) in the sample of movers as described in Section 2.3.2. Raw mean income is computed in the same sample. CZ-level characteristics are as reported in Chetty et al. (2014); Finkelstein et al. (2021); Diamond and Moretti (2021). Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

Table E.9: **ACA 2SLS Regressions (Excluding 2010–2013)**

Dependent variable:	(1) Log Income	(2) Share with Schedule SE	(3) Share Retired
Share Insured ($I_{c,t}$)	0.412	0.408	-0.099
	(0.111)	(0.087)	(0.032)
Mean of Dependent Variable	12.520	0.429	0.101
Std. Dev. of Dependent Variable	0.896	0.495	0.301
Mean of Independent Variable	0.888	0.887	0.888
Std. Dev. of Independent Variable	0.050	0.050	0.050
Number of Observations	1,221,000	1,193,000	1,820,000
Physician Age Range	40-55	40-55	44-70

Notes: This table displays the results of a 2SLS specification that is described in Section 3.2 and in the notes to Table 4. These are instrumented parametric difference-in-differences estimates of the effects of the ACA insurance expansions on the outcomes indicated in column names, in which we treat the rate of insurance in the under-65 population as the endogenous variable of interest and the rate of uninsured population in 2013 as an instrument. This table replicates columns (5) to (7) of Table 4, except that we drop the post-ACA passage and pre-implementation period (2011-2013). Disclosure Review Board approval CBDRB-FY24-0456.

Table E.10: **Inputs to Analysis in Section 4.1**

	(1) Log $\left(\frac{\text{GAF}}{\text{Price Index}}\right)$	(2) Log GAF	(3) Diamond and Moretti (2021) Price Index	(4) CZ Fixed Effects Physicians
Log Median Household Income	-0.23	0.09	0.33	-0.13
	(0.01)	(0.01)	(0.02)	(0.05)
Constant	2.44	-1.07	-3.57	1.52
	(0.13)	(0.10)	(0.19)	(0.49)
N	500	700	500	700

Notes: Column (1) of this table reports the regression estimate of the bivariate relationship between Medicare's implicit subsidy and the level of CZ earnings graphed in Figure E.12. CZ earnings are measured as 2016 log median household income, reported in Chetty et al. (2014). Columns (2) and (3) show the separate relationships between the numerator and the denominator of the implicit subsidy measure and CZ-level log median household income. In column (4), we show the bivariate relationship between our CZ fixed effects for physician earnings and log median household income. Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024, CBDRB-FY24-0456.

