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ABSTRACT

This paper examines how selection into medicine in the United States influences physician practice styles. We exploit macroeconomic conditions at the time of medical school application as an exogenous shock to the applicant pool and find that labor market downturns in other prestigious fields increase applications to medical school and shift the composition of matriculating cohorts toward students from non–pre-medical backgrounds. Strikingly, these low–outside-option cohorts practice medicine differently decades later. Conditional on experience, specialty, and outcome year, recession-cohort physicians bill Medicare more per beneficiary despite seeing healthier patients—particularly on discretionary, revenue-generating services—and sort into practice settings with higher-powered financial incentives. The additional spending does not produce detectable improvements in patient health. Taken together, our findings point to stronger financial motivation among those drawn into medicine when outside options are weak and suggest that medical school admissions screen imperfectly on the attributes that ultimately shape how physicians practice.

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I Introduction

Physician decision-making plays a central role in health care delivery. Physicians influence treatment choices, the adoption of new technologies, and the use of high-cost procedures, all of which contribute to regional and provider-level variation in health care expenditures (Cutler et al., 2019; Badinski et al., 2023). While a large literature examines how factors such as financial incentives (Clemens and Gottlieb, 2014; Alexander, 2020), organizational structures (Molitor, 2018; Doyle and Staiger, 2025), and training (Epstein and Nicholson, 2009; Schnell and Currie, 2018) shape physician behavior, most existing work takes the population of practicing physicians as given. This paper investigates a fundamental but understudied question: who decides to become a doctor, and how does this shape the way medicine is practiced? This question is particularly important in high-income countries like the United States, where physicians' high earnings may attract some individuals motivated more by financial returns than by patient welfare (Li et al., 2017; Gottlieb et al., 2025; Buehler et al., 2026).

To examine how selection into medicine influences the delivery of care, we exploit macroeconomic conditions at the time of medical school application as an exogenous shock to the applicant pool. Because physician unemployment is low and acyclical, downturns that weaken outside options may shift highly skilled individuals toward medicine and away from other high-paying, prestigious careers. Consistent with this idea, we find that over the past three decades, significantly more people applied to medical school when unemployment in other high-skill occupations was higher. These downturns also changed the composition of entering medical school classes, with a greater share of matriculants majoring in non-traditional premedical fields. Strikingly, cohort-level differences based on entry conditions persist decades later: physicians who applied to medical school during downturns in competing occupations bill more to Medicare—particularly for discretionary, high-margin services such as elective procedures and physician-administered drugs—and are more likely to practice in smaller, higher-earning settings. These cohorts treat slightly healthier patients on average, yet we find no measurable differences in their impacts on patient health. Taken together, these findings indicate that selection into the medical profession has enduring implications for the delivery and effectiveness of health care.

We begin by constructing a business cycle measure that captures economic conditions

relevant to potential physicians. Because few undergraduates deciding whether to apply to medical school are choosing between medicine and low-skill occupations, aggregate unemployment rates may be a poor proxy for the economic conditions they face. We therefore develop a "medical-school-applicant-relevant" business cycle that reflects downturns in other occupations plausibly considered by students contemplating medicine. Using data from the Current Population Survey (CPS), the American Community Survey (ACS), and the Association of American Medical Colleges (AAMC) Applicant Matriculant File, we link undergraduate majors to occupations and compute an annual weighted unemployment rate that reflects the labor market conditions facing students in majors common among medical school applicants. Averaged over a four-year window to capture conditions throughout college, this measure provides a more targeted proxy for the outside options facing potential medical school applicants than the overall business cycle.

Using this measure, we find that downturns in competing occupations expand and reshape the medical school pipeline. Drawing on the AAMC's Applicant Matriculant Files, we estimate that for those applying between 1989 and 2007—the cohorts whose eventual practice patterns we observe in claims data—a one-percentage-point increase in unemployment in competing fields was associated with an increase of 0.81 applicants per available spot (p-value = 0.006), or a 36 percent rise relative to the mean of 2.3. Although this increase occurs among both pre-medical and non–pre-medical applicants, the composition of matriculants shifts during downturns: a one-percentage-point increase in the applicant-relevant unemployment rate raises the share of matriculants with non–pre-medical undergraduate majors by 4.7 percentage points (p-value < 0.001; 15.1 percent relative to the mean of 31 percent). Although applicant numbers respond more strongly among men, the male share of matriculants does not move with the business cycle, suggesting that selectivity among men rises more sharply during downturns. Similar patterns emerge when exploiting state-level variation: applications rise more in states experiencing more severe downturns, and the increase is proportionally larger among non–pre-medical majors. Taken together, these results suggest that economic downturns expand the pool of potential physicians by drawing in individuals who would otherwise have pursued other professional paths, reshaping the composition of the future physician workforce.

Entry conditions have lasting consequences for how medicine is practiced, with practice differences concentrated on margins associated with financial motivation: recession-cohort

physicians bill more, particularly on discretionary, revenue-generating services; treat healthier patient panels; and sort into more lucrative practice settings. These differences arise within specialty rather than through specialty choice, as entry conditions have no detectable effect on which specialties physicians enter.[1] Using the public-use Medicare Part B files from 2013 to 2019 and conditioning on experience, specialty, and outcome year fixed effects, we estimate that cohorts facing a one-percentage-point higher unemployment rate in competing occupations at the time of medical school application bill $14.17 more per patient annually (p-value = 0.006), a 4.7 percent increase relative to mean spending of $301.4. This estimate is very similar in restricted Medicare Part B data from 2001 to 2016 ($13.72 per beneficiary, p-value < 0.001), which we use to decompose billing into service categories. The largest increases in billing come from physician-administered drugs ($6.26 per beneficiary per year, p-value = 0.004), ambiguous procedures that could be discretionary or non-discretionary depending on how the patient presents ($3.18, p-value = 0.004), and elective procedures ($2.05, p-value = 0.008). In contrast, we find no impact of entry conditions on non-elective procedures ($0.25, p-value = 0.928) or evaluation and management (E&M) services ($0.83, p-value = 0.606), categories with limited physician discretion and weaker revenue-generating potential. These patterns suggest that the spending response operates through the margins where physicians have the most discretion over treatment and the strongest revenue incentives.

Recession-cohort physicians bill more despite treating healthier patients, and this greater treatment intensity does not produce better outcomes. In the public-use files, a one-percentage-point increase in the applicant-relevant unemployment rate at the time of medical school application reduces the share of a physician's Medicare patients who are dually eligible for Medicaid—a population that is typically sicker than Medicare-only beneficiaries (MedPAC, 2023)—by 0.34 percentage points (p-value = 0.002), a 1.4 percent decline relative to the mean of 24.1 percent. While the corresponding reduction in average patient risk scores is imprecise, evidence from the restricted Part B files also suggests that recession-cohort physicians see healthier patients: those who applied when applicant-relevant unemployment was one percentage point higher treat beneficiaries who are 0.27 percentage points less likely to have a non-avoidable emergency department (ED) visit in the following year (p-value = 0.016; 1.8

[1] This finding is consistent with the supply of residency slots being largely fixed across specialties regardless of cohort preferences. Our estimates are accordingly very similar with and without specialty controls.

percent) and who have 0.18 percentage points lower average predicted one-year mortality (p-value = 0.014; 1.6 percent). Nevertheless, despite billing more for these healthier patients, we find no detectable effects on patient health outcomes. The coefficient estimates are small and insignificant, with neither avoidable ED visits (−0.15 percentage points, or 0.6 percent of the mean; p-value = 0.388) nor actual one-year mortality conditional on predicted (−0.003 percentage points, 0.02 percent; p-value = 0.956) responding to entry conditions.

Recession-cohort physicians also sort into more lucrative practice settings. Drawing on the Centers for Medicare & Medicaid Services (CMS) Physician Compare database, we find that physicians who applied when applicant-relevant unemployment was one percentage point higher work in practices that on average have 13.62 fewer clinicians (p-value = 0.010), a 2.6 percent decline relative to the mean of 518.9. Because physician earnings tend to fall with practice size (Gottlieb et al., 2025), this smaller-practice sorting is consistent with stronger financial motivation. IQVIA's SK&A Office-Based Physician Database shows that these physicians also sort by practice ownership and affiliation: cohorts that applied when applicant-relevant unemployment was one percentage point higher are 1.76 percentage points less likely to work in hospital- or health-system-owned practices (p-value < 0.001; 4.0 percent) and 2.15 percentage points more likely to work in hospital-affiliated independent practices (p-value < 0.001; 4.3 percent). Because independent practices are typically paid fee-for-service while hospital- and system-owned settings more often pay salaries, these patterns are consistent with recession-cohort physicians sorting into settings with higher-powered financial incentives.

The impacts of entry conditions on eventual practice patterns are more pronounced among male physicians. Although the share of women among matriculants is stable across cohorts, the number of male applicants per medical school seat rises somewhat more than the number of female applicants during downturns, suggesting that men who matriculate during recessions may be a more differentially selected group than the corresponding pool of women. Consistent with this, the practice differences we document are generally larger among men. In the restricted Medicare data, the effect on revenue per beneficiary is nearly three times as large for male as for female physicians ($19.69 versus $6.70, p-value = 0.014 on the difference). Within service categories, the effect on ambiguous-procedure billing is significantly larger for men than for women ($4.39 versus $0.70, p-value = 0.050 on the difference). The effect on physician-administered drugs is more than twice as large for men ($8.28 versus $3.57),

though the difference is imprecise (p-value = 0.142). The sorting into hospital-affiliated, independent practices and away from hospital- or system-owned employment is likewise larger among men (p-values on the difference of 0.068 and 0.014, respectively). These patterns suggest that the behavioral shifts documented above operate disproportionately through male physicians within cohorts, even though the gender composition of medical school classes is uncorrelated with the business cycle.

Finally, we examine the channels through which outside labor market conditions shape physician behavior. When unemployment in competing fields is higher, both the selectivity of medical school admissions and the composition of matriculating cohorts shift. Consistent with heightened selectivity, matriculant Medical College Admission Test (MCAT) scores rise modestly during downturns even as average applicant scores do not. Yet our main findings are difficult to reconcile with a story in which heightened selectivity primarily raises clinical skill: recession-cohort physicians sort into smaller, more lucrative practice settings; bill more on discretionary, revenue-generating services while treating healthier patients; and produce no measurable improvements in patient health. While a skill-based story would likely predict better patient outcomes or a willingness to manage more complex patients, the observed pattern of effects instead points toward stronger financial motivation among recession-cohort physicians, potentially extending beyond marginal entrants through peer effects within cohorts. A regression-based mediation analysis comparing admissions selectivity with changes in cohort composition in the spirit of Heckman et al. (2013) further supports this interpretation: changes in the share of matriculants from non-traditional pre-medical backgrounds can statistically explain a meaningful share of the effects on income-related margins—including Medicare revenue per beneficiary and sorting into smaller practice settings with greater earnings potential—whereas selectivity plays a smaller role.[2]

This paper builds on three literatures. First, we contribute to the extensive literature examining the determinants of physician behavior. Numerous studies document that providers vary widely in the intensity and type of care they deliver, with important implications for health care costs and patient outcomes (e.g., Currie et al., 2016; Van Parys, 2016; Gowrisankaran et al., 2023; Ginja et al., 2024; Currie and Zhang, 2025). Building on this

[2]The share of non–pre-medical matriculants captures only one dimension of cohort composition and likely understates such compositional shifts if undergraduates adjust their majors in response to the business cycle (Blom et al., 2021). This exercise is thus provided solely as suggestive evidence on mechanisms rather than as a formal decomposition of the channels at play.

evidence, subsequent research has sought to uncover the sources of such variation, examining how differences in financial incentives (Clemens and Gottlieb, 2014; Alexander, 2020; Alexander and Schnell, 2024), organizational structures and peer environments (Molitor, 2018; Doyle and Staiger, 2025), training (Epstein and Nicholson, 2009; Schnell and Currie, 2018), skill (Currie and MacLeod, 2017, 2020; Chan et al., 2022), and beliefs (Cutler et al., 2019) shape provider behavior. In contrast to this literature, which treats the composition of the physician workforce as given, we show that part of the variation in clinical behavior arises from who becomes a physician in the first place. By endogenizing entry into medicine, our analysis identifies selection into the profession as an important but often overlooked determinant of heterogeneity in clinical decision-making.

Second, our work builds on the broad literature examining the determinants of occupational choice. A long tradition in labor economics, beginning with Roy (1951) and extended by dynamic frameworks (McCall, 1970; Keane and Wolpin, 1997), studies how individuals select into professions based on expected earnings, non-pecuniary attributes, and preferences (Willis and Rosen, 1979; Heckman and Sedlacek, 1985). Related research on signaling and screening emphasizes how education and training reveal and filter worker ability and motivation (Spence, 1973; Stiglitz, 1975), while more recent work highlights the role of intrinsic and mission-driven motives in shaping who selects into public-service and health-related occupations (Besley and Ghatak, 2005; Delfgaauw and Dur, 2007). A growing empirical literature, primarily in lower- and middle-income settings, shows that career incentives and recruitment design shape both who enters such occupations and how they perform once in them (Dal Bó et al., 2013; Deserranno, 2019; Ashraf et al., 2020; Bertrand et al., 2020). We contribute to this literature by linking students' outside options at the time of medical school application to U.S. physicians' clinical practice patterns decades later, showing that selection effects at entry can have persistent consequences for how care is delivered. Our findings suggest that screening in medicine is imperfect: if medical schools could fully identify applicants with the attributes they seek, cohorts chosen during labor market downturns, when the applicant pool expands, should not perform worse than other cohorts.

More specifically, our work contributes to research examining how macroeconomic conditions influence career entry and long-run occupational outcomes. A large body of evidence shows that graduating in a recession leads to persistently lower earnings, slower career advancement, and worse life outcomes (Kahn, 2010; Oreopoulos et al., 2012; Schwandt and

von Wachter, 2019). Recognizing that such downturns can also shape behavior before labor-market entry, subsequent work examines how macroeconomic conditions influence educational and occupational choices (Böhm and Watzinger, 2015; Blom et al., 2021; Weinstein, 2022; Cotofan et al., 2023). Most directly related, several papers show that economic conditions at the time of career entry shape both the composition and on-the-job performance of U.S. workers in specific high-skill professions, including teaching (Hoxby and Leigh, 2004; Nagler et al., 2020), academic economics (Oyer, 2006), and finance (Oyer, 2008). Our paper offers a complementary perspective by showing that macroeconomic shocks affect who enters medicine—a recession-resistant profession (Dillender et al., 2021)—with persistent consequences for physician practice styles and health care delivery.

The remainder of the paper is organized as follows. Section II outlines the construction of a business cycle measure that captures macroeconomic fluctuations relevant to high-skilled undergraduates. Section III examines how these fluctuations affect both the number and characteristics of individuals applying to and matriculating into medical school. In Section IV, we explore how entry conditions affect cohort-level practice styles, and Section V provides a discussion of potential mechanisms. Section VI concludes.

II Constructing the relevant business cycle

Our empirical strategy requires a measure of the outside options facing individuals deciding whether to apply to medical school. Because few undergraduates who might become physicians are also considering careers in fields such as construction, downturns in those sectors are not relevant for our analysis. Accordingly, we construct a "medical-school-applicant-relevant" business cycle to capture the economic conditions faced by individuals on the margin of applying to medical school. As outlined below, this measure is a weighted average of occupation-level unemployment rates, where the weights reflect both the distribution of occupations that graduates of a given major typically enter and the share of medical school applicants from each major.

We construct this business cycle in five steps using three data sources. First, we use individual-level data on labor force status from the CPS, accessed via IPUMS (Flood et al., 2023), to construct unemployment rates by occupation-year (or by occupation-state-year for state-level analyses) using survey weights. Occupations are defined using the IPUMS

harmonized 2010 occupation codes.[3] These data offer three key advantages: (1) they allow us to examine historical trends dating back to the 1970s, (2) they enable the construction of unemployment rates at the occupation level rather than the broader industry level, and (3) they align with the series used in official U.S. unemployment statistics.

Second, we use ACS data for 2009–2019 to group undergraduate majors into clusters based on the occupations their graduates ultimately enter. Specifically, we start with a list of all undergraduate majors represented among medical school applicants in the AAMC data described below and map this list to the ACS. For each major, we compute the share of its graduates entering each occupation and apply k-means clustering to these occupation-share vectors, so that majors whose graduates enter similar sets of occupations are grouped together. This clustering procedure generates seven major clusters: biology/chemistry, engineering, computer science/math/physics, earth sciences, humanities/applied social sciences, business/economics/political science, and education (see Table A1 for a full list of the majors in each cluster). As approximately two thirds of all medical school applicants and matriculants over the last three decades came from the biology/chemistry cluster, we refer to it as the "traditional pre-medical" cluster throughout.

Third, we construct an annual unemployment rate for each major cluster as the weighted average of the occupation-specific rates from step one, where the fixed weights reflect the share of individuals in that major cluster who enter each occupation on average over all years available in the ACS.[4] We use average weights constructed using all available data rather than year-specific weights both because year-specific weights would introduce endogeneity concerns, since the share of a given major cluster entering each occupation is partly driven by contemporaneous labor market conditions, and because the ACS is not available over our entire sample. Nevertheless, as shown in Figure A1(a), the medical-school-applicant-relevant business cycle is very similar when we use weights from a single ACS year applied to the

[3]We construct unemployment rates at the occupation level rather than the industry level, as occupation-based measures better capture the employment prospects of high-skilled professions within an industry. In particular, unemployment rates can vary substantially between high-skilled occupations (e.g., lawyers or physicians) and lower-skilled roles that constitute the majority of employment within an industry (e.g., administrative assistants or home health workers). As in official U.S. unemployment statistics, we restrict the data to individuals aged 16 or older and in the labor force.

