THE EARNINGS AND LABOR SUPPLY OF U.S. PHYSICIANS*

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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 health care policies on these earnings, physicians' labor supply, and the 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 make up 8.6% of national health care spending. Business income makes up 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.

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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. *JEL codes:* J44, I18, H51.

> *The [health care] 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[1]

> *My hand surgeon should have been paid $4.5 billion for fixing my broken wrist, not $1,000.*
>
> —Crawford[2]

I. Introduction

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 but 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. Because 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 make up 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 labor market for physicians may give it unique power to drive talent allocation of these quintessential "human capitalists"

1. Case and Deaton (2020, 193).
2. Crawford (2019).

(Smith et al. 2019). If reimbursement policies and entry barriers shape physicians' 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 long-standing 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.[3] We document the level and composition of physicians' earnings, how earnings evolve over time, and the pronounced differences across geography and specialty. We find that physicians' earnings make up 8.6% of total health care 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 physicians' 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 health care 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 changes to payment rates per service. In both cases, the government's influence is substantial.

3. 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, Chandra and Jena 2013; Altonji and Zhong 2021; Lo Sasso 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. Online Appendix A presents a survey on the public's beliefs about physicians' earnings.

In terms of physicians' earnings, a quarter of marginal revenue induced by Medicare reimbursement changes accrues to physicians personally. When health care 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 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 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.[4] This means that policies subsidizing surgery or primary care have the power to at-

4. 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

tract more top talent into those specialties, potentially changing quality of care for a generation.[5] We calculate that using taxes to replicate the same 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—especially public insurance coverage—play important roles in these earnings and in allocating talent within medicine. Put simply, the expansion of government health care 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 effects, and thus social returns, to physician ability in different specialties—currently unknown parameters. We encourage future work to estimate these to determine the welfare impact of talent allocation and hence insurance policies.

II. 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; Online Appendices B.1 and B.2 provide details.

II.A. Career in Medicine

A career in medicine is competitive and follows a relatively rigid script. Practicing physicians choose specializations early,

number of U.S. applicants continues to decline, suggesting that the concern may be about applicant quality rather than numbers per se.

5. Doyle, Ewer and Wagner (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 their estimates for cross-specialty analysis.

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 United States. 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 a medical school. 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 University admits 2.2% of applicants and Harvard University 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, 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 and vary substantially in their competitiveness and length.[6] 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 a W-2, plus a share of profits that remain after all practice expenses are met. The exact legal structure affects the tax liability and the profit-sharing incentives within the practice.

6. 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).

II.B. 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 health care providers in the United States. We augment these with additional administrative and survey data sets, detailed in Online Appendix B.1.

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.[7] 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.

2. *Physician Registry.* We merge tax data with the administrative registry of physicians (the National Plan and Provider Enumeration System, NPPES) using the Census Bureau's Protected Identification Key (PIK)-based data-linkage infrastructure. NPPES lists all physicians and their specialties.[8] We augment this with medical school name from the Centers for Medicare and Medicaid Services (CMS) Doctors and Clinicians file and the school's *U.S. News and World Report* ranking.

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

7. For ease of exposition, we refer to the EIN tax unit as a "firm" throughout.

8. Physician specialty is defined at varying levels of detail; in this article, we primarily use Medicare's specialty codes or broader aggregates that we define in Online Appendix B.1.

4. *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 the ACS between 2005 and 2017.[9] This provides self-reported earnings and work hours. We also use the ACS to construct a sample of lawyers for comparison.

5. *Medicare Data.* We add data on treatments that 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 details by physician. We add data on Medicare reimbursement rates for each service-year from the CMS Physician Fee Schedule files.

6. *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 IV.C.

II.C. Income and Retirement Definitions

Physicians' incomes come through diverse and changing mechanisms. This mishmash of sources makes it particularly challenging to study physicians' 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 pretax deferrals to retirement plans or the like;[10] individual total business income; and adjusted gross income (AGI) at the household level. We define retirement as the year in which an individual who is older than 40 first receives form 1099-SSA, Social Security Benefit Statement. Details are in Online Appendix B.2.

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

10. 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.

II.D. Sample Definitions

Our main sample is a physician-year panel from 2005 to 2017 for physicians aged 20–70. 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 I). In many of our analyses we also use two age-based subsamples: a peak-earnings sample of ages 40–55 and a high-retirement-risk sample of ages 56–70 (350,000 and 287,000 physicians, respectively, in 2017).[11]

III. Sources of Variation in Physician Earnings

III.A. Basic Facts

Table I reports summary statistics for the full sample (column (1)), the 2017 cross section (column (2)), and two age-based subsamples 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. A third of physicians have business income exceeding $25,000. At the tax-unit level, median AGI 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 Online Appendix Figure E.3). Table I reports additional characteristics of physicians and their work environments, including specialty, firm size, work hours, and medical school characteristics. We discuss these aspects in Online Appendix B.3.

We find that physicians in aggregate earn $297 billion in pretax dollars measured by total individual income, or 8.6% of total U.S. health care spending in 2017.[12] Put differently, out of $10,611 that an average American spent on health care in 2017, physicians earned $913. While billing for physicians' clinical services makes up one-fifth of spending, less than half of this amount is physicians' own pay.[13] Subtracting individual income tax pay-

11. All numbers in this article are rounded according to U.S. Census Bureau disclosure protocols.

12. Total health care spending in 2017 was $3.4 trillion according to CMS (2019, table 1).

13. 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 (Ganguli et al. 2020).