Table E.11: **Specialty Choice Model**

	Reduced Form		OLS		2SLS	
	(1) Estimate	(2) Standard Error	(3) Estimate	(4) Standard Error	(5) Estimate	(6) Standard Error
Hourly RVUs	-0.229	0.060	-	-	-	-
Hourly Income	-	-	-0.007	0.002	-0.012	0.005
Hourly RVUs/Income × USMLE Score						
× ≤ 190	Reference	-	Reference	-	Reference	-
× 191-200	-0.046	0.050	-0.001	0.001	-0.002	0.002
× 201-210	0.012	0.046	0.003	0.001	0.000	0.002
× 211-220	0.004	0.049	0.004	0.001	0.000	0.002
× 221-230	0.117	0.047	0.009	0.001	0.005	0.002
× 231-240	0.250	0.042	0.014	0.001	0.010	0.001
× 241-250	0.361	0.042	0.018	0.001	0.014	0.001
× 251-260	0.444	0.046	0.021	0.001	0.017	0.002
× > 260	0.516	0.052	0.024	0.001	0.020	0.002
USMLE Score Fixed Effects						
≤ 190	Reference	-	Reference	-	Reference	-
191-200	0.086	0.132	0.122	0.133	0.196	0.186
201-210	0.447	0.116	0.239	0.116	0.419	0.168
211-220	0.801	0.104	0.443	0.110	0.793	0.171
221-230	1.153	0.099	0.497	0.106	0.871	0.160
231-240	1.365	0.101	0.397	0.103	0.762	0.146
241-250	1.757	0.110	0.540	0.107	0.885	0.147
251-260	1.954	0.130	0.565	0.139	0.882	0.178
> 260	2.146	0.145	0.581	0.161	0.899	0.200
Specialty Fixed Effects						
Anesthesiology	Reference	-	Reference	-	Reference	-
Dermatology	-2.374	0.348	-2.767	0.238	-2.293	0.385
Emergency Medicine	0.167	0.072	0.228	0.095	0.025	0.150
Internal Medicine	0.574	0.147	0.656	0.144	0.273	0.266
OBY-GYN	-1.933	0.327	-1.645	0.285	-2.336	0.522
Orthopaedic Surgery	0.626	0.188	0.251	0.242	1.143	0.601
Pathology	-2.350	0.184	-2.218	0.170	-2.587	0.272
Pediatrics	-1.284	0.349	-0.921	0.326	-1.852	0.658
Physical Medicine and Rehabilitation	-2.790	0.218	-2.646	0.197	-3.179	0.367
Plastic Surgery	-1.616	0.160	-1.744	0.148	-1.485	0.214
Psychiatry	-1.892	0.243	-1.634	0.249	-2.384	0.505
Radiation Oncology	-1.955	0.241	-2.368	0.174	-1.756	0.431
Radiology	-0.464	0.132	-0.598	0.125	-0.323	0.212
Surgery	0.601	0.132	0.491	0.095	0.595	0.117
Specialty Characteristics						
Std. Dev. Hourly Income*	0.002	0.006	-0.004	0.006	0.014	0.013
Mean Employer Size*	0.360	0.161	0.350	0.105	0.371	0.107
Share Female	5.838	0.983	5.287	0.733	6.674	1.129
N	750	750	750	750	750	750
					First Stage	
Choice Model Medicare Price Instrument	-	-	-	-	25.880	1.294
N	-	-	-	-	80	80

Notes: The estimates are based on the discrete choice model specified in equation (9). This regression is estimated on group data at the USMLE Step 1 score group by year by specialty level. For each USMLE Step 1 group and year, the outcome variable is the difference in the log probability of choosing an index specialty and log probability of choosing family medicine, which is the reference specialty in the model. For 2SLS estimates in columns (5) and (6) we report the results of an example first stage for one of the interaction terms. For variables indicated by an asterisk (*), the coefficient and standard error have been multiplied by 1,000 to improve readability. Disclosure Review Board approval CBDRB-FY24-0456.

Table E.12: **Own and Cross-Income Elasticities From Specialty Choice Model: Reduced Form**