[4]In particular, we use occupation weights constructed from all one-year ACS samples with detailed education information that overlap with our study period (2009–2019). As an example of this methodology, suppose that an average of 30 percent of individuals in the engineering major cluster become aerospace engineers and an average of 70 percent become mechanical engineers. Then the unemployment rate for the engineering cluster in a given year would be $0.3 * U_y^{aero} + 0.7 * U_y^{mech}$.

entire period, rather than average weights constructed over 2009–2019.

Fourth, we use the AAMC Applicant Matriculant Files to aggregate across major clusters, weighting each cluster's unemployment rate by its share of medical school applicants to obtain an annual applicant-relevant unemployment rate. The AAMC data contain detailed information on applicants and matriculants to U.S. medical schools for application cycles from 1989 to 2019, including undergraduate major. We again use fixed weights for aggregation, here each cluster's average share of applicants over 1989–2019, as year-specific weights would introduce endogeneity concerns due to contemporaneous labor market conditions affecting the distribution of undergraduate majors among medical school applicants in a given year. Nevertheless, as shown in Figure A1(b), the business cycle relevant for medical school applicants is similar when major cluster weights from a single application cycle are applied to the entire period, rather than average weights constructed over 1989–2019.

Finally, we average this annual rate over the application year and the three preceding years. This window is meant to capture labor market conditions toward the end of a potential applicant's undergraduate degree, when marginal students are most likely deciding whether to apply to medical school. Two factors complicate the timing of our analysis. First, while specific coursework is not always formally required, applicants typically complete key premedical courses during college to prepare for the MCAT and strengthen their applications. This motivates consideration of economic conditions prior to the completion of undergraduate education. Second, many students take one or more gap years after graduating, and we observe only the year in which they apply to medical school. This motivates a consideration of economic conditions after undergraduate completion. The four-year window spans both margins, and we denote the resulting measure by $\bar{U}_y$ for application year y. We further examine the robustness of our results to alternative timing assumptions in Section IV.D.

Figure 1(a) plots the economy-wide unemployment rate, the national unemployment rate for physicians, and the medical-school-applicant-relevant business cycle $\bar{U}_y$ from 1979 to 2019.[5] Because majors that commonly lead to medical school tend to feed into occupations with relatively low unemployment rates, it is unsurprising that the applicant-relevant business cycle consistently falls below the overall economy-wide measure. While recessions are visible across all series, the relative severity of downturns differs for those considering

[5]Because detailed occupation-level information is available in the CPS beginning in 1976, and our baseline measure averages the annual medical-school-applicant-relevant unemployment rate in the application year and the three preceding years, this measure can be constructed starting in 1979.

medicine relative to workers in the broader labor market. Notably, physicians display an essentially flat unemployment profile over the business cycle, unlike other high-income or high-prestige occupations, which exhibit much more pronounced cyclicality. Finally, as shown in Figure 1(b), there is substantial cross-state variation in the applicant-relevant business cycle, with certain states experiencing more pronounced downturns during specific recessions than others.

III Medical school applicants and matriculants

III.A Data

As outlined above, data on medical school applicants and matriculants come from the AAMC's Applicant Matriculant Files covering 1989–2019. These data report the number of unique applicants and matriculants to U.S. MD-granting medical schools at the state–year–major level, where state indicates the applicant's legal residence at the time of application, year corresponds to the year of application (i.e., the year before potential matriculation), and major reflects the applicant's detailed undergraduate field of study. In what follows, we limit the sample to applicants residing in the 50 states and the District of Columbia unless otherwise noted, as they are expected to be more sensitive to the business cycle fluctuations that we consider. We supplement these counts with AAMC data on average composite and subcomponent MCAT scores, available at the national cohort level separately for applicants and matriculants from 1991 to 2014, the year before the exam's 2015 redesign.

To examine how both the number and characteristics of future physicians respond to business cycle fluctuations, we consider three key sets of outcomes from the AAMC data. First, to analyze how the number of individuals seeking to pursue a career in medicine changes when there are downturns in competing occupations, we examine the total number of applicants, both nationally and by state. Second, to study the types of individuals who respond to such shocks, we examine the characteristics of applicants: their undergraduate major grouping and gender, both nationally and by state, as well as their academic preparation as measured by MCAT scores at the national level. Finally, to assess how business cycle fluctuations affect the composition of future physicians, we consider the characteristics of individuals who ultimately matriculate to medical school, including their undergraduate

major, gender, and MCAT scores.

Major choice and gender can serve as informative signals of the types of people entering medicine, as both plausibly proxy for underlying preferences. While direct measures of individual preferences are unavailable in standard data sources, major choice may capture heterogeneity in, for example, enjoyment of coursework and the relative importance individuals attach to expected earnings and non-pecuniary job attributes (Arcidiacono, 2004; Zafar, 2013; Wiswall and Zafar, 2015; Kirkeboen et al., 2016), and differences in average preferences by gender are well documented (Croson and Gneezy, 2009; Falk and Hermle, 2018; Burbano et al., 2024). Consistent with this logic, experimental evidence on practicing physicians suggests that social preferences differ by both undergraduate major and gender.[6] To the extent these characteristics proxy for preferences, cyclical shifts in the majors and genders of applicants, and in turn matriculants, signal changes in the preferences of those entering medicine.

III.B National patterns

Number of applicants We begin by examining aggregate patterns in the number of applicants at the national level. Figure 2(a) plots the time series of the total number of U.S. applicants per available spot alongside the medical-school-applicant-relevant business cycle from 1989 to 2019. Applications to medical school are strongly countercyclical, with more students applying when outside options weaken due to poor economic conditions. The magnitude of this relationship is sizable: Figure 2(b) plots the number of U.S. applicants per spot against the medical-school-applicant-relevant unemployment rate and shows that from 1989 to 2019, a one-percentage-point increase in $\bar{U}_y$ was associated with 0.094 additional applicants per spot (p-value = 0.044),[7] an increase of 4.1 percent relative to the average of 2.3 applicants per spot (see Table A2). These patterns are consistent with findings from the education literature showing that educational attainment tends to rise when young adults

[6] Li et al. (2022) measure altruism among 284 physicians in an incentivized choice experiment. In their publicly available data, physicians from a traditional pre-medical major (defined as biology, chemistry, biochemistry, or nursing using their major categories, which are coarser than those in our main analysis) are more altruistic than those from non-traditional majors: though imprecise given the small sample, the gap is sizable (Figure A2(a)) and comparable to the male-female gap in the same data (Figure A2(b)). These differences hold within gender and within major groups (Figure A2(c)), so they are not driven by a correlation between major and gender.

[7] Throughout this section, p-values are computed using a wild bootstrap.

face adverse labor market conditions.[8]

Although an increase in applicants emerges following the Great Recession, the timing appears more delayed than for earlier recessions (Figure 2(a)). As the time between undergrad completion and medical school application has been increasing across cohorts (Laszcz, 2024), it is natural that the timing of the response to economic conditions has shifted over the 1989–2019 period. We therefore also examine 1989 to 2007 separately, as this is the window for which our baseline timing assumption—that the medical-school-applicant-relevant business cycle averages unemployment over the application year and the three preceding years—appears to most closely match application behavior. This window also aligns with the period covered by our analysis of eventual practice styles in Section IV, in which the timing of our outcome data and our requirement that physicians have completed their training limit that sample to those who applied before 2008. As shown in Figure 2(b) and Table A2, the relationship between the number of applicants and our baseline measure of relevant unemployment is substantially stronger in this earlier window: from 1989 to 2007, a one-percentage-point increase in $\bar{U}_y$ was associated with 0.81 additional U.S. applicants per spot (p-value = 0.006), a 35.8 percent increase over the mean.

Heterogeneity in applicant responses We next examine how these aggregate responses differ by domestic versus international status, undergraduate major grouping, and sex (Figure A3 and Table A2). The response is essentially absent among international applicants, for whom a one-percentage-point increase in $\bar{U}_y$ over 1989 to 2007 raised applicants per spot by only 0.017 (p-value = 0.098), about two percent of the domestic increase. This is unsurprising, as labor markets in the United States likely poorly reflect the outside options of international applicants. Among U.S. applicants, both major groupings (traditional and non-traditional pre-medical majors) and both sexes responded countercyclically over 1989–2007. Traditional pre-medical and non-traditional applicants responded similarly in absolute terms (0.43 and 0.31 additional applicants per spot per percentage point increase in $\bar{U}_y$ with p-values = 0.010), although the percent increase was larger for non-traditional majors given their smaller share of the applicant pool. However, men responded more strongly than women, with a one-percentage-point increase in $\bar{U}_y$ being associated with 0.53 additional male applicants per spot (p-value = 0.016; a 42.8 percent increase relative to the mean of

[8]Prior work documents the countercyclical nature of both college (Betts and McFarland, 1995; Barr and Turner, 2015) and graduate school (Bedard and Herman, 2008; Johnson, 2013) enrollment.

1.2) and 0.28 additional female applicants per spot (p-value < 0.001; a 27.3 percent increase relative to the mean of 1.0). Consistent with the earlier discussion of timing, these responses were weaker over the full 1989–2019 period.

We further decompose the U.S. applicant response by undergraduate major cluster (Figure A4).[9] Every cluster applied in greater numbers when conditions in competing occupations declined over 1989–2007. In absolute terms, the response was largest among traditional pre-medical applicants, with 0.43 additional applicants per spot per percentage point increase in $\bar{U}_y$, followed by Humanities and Applied Social Sciences (a cluster in which Psychology is the largest contributor, 0.11); Business, Economics, and Political Science (0.07); and Engineering (0.06). In percent terms, however, the largest responses came from clusters with the strongest outside labor market options: Engineering (approximately 100 percent of the cluster mean); Business, Economics, and Political Science (approximately 60 percent); and Computer Science, Math, and Physics (approximately 60 percent). As before, responses were more muted, though generally still positive, over the full 1989–2019 period.

Downturns also appear to shift the academic preparation of the applicant pool (Figure A5 and Table A3), not just its size and composition.[10] Over 1991–2007, the average applicant's composite MCAT score was unrelated to relevant unemployment (p-value $= 0.816$), though this null result masks an increase of 0.11 points per percentage point increase in $\bar{U}_y$ for the verbal reasoning component (p-value $= 0.070$; 0.05 within-cohort standard deviations). Over the full 1991–2014 period, average composite scores instead declined by 0.22 points as unemployment rose by one percentage point (p-value $= 0.016$; 0.04 within-cohort standard deviations), driven by a reduction in average biological sciences scores. Taken together, the evidence suggests that weaker outside options draw in a wider and, on average, slightly less pre-medically focused applicant pool.

Matriculant composition These shifts in the applicant pool will reshape future physician cohorts only if they survive the admissions stage: if medical schools continue to admit the students who would have applied regardless of economic conditions, the business cycle need not affect the delivery of care years later. Figure 3 plots the share of medical school

[9] Results are very similar if major-cluster-specific business cycles are used in place of $\bar{U}_y$, which weights across these major-cluster-specific unemployment rates (see Section II).

[10] Because MCAT scores trend upward over the sample period, all MCAT regressions include a linear time trend; the figures plot scores in levels and after residualizing from this trend.

matriculants who are from the United States (subfigure (a)), the share of U.S. matriculants from non-traditional pre-medical majors (subfigure (b)), and the share of U.S. matriculants who are female (subfigure (c)) against the medical-school-applicant-relevant unemployment rate from 1989 to 2019, with each subfigure showing the time series in the left panel and the scatterplot in the right panel.

The matriculant composition responds along some margins but not others. Despite the differential applicant response across U.S. and international applicants, the share of matriculants who come from the United States is essentially flat with respect to the business cycle (Figure 3(a)), indicating that U.S. medical schools admit a stable mix of domestic and international students regardless of applicant-pool composition.[11] Likewise, the gender composition of U.S. matriculants does not vary systematically with the business cycle (Figure 3(c)): although the applicant response is more pronounced among men, this does not translate into a more male matriculating class.

The undergraduate major composition, by contrast, shifts systematically. As shown in Figure 3(b), a one-percentage-point increase in $\bar{U}_y$ over 1989–2019 was associated with a 1.1 percentage point increase in the non-traditional share (p-value = 0.002), a 3.5 percent increase relative to the mean of 31.4 percent (Table A2). This pattern was far more pronounced before the Great Recession, where the corresponding increase was 4.7 percentage points (p-value < 0.001), or more than 15 percent of the mean. This shift is mirrored by a sharp decline in the traditional pre-medical share during downturns, with the offsetting gains concentrated in the Business, Economics, and Political Science; Engineering; and Humanities and Applied Social Sciences clusters (Figure A6).

There is also some evidence that the academic preparation of matriculants shifts with the cycle (Figure A7 and Table A3). Over 1991–2007, a one-percentage-point increase in $\bar{U}_y$ was associated with a 0.55 point increase in the average matriculant's composite MCAT score (p-value = 0.032; 0.12 within-cohort standard deviations), with gains in both the biological sciences (0.18 points, p-value = 0.056; 0.10 standard deviations) and verbal reasoning (0.29 points, p-value < 0.001; 0.16 standard deviations) components. These associations largely disappear over the full 1991–2014 period, where the composite effect is small and insignificant

[11] The full-sample estimate is small but statistically significant ($\beta = -0.12$, p-value = 0.028 over 1989–2019), while the pre–Great-Recession estimate has the opposite sign and is not statistically significant ($\beta = 0.27$, p-value = 0.192 over 1989–2007). The sign flip and small magnitudes suggest that any compositional shift along this margin is negligible.

(-0.01, p-value $= 0.902$).

Together, the evidence shows that matriculating cohorts tilt toward non-traditional majors and, before 2008, toward higher MCAT scores. These shifts in background and preparation may affect how these cohorts practice, which is the focus of Section IV.

III.C State-level analysis

We next exploit cross-state variation in economic conditions, a distinct source of identifying variation from the national time series examined above (see Figure 1(b)). The benefit of using state-level variation is that we can include year fixed effects in the specifications, thereby controlling for factors common to all states in a given year, including secular trends or national policy changes that move with the business cycle. The cost is that the year fixed effects absorb the aggregate time-series variation, leaving a smaller and noisier source of identification. Moreover, because admissions decisions are made in what is effectively a national market, national economic conditions are likely more relevant for the composition of matriculating cohorts than local conditions. Given these trade-offs, we view the national and state-level analyses as complementary.

We estimate regressions of the following form using data at the state-year level:

$$Outcome_{sy} = \beta \cdot \bar{U}_{sy} + \lambda_s + \lambda_y + \epsilon_{sy} \tag{1}$$

where $Outcome_{sy}$ denotes the number of applicants or matriculants per 100,000 state residents from state s in year y; $\bar{U}_{sy}$ is the medical-school-relevant unemployment rate constructed at the state level; and λ_s and λ_y are state and year fixed effects, respectively. The coefficient of interest, $\hat{\beta}$, captures how much more strongly applicants respond in states experiencing more severe downturns than in less-affected states in the same year. Because the cross-state variation is limited relative to the variation in the national cycle, we estimate equation (1) over the full 1989–2019 period. Observations are weighted by state population, standard errors are clustered by state, and p-values are computed using a wild cluster bootstrap (Cameron et al., 2008).

Results from estimation of equation (1) are shown in Table 1. Consistent with the national results, applications to medical school are strongly countercyclical: a one-percentage-point increase in the relevant state-level unemployment rate is associated with 0.38 additional

applicants per 100,000 residents (p-value = 0.024), a 2.7 percent increase relative to the mean (column (2) of panel (a)). The number of matriculants from a given state rises more modestly and is not statistically significant once both state and year fixed effects are included (column (4) of panel (a)), consistent with admissions clearing in a national market. For this reason, we examine the composition of matriculants only in the national data.

The remainder of Table 1 reports the applicant response by undergraduate major grouping (panel (b)) and sex (panel (c)). As in the national data, both traditional and non-traditional pre-medical majors respond countercyclically, with somewhat larger proportional increases among non-traditional majors. Applications by both men and women similarly rise during downturns, with men again responding more strongly. The fact that these cross-state patterns mirror the national results, despite relying on different variation, strengthens our confidence that the identified relationships reflect the effects of economic conditions on medical school application behavior.

IV Physician practice style and organization

IV.A Data

To examine how selection into medicine across the business cycle affects physician practice styles, we draw on a number of data sources to measure physician outcomes and characteristics.