TABLE I
SUMMARY STATISTICS

	Years:	2005–2017	2017		
	Ages:	All (1)	All (2)	40–55 (3)	56–70 (4)
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 (years)	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

ments at a rate of 30% implies that physicians' total after-tax earnings is closer to 6% of total U.S. health care spending, or 1% of GDP. This puts an upper bound on the magnitudes at play in policy discussions that suggest lowering health care spending by cutting physician pay (e.g., Baker 2017). A common version

TABLE I
CONTINUED

	Years: 2005–2017	2017		
Ages:	All (1)	All (2)	40–55 (3)	56–70 (4)
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 20–70. Columns (2)–(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 United States with the universe of individual income tax return data. Section II and Online Appendix B.2 provide details on data sources and measurement of each variable. Disclosure Review Board approval no. CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

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.[14]

1. *Earnings Variation.* Average earnings mask substantial heterogeneity. More than 25% of physicians in 2017 earn more than $425,000, and the top 1% of physicians earns more than $1.7 million (Online Appendix Figure E.1).[15] Online Appendix Table E.2 asks what share of this variation relates to observable charac-

14. Chen, Persson and Polyakova (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. health care expenditures.

15. Online Appendix Figures E.1 and E.2 show the full distribution of individual earnings and AGI, respectively.

teristics. 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 the R^2 up to 0.19. Women earn 30% less than men. We consider the explanatory power of covariates that physicians have (at least some) control over throughout their careers: attending a top-five medical school, specialty, location (commuting zone), size of practice, and presence of business earnings. Specialty and firm size (statistically) explain 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, predetermined demographics and observable career attributes explain up to 37% of the observed variation.

These results highlight two facts that guide our 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 below, using two-way fixed effects to fully decompose the variation into individual- and market-specific factors.

2. *Age Profile.* Figure I, Panel A 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 (Online Appendix Figures E.4A and E.4B), but earnings remain close to \$270,000 into their late sixties. This age pattern motivates our focus on income during ages 40–55 in much of the subsequent analyses, as that age interval reflects physicians' maximum earnings period.

Figure I, Panel B 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 40–55, while

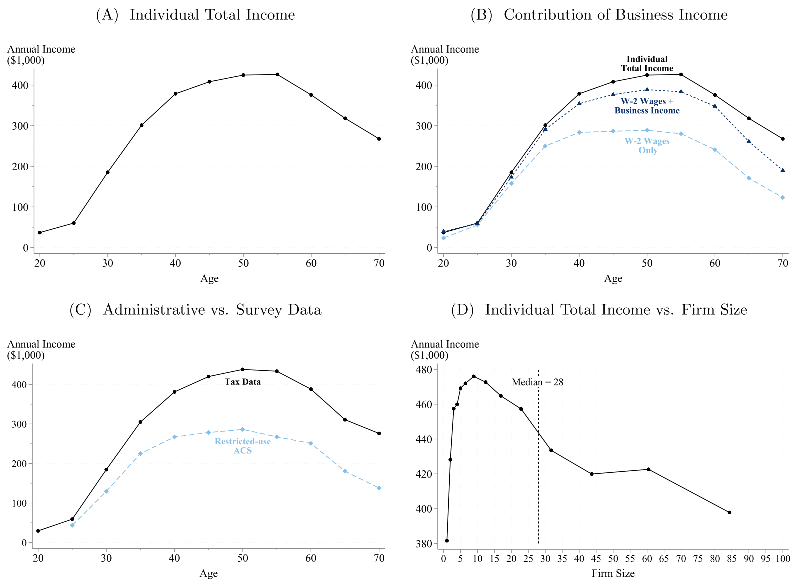

FIGURE I

Physicians' Earnings over the Life Cycle and by Firm Size

The figure plots mean individual total income in our 2017 sample of physicians by five-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 pretax 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 40–55 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. Online Appendix B.2 provides measurement details. Disclosure Review Board approval no. CBDRB-FY23-0319, CBDRB-FY2023-CES 005-024.

business income (along with the probability of filing Schedule C; see Online Appendix Figure E.4C) grows steadily and accounts for nearly a quarter of earnings at age 50.

3. *Administrative Versus Survey Data.* To highlight the differences between survey and administrative data, Figure I, Panel C 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 mean of the administrative data. 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 Online Appendix B.4)—a crucial part of physicians' earnings, as discussed above.

4. *Firm Size.* Figure I, Panel D shows the relationship between earnings and firm size among 40–55-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 firm sizes that correspond to small group practices of 8–10 physicians. Moving to larger firms, such as large physician organizations or hospitals, average earnings decline.

5. *Top Earners Among Physicians.* Table II examines the long right tail of physicians' income distribution, showing how top earners differ from average physicians. We focus on physicians age 40–55 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%.[16]

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-five medical schools and 62% less likely to work in primary care. Top earners are six times more likely to

16. 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 two throughout the top half of the physician distribution, as Gottlieb et al. (2023a) assumed previously when relating physician and nonphysician top earnings.

TABLE II

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 nonlabor		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 I), by selected percentiles of the individual total income distribution (as specified in column titles). Variables are as defined in Table I. Section II and Online Appendix B.2 provide more details on data sources and measurement of each variable. Disclosure Review Board approval no. CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

be neurosurgeons, one of the specialties with the most extensive training.

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.

III.B. The Importance of Specialty

1. *Earnings by Specialty.* Earnings vary substantially across specialties (Online Appendix Table E.5). Primary care physicians (PCPs), the most common specialty category, are also the lowest-earning. Average total individual income among 40–55-year-old physicians is $282,300 ($235,300 at the 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 PCPs 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.

2. *Correlates of Specialty Income.* Higher earnings could make one specialty more attractive than another or could represent a compensating differential. While Section IV.C 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 compensating for disamenities.

We examine how earnings differences across specialties relate to two key job amenities: working hours and training length. Figure II, Panel A shows the relationship between total individual income (ages 40–55) 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 a 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 II, Panel B shows a very strong relationship between average income in specialty and