	USMLE Score > 260		USMLE Score 251-260		USMLE Score 241-250		USMLE Score 231-240		USMLE Score 221-230		USMLE Score 211-220		USMLE Score 201-210		USMLE Score 191-200		USMLE Score ≤ 190	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
	$\epsilon^{\text{Inc.}}_{i,j}$ $(i=j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i\neq j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i=j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i\neq j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i=j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i\neq j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i=j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i\neq j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i=j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i\neq j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i=j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i\neq j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i=j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i\neq j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i=j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i\neq j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i=j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i\neq j)$
Anesthesiology	0.337	-0.010	0.248	-0.011	0.149	-0.010	0.023	-0.002	-0.125	0.011	-0.255	0.019	-0.252	0.013	-0.319	0.014	-0.249	0.029
Dermatology	1.729	-0.141	1.293	-0.104	0.817	-0.040	0.129	-0.003	-0.730	0.005	-1.466	0.007	-1.412	0.009	-1.786	0.008	-1.470	0.026
Emergency Medicine	0.366	-0.020	0.265	-0.024	0.161	-0.016	0.024	-0.003	-0.134	0.018	-0.274	0.030	-0.269	0.025	-0.350	0.021	-0.304	0.005
Family Medicine	0.415	-0.011	0.310	-0.008	0.188	-0.008	0.028	-0.002	-0.152	0.016	-0.293	0.042	-0.270	0.054	-0.303	0.105	-0.263	0.078
Internal Medicine	0.616	-0.182	0.457	-0.139	0.283	-0.083	0.044	-0.013	-0.245	0.069	-0.488	0.141	-0.469	0.138	-0.624	0.142	-0.482	0.157
OB-GYN	0.142	-0.003	0.104	-0.003	0.063	-0.003	0.010	-0.001	-0.052	0.005	-0.104	0.010	-0.101	0.009	-0.127	0.012	-0.111	0.004
Orthopaedic Surgery	0.535	-0.082	0.411	-0.050	0.258	-0.024	0.041	-0.002	-0.237	0.006	-0.480	0.006	-0.465	0.003	-0.587	0.005	-0.485	0.009
Pathology	0.369	-0.010	0.278	-0.005	0.171	-0.002	0.026	-0.000	-0.147	0.002	-0.294	0.004	-0.283	0.005	-0.357	0.006	-0.298	0.005
Pediatrics	0.218	-0.025	0.164	-0.018	0.097	-0.015	0.015	-0.002	-0.081	0.015	-0.162	0.030	-0.153	0.032	-0.200	0.034	-0.167	0.027
Physical Medicine and Rehabilitation	0.717	-0.001	0.534	-0.003	0.326	-0.003	0.050	-0.001	-0.278	0.004	-0.552	0.014	-0.535	0.011	-0.669	0.020	-0.565	0.010
Plastic Surgery	0.482	-0.018	0.363	-0.011	0.225	-0.004	0.035	-0.000	-0.196	0.001	-0.393	0.001	-0.380	0.001	-0.479	0.001	-0.393	0.007
Psychiatry	0.450	-0.008	0.333	-0.009	0.205	-0.005	0.031	-0.001	-0.170	0.011	-0.334	0.027	-0.315	0.034	-0.388	0.052	-0.341	0.026
Radiation Oncology	1.422	-0.049	1.073	-0.026	0.662	-0.013	0.103	-0.001	-0.576	0.003	-1.156	0.002	-1.115	0.003	-1.410	0.002	-1.156	0.021
Radiology	0.554	-0.030	0.406	-0.030	0.252	-0.016	0.039	-0.002	-0.222	0.007	-0.450	0.010	-0.435	0.009	-0.553	0.007	-0.451	0.016
Surgery	0.647	-0.130	0.505	-0.076	0.314	-0.042	0.049	-0.006	-0.278	0.028	-0.563	0.049	-0.561	0.030	-0.718	0.027	-0.611	0.011

Notes: This table presents own- and cross-income elasticities of specialty choice probability computed based on the reduced form version of the discrete choice model specified in equation (9). Table E.11 reports the full set of estimates for this specification. The own-income elasticity (reported in odd-numbered columns) for a specialty i within a score group a is computed as the product of the coefficient on RVUs term for this score group, δ_a, the mean hourly RVUs in specialty i, and 1 minus the share of physicians in score group a who chose specialty i. The cross-income elasticity (reported in even-numbered columns) for a specialty i vis-à-vis RVUs in specialty j is computed as -1 times the product of the coefficient on RVUs term for this score group, δ_a, the mean hourly RVUs in specialty j, and the share of physicians in score group a who chose specialty j. Mean hourly RVUs and observed choice shares are at 2016 levels (the last year of NRMP data). Disclosure Review Board approval CBDRB-FY24-0456.