Outcome data We construct our outcome measures from four sources. First, we draw on the public-use Medicare Part B files at the physician-year level from 2013 to 2019. We construct three outcomes from these data. The first is the average annual Medicare revenue per beneficiary, which serves as a proxy for treatment intensity. We calculate this measure by dividing the total standardized Medicare payment reported for each physician-year by the number of unique beneficiaries treated.[12] Because Medicare prices are administratively set at the federal level, and because standardized payments exclude geographic adjustments and special program payments unrelated to resource use, variation in this measure primarily

[12]We truncate the number of beneficiaries at the 99th percentile to limit the influence of likely data errors—or incident-to billing where care is delivered by a different provider—associated with exceptionally large patient panels.

reflects differences in the amount and mix of care provided. The second outcome is the average risk score of patients treated by each physician, which provides a summary measure of patient health based on CMS's Hierarchical Condition Category model. Higher risk scores correspond to sicker patient populations and greater expected spending. Finally, because beneficiaries who are dually eligible for Medicaid—the public health insurance program for low-income and disabled individuals in the United States—tend to be sicker, and because physicians are reimbursed at lower rates for treating such patients in most states, we also examine the share of each physician's Medicare patients who are dually eligible for Medicaid.

Second, we complement the public-use Part B files with a 20 percent sample of patient-level Medicare carrier claims from 2001 to 2016. We construct four sets of outcomes at the physician-year level, the first two of which center on physician billing. First, we compute average Medicare revenue per beneficiary in parallel to our public-use measure, both as a verification that the billing response replicates in this sample and as the basis for the decomposition that follows. Second, we decompose total revenue into eight mutually exclusive service categories using Berenson-Eggers Type of Service (BETOS) codes: E&M, elective procedures, non-elective procedures, ambiguous procedures, tests, imaging, physician-administered drugs, and other.[13]

The remaining two sets of outcomes use these claims to examine impacts on patient mix and patient health outcomes. The first captures subsequent ED use among a physician's Medicare beneficiaries. We classify ED visits using the algorithm of Billings et al. (2000), which assigns each visit probabilities of falling into four categories based on its primary discharge diagnosis code: (1) emergent, not preventable or avoidable, (2) non-emergent, (3) emergent but primary-care treatable, and (4) emergent, preventable or avoidable. We assign an ED visit to a category when its probability exceeds 50 percent, and in our pri-

[13]BETOS codes are a Healthcare Common Procedure Coding System (HCPCS)-level classification system maintained by CMS and available through ResDAC at https://resdac.org/cms-data/variables/line-berenson-eggers-type-service-betos-code. The classification assigns each HCPCS code to one of seven top-level categories, identified by letter prefix: E&M (M), procedures (P), imaging (I), tests (T), durable medical equipment (D), other services (O), and exceptions/unclassified (Y). We use the BETOS letter prefix to assign HCPCS codes to E&M, imaging, and tests. We separate physician-administered drugs, which BETOS classifies within "other services," into their own category; combine the remainder of "other services" with durable medical equipment and exceptions/unclassified into a single "other" category; and split procedures into three subcategories (elective, non-elective, and ambiguous) based on the degree of physician discretion over whether and when to intervene. This yields eight categories in total. Table A4 provides the full mapping of procedure subcategories to these classifications. Because the "other" category is a residual grouping, we exclude it from our analyses; average physician-year billing per beneficiary in this category is only $4.10.

mary analyses, label the first as "non-avoidable" and group (2)–(4) as "avoidable." For each physician-patient visit, we then construct indicators for whether the beneficiary had any subsequent ED visit of each type within one year. Averaging across a physician's visits in a year yields the share of visits followed by an ED visit of that type, or equivalently the visit-weighted share of beneficiaries with such a visit.[14] The second captures predicted and actual one-year mortality. Following Chan et al. (2023), we predict individual-level mortality at the visit level based on patient demographics and Elixhauser comorbidities, capturing expected mortality given observable health status at the time of treatment.[15] As with ED visits, we then aggregate visit-level mortality measures to the physician-year level by averaging across visits with each physician in each year. Non-avoidable ED visits and predicted mortality characterize patient mix, capturing the severity of the cases a physician treats, while avoidable ED visits and actual mortality conditional on predicted mortality capture patient health outcomes.

The remaining two data sources capture the organizational structure of practices in which physicians choose to work. The first, CMS's Physician Compare database, provides our measure of practice size—the average annual number of clinicians reported in each organization with which the physician is affiliated—for the period 2014 to 2019.[16] Recent work using administrative tax data documents a strong negative relationship between physician income and firm size (Gottlieb et al., 2025), suggesting that physicians with stronger financial motivation may prefer to practice in smaller organizations.[17] The second, IQVIA's SK&A Office-Based

[14]Visits the algorithm does not score (injury, mental health, alcohol, drug-related, and unmapped diagnoses) are excluded from the categorized measures but are included in the any-ED-visit measure. Results are robust to using an 80 percent threshold and to shorter windows over which future ED visits are measured (one month or one quarter).

[15]We generate Elixhauser comorbidities using ICD diagnosis codes from the carrier, outpatient, and inpatient claims files, supplementing the carrier claims with the other settings since comorbidity burden is best characterized using diagnoses from all care settings. Merging these data with the Master Beneficiary Summary File for age, sex, and race, we construct indicators for whether each Elixhauser condition ever appeared and whether it occurred within the past year, and regress actual one-year mortality on these indicators, ten age-bin dummies, sex, and race. Predicted mortality at the visit level is the prediction from this model.

[16]We use December snapshots of the Physician Compare files for all years except 2015, for which only a November snapshot is available. Practice size is based on the number of clinicians affiliated with each Group Practice PAC ID, a unique identifier assigned in CMS's Provider Enrollment, Chain, and Ownership System (PECOS). Medicare defines group membership using the Medicare Data on Provider Practice and Specialty (MD-PPAS) file, which links providers to practices based on their National Provider Identifier (NPI) and Tax Identification Number (TIN).

[17]Gottlieb et al. (2025) show that average annual earnings for physicians peak at approximately $470,000 in firms with eight to ten clinicians before declining as firm size increases. This pattern likely reflects the additional business income earned through ownership in smaller group practices.

Physician Database, provides information on practice ownership and health system affiliation for office-based physicians in the United States from 2008 to 2016. These data allow us to distinguish among physician practices that are independent and unaffiliated, independent and hospital-affiliated, and hospital- or system-owned.[18] Compensation models differ systematically across these ownership types: physicians in hospital- and system-owned practices are more likely to be salaried, whereas those in independent practices—most of which are affiliated with hospitals—are typically paid on a fee-for-service basis (AMA, 2025). As a result, more financially motivated providers may systematically sort away from hospital- or system-owned practices and into independent ones.

Physician characteristics and sample selection We obtain information on physician characteristics—graduation year, medical school, specialty, credential, and sex—by combining two sources: CMS's Physician Compare database referenced above, covering 2014 to 2019, and a 2018 extract of the AMA Masterfile. Each source is restricted to a particular set of providers. Physician Compare includes only clinicians who were enrolled in Medicare and had recently submitted a Medicare fee-for-service claim, whereas the AMA extract is restricted to providers appearing in an IQVIA dataset that includes all providers who wrote at least one opioid, antidepressant, anti-anxiety, or antipsychotic prescription between 2006 and 2018.[19]

The two sources differ in how cleanly they identify these characteristics. The AMA extract reports a single value per provider, so no reconciliation is required. In the Physician Compare data, by contrast, a provider can appear across multiple annual files with conflicting entries, so we assign a single value for each characteristic before combining the sources.[20]

[18]We define four exhaustive practice categories: independent and hospital-affiliated, independent and unaffiliated, hospital- or health system-owned, and other. Approximately 20 percent of providers report more than one practice location in a given year.

[19]To be listed in Physician Compare, a clinician must be in approved status in CMS's PECOS, report at least one practice location and one specialty, and have submitted a Medicare fee-for-service claim (or be newly enrolled) within the prior six months. The four IQVIA drug classes are among the most commonly prescribed in the United States, so the requirement of having written at least one such prescription captures the large majority of practicing physicians.

[20]For specialty and credential, we use the value reported in the most recent file in which the provider appears if only one value is ever listed, and otherwise the most common value across all years. For sex, we take the most common value, setting it to missing in the event of a tie. For medical school and graduation year, we again take the most common value; in the event of a tie, we use the value from the earliest file in which the provider appears, since later changes often reflect subsequently reported training rather than initial medical education, which is what we wish to capture.

For providers appearing in both, we use the AMA values, as the Masterfile is the standard authoritative source for physicians' training and credential information and records initial medical education directly; for those appearing in only one, we use whichever is available. Results are robust to constructing the analysis sample using either source alone.

From this combined file, we construct our analysis sample. We keep only providers in specialties that require a physician degree (MD or DO), dropping non-physician clinicians such as physical therapists. Among these, we retain only allopathic (MD) physicians who earned their medical degree in the United States.[21] We further limit the sample to individuals who applied to medical school during the period covered by our application data (1989–2019), allowing us to link cohort-level variation in practice outcomes to cohort-level variation in applicant and matriculant characteristics. Finally, to focus on long-run practice patterns rather than transitory early-career behavior, we restrict to physician-year observations occurring at least seven years after medical school graduation. By this point, physicians have completed residency, and those who pursued fellowship training have largely completed that as well. Given the coverage of our outcome data, this limits the analysis to physicians who applied between 1989 and 2007 when using the public-use Medicare data and between 1989 and 2004 when using the restricted Medicare and SK&A data.[22]

Table 2 reports summary statistics for the resulting samples. Column (1) describes the universe of physicians in the combined attribute files who meet the sample inclusion criteria, while columns (2) through (4) describe the analysis samples underlying the public-use Medicare data, SK&A data, and restricted Medicare data, respectively. Here and in the subsequent analyses, we classify each physician's primary specialty into one of 14 mutually exclusive and exhaustive categories based on training pathway and practice setting.[23] Across

[21] We classify a physician as osteopathic if the source from which we draw their characteristics lists their credential as DO or records a DO-granting medical school, whereas we classify a physician as a U.S. medical graduate if that source places their medical school in the United States. We restrict our attention to U.S.-trained MDs for two reasons. First, our applicant and matriculant analysis covers U.S. MD-granting institutions, so the restriction aligns the samples across the contemporaneous and downstream outcomes. Second, if tighter U.S. MD admissions during downturns divert some would-be MD entrants into DO or foreign medical programs, then recession cohorts from these groups mix selection into medicine with selection across training pathways, providing a less clean test of selection into medicine.

[22] Both sets of outcomes we construct from the restricted Medicare carrier claims—subsequent ED use and one-year mortality—are measured over the year following each visit. These outcomes are therefore constructed for visits through 2015, using claims through 2016. This additional restriction requires us to limit the sample to physicians who applied between 1989 and 2003.

[23] Specialties corresponding to a distinct graduate medical education training pathway are grouped on that basis: primary care (internal medicine, family medicine, general pediatrics, and combined programs such as med-peds), surgical specialties, obstetrics/gynecology, neurology, dermatology, and behavioral health

all samples, the composition is broadly similar: primary care is the largest category (roughly a quarter to a third of physicians), followed by surgical and facility-based specialties. The average sample physician applied to medical school in the mid to late 1990s and has over a decade of post-graduation experience, and roughly 40 to 45 percent of sample physicians are female. The samples differ somewhat in how their members are drawn from the two source files: nearly all physicians appear in the AMA extract (96–97 percent), while the share appearing in Physician Compare in the outcome samples ranges from about 88 percent in the SK&A data to 98 percent in the public-use Medicare sample, reflecting that the public-use Medicare sample is itself constructed from a CMS source.

IV.B Empirical specification

Our goal is to analyze how variation in physician-level practice outcomes relates to the business cycle at the time of medical school application. To do so, we regress measures of physician-year practice outcomes on the medical-school-applicant-relevant unemployment rate at the time of application while controlling for physician experience, specialty, and outcome year. Specifically, we estimate specifications of the following form:

$$Outcome_{it} = \beta \cdot \bar{U}_{y(i)} + \lambda_{specialty} + \lambda_{experience} + \lambda_t + \epsilon_{it} \tag{2}$$

where $Outcome_{it}$ is an outcome measure for physician i in year t; $\bar{U}_{y(i)}$ is the medical-school-applicant-relevant unemployment rate for application year y; $\lambda_{specialty}$ and $\lambda_{experience}$ denote fixed effects for the specialty and number of years since medical school graduation for physician i, respectively; and λ_t denote outcome year fixed effects. The specialty, experience, and outcome year fixed effects absorb differences in practice patterns across specialties, career stages, and calendar years, as well as any component of $\bar{U}_{y(i)}$ that is linear in application year.[24] Although specialty could itself respond to entry conditions—which would make it

(psychiatry and related behavioral specialties). Internal medicine subspecialties typically entered through fellowship are grouped as medical subspecialties, with oncology broken out separately because per-physician spending is far higher for this group than for other internal medicine subspecialties. The remaining categories are defined by practice setting or clinical function rather than a single training pathway, reflecting the heterogeneous routes into these roles: facility-based specialties (anesthesiology, emergency medicine, critical care, and hospitalists), diagnostic specialties (radiology and pathology), musculoskeletal/rehabilitation, non-surgical proceduralists (e.g., interventional cardiology and radiology, electrophysiology), and supportive/palliative care. Physicians whose specialty does not fall into any of these groups—including preventive medicine, occupational medicine, and unspecified specialties—are assigned to an "other physician" category.

[24] Since medical school is four years, and applicants apply the year before matriculation, we subtract five

a "bad control" (Angrist and Pischke, 2009)—we show below that specialty composition is stable across entry cohorts (Section IV.D), consistent with fixed residency slots constraining the specialty distribution regardless of shifts in cohort preferences.[25] P-values and confidence intervals are calculated using a wild cluster bootstrap clustered by application cohort (Cameron et al., 2008).

Estimation of equation (2) delivers a single estimate summarizing the relationship between the economic conditions in competing occupations at the time of medical school application and practice outcomes for physicians later in their careers. To examine how the effect size varies across the distribution of observed economic conditions, we further estimate versions that replace the continuous medical-school-applicant-relevant unemployment rate with indicators for quartiles of this measure. Letting Q_q denote the range of values of unemployment rates in quartile q, we estimate the following regression:

$$Outcome_{it} = \sum_{q=2}^{4} \beta_q \cdot \mathbb{1}\left\{\bar{U}_{y(i)} \in Q_q\right\} + \lambda_{specialty} + \lambda_{experience} + \lambda_t + \epsilon_{it} \tag{3}$$

where $\mathbb{1}\left\{\bar{U}_{y(i)} \in Q_q\right\}$ is an indicator denoting whether the medical-school-applicant-relevant unemployment rate for application year y was in quartile q over 1989 through 2007, and all other variables are defined as in equation (2). Figure A8 illustrates the binning procedure, showing how application years are grouped into quartiles based on the medical-school-applicant-relevant unemployment rate.

IV.C Main results

Physician billing We begin by considering impacts on physician billing to Medicare, finding that physicians who applied to medical school during downturns bill substantially more per beneficiary years later, particularly on discretionary margins. Column (1) of Table 3 reports results from estimation of equation (2) using average billing per Medicare beneficiary from the public-use Medicare files from 2013–2019 at the physician-year level as the outcome.

from the year of medical school graduation available in different data sources to assign application year y to practicing physicians. Application year is therefore a deterministic function of outcome year and experience, so cohort fixed effects cannot be included alongside λ_t and $\lambda_{experience}$. We view this as a feature of the design: secular changes across entry cohorts are differenced out, and identification comes instead from the repeated peaks and troughs in the applicant-relevant business cycle over our sample period (Figure 1(a)).

[25] Consistent with this, we show in Section IV.D that our estimates are nearly identical with and without specialty controls.

The estimate shows that a one-percentage-point increase in $\bar{U}_{y(i)}$ leads to a \$14.17 increase in average annual Medicare revenue per beneficiary (p-value = 0.006), corresponding to a 4.7 percent increase relative to the mean of \$301.4. This response replicates in the restricted Medicare data over the longer 2001–2016 period: column (1) of Table 4 shows that a one-percentage-point increase in $\bar{U}_{y(i)}$ raises average annual Medicare revenue per beneficiary by \$13.72 (p-value < 0.001), a 4.7 percent increase relative to the mean of \$291.7. As shown in Figure 4(a), which overlays the coefficients on quartiles of $\bar{U}_{y(i)}$ from estimation of equation (3) in both datasets, this effect is not driven by any single quartile of the unemployment rate in competing occupations; rather, average revenue per beneficiary increases steadily as employment in alternative occupations declines.

The remaining columns of Table 4 decompose the increase in total billing per beneficiary across the seven reported service categories in the restricted Medicare data over 2001–2016: E&M, elective procedures, non-elective procedures, ambiguous procedures, tests, imaging, and physician-administered drugs.[26] The largest absolute increase comes from physician-administered drugs (column (8)): a one-percentage-point increase in $\bar{U}_{y(i)}$ raises drug spending per beneficiary by \$6.26 (p-value = 0.004), a 26.8 percent increase relative to the mean of \$23.3 and accounting for roughly 46 percent of the total increase in billing. Billing on procedures classified as elective or ambiguous also responds strongly: revenue from elective procedures rises by \$2.05 (p-value = 0.008; 5.2 percent of the mean) and revenue from ambiguous procedures rises by \$3.18 (p-value = 0.004; 7.1 percent of the mean). Billing on tests shows a smaller, marginally significant increase (\$1.05, p-value = 0.094; 3.9 percent of the mean). In contrast, spending categories with limited physician discretion show no detectable response: the coefficients on E&M services (column (2)) and non-elective procedures (column (4)) are statistically indistinguishable from zero (p-values of 0.606 and 0.928, respectively). We also find no detectable effect on imaging (column (6), p-value = 0.716); unlike tests, which are often physician-billable, imaging in carrier claims is largely billed by interpreting radiologists rather than the ordering physician, so imaging generates little direct revenue for most of our sample.[27] These patterns indicate that the additional spending by

[26] Figure A9 plots the corresponding quartile coefficients. Recall that we omit the residual "other" category from these analyses, as it accounts for less than 2 percent of total billing and can be recovered from the other columns.