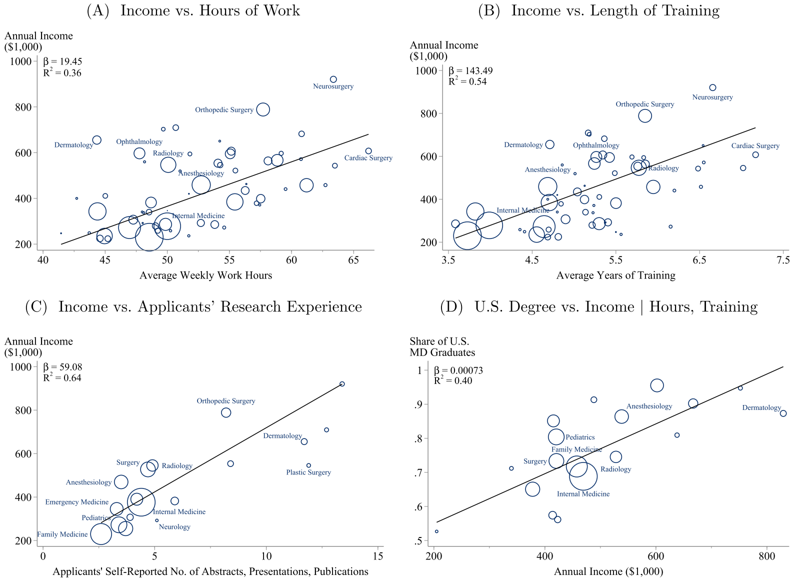

FIGURE II

Correlates of Specialty Income

This figure shows relationships between specialty earnings and specialty characteristics. Specialty earnings are measured as mean individual total income among 40–55-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 40–55 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 Online 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 no. CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

physicians' average length of training.[17] Each extra year of training is associated with $143,000 in extra annual income.

17. 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. Online Appendix B.2 provides details. Our measure ranges from 3.7 years for family practice to 7.2 years for cardiac surgery.

The challenges of medical training extend beyond its length. 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.[18]

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 among 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 physicians. Figure II, Panel D relates this share to the unexplained part of specialty earnings.[19] We residualize both the share of U.S. MD-trained physicians and specialty mean income with respect to training duration and work hours. We 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.[20] This suggests that income above the regression lines in Panels A

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

19. Panels C and D use NRMP specialty definitions. NRMP data allow 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.

20. A similar pattern emerges when we compare the share of graduates from top-five medical schools in a specialty to the specialty's average hourly income.

and B is largely an attractive feature of a specialty rather than a compensating differential. Section IV.C moves beyond this descriptive relationship and estimates a formal model of specialty choice.

III.C. The Importance of Geography

The geographic pattern of physician earnings is striking. Figure III, Panel A shows average earnings for physicians aged 40–55 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 use event studies to implement the movers strategy of Finkelstein, Gentzkow, and Williams (2016), Molitor (2018), and others, to determine the causal importance of location. We delve into a finer decomposition of place- and person-specific factors based on the methods of Card, Rothstein, and Yi (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 III.B 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 IV and V.

1. *Event Study.*

i. 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 $\Delta\overline{\ln y}_{(c,c')}$.[21] Using data from 12 years around the

We find that surgeons and procedural specialists have nearly twice the share of top-five graduates and the highest incomes (Online Appendix Figure E.5).

21. Our regression sample is all 40–55-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 40–55-year-old physicians. We use CZs to capture within- and cross-state variation; unadjusted CZ average incomes are shown in Online Appendix Figure E.6.

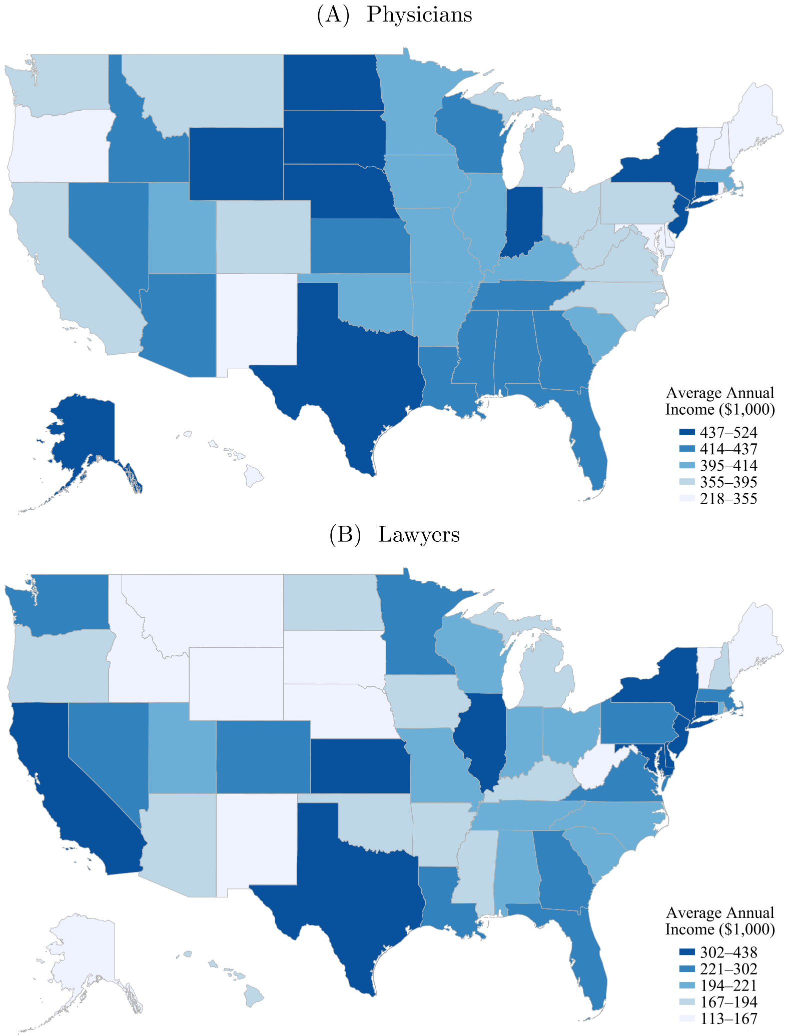

FIGURE III

Geographic Variation in Earnings

This figure plots mean individual total income among 40–55-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 II and Online Appendix B.1. Online Appendix B.2 provides more details on income measurement. Disclosure Review Board approval no. CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

move, we estimate:

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

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.[22]

ii. Results. Figure IV 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.[23] Having established that location influences earnings, we examine the patterns of these locations' effects and how physicians sort across them.