Table E.13: **Own and Cross-Income Elasticities From Specialty Choice Model: OLS**

	USMLE Score > 260		USMLE Score 251-260		USMLE Score 241-250		USMLE Score 231-240		USMLE Score 221-230		USMLE Score 211-220		USMLE Score 201-210		USMLE Score 191-200		USMLE Score $\leq$ 190	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
	$\epsilon^{\text{Inc.}}_{i,j}$ $(i=j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i \neq j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i=j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i \neq j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i=j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i \neq j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i=j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i \neq j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i=j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i \neq j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i=j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i \neq j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i=j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i \neq j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i=j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i \neq j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i=j)$	$\epsilon^{\text{Inc.}}_{i,j}$ $(i \neq j)$
Anesthesiology	2.686	-0.083	2.188	-0.100	1.681	-0.115	1.016	-0.082	0.216	-0.020	-0.525	0.039	-0.808	0.041	-1.378	0.062	-1.150	0.135
Dermatology	4.182	-0.341	3.458	-0.278	2.796	-0.136	1.759	-0.034	0.382	-0.003	-0.917	0.004	-1.378	0.009	-2.340	0.011	-2.062	0.037
Emergency Medicine	2.561	-0.140	2.046	-0.185	1.591	-0.160	0.948	-0.123	0.202	-0.027	-0.495	0.055	-0.758	0.070	-1.324	0.080	-1.231	0.022
Family Medicine	1.568	-0.040	1.294	-0.035	1.002	-0.041	0.598	-0.040	0.124	-0.013	-0.286	0.041	-0.411	0.082	-0.621	0.215	-0.576	0.170
Internal Medicine	1.787	-0.527	1.466	-0.445	1.161	-0.339	0.710	-0.208	0.154	-0.043	-0.366	0.106	-0.549	0.161	-0.979	0.223	-0.810	0.264
OB-GYN	2.253	-0.042	1.835	-0.061	1.411	-0.077	0.850	-0.060	0.179	-0.016	-0.426	0.041	-0.647	0.057	-1.092	0.101	-1.027	0.037
Orthopaedic Surgery	3.894	-0.594	3.304	-0.403	2.658	-0.252	1.689	-0.091	0.372	-0.009	-0.903	0.011	-1.367	0.010	-2.315	0.018	-2.046	0.037
Pathology	2.311	-0.060	1.927	-0.032	1.518	-0.020	0.927	-0.013	0.199	-0.003	-0.476	0.007	-0.714	0.013	-1.212	0.021	-1.081	0.019
Pediatrics	1.608	-0.186	1.335	-0.148	1.007	-0.156	0.611	-0.101	0.129	-0.023	-0.308	0.057	-0.456	0.094	-0.798	0.134	-0.716	0.117
Physical Medicine and Rehabilitation	2.063	-0.003	1.697	-0.009	1.327	-0.012	0.809	-0.010	0.173	-0.003	-0.410	0.011	-0.620	0.013	-1.043	0.031	-0.942	0.017
Plastic Surgery	2.909	-0.111	2.425	-0.070	1.925	-0.033	1.191	-0.007	0.256	-0.001	-0.613	0.002	-0.925	0.001	-1.567	0.002	-1.377	0.025
Psychiatry	1.844	-0.035	1.511	-0.041	1.187	-0.031	0.714	-0.031	0.150	-0.009	-0.354	0.028	-0.520	0.056	-0.861	0.115	-0.810	0.061
Radiation Oncology	4.055	-0.141	3.384	-0.082	2.669	-0.052	1.652	-0.012	0.355	-0.002	-0.853	0.002	-1.284	0.003	-2.178	0.003	-1.913	0.034
Radiology	2.982	-0.163	2.416	-0.182	1.919	-0.120	1.185	-0.062	0.259	-0.009	-0.627	0.013	-0.945	0.019	-1.615	0.020	-1.408	0.051
Surgery	2.507	-0.504	2.164	-0.324	1.724	-0.229	1.060	-0.134	0.233	-0.023	-0.564	0.049	-0.877	0.046	-1.508	0.058	-1.373	0.025

Notes: This table presents own- and cross-income elasticities of specialty choice probability computed based on the OLS version of the specialty choice model specified in equation (9). Table E.11 reports the full set of estimates for this specification. The own-income elasticity (reported in odd-numbered columns) for a specialty i within a score group a is computed as the product of the coefficient on income term for this score group, δ_a, the mean hourly income in specialty i, and 1 minus the share of physicians in score group a who chose specialty i. The cross-income elasticity (reported in even-numbered columns) for a specialty i vis-à-vis income in specialty j is computed as -1 times the product of the coefficient on income term for this score group, δ_a, the mean hourly income in specialty j, and the share of physicians in score group a who chose specialty j. Mean hourly income and observed choice shares are at 2016 levels (the last year of NRMP data). Disclosure Review Board approval CBDRB-FY24-0456.