[27] Because our estimates measure what each physician bills rather than what they induce others to bill, the null is also consistent with recession-cohort physicians ordering more imaging but with the additional spending recorded under the radiologists who read it. In that case our estimates understate the spending

recession-cohort physicians is concentrated on margins where physicians exercise the most discretion and where they directly capture the resulting revenue.

A natural way to benchmark the increase in revenue per beneficiary is to scale the average spending effect by the change in the share of non-traditional entrants to medicine. Over 1989–2007, a one-percentage-point increase in applicant-relevant unemployment increases the share of matriculants from non-traditional majors by 4.7 percentage points (Figure 3(b)) and raises average Medicare revenue per beneficiary by around $14 per physician-year in both data sources (columns (1) of Tables 3 and 4). Under the strong assumptions that (1) the change in matriculants from non–pre-medical majors fully captures the share of marginal entrants and (2) only marginal entrants change their behavior, the implied spending increase among marginal entrants is around $300 per beneficiary ($14 / 0.047), or roughly 100 percent of the mean in either data source.

While a useful benchmark, this calculation likely overstates behavioral changes among these marginal entrants. First, some individuals may switch into traditional pre-medical majors (e.g., from economics to pre-med) before we observe undergraduate major, in which case they would be misclassified as inframarginal even though their decision to enter medicine responded to labor market conditions. Second, some individuals in traditional pre-medical majors may initially have intended to pursue non-medical careers but switched into medicine in response to weaker outside options. Both forms of unobserved switching would lead us to understate the share of marginal entrants and therefore overstate the implied spending effect among them. This interpretation is reinforced by the fact that applications increase among both traditional and non-traditional majors during downturns (Figure A3(b)), suggesting that some marginal entrants are likely classified as traditional in our data. Third, if inframarginal physicians also respond—for example, through peer effects (see Section V)—then part of the observed average increase in spending will reflect changes among inframarginal physicians, mechanically implying a smaller effect among marginal entrants.

Patient mix Higher revenue per beneficiary could reflect either greater patient need or higher intensity of physician practice styles. While equation (2) includes controls for outcome year, physician experience, and physician specialty—which account for secular trends in patient need and average differences in patient severity across experience and specialty

these physicians induce. The same holds for any service a physician orders but does not perform.

profiles—we directly examine whether the composition of patients treated by a physician varies with labor market conditions at the time the physician applied to medical school. The evidence presented in Tables 3 and 5, with the corresponding quartile patterns in Figures 4 and 5, indicates that physicians who applied to medical school when outside options were more limited treat slightly healthier patients. This suggests that their higher revenue per beneficiary reflects greater practice and/or billing intensity rather than greater patient need.

The public-use Medicare files provide two proxies for patient severity. Column (3) of Table 3 shows that a one-percentage-point increase in $\bar{U}_{y(i)}$ leads to a 0.34 percentage-point reduction (p-value = 0.002) in the share of Medicare beneficiaries who are dually eligible for Medicaid—a population that is typically sicker and more medically complex than Medicare-only beneficiaries (MedPAC, 2023)—corresponding to a 1.4 percent decline relative to the mean of 24.1 percent.[28] The point estimate on average risk scores (column (2)) is also negative but is not statistically distinguishable from zero (p-value = 0.244).

The restricted Medicare data provide two further measures of patient composition. Column (1) of Table 5 shows that a one-percentage-point increase in $\bar{U}_{y(i)}$ reduces the share of a physician's beneficiaries with a non-avoidable ED visit in the following year by 0.27 percentage points (p-value = 0.016), a 1.8 percent decline relative to the mean of 15.1 percent. Because these are emergent visits that appropriate ambulatory care could not have prevented, they reflect underlying patient severity rather than the quality of care a physician delivers. Column (3) shows a corresponding reduction in predicted one-year mortality of 0.18 percentage points (p-value = 0.014), a 1.6 percent decline relative to the mean of 10.9 percent. As mortality is predicted from patient demographics and Elixhauser comorbidities observed at the time of treatment, this measure again reflects patient mix rather than any subsequent effect of the physician's care.

Patient health outcomes While the additional billing by physicians who applied to medical school during downturns is concentrated on margins typically involving clinical discretion, suggesting that the services may not be strictly necessary, the higher intensity of service provision could still improve patient health. We therefore examine impacts on patient

[28]The negative relationship between $\bar{U}_{y(i)}$ and the share of dually eligible Medicare beneficiaries is also consistent with greater financial motivation among individuals entering medicine during economic downturns, given that physicians face lower reimbursement rates and higher administrative burdens when treating dually eligible patients (Li, 2023).

outcomes directly using the restricted Medicare data in Table 5 and Figure 5.

The evidence suggests that the higher billing intensity of physicians who applied to medical school during downturns does not translate into measurably different patient outcomes. Column (2) of Table 5 shows no detectable effect on the share of a physician's beneficiaries with an avoidable ED visit in the following year: a one-percentage-point increase in $\bar{U}_{y(i)}$ is associated with a statistically insignificant 0.15 percentage-point reduction (p-value = 0.388; -0.6 percent of the mean).[29] Unlike the non-avoidable visits discussed above, these are visits that appropriate ambulatory care could plausibly have prevented, making them the margin that differences in service intensity could affect. Similarly, column (4) shows that the additional spending does not translate into reduced mortality, with a one-percentage-point increase in $\bar{U}_{y(i)}$ having no impact on actual one-year mortality conditional on predicted (p-value = 0.956). Because the specification conditions on predicted mortality, this coefficient captures differences in realized survival among patients with similar observable health status at the time of treatment, the margin over which a physician's care could plausibly matter.

Both nulls are informative. Our 95 percent confidence intervals rule out improvements larger than 0.56 percentage points in avoidable ED visits (2.2 percent of the mean) and 0.14 percentage points in one-year mortality (1.1 percent of the mean). In each case the same specification identifies a difference in the corresponding measure of patient mix, constructed from the same underlying data: avoidable and non-avoidable ED visits are the same visits split by the Billings classification of the discharge diagnosis, and predicted mortality is computed for the same patients and visits as realized mortality. That differences appear on the patient-mix side and not on the outcome side is more consistent with the absence of an effect than an inability to detect one.

Physician practice environments The business cycle at the time of application to medical school also affects the practice environments in which physicians ultimately work: those who applied during downturns work in smaller organizations and sort into practice arrangements with higher-powered financial incentives. As shown in column (4) of Table 3 and Figure 4(d), a one-percentage-point increase in $\bar{U}_{y(i)}$ reduces the size of the average physician's practice by 13.62 providers (p-value = 0.010), a 2.6 percent decline relative to the

[29] Table A5 reports effects separately on each of the three subcategories that make up the avoidable group. We find no detectable effect on any of the three, indicating that the null for the avoidable group does not mask offsetting movements across its components.

mean. Because physician compensation is generally decreasing in firm size (Gottlieb et al., 2025), this pattern is consistent with greater financial motivation among individuals entering medicine when outside options are weaker.

Results from the SK&A data show how office-based physicians sort across organizational forms as a function of labor market conditions at entry. Table 6 and Figure 6 show that cohorts who applied when outside options were limited are more likely to work in hospital-affiliated independent practices (2.15 percentage points, p-value < 0.001) and are less likely to work in hospital- or health-system-owned practices (-1.76 percentage points, p-value < 0.001). Both effects amount to changes of around 4 percent relative to their respective means. As shown in column (3) of Table 6 and Figure 6(c), we observe no differential sorting into independent, non-affiliated practices (-0.23 percentage points, p-value $= 0.270$), which likely reflects their rarity over our sample period (6.7 percent of physician-year observations, compared with 49.7 and 43.5 percent for hospital-affiliated and hospital-owned practices). Because earnings are typically tied more directly to effort and billing intensity in independent practices, whereas hospital- and system-owned settings are more often salaried, these patterns again indicate that physicians entering medicine when outside options are weak sort into practice arrangements with higher-powered financial incentives.

IV.D Additional analyses

Impacts on specialty choice A natural question is whether physicians entering under different conditions sort into different specialties, which could shape practice patterns directly and would raise the concern that conditioning on specialty in equations (2) and (3) constitutes a "bad control" (Angrist and Pischke, 2009). We find no consistent evidence of such sorting. In particular, we estimate analogs of equations (2) and (3) with indicators for five mutually exclusive and exhaustive specialty groups on the left-hand side—primary care, surgical specialties, facility-based specialties, medical subspecialties, and other—and excluding specialty controls on the right-hand side. As shown in Table 7, we find no statistically significant relationship between applicant-relevant unemployment and specialty, whether a single specialty is assigned to each physician (panel (a)), as when using specialty as a control in our main specifications, or measured at the physician-year level (panel (b)).[30] The quartile versions show the same pattern (see Figure A10).

[30] Estimated separately by sex, the specialty results show no consistent pattern for either men or women.

This pattern is consistent with residency slots being largely fixed across specialties: even though physician preferences are correlated with specialty choice (Li, 2018; Attema et al., 2023), the fixed supply of slots constrains the specialty distribution even when the preference composition of a cohort shifts. The practice style and organizational effects documented above therefore operate within specialty rather than through which specialties physicians enter.

Heterogeneity by physician sex We next examine heterogeneity in the practice style and organizational effects by physician sex. As shown in Section III.B, labor market conditions at the time of application do not affect the gender composition of medical school matriculants: across cohorts, the share of women among matriculating physicians is stable with respect to business cycle conditions in competing occupations (see Figure 3(c) and Table A2(d)). At the same time, application behavior differs by sex, with the number of male applicants per available medical school seat increasing somewhat more than the number of female applicants per seat during economic downturns (Table 1(c), Table A2(c), and Figure A3(c)). As a result, even though the observed gender composition of matriculants remains unchanged, the pool of men who ultimately matriculate may be more differentially selected during downturns than the corresponding pool of women.

Consistent with this pattern, we observe heterogeneity by physician sex in subsequent practice outcomes, particularly those relating to revenue incentives. As shown in column (1) of Table A6, the effect of $\bar{U}_{y(i)}$ on Medicare spending per beneficiary in the public-use files is over twice as large for male physicians as for female physicians, although the difference is not statistically significant (p-value on difference $= 0.342$). The same pattern emerges in the restricted Medicare data over 2001–2016: as shown in column (1) of Table A7, a one-percentage-point increase in $\bar{U}_{y(i)}$ raises spending per beneficiary by \$19.69 among male physicians (p-value < 0.001) and by \$6.70 among female physicians (p-value $= 0.126$), a difference that is statistically significant at the 5 percent level (p-value on difference $=$ 0.014).

Decomposing the overall spending effect by service category reveals that the gender gap is concentrated in the most discretionary, revenue-generating categories. As shown in Table A7, a one-percentage-point increase in $\bar{U}_{y(i)}$ raises physician-administered drug spending by \$8.28 among male physicians (p-value < 0.001), compared to \$3.57 among female physicians

(p-value = 0.212), although the difference is not statistically significant (p-value on difference = 0.142). The same pattern holds for "other procedures"—those that may be discretionary or non-discretionary depending on patient presentation—where the increase is \$4.39 among male physicians (p-value = 0.004) and only \$0.70 among female physicians (p-value = 0.510; p-value on difference = 0.050). In contrast, the increases in spending on elective procedures are more similar for male and female physicians (\$2.35 [p-value = 0.066] for men versus \$1.56 [p-value = 0.214] for women; p-value on difference = 0.652). Neither group exhibits a significant response on the remaining categories.

Gender differences in responsiveness are also pronounced for practice organization. As shown in Table A8, the increase in employment in hospital-affiliated independent practices and the corresponding decline in hospital- or health-system-owned employment caused by higher $\bar{U}_{y(i)}$ are significantly larger among male physicians, with the difference in responsiveness between male and female physicians being statistically significant (p-values on difference of 0.068 and 0.014, respectively).

There is less of a gender divide for impacts on patient mix and patient health. If anything, the reductions in non-avoidable ED visits and predicted mortality are somewhat larger among female physicians, but in neither case is the difference between men and women statistically significant (p-values on difference of 0.336 and 0.804; columns (1) and (3) of Table A9). The patient health outcomes remain null for both groups, consistent with the overall results (columns (2) and (4)).

Robustness The impacts of entry conditions on practice styles, patient composition, patient outcomes, and practice organization are not sensitive to a wide range of empirical choices.

Table A10 summarizes results using alternative specifications and samples for outcomes in the public-use Medicare data. Column (1) reports our baseline results for reference. Column (2) additionally controls for average patient risk scores; because cohorts who entered medicine during economic downturns tend to see slightly healthier patients, the gradient in spending per beneficiary is modestly larger when controlling for patient risk. Column (3) weights observations by the total number of beneficiaries at the provider-year level to reduce noise from providers with relatively few Medicare patients; although estimates are somewhat attenuated, qualitative patterns remain unchanged. Finally, columns (4)–(6) vary

the sample. Column (4) requires at least ten years since medical school graduation, rather than seven in our baseline specification, to accommodate longer fellowship pathways and focus on more established practice patterns. Column (5) extends the outcome period to include the COVID-19 years (2020–2023), rather than ending the sample in 2019. Column (6) includes application cohorts who applied before the start of the AAMC data in 1989.[31] Results are robust across these alternative samples.

We also verify that our estimates are not driven by the specific set of fixed effects we include. The results presented in Tables 3–6 are very similar when specialty fixed effects are excluded and remain highly stable when physician sex and practice ZIP code fixed effects are added (see Figures A11–A14). Together with the absence of specialty sorting documented above, these patterns indicate that the impacts of entry conditions on practice styles and organization are not driven by changes in specialty, gender, or location across cohorts, but rather reflect differences in behavior within specialty and market.

Tables A11–A15 report results using alternative measures of economic conditions as the independent variable of interest for outcomes in the public-use Medicare data (Table A11), the restricted Medicare data (Tables A12–A14), and the SK&A data (Table A15). Column (1) again reports our baseline results from estimation of equation (2) for reference, which use the medical-school-applicant-relevant business cycle averaged over the year of application to medical school and the three preceding years. As shown in column (2), using the overall business cycle—also averaged over the year of application and the three preceding years—yields smaller estimates for all outcome measures, consistent with the idea that some fluctuations in the overall economy are not relevant for individuals on the margin of applying to medical school. Because increased unemployment among physicians should have opposite theoretical implications from increased unemployment in other high-skilled occupations, column (3) constructs the applicant-relevant business cycle holding physician employment fixed. Results using this alternative measure are very similar to the baseline, and in some cases more pronounced. Finally, columns (4)–(6) use the baseline medical-school-applicant-relevant unemployment rate but vary the averaging window. Estimates are substantially attenuated for most outcomes when only employment conditions in the year of application are considered (column (4)), consistent with the idea that students require time to adjust

[31] We exclude physicians with more than 30 years of experience since medical school graduation to avoid changes in practice patterns associated with retirement decisions. This restriction allows us to include physicians who applied to medical school from 1978 onward in this exercise.

their coursework in order to pivot toward medicine. In contrast, results tend to be very similar when averaging over the year of application and the two preceding years (column (5)) or the four preceding years (column (6)).

Finally, because different application cohorts mechanically contribute observations at different experience levels in different outcome years, our baseline samples have an unbalanced cohort–year–experience structure. As a result, cohort-driven differences may be partially absorbed by the year or experience fixed effects, and cohorts that disproportionately populate certain cells may exert greater influence on the estimated effects. Tables A16–A20 report results from alternative sample restrictions and weighting schemes that address these concerns by imposing support or balance across cohorts, years, and experience levels, dropping cohorts at the extremes of the unemployment distribution, and reweighting experience–year cells.[32] Reassuringly, the results are robust across these alternatives.

V Potential mechanisms

When the medical-school-applicant-relevant unemployment rate is higher, several things change that could affect the performance of the resulting physician cohort.

Selection into medicine We focus first on two mechanisms that operate through who chooses to apply to medical school, and in turn the composition of the matriculating class. First, when outside options weaken, more individuals apply for a largely fixed number of medical school slots, reducing acceptance rates and increasing selectivity (Figure 2). If medical schools can effectively screen on clinical aptitude, heightened selectivity could raise the average skill of matriculating students, thereby improving cohort performance. Notably, we find some evidence that MCAT scores among matriculating cohorts rise during downturns,

[32]Each table reports six specifications. Column (1) reproduces the baseline estimate. Column (2) restricts to a support-balanced sample, requiring each cohort, outcome year, and experience level to be observed with at least four values along the other two dimensions (see Figures A15–A17(b)). Columns (3) and (4) impose balance on two of the three dimensions: column (3) balances across outcome years and cohorts (Figures A15–A17(c)), so that differential cohort representation across years does not influence the outcome year fixed effects, and column (4) balances across outcome years and experience levels (Figures A15–A17(d)), so that the results are not driven by shifts in experience composition over time. Column (5) differs by data source according to the length of the outcome window: the restricted Medicare data span enough outcome years to balance across experience and cohort (see Figure A16(e)), whereas the shorter public-use Medicare and SK&A windows do not, so there we instead drop the cohorts with the highest and lowest values of $\bar{U}_y$. Column (6) weights observations by the inverse share of physician-year observations in each experience–outcome year cell, preventing heavily represented cells from receiving greater implicit weight.

although only modestly (Table A3). Second, the composition of matriculating classes may shift along dimensions other than skill that bear on later practice. Downturns draw entrants away from competing fields that are more subject to business cycle fluctuations, such as business, finance, and engineering (Figure A6). If these entrants have different preferences—for example, if they weight financial returns more heavily—such composition changes could lead to systematic differences in practice style even conditional on skill.