2. *Place versus Physician Factors: Variance Decomposition.*

i. 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:

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

22. Calendar year fixed effects are implicitly included as a linear combination of the other fixed effects.

23. Online Appendix 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 geographic patterns.

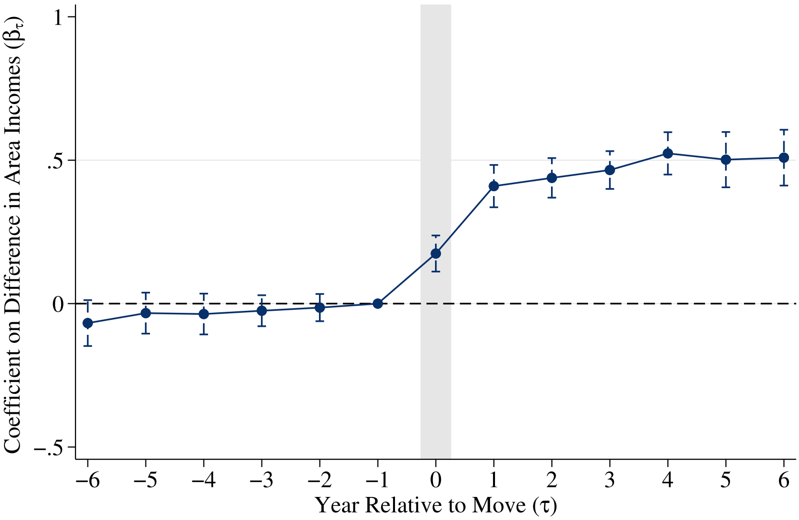

FIGURE IV

Event Study: Physician Movers

This figure shows coefficient estimates on the difference between mean individual total income between origin and destination commuting zones ($\Delta\overline{\ln y}_{(c,c')}$) from equation (1). The coefficient is normalized to zero in the year before the move ($\tau = -1$). The dashed lines indicate 95% confidence intervals. The outcome is log individual total income. The independent variables include $\Delta\overline{\ln y}_{(c,c')}$ 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 40–55 during that change. Disclosure Review Board approval no. CBDRB-FY23-0319, CBDRB-FY2023-CES005-024, CBDRB-FY24-0456.

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 pretrends from earlier 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, Saggio, and Sølvsten (2020) heteroskedastic correction, along with a direct fixed-effects estimation (Abowd et al. 1999).[24]

24. 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

ii. Results. Figure V, Panel A shows the key results. The first three bars show the estimated variance of location effects, Var(ψ_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, 2Cov(α_i, ψ_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; the ratio of covariance to variance is between 0.6 and 0.8 (Online Appendix 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, but with a differently structured labor market. Again we initially find a negative covariance when using the standard fixed-effects estimator, but the limited-mobility bias corrections reverse the sign for lawyers.[25] 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, Rothstein, and Yi (2021) and the broader literature on worker-firm matching—when the pattern exists. Physicians' pattern is unique.[26]

location becomes difficult. This tends to bias downward 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, Saggio and Sølvsten (2020) heteroskedastic correction uses a leave-one-out variance component estimator. We use the Bonhomme et al. (2023) PyTwoWay package to implement all estimators.

25. 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 Online 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.

26. 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 equation (2). As we add covariates, the correlation of individual fixed effects with

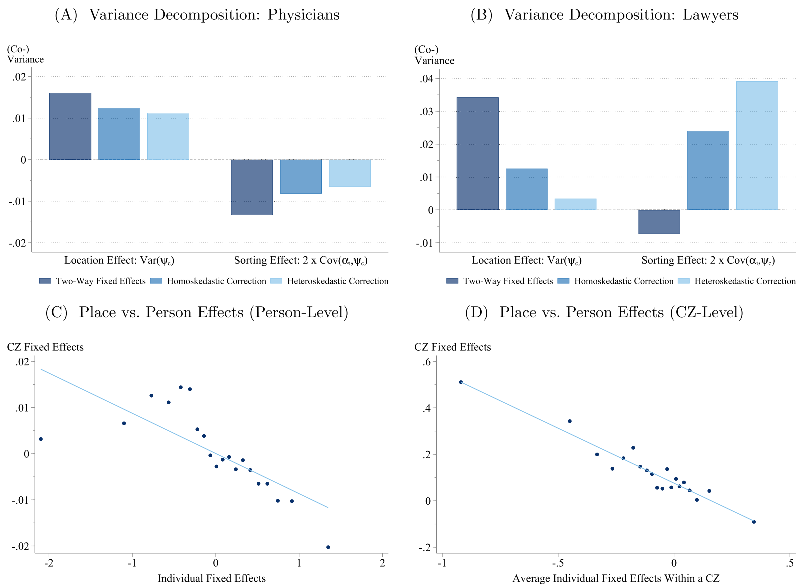

FIGURE V

Place Versus Physician Contributions to Earnings

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 the definition in Figure IV). 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, Var(ψ_c). The effect of sorting of people to locations, 2Cov(α_i, ψ_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, Saggio, and Sølvsten (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 the individual fixed-effects distribution. In Panel D we collapse the data to the CZ level by averaging individual fixed effects within a CZ as in Card, Rothstein, and Yi (2021). The line of best fit is based on a bivariate OLS regression using underlying data points. Disclosure Review Board approval no. CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

iii. Importance of Sorting for Geographic Patterns. What does this sorting mean for the overall pattern of earnings across space? We follow Card, Rothstein, and Yi (2021) and address this ques-

the conditional CZ effects becomes increasingly negative, trending toward the pattern in Figure V, Panel C.

tion 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 $\overline{\ln y}_c$[27] 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.[28] The variance decomposition of equation (3) reveals what share of variation in areas' average incomes come from the places themselves, the composition of workers, and sorting of those workers across locations.

Figure V, Panel D 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 Online Appendix 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, Rothstein, and Yi (2021) explains 1.8 times the magnitude of the CZ-industry effects.[29] The relative magnitude of our sorting is 1.2 times that of location effects, but with the opposite sign.

iv. 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 equation (2). The results, shown in Online Appendix Table E.6, Panel C and Online Appendix Figure E.8, Panel A, are broadly consistent with the location-physician decomposition. The raw and bias-corrected covariances between the physician's individual and firm effects are negative. This negative

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

28. 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 effect on the estimates of location variance and sorting (Online Appendix Table E.7).