Table E.14: **Own and Cross-Income Elasticities From Specialty Choice Model: 2SLS**

	USMLE Score > 260		USMLE Score 251-260		USMLE Score 241-250		USMLE Score 231-240		USMLE Score 221-230		USMLE Score 211-220		USMLE Score 201-210		USMLE Score 191-200		USMLE Score $\leq$ 190	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
	$\epsilon_{i,j}^{\text{Inc.}}$ $(i = j)$	$\epsilon_{i,j}^{\text{Inc.}}$ $(i \neq j)$	$\epsilon_{i,j}^{\text{Inc.}}$ $(i = j)$	$\epsilon_{i,j}^{\text{Inc.}}$ $(i \neq j)$	$\epsilon_{i,j}^{\text{Inc.}}$ $(i = j)$	$\epsilon_{i,j}^{\text{Inc.}}$ $(i \neq j)$	$\epsilon_{i,j}^{\text{Inc.}}$ $(i = j)$	$\epsilon_{i,j}^{\text{Inc.}}$ $(i \neq j)$	$\epsilon_{i,j}^{\text{Inc.}}$ $(i = j)$	$\epsilon_{i,j}^{\text{Inc.}}$ $(i \neq j)$	$\epsilon_{i,j}^{\text{Inc.}}$ $(i = j)$	$\epsilon_{i,j}^{\text{Inc.}}$ $(i \neq j)$	$\epsilon_{i,j}^{\text{Inc.}}$ $(i = j)$	$\epsilon_{i,j}^{\text{Inc.}}$ $(i \neq j)$	$\epsilon_{i,j}^{\text{Inc.}}$ $(i = j)$	$\epsilon_{i,j}^{\text{Inc.}}$ $(i \neq j)$	$\epsilon_{i,j}^{\text{Inc.}}$ $(i = j)$	$\epsilon_{i,j}^{\text{Inc.}}$ $(i \neq j)$
Anesthesiology	1.351	-0.042	0.873	-0.040	0.341	-0.023	-0.345	0.028	-1.149	0.105	-1.866	0.138	-1.857	0.094	-2.230	0.100	-1.813	0.213
Dermatology	2.104	-0.171	1.379	-0.111	0.567	-0.028	-0.597	0.012	-2.033	0.015	-3.256	0.015	-3.166	0.020	-3.788	0.018	-3.252	0.058
Emergency Medicine	1.289	-0.071	0.816	-0.074	0.323	-0.033	-0.322	0.042	-1.077	0.145	-1.759	0.194	-1.742	0.161	-2.144	0.129	-1.942	0.035
Family Medicine	0.789	-0.020	0.516	-0.014	0.203	-0.008	-0.203	0.014	-0.659	0.069	-1.016	0.147	-0.944	0.189	-1.005	0.349	-0.909	0.269
Internal Medicine	0.899	-0.265	0.585	-0.177	0.236	-0.069	-0.241	0.070	-0.818	0.229	-1.298	0.375	-1.260	0.370	-1.585	0.361	-1.277	0.416
OB-GYN	1.133	-0.021	0.732	-0.024	0.286	-0.016	-0.288	0.020	-0.954	0.085	-1.513	0.147	-1.486	0.130	-1.768	0.163	-1.620	0.059
Orthopaedic Surgery	1.959	-0.299	1.318	-0.161	0.539	-0.051	-0.573	0.031	-1.981	0.051	-3.207	0.039	-3.138	0.022	-3.746	0.029	-3.226	0.058
Pathology	1.163	-0.030	0.769	-0.013	0.308	-0.004	-0.315	0.004	-1.060	0.014	-1.689	0.026	-1.640	0.030	-1.961	0.034	-1.705	0.030
Pediatrics	0.809	-0.094	0.533	-0.059	0.204	-0.032	-0.207	0.034	-0.689	0.124	-1.095	0.203	-1.048	0.216	-1.292	0.218	-1.129	0.184
Physical Medicine and Rehabilitation	1.038	-0.002	0.677	-0.004	0.269	-0.003	-0.274	0.004	-0.922	0.014	-1.456	0.038	-1.425	0.030	-1.688	0.051	-1.485	0.027
Plastic Surgery	1.464	-0.056	0.967	-0.028	0.391	-0.007	-0.404	0.002	-1.362	0.005	-2.179	0.005	-2.124	0.003	-2.537	0.004	-2.171	0.039
Psychiatry	0.928	-0.017	0.603	-0.016	0.241	-0.006	-0.242	0.011	-0.800	0.050	-1.258	0.101	-1.195	0.128	-1.394	0.187	-1.278	0.096
Radiation Oncology	2.041	-0.071	1.350	-0.033	0.542	-0.010	-0.561	0.004	-1.890	0.010	-3.029	0.006	-2.948	0.007	-3.525	0.005	-3.017	0.054
Radiology	1.500	-0.082	0.964	-0.072	0.389	-0.024	-0.402	0.021	-1.378	0.046	-2.227	0.048	-2.171	0.045	-2.614	0.032	-2.221	0.081
Surgery	1.262	-0.254	0.863	-0.129	0.350	-0.047	-0.360	0.046	-1.241	0.123	-2.004	0.175	-2.014	0.107	-2.441	0.093	-2.165	0.039