Importantly, both of these channels could affect not only the marginal physicians themselves, but also their "always-taker" peers through peer effects. Changes in the distribution of skills or preferences could influence training environments, peer learning, professional networks, and ultimately practice norms for everyone in the cohort, propagating the effects of entry-time conditions beyond marginal entrants to inframarginal physicians as well.

The empirical findings shown in Section IV.C are more consistent with the impacts of entry conditions on future practice styles being driven by changes in physician preferences, potentially amplified through peer effects, than with changes in clinical skill. Cohorts entering medicine when outside options are weak bill more per beneficiary despite treating healthier patients, with the additional billing concentrated in services where physicians have the most discretion and the strongest revenue incentive (Tables 3–4), and they sort into smaller, more lucrative practice settings (Tables 3 and 6). However, their patients do not experience better outcomes (Table 5). If heightened selectivity were primarily raising clinical skill, one might expect improvements in patient outcomes or a greater willingness to manage more complex patients. Moreover, the fact that recession-cohort physicians bill more per beneficiary is itself difficult to reconcile with the skill story, since existing work tends to find that better providers do more with less (Doyle et al., 2010; Currie and Zhang, 2025; Abaluck et al., 2026). Instead, the patterns suggest that physicians entering during downturns exhibit stronger financial motivation, sorting into practice environments where financial incentives are greater and practicing medicine in ways that generate higher revenue without measurable improvements in patient outcomes.[33]

[33]In contrast, Nagler et al. (2020) find that teachers who entered the profession during recessions are subsequently more effective. The opposing signs across professions are consistent with a simple model of occupational sorting based on skill and financial motivation. When downturns contract high-paying opportunities that are typically filled by high-skill individuals in good times, the displaced sort across the remaining options by how much they value financial returns: those who value money relatively less opt into low-paying professions such as teaching, while those who value it more opt into other high-paying professions such as medicine. This raises aptitude in teaching, which cannot attract the same talent when times are good and outside options are plentiful. Medicine, by contrast, always attracts the highly skilled through

To assess how much of the reduced-form relationship between applicant-relevant unemployment and physician outcomes may operate through changes in the skill versus the preferences of entering cohorts, we implement a regression-based mediation decomposition in the spirit of Heckman et al. (2013). As described in detail in Appendix C, the decomposition is broadly consistent with the preference-based interpretation above: for outcomes most directly tied to physician income, such as Medicare revenue per beneficiary and choice of practice ownership, the share of matriculants from non-traditional pre-medical backgrounds (our proxy for cohort preferences) accounts for a meaningful portion of the reduced-form effect, whereas the acceptance rate (our proxy for cohort skill) plays a more prominent role in explaining differences in patient risk.[34] We caution, however, that the channel-specific components are estimated imprecisely. Because each is a product of a first-stage and a structural coefficient, its sampling variability compounds that of both, yielding wide confidence intervals even where the total and residual effects are more precisely estimated. We therefore treat the decomposition as suggestive evidence on mechanisms rather than as precise estimates of the share attributable to each channel.

Non-selection mechanisms Two further mechanisms could in principle link entry-time conditions to later practice behavior through channels other than selection, though the evidence is less consistent with each. The first is psychological "scarring" (Malmendier and Nagel, 2011; Oreopoulos et al., 2012): the experience of applying during a recession might durably shift entrants' attitudes toward money or risk in ways that later manifest as more revenue-focused practice. This mechanism is difficult to reconcile with the timing of our identifying variation. For scarring to explain our findings, an imprint left at medical school application would have to survive the structured, acyclical environment of medical school and residency, a long period through which future physicians are largely insulated from the labor market. Moreover, because training varies in length, those who applied during a recession often begin practicing in different economic conditions, including a recovered economy, while those who applied in good times can enter practice during a downturn. Any scarring from economic conditions at the time of application would thus have to persist for more

high wages, so the main effect of a recession is instead an infusion of the more financially motivated.

[34] Because higher unemployment lowers the acceptance rate, a negative acceptance-rate component implies that more selective cohorts treat patients with lower average risk scores. As with our main results, this suggests that admissions screening is imperfect at identifying clinical aptitude.

than a decade and outweigh any scarring from the economic conditions encountered at other formative junctures, including entry into residency, the choice of fellowship, and the start of independent practice.

The second alternative operates through financing rather than psychology: students who apply during recessions may face tighter household budgets or diminished family resources, leading them to take on greater educational debt, which could in turn push cohorts that entered during downturns toward higher-revenue practice styles later in their careers.[35] Several features of the setting again suggest that this mechanism is unlikely to account for our findings. The financing that generates medical school debt is determined year by year over the four years of enrollment, with loan amounts and interest rates set annually rather than fixed at matriculation (AAMC, n.d.). Since a student's debt therefore reflects economic conditions across all four years of medical school, not the conditions leading up to application that define our variation, a debt mechanism should affect students who entered under a range of conditions. As with the scarring mechanism, the timing is therefore again at odds with our identifying variation. Beyond this, publicly available AAMC data show that real median education debt at U.S. medical schools rose only gradually across application cohorts from 2004 to 2011 (graduating classes of 2009–2016), with no discernible break around the cohorts that applied during the Great Recession at either public or private institutions (Youngclaus et al., 2017). This suggests that recession-driven debt changes are unlikely to be first order. Finally, the population entering medicine is drawn overwhelmingly from the top of the income distribution: roughly three-quarters of matriculants come from families in the top two income quintiles, a concentration that has changed little over the three decades through 2017 (Youngclaus and Roskovensky, 2018). Credit constraints and recession-driven resource losses bind least for this group, making differential debt accumulation an unlikely driver of the differences we document.

Taken together, these results suggest that the conditions under which physicians enter medicine leave a lasting imprint on how they later practice, and that this imprint operates primarily through the preferences of those drawn into the profession rather than through their clinical aptitude or through scarring or financial constraints associated with entering

[35]The timing of our effects further runs contrary to a debt-burden story. Educational debt is heaviest immediately after training and declines over a physician's career, yet we find that the spending effects, if anything, grow stronger when we restrict to cohorts with more time since medical school graduation (Table A10).

during a downturn. Policy levers that change the relative attractiveness of medicine, such as physician reimbursement or the cost of medical education, may therefore shape not only how many people aim to enter the profession but the kind of entrants it draws, with consequences for practice patterns and spending that persist long after entry.

VI Conclusion

This paper examines how selection into medicine shapes the delivery of health care in the United States. We exploit macroeconomic conditions at the time of medical school application as an exogenous shock to the applicant pool, leveraging the fact that downturns in competing high-skill occupations expand the set of individuals on the margin of pursuing medicine.

We document two main sets of findings. First, applications to medical school rise substantially when alternative occupations weaken, and the composition of matriculating classes shifts toward students from non–pre-medical undergraduate backgrounds. Applications rise more sharply among men while the gender mix of matriculants is unchanged, suggesting that selectivity rises more for male than for female applicants when outside options decline.

Second, these compositional shifts have lasting consequences for the delivery of care. Cohorts entering medicine during downturns bill Medicare more per beneficiary as practicing physicians, despite treating somewhat healthier patients. The additional billing is concentrated on margins where physicians have the most latitude over treatment choices and the strongest revenue incentives, while spending on services with limited discretion is much less responsive. These cohorts also gravitate toward smaller, fee-for-service practice arrangements and away from salaried hospital- and system-owned employment. The shifts in billing and practice arrangements are substantially more pronounced among male physicians, consistent with men in recession cohorts being a more differentially selected group. Despite the higher spending on healthier patients, we find no detectable improvement in avoidable ED visits or actual one-year mortality conditional on predicted mortality.

Together, the results suggest that individuals entering medicine during downturns place greater weight on financial returns, and that this shift in preferences, rather than heightened clinical skill from increased admissions selectivity, is the dominant channel through which entry conditions shape downstream practice. While an increase in clinical skill stemming from

medical schools' ability to pick more qualified applicants during times of increased selectivity would predict that recession-cohort physicians manage more complex patients, use fewer resources, or achieve better patient outcomes, we find the opposite: these physicians treat healthier patients, bill more on discretionary, revenue-generating services, sort into practice settings with stronger links between effort and earnings, and produce no detectable improvements in patient health. A regression-based mediation analysis is further consistent with the preferences channel playing a key role, particularly for income-related margins such as spending per beneficiary and sorting into smaller, more lucrative practice settings, although limited statistical power means we cannot fully separate the contributions of selectivity and composition.

The differences in billing we document matter for public budgets. Consider a counterfactual in which the entire physician workforce were drawn from an applicant pool facing a one-percentage-point higher applicant-relevant unemployment rate, comparable to the gap between the highest- and lowest-unemployment cohorts in our estimation sample. Our estimates imply such a workforce would bill Medicare Part B 4.7 percent more per beneficiary (column (1) of Tables 3 and 4). Applied to total Part B spending of roughly $576 billion in 2025 (KFF, 2026), this amounts to about $27 billion per year. While sizable, the impacts on aggregate spending are likely even larger. Our outcomes capture only what each physician bills to Medicare under their own identifier in Part B claims, yet physicians also set in motion spending recorded elsewhere, including the admissions they generate under Part A and the prescriptions they write under Part D. Were billing differences of similar proportional magnitude to extend to all Medicare spending, which totaled nearly $1.2 trillion in 2025, the budgetary implications would approach $56 billion per year. While population health and administered prices are well-recognized drivers of government health spending, our findings point to a third margin: who is drawn into the profession, and how this shapes the intensity of their practice styles.

Our findings also have implications for the design of medical education and health care policy more generally. Policies that affect the relative attractiveness of medicine—such as physician reimbursement, the cost of medical training, or the working conditions and bargaining power of trainees—are likely to shape practice patterns through different mechanisms than those that affect admissions selectivity, such as the number of medical school slots. More fundamentally, the fact that cohort performance does not improve when the applicant

pool expands indicates that medical school admissions screen imperfectly on the attributes that ultimately shape physician behavior: with effective screening, larger applicant pools should yield better cohorts, not ones that practice more expensive, revenue-driven medicine.

Taken together, our results identify selection into the medical profession as an important and understudied determinant of variation in clinical decision-making. Although much research has examined what shapes physician behavior after entry, far less attention has been paid to who chooses to enter in the first place. Macroeconomic conditions at the time of career choice are one force shaping selection, but surely not the only one. For example, cross-country differences in physician training, compensation, and outside options may similarly shape who selects into medicine, helping to explain international differences in spending, utilization, and practice styles. Understanding what draws individuals to medicine, and how those motivations translate into clinical practice, is essential for any effort to shape the future physician workforce.

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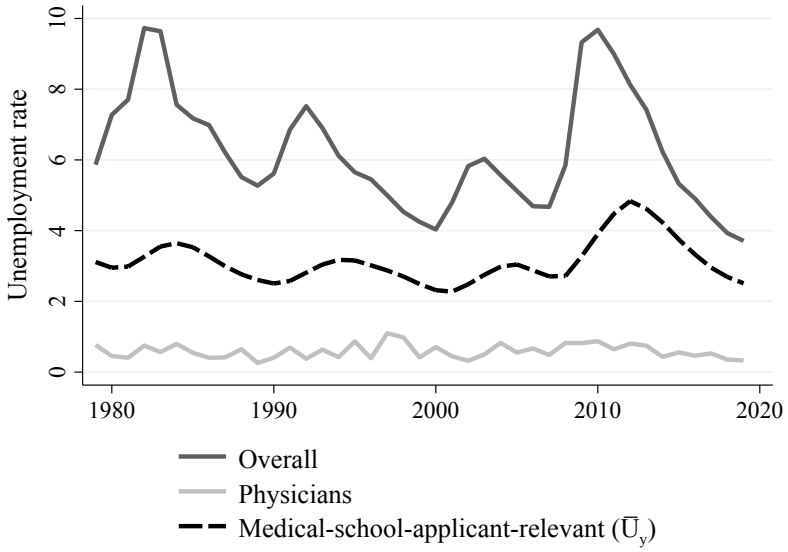
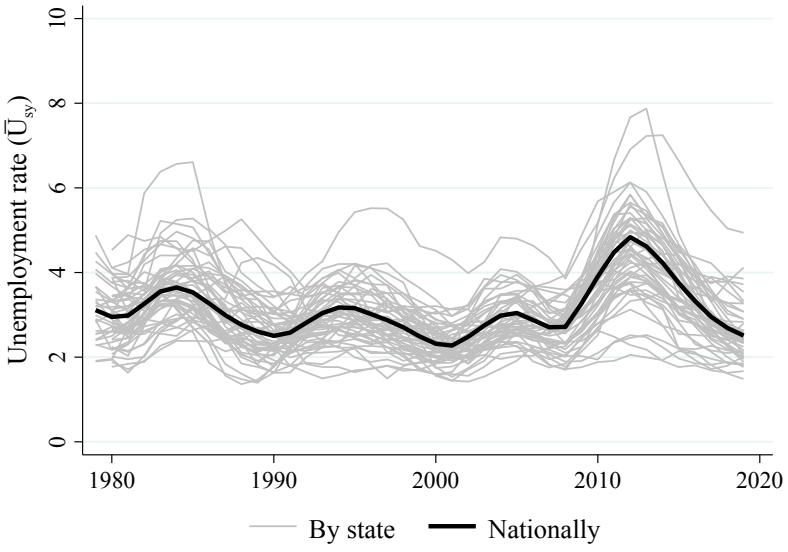
VII Figures

Figure 1: Unemployment rates overall and by state over time

(a) Overall and by occupation nationally



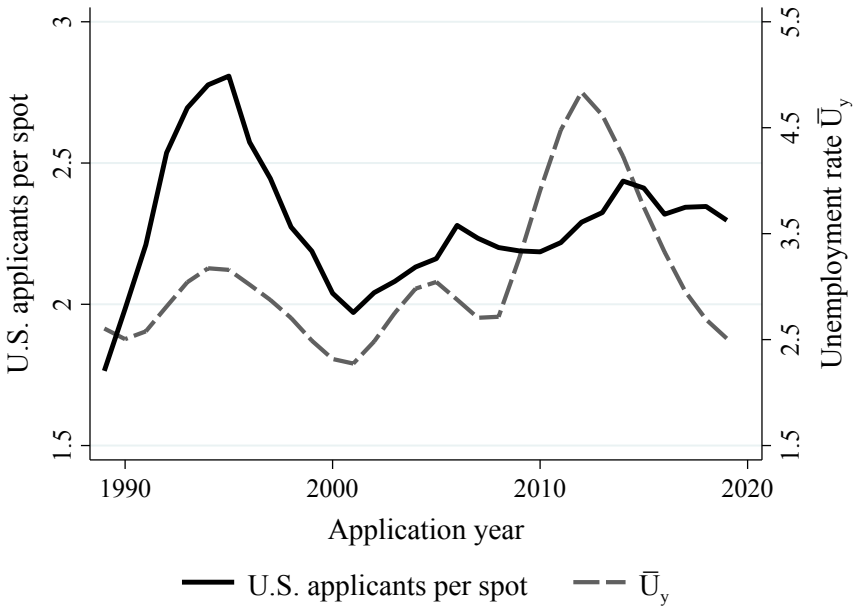
(b) Medical-school-relevant unemployment by state



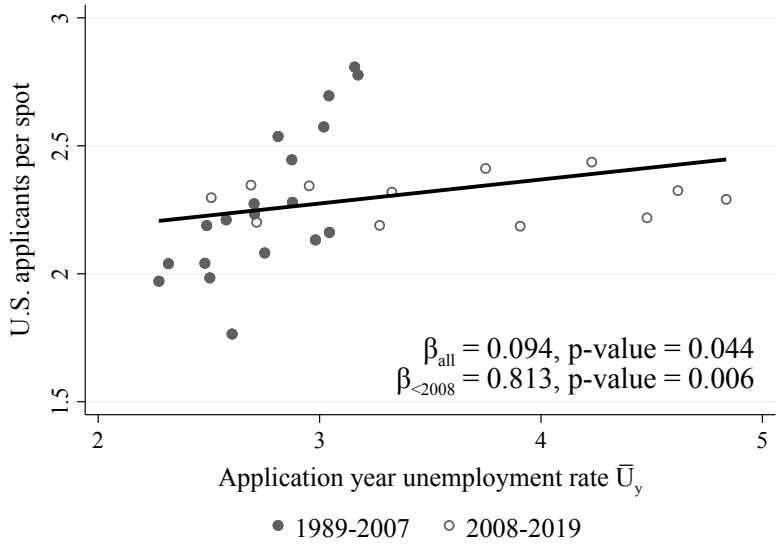
Notes: The above figures plot unemployment rates nationally (subfigure (a)) and by state (subfigure (b)) from 1979 to 2019. Subfigure (a) shows the economy-wide unemployment rate, the national unemployment rate for physicians, and the medical-school-applicant-relevant business cycle ($\bar{U}_y$); see Section II for details on the construction of $\bar{U}_y$. Subfigure (b) plots medical-school-applicant-relevant business cycle at both the national ($\bar{U}_y$) and state ($\bar{U}_{sy}$) levels. All unemployment rates are constructed using data from the CPS.