29. Card, Rothstein and Yi (2021) consider CZ-by-industry, while we consider location effects for one occupation. Our data differ from the Longitudinal Employer-Household Data (LEHD) which Card, Rothstein and Yi (2021) use in that we include the self-employed and nonwage income.

worker-firm covariance reinforces the uniqueness of physicians' labor markets.[30]

3. *Correlates of Place Fixed Effects.* We explore the economics of these places by projecting the place fixed effects on observable characteristics.[31] Figure VI shows a series of correlations between location characteristics and our estimated place fixed effect, and between the same characteristics and the location's mean log earnings. Measures of the location's general economic strength tend to be uncorrelated or to 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, Gentzkow and Williams 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.[32] This pattern holds up whether comparing physicians' 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 health care (Dingel et al. 2023). Second, physicians may

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

31. We use estimates for commuting-zone fixed effects based on equation (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 III, so we do not present them further.

32. As Online Appendix 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. Online Appendix Table E.8 reports regression coefficients and standard errors for all regressions shown in Figure VI.

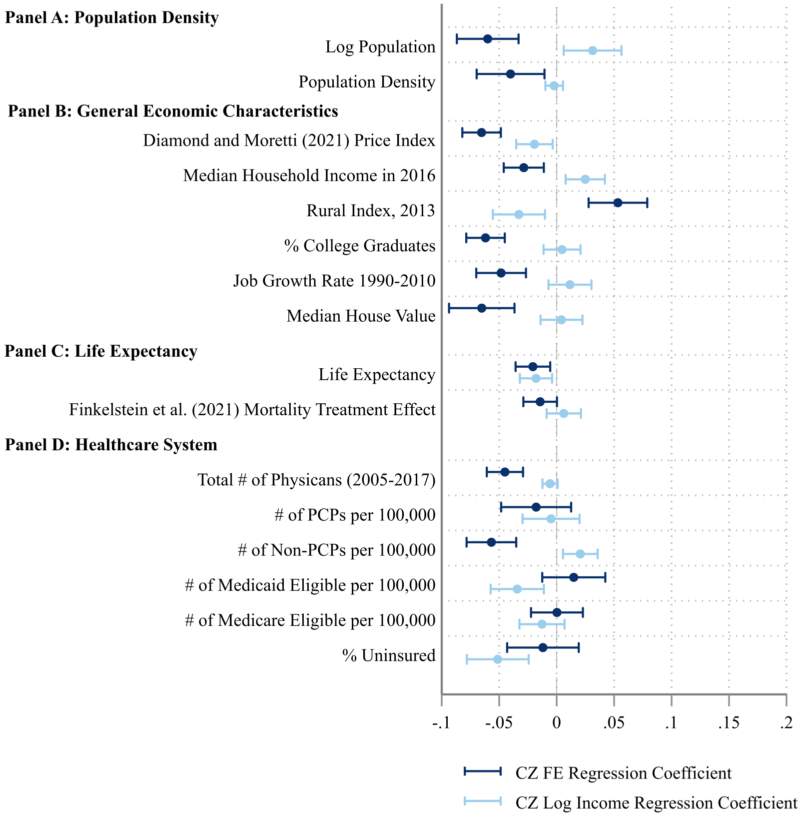

FIGURE VI

Correlates of Place Effects

This figure plots the results of bivariate OLS regressions of raw average individual total income in a commuting zone (light blue/gray; color version available online), as well as of place treatment effect on earnings (dark blue/gray), 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 the definition in Figure IV). Raw mean income is computed in the same sample. CZ-level characteristics are as reported in Chetty et al. (2014), Finkelstein, Gentzkow, and Williams (2021), and Diamond and Moretti (2021). Disclosure Review Board approval no. CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

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.[33] Third, the income gradient may reflect compensating differentials for skilled workers' preferences to live in higher-income locales. It is not clear why this would be true for only physicians and not lawyers. Finally, government's major role in the health care market, and the complex political economy of this role, could cause outcomes to differ from other industries. Federal and state governments purchase medical services on behalf of lower-income and rural residents, increasing these consumers' effective purchasing power for health care relative to other goods or services. Section IV measures this influence and Section V.A asks how it affects the geographic earnings gradient.

IV. Policy Influence on Earnings and Labor Supply

We use multiple empirical strategies to investigate the government's influence on physicians' earnings and labor supply. First we use short-run price changes, which occur as Medicare adjusts its reimbursements for each procedure. We 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.

IV.A. Transient Price Shocks and Short-Run Supply Responses

We use physician payment adjustments in the $900 billion/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

33. Clemens and Gottlieb (2017, fig. 2) report 20% higher private payments in the most concentrated markets compared with the least. Dunn and Shapiro (2014) find that a 10% increase in physicians' market concentration increases prices by 1%. In hospital pricing, Cooper et al. (2019) report 12% higher prices in monopoly markets than in 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. Furthermore, 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.

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.[34] We use changes in the RVUs assigned to each service to estimate how much Medicare payments influence physicians' contemporaneous incomes and labor supply.

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. Although 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 compute the average number of times each service k was performed across all years of our utilization data (2012–2017), denoted $\overline{q_{i,k}}$.[35] 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:

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

34. 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 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 predetermined 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 Update Committee meeting. Online Appendix C.1 provides more details about the institutional setting and our empirical approach.

35. 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.

We label this the Medicare price instrument. Online Appendix 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.[36] We control for physician fixed effects, α_i, physician-age fixed effects, $\theta_{a(i,t)}$, 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.[37] We run our analysis separately for 40–55- and 56–70-year-old physicians; the former group comprises 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 $\ln Q_{i,t}$.[38] Because each procedure's RVU weight enters into both this total and into our instrument $P_{i,t}$, we expect a mechanical coefficient of one in the absence of any behavioral response. With total RVUs as an outcome, the difference between the coefficient and one yields the supply elasticity. A coefficient below one indicates income-targeting behavior, while a coefficient above one 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

36. To the extent that changes in Medicare's fee schedule can trigger changes in private insurers' reimbursement 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.