Notes: This table presents own- and cross-income elasticities of specialty choice probability computed based on the 2SLS version of the specialty choice model specified in equation (9). Table E.11 reports the full set of estimates for this specification. The own-income elasticity (reported in odd-numbered columns) for a specialty i within a score group a is computed as the product of the coefficient on income term for this score group, δ_a, the mean hourly income in specialty i, and 1 minus the share of physicians in score group a who chose specialty i. The cross-income elasticity (reported in even-numbered columns) for a specialty i vis-à-vis income in specialty j is computed as -1 times the product of the coefficient on income term for this score group, δ_a, the mean hourly income in specialty j, and the share of physicians in score group a who chose specialty j. Mean hourly income and observed choice shares are at 2016 levels (the last year of NRMP data). Disclosure Review Board approval CBDRB-FY24-0456.

Table E.15: **Lifetime Earnings of Physicians and Lawyers**

	(1) All Physicians	(2) Primary Care Physicians	(3) Lawyers
Mean PDV Lifetime Income ($\beta = 0.97$, at Age 20)	\$10,100,000	\$6,500,000	\$7,100,000
Undergrad & Graduate Tuition	\$250,688	\$250,688	\$186,273
PDV Lifetime Income Net of Tuition	\$9,849,312	\$6,249,312	\$6,913,727
Relative to Lawyers	142%	90%	100%
Mean Lifetime Hours Worked	112,900	108,700	105,500
Relative to Lawyers	107%	103%	100%
Higher Weight for Hours >40 / Week	112%	106%	100%

Notes: This table reports our estimates of absolute and relative lifetime earnings between physicians and lawyers. The present discounted value (PDV) of earnings from age 20 to 70 is computed based on simulations described in Appendix B.2. Undergraduate and graduate tuition costs were obtained from the Association of American Medical Colleges and the American Bar Association as also detailed in Appendix B.2. Average annual hours worked are computed by multiplying weekly hours worked by the number of weeks worked reported in ACS. Annual hours worked are averaged within each year of age, and then summed across ages to obtain lifetime hours worked. The final row uses an adjusted work hours measure, which increases the weights for hours worked over 40 per week based on the return to weekly hours worked estimated in Goldin (2014, Table 3, column 5). Disclosure Review Board approval CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

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