Figure 2: Entry conditions and medical school applicants per spot

(a) Domestic applicants per spot and unemployment over time



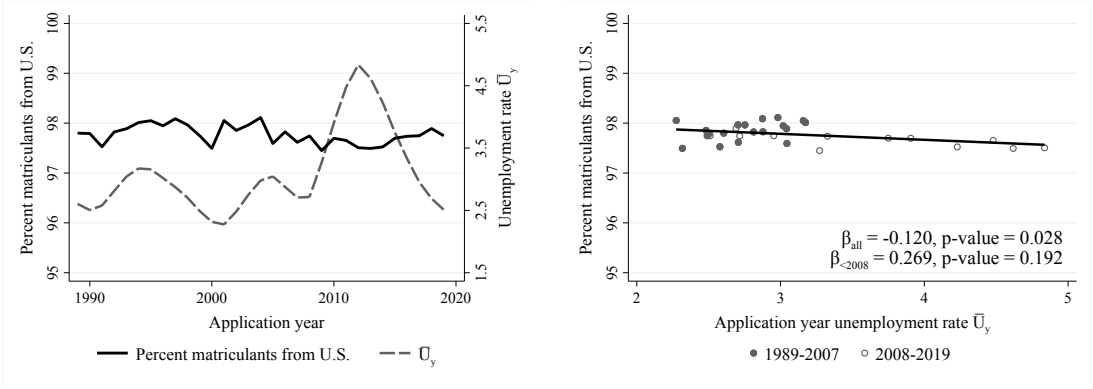
(b) Applicants per spot versus relevant unemployment



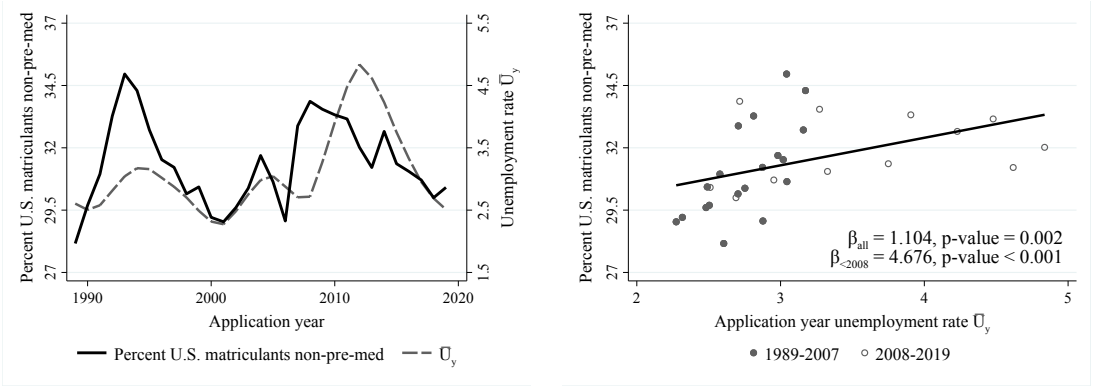
Notes: The above figures show how the annual number of applicants from the United States to U.S. medical schools, divided by the number of matriculants to U.S. medical schools ("spots"), covaries with the medical-school-applicant-relevant business cycle ($\bar{U}_y$) from 1989 to 2019. Subfigure (a) presents the raw time series of these variables, while subfigure (b) shows a scatterplot of their relationship. In subfigure (b), the relationship is shown separately for the period before the Great Recession (1989–2007; solid circles)—the period for which we can examine downstream outcomes on eventual practice styles—and the post–Great Recession period (2008–2019; hollow circles). The coefficients reported in subfigure (b) come from bivariate regressions of applicants per spot on $\bar{U}_y$; corresponding p-values are calculated using a wild bootstrap. See Section II for details on the construction of $\bar{U}_y$. Data on applicants and matriculants are from the AAMC. Figure A3 shows analogous results for international applicants, as well as results by undergraduate major category and sex.

Figure 3: Entry conditions and the composition of medical school matriculants

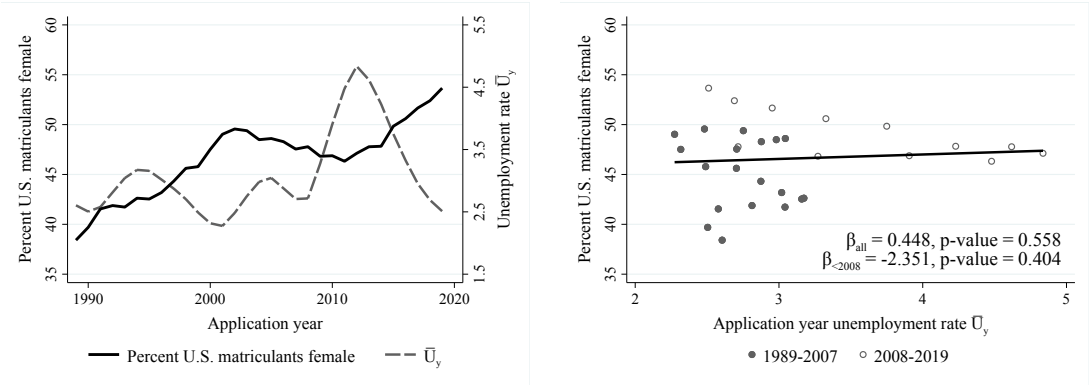
(a) Percent of matriculants from United States



(b) Percent non–pre-med majors among U.S. matriculants



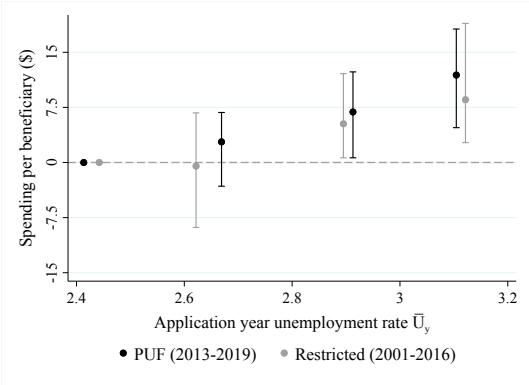
(c) Percent female among U.S. matriculants



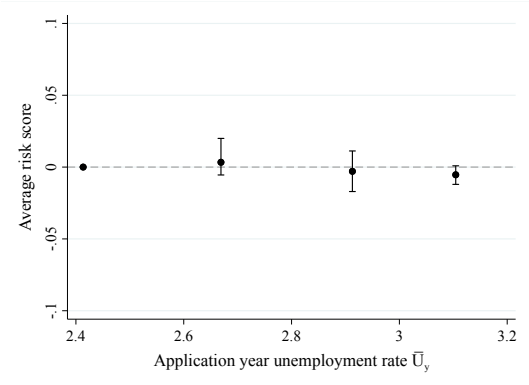
Notes: The above figures show how the annual percent of U.S. medical school matriculants from the United States (subfigure (a)), the percent of U.S. matriculants from non–pre-medical undergraduate majors (subfigure (b)), and the percent of U.S. matriculants who are female (subfigure (c)) covary with the medical-school-applicant-relevant business cycle ($\bar{U}_y$) from 1989 to 2019. In each subfigure, the left subplot presents the raw time series of these variables, while the right subplot shows a scatterplot of their relationship. In the right subplots, the relationship is shown separately for the period before the Great Recession (1989–2007; solid circles)—the period for which we can examine downstream outcomes on eventual practice styles—and the post–Great Recession period (2008–2019; hollow circles). The coefficients reported in the right subplots come from bivariate regressions of the percent of domestic, non–pre–medical, or female matriculants on $\bar{U}_y$; corresponding p-values are calculated using a wild bootstrap. See Section II for details on the construction of $\bar{U}_y$. Data on matriculants are from the AAMC.

Figure 4: Entry conditions, billing, patient mix, and practice size (public Medicare)

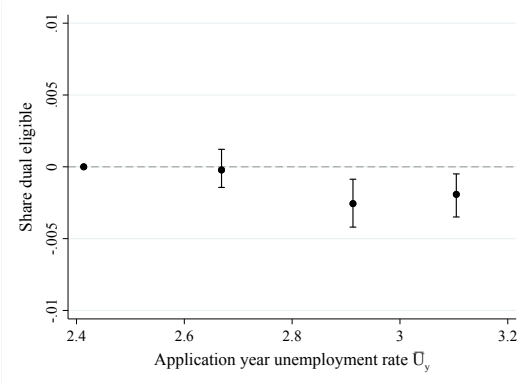
(a) Average Medicare revenue per beneficiary



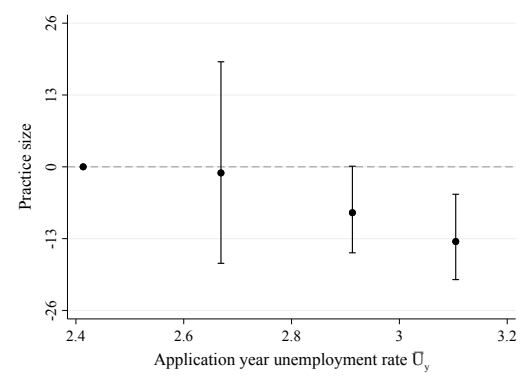
(b) Average patient risk scores



(c) Share Medicare beneficiaries dually eligible

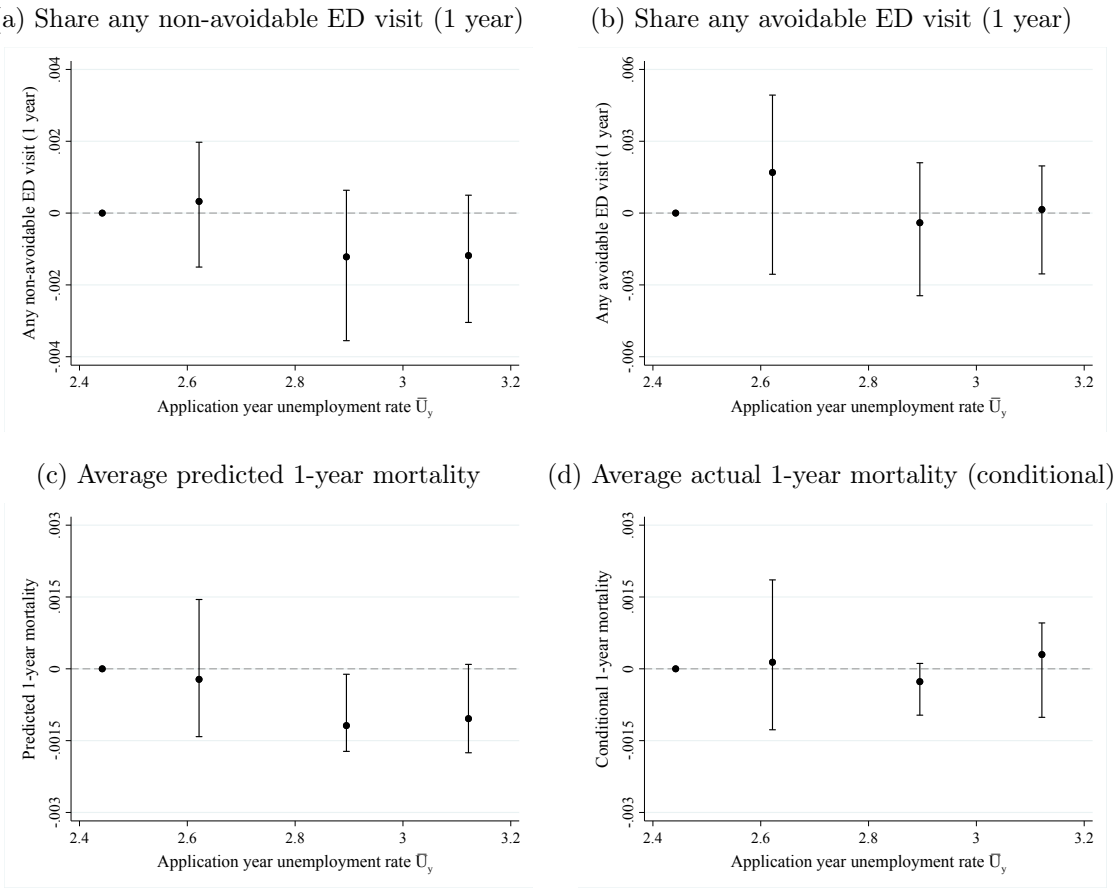


(d) Practice size



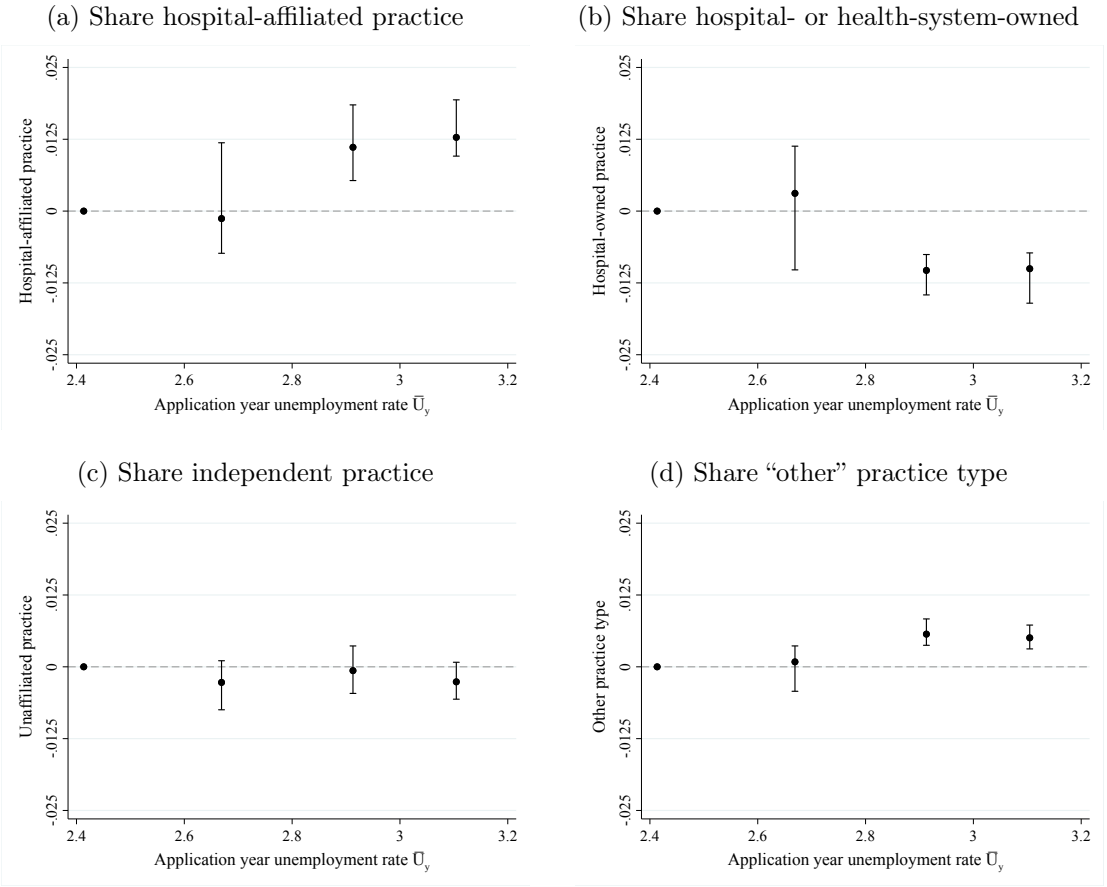
Notes: The above figures present results from estimation of equation (3) using average annual Medicare billing per beneficiary (subfigure (a)), average risk scores among the physician's Medicare beneficiaries (subfigure (b)), the share of a physician's Medicare beneficiaries that are dually eligible for Medicaid (subfigure (c)), and physician practice size (subfigure (d)) as outcomes. Confidence intervals are calculated using a wild cluster bootstrap clustered by application cohort (Cameron et al., 2008). The y-axes are scaled to span from –5 percent to +5 percent of the outcome mean. All regressions include specialty, experience, and outcome year fixed effects. The sample is restricted to allopathic physicians who completed their initial medical education in the United States, applied to medical school in or after 1989, and have at least seven years of experience in a given outcome year. See Section II for details on the construction of $\bar{U}_y$. Outcome data are drawn from the public-use Medicare Part B files from 2013–2019 and the public-use Medicare Physician Compare files from 2014–2019. In subfigure (a), results estimated using the restricted-use Medicare Part B data from 2001–2016 are overlaid with those from the public-use data.

Figure 5: Entry conditions, patient mix, and patient health outcomes (restricted Medicare)



Notes: The above figures present results from estimation of equation (3) using the share of a physician's beneficiaries with an emergent, non-avoidable emergency department (ED) visit in the following year (subfigure (a)), the share with an avoidable ED visit in the following year (subfigure (b)), the average predicted one-year mortality rate among the physician's beneficiaries (subfigure (c)), and the average actual one-year mortality rate conditional on predicted (subfigure (d)) as outcomes. All four measures are visit-weighted, as described in Section IV.A. ED visits are classified using the algorithm of Billings et al. (2000); avoidable visits comprise the non-emergent, emergent/primary care treatable, and emergent/preventable or avoidable categories. Confidence intervals are calculated using a wild cluster bootstrap clustered by application cohort (Cameron et al., 2008). The y-axes are scaled to span from –2.5 percent to +2.5 percent of the outcome mean. All regressions include specialty, experience, and outcome year fixed effects. The sample is restricted to allopathic physicians who completed their initial medical education in the United States, applied to medical school in or after 1989, and have at least seven years of experience in a given outcome year. See Section II for details on the construction of $\bar{U}_y$. Outcome data are drawn from restricted Medicare data from 2001–2016.