37. 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.

38. This can be interpreted as the number of services a physician provides, weighted by value. It is formally defined in Online 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.

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 these IV specifications using 2SLS.

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:

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

where φ_κ is a set of procedure fixed effects.[39] To measure effects 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.

1. *Results.* Table III and Online Appendix Figure E.10 report the results. A 10% increase in Medicare's payment rate leads to a 2.4% increase in professional earnings of 40–55-year-old physicians, that is, 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)).[40] 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.[41] The procedure-level analysis directly shows that a 10% increase in a 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 pro-

39. 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.

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

41. 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.

TABLE III
RVU REGRESSION TABLE

	NPI-level			Procedure-level		2SLS	
Dependent variable:	Log income (1)	Log total RVUs billed (2)	Log number of unique procedures (3)	Log number of unique patients (4)	Log total RVUs billed (5)	Log income (6)	Retired (7)
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 var. (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

TABLE III

CONTINUED

Dependent variable:	NPI-level			Procedure-level		2SLS	
	Log income (1)	Log total RVUs billed (2)	Log number of unique procedures (3)	Log number of unique patients (4)	Log total RVUs billed (5)	Log income (6)	Retired (7)
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 var. (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 2012 and 2017. Disclosure Review Board approval no. CBDRB-FY23-0319, CBDRB-FY2023-CES005-024.

viding the procedure more frequently for the same number of patients.

Intensive-margin responses are broadly similar among 55–70-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 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, Gottlieb, and Molnár 2017), we get a lower pass-through of \$25 for each \$100 of any insurance spending.[42] 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 and 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, Carey, and Miller (2021), but is similar to other estimates of compensated wage elasticities (Nicholson and Propper 2011).

IV.B. Persistent Demand Shocks and Medium-Run Supply Responses

Next we study the impact of a persistent demand shock on physicians' earnings and behavior. The ACA 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.

42. Online Appendix C.1 details these calculations.

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 2015, coinciding with the rollout of marketplaces in 2014.[43]

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 before 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 (Online Appendix 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 effect of insurance expansions on physician-level outcomes $Y_{i(c),t}$. We use our physician-year panel covering four years post-expansion to run:

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

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 the 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 nonelderly population in 2013, conditional on covariates. While this parallel-trends assumption is not directly

43. Online Appendix C.2 provides more details on our definitions and sources.

testable, the event-study specification in equation (7) allows us to assess whether counties with different uninsurance rates in 2013 followed parallel trends in outcomes before 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 equation (7), but with $I_{c(i),t}$ as the outcome and normalizing to 2013, the year before 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 effects into the pre- and post-implementation periods.[44] To interpret this estimate, we assume that 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 40–55). For the population at a higher risk of retirement, we measure the effect of ACA expansion on the probability of retirement.[45]

1. *Results.* Figure VII plots coefficients $\hat{\beta}_t$ for the first stage and the reduced form for individual income and retirement. Table IV reports the first-stage, reduced-form, and 2SLS coefficients for all outcomes. The first-stage estimate (Table IV, column

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

45. As in Section IV.A, we consider ages 56–70 to be at a higher risk of retirement. To capture all physicians who turn 56 during our event study's time window, the regression is estimated on all physicians age 44–70.

TABLE IV

ACA Regression Table

	First stage	Reduced form			2SLS		
Dependent variable	Share insured (1)	Log income (2)	Share with schedule SE (3)	Share retired (4)	Log income (5)	Share with schedule SE (6)	Share retired (7)
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 ⩾ 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 var.	0.047	0.924	0.499	0.283	0.911	0.498	0.290
Mean of independent var.	0.147	0.147	0.147	0.148	0.851	0.851	0.849
Std. dev. of independent var.	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 the 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. Online 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)–(4) report reduced-form estimates. Columns (5)–(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. Online Appendix Table E.9 reports the same specifications, but drops the post-ACA passage and pre-implementation years (2011–2013). Disclosure Review Board approval no. CBDRB-FY23-0319, CBDRB-FY2023-CES005-024, CBDRB-FY24-0456.

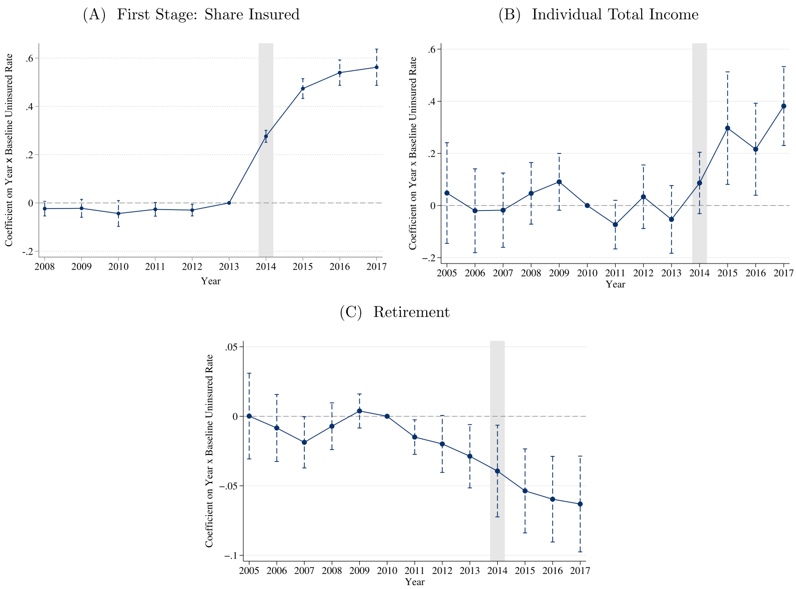

FIGURE VII

Effect of ACA Insurance Expansion

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 Online 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 no. CBDRB-FY23-0319, CBDRB-FY2023-CES005-024, CBDRB-FY24-0456.

(1)) shows that counties with a 10 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 VII, Panel B and Table IV, columns (2) and (5) show that earnings among physicians aged 40–55 grew faster in these more affected areas. We estimate that a 10 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 10 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.