Figure 6: Entry conditions and physician practice ownership and affiliation (SK&A)



Notes: The above figures present results from estimation of equation (3) using as outcomes the share of physicians in each practice category: independent but hospital affiliated (subfigure (a)), hospital- or health-system-owned (subfigure (b)), independent and unaffiliated (subfigure (c)), and "other" (subfigure (d)). Confidence intervals are calculated using a wild cluster bootstrap clustered by application cohort (Cameron et al., 2008). All regressions include specialty, experience, and outcome year fixed effects. The sample is restricted to allopathic physicians who completed their initial medical education in the United States, applied to medical school in or after 1989, and have at least seven years of experience in a given outcome year. See Section II for details on the construction of $\bar{U}_y$. Outcome data are drawn from the SK&A data from 2008–2016.

VIII Tables

Table 1: Relevant unemployment and medical school applicants and matriculants by state

a. Overall	Applicants per 100,000		Matriculants per 100,000	
	(1)	(2)	(3)	(4)
Unemployment rate $\bar{U}_{sy}$	0.71***	0.38**	0.14**	0.13
	(0.14)	(0.16)	(0.06)	(0.09)
	[<0.001]	[0.024]	[0.024]	[0.214]
Mean dependent variable	14.2	14.2	6.1	6.1
Percent of mean	5.0	2.7	2.4	2.1
b. Applicants by major group	Pre-medical applicants per 100,000		Other applicants per 100,000	
	(1)	(2)	(3)	(4)
Unemployment rate $\bar{U}_{sy}$	0.19**	0.38**	0.21***	0.25***
	(0.09)	(0.19)	(0.04)	(0.09)
	[0.040]	[0.032]	[<0.001]	[0.006]
Mean dependent variable	8.5	8.5	3.7	3.7
Percent of mean	2.3	4.4	5.7	6.6
c. Applicants by sex	Male applicants per 100,000		Female applicants per 100,000	
	(1)	(2)	(3)	(4)
Unemployment rate $\bar{U}_{sy}$	0.42***	0.24**	0.29***	0.14
	(0.08)	(0.10)	(0.07)	(0.08)
	[<0.001]	[0.016]	[<0.001]	[0.108]
Mean dependent variable	7.5	7.5	6.6	6.6
Percent of mean	5.6	3.1	4.3	2.1
Observations	1,581	1,581	1,581	1,581
State fixed effects	✓	✓	✓	✓
Year fixed effects		✓		✓

Notes: The above table shows output from estimation of equation (1) using data at the state-year level from 1989–2019. Outcomes are expressed per 100,000 state residents, and observations are weighted by state population. Standard errors, clustered by state, are shown in parentheses; p-values, calculated using a wild cluster bootstrap (Cameron et al., 2008) clustered by state, are shown in brackets. See Section II for details on the construction of $\bar{U}_{sy}$. Data on applicants and matriculants are from the AAMC, and data on annual state population are from the U.S. Census Bureau's intercensal population estimates. Significance stars are based on the wild cluster bootstrap p-values: $^{*}\, p < 0.10$, $^{**}\, p < 0.05$, $^{***}\, p < 0.01$.

Table 2: Sample statistics and overview of outcome data sources

	Physician attribute files (1)	Public Medicare (2)	Restricted Medicare (3)	SK&A data (4)
Outcome data years				
Earliest year	–	2013	2001	2008
Latest year	–	2019	2016	2016
Selection criterion				
Percent allopathic (MD)	100	100	100	100
Percent US medical graduate	100	100	100	100
Earliest application year	1989	1989	1989	1989
Latest application year	2007	2007	2004	2004
Sample characteristics				
Average experience (years)	–	13.4	11.0	12.6
Average application year	1998.2	1998.1	1996.3	1995.6
Percent female	44.8	39.9	41.6	42.9
Percent in AMA extract only	14.5	1.56	8.32	12.1
Percent in PC only	3.77	4.23	3.59	2.75
Percent in both	81.7	94.2	88.1	85.1
Specialties (percent)				
Primary care	29.5	24.2	27.4	32
Surgical specialties	16.8	19.2	18	18.4
Facility based	16.4	18.5	17.1	11.6
Medical subspecialties	8.46	8.87	9.07	9.67
Diagnostic	6.54	7.41	6.21	5.12
OB/GYN	5.84	5.78	6.3	6.99
Behavioral health	4.61	3.22	3.89	3.55
Non-surgical oncology	2.94	3.07	3.01	3.35
Musculoskeletal/rehab	2.83	3.11	2.98	3.17
Dermatology	2.06	2.48	2.25	2.37
Neurology	1.94	2.09	1.93	2.07
Non-surgical proceduralist	1.41	1.75	1.55	1.46
Supportive/palliative	0.23	0.24	0.19	0.11
Other physician	0.36	0.14	0.21	0.16
Unique physicians	290,563	222,812	212,202	170,769
Average years per physician	–	5.29	7.1	5.46

Notes: The above table reports summary statistics for the data sources used to examine the impacts of entry conditions on physician practice styles and organization. As outlined in Section IV.A, our analyses focus on allopathic physicians who completed their initial medical education in the United States, applied to medical school in or after 1989, and have at least seven years of experience in a given outcome year. Based on the latest year available for each outcome data source, this final sample criterion limits the sample to physicians applying to medical school in or before 2007 in column (2) and 2004 in columns (3) and (4). Column (1) reports summary statistics for the full physician sample constructed by combining an extract of the AMA Masterfile from 2018 with the Medicare Physician Compare files from 2014–2019. Column (2) reports statistics for physicians from this full file who appear at least once in the public-use Medicare Part B files from 2013–2019, column (3) reports statistics for physicians who appear in the restricted Medicare Part B files from 2001–2016, and column (4) reports statistics for physicians who appear at least once in the SK&A data from 2008–2016. Specialties are classified into 14 mutually exclusive and exhaustive categories; see Section IV.A.

Table 3: Impacts of entry conditions on billing, patient mix, and practice size: public-use Medicare data (2013–2019)

	Revenue per beneficiary (1)	Average risk score (x100) (2)	Dual eligible benes. (%) (3)	Practice size (4)
Unemployment rate $\bar{U}_y$	14.17***	-0.75	-0.34***	-13.62**
	(3.43)	(0.59)	(0.07)	(4.67)
	[0.006]	[0.244]	[0.002]	[0.010]
Observations	1,178,466	1,178,466	1,178,466	927,400
R^2	0.11	0.19	0.09	0.02
Mean dependent variable	301.4	174.1	24.1	518.9
Percent of mean	4.7	-0.4	-1.4	-2.6
Specialty fixed effects	✓	✓	✓	✓
Experience fixed effects	✓	✓	✓	✓
Outcome year fixed effects	✓	✓	✓	✓

Notes: The above table shows output from estimation of equation (2) using data at the physician-year level. Outcomes are the average annual Medicare revenue per beneficiary (column (1)), average risk scores among the physician's Medicare beneficiaries times 100 (column (2)), the percent of a physician's Medicare beneficiaries that are dually eligible for Medicaid (column (3)), and physician practice size (column (4)). Standard errors, clustered by application cohort, are shown in parentheses; p-values, calculated using a wild cluster bootstrap (Cameron et al., 2008) clustered by cohort, are shown in brackets. See Section II for details on the construction of $\bar{U}_y$. Outcome data are drawn from the public-use Medicare Part B files from 2013–2019 and the public-use Medicare Physician Compare files from 2014–2019. Significance stars are based on the wild cluster bootstrap p-values: $^{*}\, p < 0.10$, $^{**}\, p < 0.05$, $^{***}\, p < 0.01$.

Table 4: Impacts of entry conditions on physician billing per beneficiary, overall and by service category: restricted Medicare data (2001–2016)

	Revenue per beneficiary (1)	Evaluation & management (2)	Elective procedures (3)	Non-elective procedures (4)
Unemployment rate $\bar{U}_y$	13.72***	0.83	2.05***	0.25
	(2.38)	(1.36)	(0.64)	(1.89)
	[<0.001]	[0.606]	[0.008]	[0.928]
Observations	1,505,912	1,505,912	1,505,912	1,505,912
R^2	0.18	0.12	0.24	0.11
Mean dependent variable	291.7	103.3	39.7	18.2
Percent of mean	4.7	0.8	5.2	1.4
	Other procedures (5)	Imaging (6)	Tests (7)	Phys.-admin. drugs (8)
Unemployment rate $\bar{U}_y$	3.18***	–0.34	1.05*	6.26***
	(0.94)	(0.69)	(0.51)	(1.58)
	[0.004]	[0.716]	[0.094]	[0.004]
Observations	1,505,912	1,505,912	1,505,912	1,505,912
R^2	0.14	0.09	0.04	0.07
Mean dependent variable	44.7	31.2	27.1	23.3
Percent of mean	7.1	–1.1	3.9	26.8
Specialty fixed effects	✓	✓	✓	✓
Experience fixed effects	✓	✓	✓	✓
Outcome year fixed effects	✓	✓	✓	✓

Notes: The above table shows output from estimation of equation (2) using data at the physician-year level. Outcomes are the average annual Medicare revenue per beneficiary overall (column (1)) and by service category (columns (2)–(8)). We divide all Medicare Part B physician billing into eight mutually exclusive and exhaustive service categories; all are shown except for the "other services" category. Standard errors, clustered by application cohort, are shown in parentheses; p-values, calculated using a wild cluster bootstrap (Cameron et al., 2008) clustered by cohort, are shown in brackets. See Section II for details on the construction of $\bar{U}_y$. Outcome data are drawn from restricted Medicare files from 2001–2016. Significance stars are based on the wild cluster bootstrap p-values: $^{*}\, p < 0.10$, $^{**}\, p < 0.05$, $^{***}\, p < 0.01$.

Table 5: Impacts of entry conditions on patient mix and patient health outcomes: restricted Medicare data (2001–2016)

	Any non-avoidable ED visit (1 year) (1)	Any avoidable ED visit (1 year) (2)	Predicted 1-year mortality (3)	Actual 1-year mortality (cond.) (4)
Unemployment rate $\bar{U}_y$	-0.27**	-0.15	-0.18**	-0.003
	(0.08)	(0.14)	(0.04)	(0.05)
	[0.016]	[0.388]	[0.014]	[0.956]
Observations	1,322,086	1,322,086	1,320,747	1,320,747
R^2	0.09	0.09	0.23	0.46
Mean dependent variable	15.1	25.5	10.9	12.3
Percent of mean	–1.8	–0.6	–1.6	–0.02
Specialty fixed effects	✓	✓	✓	✓
Experience fixed effects	✓	✓	✓	✓
Outcome year fixed effects	✓	✓	✓	✓

Notes: The above table shows output from estimation of equation (2) using data at the physician-year level. Outcomes are the percent of a physician's beneficiaries with an emergent, non-avoidable emergency department (ED) visit in the following year (column (1)), the percent with an avoidable ED visit in the following year (column (2)), the average predicted one-year mortality rate (in percent) among the physician's beneficiaries (column (3)), and the average actual one-year mortality rate (in percent) conditional on predicted (column (4)). All four measures are visit-weighted, as described in Section IV.A. ED visits are classified using the algorithm of Billings et al. (2000); avoidable visits comprise the non-emergent, emergent/primary care treatable, and emergent/preventable or avoidable categories. Predicted mortality is a fitted value from a model estimated on all Medicare patients, not only those treated by physicians in our sample; its mean therefore need not match that of actual mortality. Standard errors, clustered by application cohort, are shown in parentheses; p-values, calculated using a wild cluster bootstrap (Cameron et al., 2008) clustered by cohort, are shown in brackets. See Section II for details on the construction of $\bar{U}_y$. Outcome data are drawn from restricted Medicare files from 2001–2016. Significance stars are based on the wild cluster bootstrap p-values: $^{*}\, p < 0.10$, $^{**}\, p < 0.05$, $^{***}\, p < 0.01$.

Table 6: Impacts of entry conditions on physician practice ownership and affiliation: SK&A data (2008–2016)

	Hospital affiliated (%) (1)	Hospital or system owned (%) (2)	Unaffiliated practice (%) (3)	Other practice type (%) (4)
Unemployment rate $\bar{U}_y$	2.15***	-1.76***	-0.23	0.81***
	(0.22)	(0.24)	(0.18)	(0.10)
	[<0.001]	[<0.001]	[0.270]	[<0.001]
Observations	932,173	932,173	932,173	932,173
R^2	0.05	0.05	0.01	0.04
Mean dependent variable	49.7	43.5	6.7	7.7
Percent of mean	4.3	-4.0	-3.4	10.5
Specialty fixed effects	✓	✓	✓	✓
Experience fixed effects	✓	✓	✓	✓
Outcome year fixed effects	✓	✓	✓	✓

Notes: The above table shows output from estimation of equation (2) using data at the physician-year level. Outcomes are the percent of physicians in each practice category: independent but hospital affiliated (column (1)), hospital- or health-system-owned (column (2)), independent and unaffiliated (column (3)), and other (column (4)). Because physicians can report more than one practice location in a given year, the four shares sum to more than 100 percent. Standard errors, clustered by application cohort, are shown in parentheses; p-values, calculated using a wild cluster bootstrap (Cameron et al., 2008) clustered by cohort, are shown in brackets. See Section II for details on the construction of $\bar{U}_y$. Outcome data are drawn from the SK&A data from 2008–2016. Significance stars are based on the wild cluster bootstrap p-values: $^{*}\, p < 0.10$, $^{**}\, p < 0.05$, $^{***}\, p < 0.01$.

Table 7: Impacts of entry conditions on physician specialty choice

a. Physician level	Primary care (1)	Surgical specialty (2)	Facility based (3)	Medical subspecialty (4)	Other specialties (5)
Unemployment rate $\bar{U}_y$	3.25	-0.96	-0.96	0.39	-1.72
	(3.79)	(0.57)	(1.86)	(1.05)	(1.38)
	[0.422]	[0.148]	[0.662]	[0.702]	[0.260]
Observations	290,563	290,563	290,563	290,563	290,563
R^2	0.00	0.00	0.00	0.00	0.00
Mean dependent variable	29.5	16.8	16.4	8.5	28.8
Percent of mean	11.0	-5.7	-5.9	4.6	-6.0
b. Physician-year level (2014–2019)	Primary care (1)	Surgical specialty (2)	Facility based (3)	Medical subspecialty (4)	Other specialties (5)
Unemployment rate $\bar{U}_y$	0.38	-0.73	-0.27	1.13	-0.52
	(0.67)	(0.43)	(0.34)	(0.58)	(0.37)
	[0.574]	[0.124]	[0.428]	[0.122]	[0.186]
Observations	952,101	952,101	952,101	952,101	952,101
R^2	0.01	0.00	0.01	0.00	0.00
Mean dependent variable	26.2	21.1	13.0	10.4	29.3
Percent of mean	1.5	-3.5	-2.1	10.9	-1.8

Notes: The above table shows output from estimation of analogs of equation (2) using data at the physician level (panel (a)) and at the physician-year level from 2014–2019 (panel (b)). Standard errors, clustered by application cohort, are shown in parentheses; p-values, calculated using a wild cluster bootstrap (Cameron et al., 2008) clustered by cohort, are shown in brackets. The five specialty groupings are mutually exclusive and exhaustive. Regressions in panel (b) include experience and outcome year fixed effects. See Section II for details on the construction of $\bar{U}_y$. Outcome data are drawn from an extract of the AMA Masterfile from 2018 and the public-use Medicare Physician Compare files from 2014–2019. Significance stars are based on the wild cluster bootstrap p-values: $^{*}\, p < 0.10$, $^{**}\, p < 0.05$, $^{***}\, p < 0.01$.