Table IV, columns (3) and (6) 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, Table IV, columns (4) and (7) 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 VII, Panel C 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.[46] 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.[47] Based on our pooled estimates, 8% of the $110 billion annual spending (CBO 2016, table 3) on the ACA insurance expansion accrued to physicians.[48] Since physicians' baseline earnings as a share of medical

46. 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 (Online Appendix Table E.9).

47. The analogous question among hospitals is well studied (Garthwaite, Gross and Notowidigdo 2018).

48. Policy reports suggest that ACA expansion resulted in approximately 5.9 percentage points more people insured among the nonelderly in total (Tolbert, Orgera and Anthony 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% in-

spending is 8.6%, their gain from expansions was nearly proportional to their baseline expenditure share.

IV.C. Price Shocks and Long-Run Supply Responses: Specialty Choice

Beyond the immediate labor supply responses found in Sections IV.A and IV.B, the effect of government policies on earnings may be even more important if these policies shape talent allocation over the long run. Figure II shows suggestive patterns: in the cross section, higher-earning specialties tend to attract physicians with more qualifications along measurable dimensions. We use variation in Medicare reimbursement policies to identify the elasticities of specialty choice to Medicare reimbursement rates and to income. Because 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.[49] 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.[50] We use six years of aggregate NRMP data on the number of physicians who apply to each specialty, reported by bins of USMLE scores ($\leq$190, 191-200, 201-210, through $\geq$260). We posit that our estimates for the highest-scoring applicants reflect true preferences, while other physicians' choices reflect a combination of preferences and choice-set rationing.

crease 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.

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

50. 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 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, Ewer and Wagner 2010).

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 40–55) working in year t:[51]

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

P_{st} is the specialty-year Medicare price instrument, as defined in equation (4) but now at the specialty-year level, divided by the specialty's average number of hours worked a year. We compute P_{st} using data on 40–55-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.[52] 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. With observations at the specialty-year-score bin level, we estimate:

$$\ln \pi_{ast} - \ln \pi_{a0t} = \alpha_{base} P_{st} + \sum_{a \in [191,200]}^{a>260} \alpha_a \cdot \mathbb{1}_a \cdot P_{st} + A_s + \delta_a + \beta \phi_{st} + \xi_{ast}. \tag{9}$$

51. Because we are comparing one cohort's choices with a different cohort's 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.

52. These characteristics are the female share in the specialty, the standard deviation of hourly income in the specialty, and the average firm size of physicians in the specialty, all measured among 40–55-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 (Altonji, Arcidiacono and Maurel 2016; Arcidiacono et al. 2020). It is also consistent with evidence in Wasserman (2023) that gender-specific preferences influence specialty choice.

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).[53] P_{st} is interacted with test-score-bin fixed effects, $\mathbb{1}_a$,with the lowest bin omitted. All other independent variables are defined as in equation (8). We normalize all independent 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 alleviate this concern. By instead constructing P_{st} based on policy changes, as described in Section IV.A, we exploit variation more likely to be independent of ξ_{ast}.

We estimate three variants of this model. First, equation (9) 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 40–55-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}.[54]

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

53. 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, Hortaçsu and Joo 2021; Gandhi, Lu and Shi 2023) of choice options with zero shares; in our case, the incidence of such observations is very low. We add one nonmatching applicant to those specialty-by-score bin-by-year observations in which NRMP reports zero nonmatching applicants. Alternative approaches, including the exclusion of zero-share observations, result in very similar estimates.

54. The analysis of the relationship between specialty-level earnings and the Medicare price instrument here is conceptually analogous to the analysis in Section IV.A. Since the 2SLS version of equation (9) has an 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 Online Appendix Table E.11. The first-stage coefficients are close to Medicare's true payment rate per RVU.

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 nonfinancial 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.

1. *Results.* Table V 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 (Online Appendix Table E.11) suggests that high-ability physicians prefer the amenities offered by family medicine and that various procedural specialties appear to have relative disamenities.[55] 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.

V. Can Government Shape Earnings Variation?

We have documented dramatic variation in physicians' earnings by specialty and location and that insurance policies drive

55. 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 Online Appendix Tables E.12–E.14.

TABLE V

SPECIALTY CHOICE MODEL

	USMLE Step 1 scores								
Ability group (a)	> 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 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 the estimated interactions and the full marginal effect for each score group. Standard errors on the marginal effects are calculated using the delta method. See Section IV.C for more discussion of the interpretation. Online Appendix Table E.11 reports the full set of estimates for these specifications, including the first stage for Panel C. Online Appendix 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 no. CBDRB-FY24-0456.

these earnings and physicians' labor supply. We now put these facts together to understand the role of government policy in driving the patterns from Section III. We consider the effects 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.

V.A. Medicare's Contribution to Geographic Earnings Variation

We first connect government's influence on physicians' 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 physicians' incomes from Section IV. Medicare adjusts its rates for local input costs, but the adjustment is incomplete, resulting in effective subsidies to rural areas (GAO 2022).[56] 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.[57] 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 (Online Appendix Figure E.12). We then relate the adjusted local Medicare reimbursement to the CZ fixed effects for physicians' earnings estimated in Section III.C.

Figure VIII 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 three times the causal estimate of 0.236 from Section IV.A. The causal estimate implies that 10% higher Medicare payments increase earnings by 2.4%. In the cross section, physicians in areas with a 10%

56. Other government policies specifically intended to subsidize rural health care 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 health care workers) who commit to work in medically underserved areas.

57. We thank Rebecca Diamond for sharing these data.

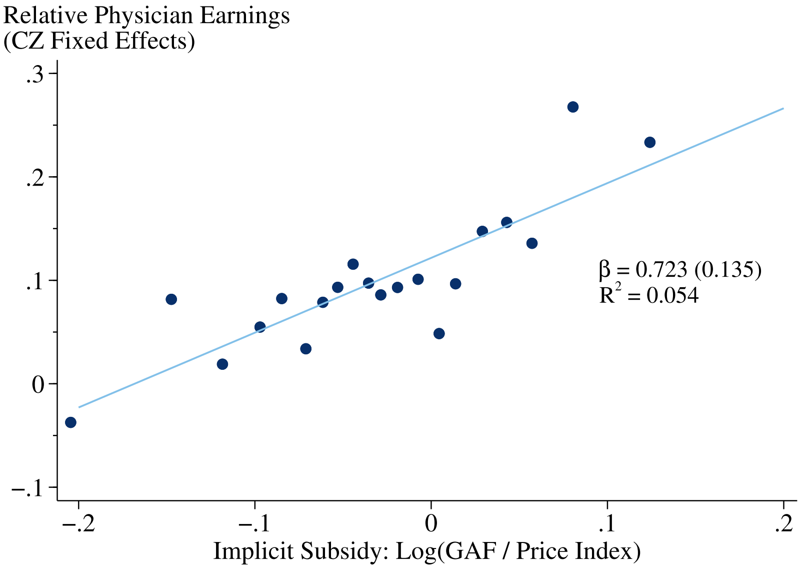

FIGURE VIII

Location Effects Versus Implicit Subsidies

This figure plots the relationship between the causal geographic component of physician earnings—our CZ fixed effects estimated in Section III.C—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 the R^2 and the line of best fit are from a bivariate OLS regression on the underlying data points. Disclosure Review Board approval no. CBDRB-FY24-0456.

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.

V.B. Allocation of Talent across Specialties

We use the specialty-choice model to illuminate the policy debate surrounding the "shortage" of PCPs and the role of entry restrictions in physicians' labor markets (Glied, Prabhu, and Edelman 2009). Policy discussions often consider increasing PCPs' incomes, through reimbursements, bonuses, or loan forgiveness. Our specialty-choice estimates from Section IV.C allow us to compute how physicians' specialty choices would respond to a change in Medicare reimbursement. Given the importance of physicians' 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.[58]

Specifically, consider an increase in internal medicine hourly Medicare reimbursements to dermatologists' level, that is, a 2.3-fold increase in internists' hourly RVU production. Figure IX shows the counterfactual distribution of internists' test scores in this regime.[59] 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 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 takeaway 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—that is, the absolute level of earnings—may affect the choice to enter medicine in the first place. Although our analysis abstracts from this decision,

58. 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 Online Appendix Figure E.13. We return to the income-based counterfactual in Section V.C, where we compare the reimbursement instrument to tax policy.

59. 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 in each score group to recompute the share of each score group in a specialty.

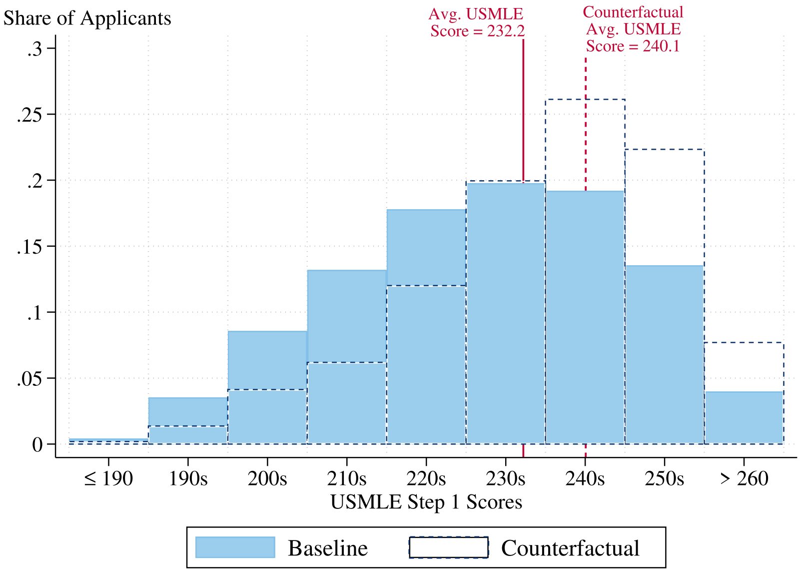

FIGURE IX

Increase Internal Medicine Medicare Payments to Dermatology Level

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 Online 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 in each USMLE score group and then renormalize the data to plot the share of each USMLE score group in one specialty: internal medicine. Disclosure Review Board Approval no. CBDRB-FY24-0456.

Online Appendix D uses our data to speculate on implications for this extensive occupational-choice margin.

V.C. *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.

Although 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 equi-

TABLE VI
PREDICTED EFFECT OF TAX CHANGES ON INCOME

Elasticity ϵ	Income growth Δy	Original tax rate (%) τ_0	New tax rate (%) 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 versus average specialist			
1.00	0.22	37	22
1.00	– 0.22	37	49
Panel D: Average specialist versus 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 III). $\Delta y = 0.22$ 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 III, 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).

librium estimates, as our empirical strategies use comparisons across physicians, specialties, or locations. We treat the supply response of physician care from Table III (measured in RVUs) as analogous to an elasticity of taxable income, denoted ϵ.[60] Given a starting tax rate τ_0, we can find the tax rate τ_1 that would generate any specific increase of Δy in log physician earnings using the formula:[61]

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

Table VI 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

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

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

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%; 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%; and by the Economic Growth and Tax Relief Reconciliation Act of 2001, which lowered the top rate from 39.6% to 35% (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.[62] 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 VI, 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 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 VI 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.

62. 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.

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.[63]

VI. Conclusion

This article 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 health care economy—half of which is government financed. We find that physicians earn $350,000 on average, and 8.6% of U.S. health care spending in aggregate. The age-earnings profile is steep, reflecting the extensive human capital investments 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 affect earnings and thus play a key role in 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 effects of health care policies, including those we investigate, we need evidence on the distribution of health effects and thus social returns to physician ability in different specialties. We do not speculate on the magnitude of such returns in this article, but our results show that quantifying the health effects of ability is an essential direction for future work and is key to formulating payment policies.

63. 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.

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Supplementary Material

An Online Appendix for this article can be found at *The Quarterly Journal of Economics* online.

Data Availability

The code underlying this article is available in the Harvard Dataverse, https://doi.org/10.7910/DVN/CJ1UM1 (Gottlieb et al. 2025).

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