Online Appendix

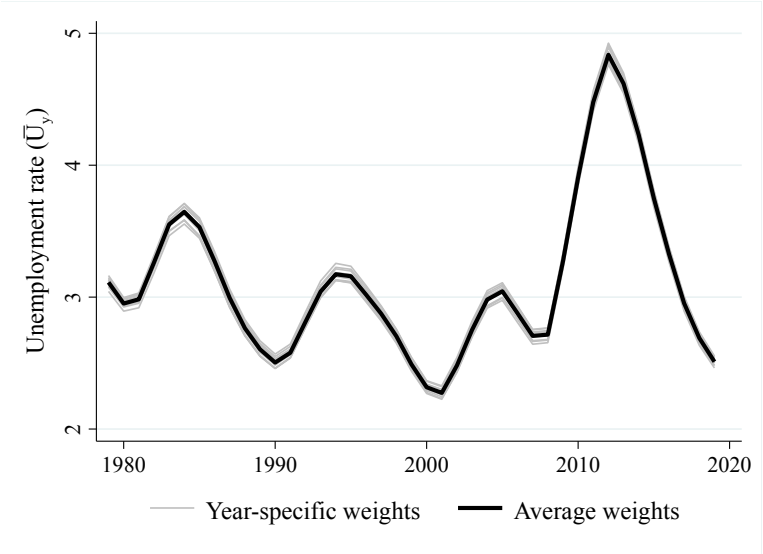
Selection into Medicine

Alexander and Schnell (2026)

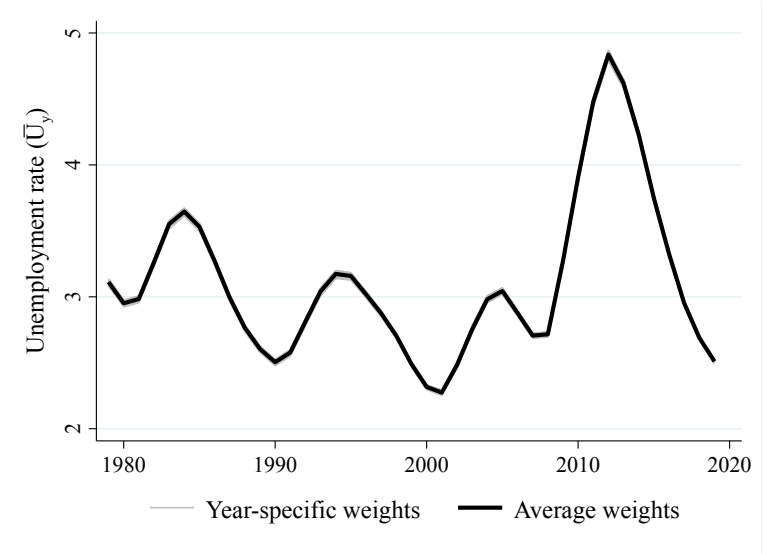
A Supplementary figures

Figure A1: Medical-school-applicant-relevant unemployment rate: alternative weights

(a) Alternative within–major-cluster, across-occupation weights



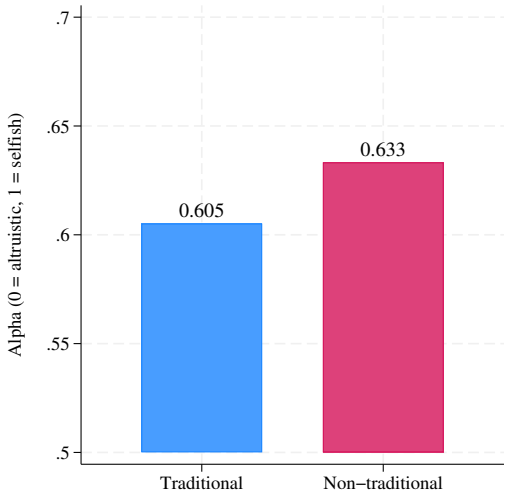
(b) Alternative across–major-cluster weights



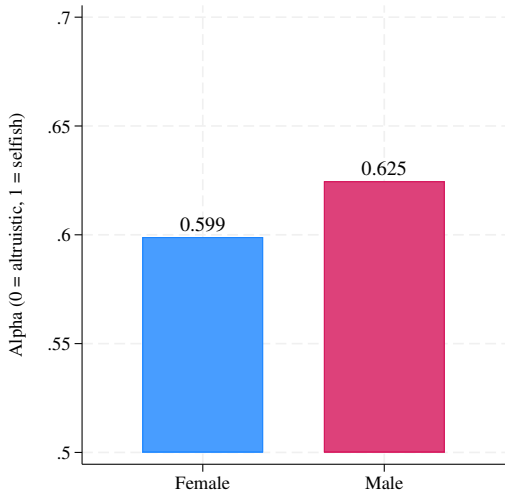
Notes: The above figures plot the medical-school-applicant-relevant business cycle ($\bar{U}_y$) from 1979 to 2019 constructed using alternative weights. Subfigure (a) plots $\bar{U}_y$ constructed using the shares of individuals in each major cluster entering each occupation from a single year of ACS data, applied to the entire period (thin, gray lines), rather than average weights constructed over all years available in the ACS (2009–2019; thick, black line). Subfigure (b) plots $\bar{U}_y$ constructed using the shares of medical school applicants coming from each major cluster from a single application cycle in the AAMC data, applied to the entire period (thin, gray lines), rather than average weights constructed over all years available in the AAMC data (1989–2019; thick, black line). See Section II for details on the construction of $\bar{U}_y$. All unemployment rates are constructed using data from the CPS.

Figure A2: Social preferences by undergraduate major and sex

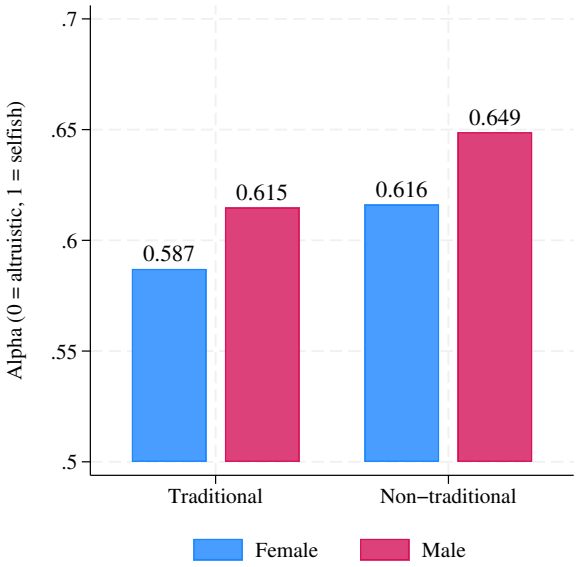
(a) By undergraduate major category



(b) By sex



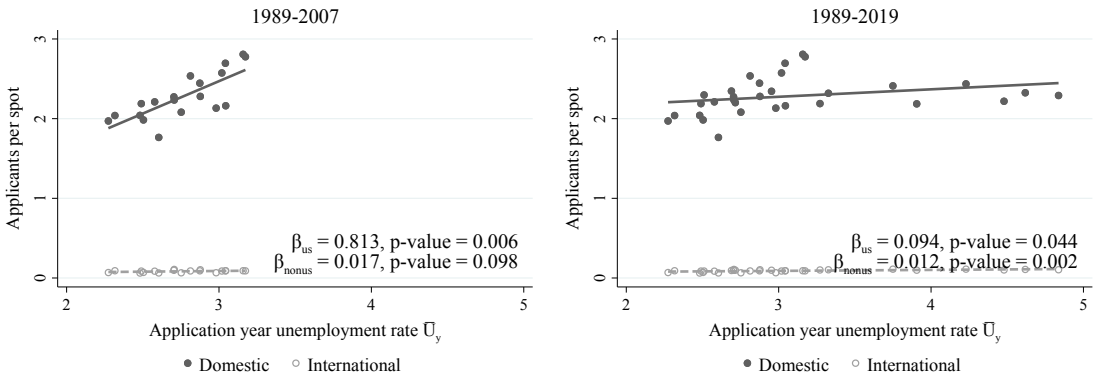
(c) By undergraduate major category and sex



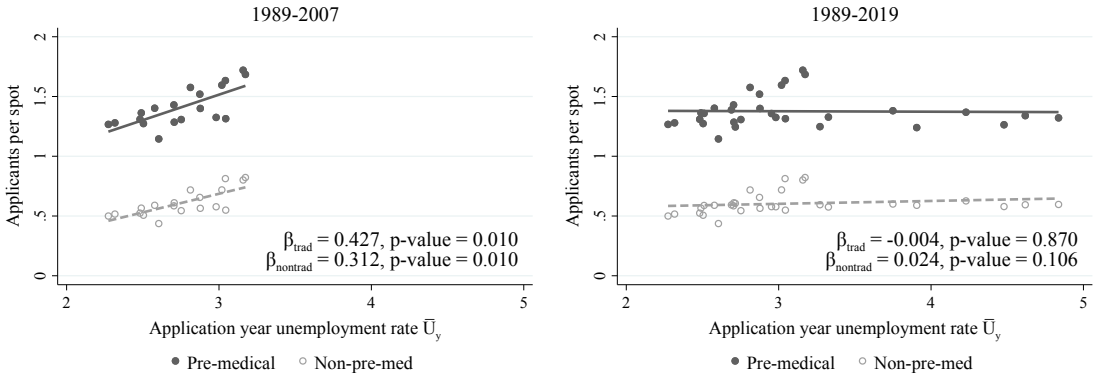
Notes: The above figures plot average measures of social preferences across undergraduate major groupings (subfigure (a)), sex (subfigure (b)), and both undergraduate major grouping and sex (subfigure (c)). Social preferences come from Li et al. (2022) and are measured using an incentivized choice experiment that elicits physicians' willingness to trade off personal income between themselves and others; "0" denotes full altruism (allocating all money to others), whereas "1" denotes full selfishness (allocating all money to oneself). "Traditional" undergraduate majors for individuals entering medicine are defined as those in the traditional pre-medical cluster (see Table A1).

Figure A3: Entry conditions and medical school applicants per spot by type

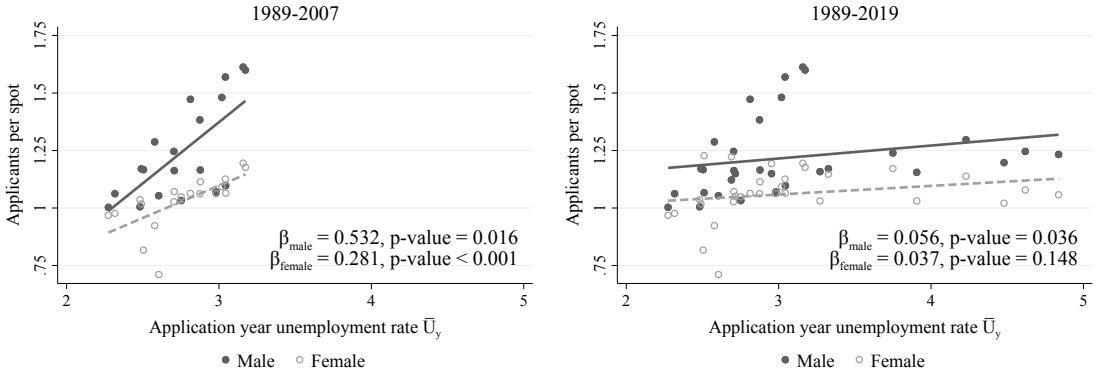
(a) By domestic versus international status



(b) By undergraduate major grouping



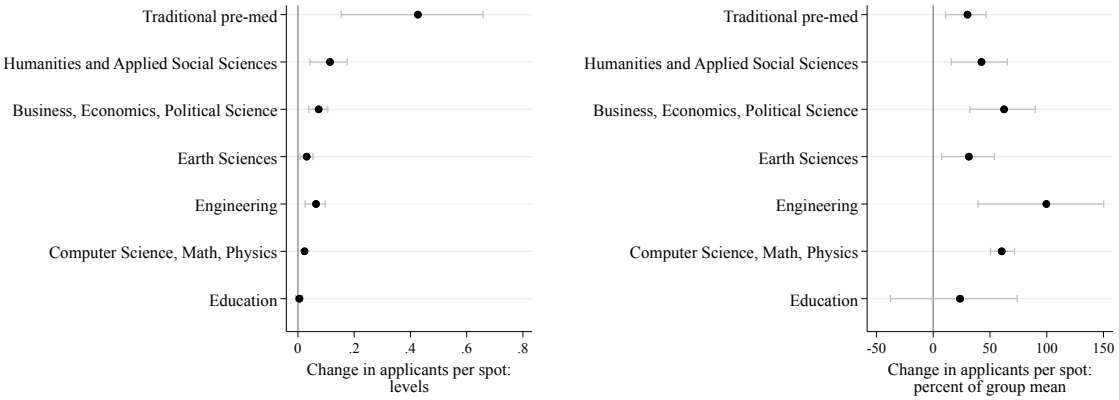
(c) By sex



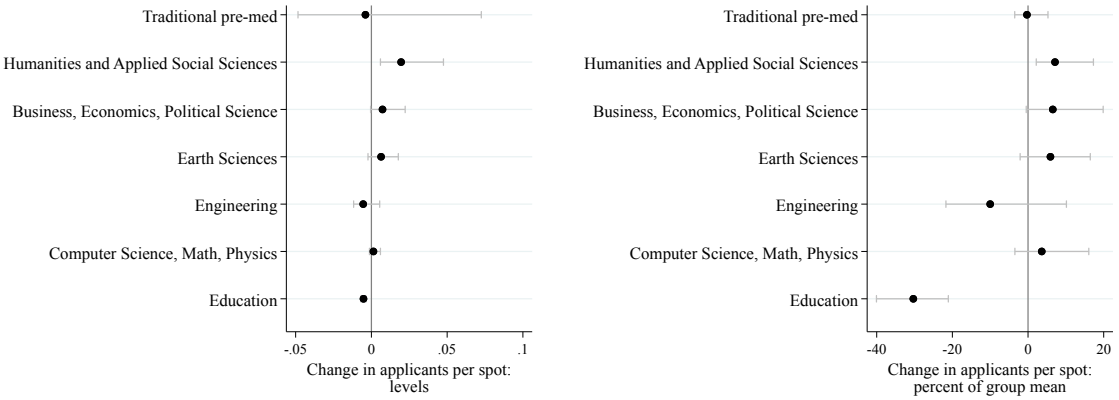
Notes: The above figures show how the annual number of applicants to U.S. medical school, divided by the number of matriculants to U.S. medical schools ("spots"), covaries with the medical-school-applicant-relevant business cycle ($\bar{U}_y$) from 1989 to 2007 (left panels) and from 1989 to 2019 (right panels). Subfigure (a) separates applicants by domestic versus international status, subfigure (b) separates U.S. applicants by undergraduate major grouping, and subfigure (c) separates U.S. applicants by sex. "Pre-medical" undergraduate majors are defined as those in the traditional pre-medical cluster (see Table A1). Reported coefficients come from bivariate regressions of applicants per spot on $\bar{U}_y$; corresponding p-values are calculated using a wild bootstrap. See Section II for details on the construction of $\bar{U}_y$. Data on applicants and matriculants are from the AAMC.

Figure A4: Entry conditions and medical school applicants per spot by major cluster

(a) 1989–2007



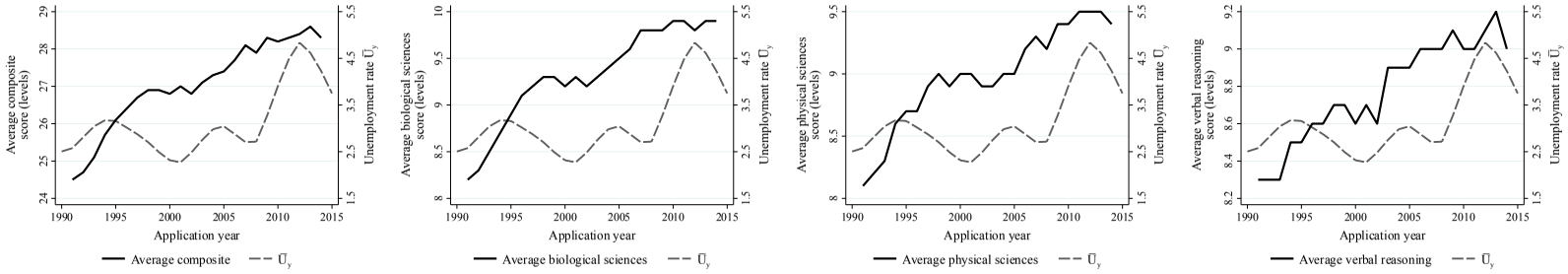
(b) 1989–2019



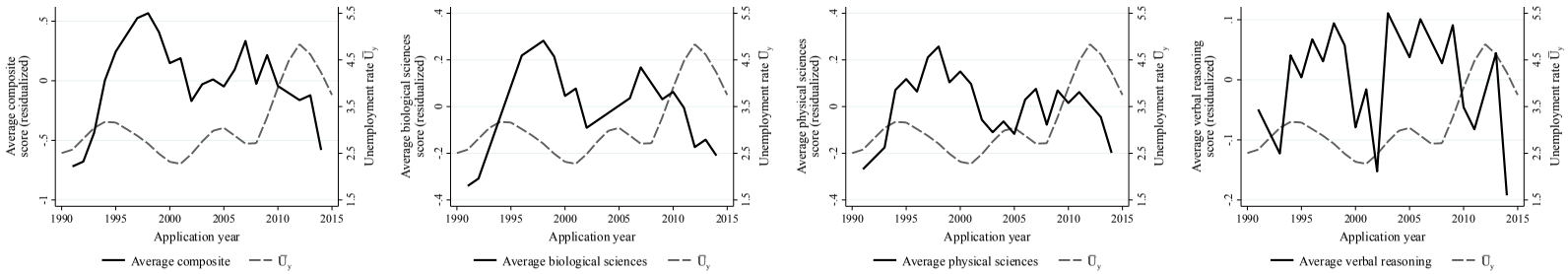
Notes: The above figures show how the annual number of U.S. applicants to U.S. medical schools from different undergraduate major clusters, divided by the number of matriculants to U.S. medical schools ("spots"), covaries with the medical-school-applicant-relevant business cycle ($\bar{U}_y$) from 1989 to 2007 (subfigure (a)) and from 1989 to 2019 (subfigure (b)). The figures plot coefficients and 95 percent confidence intervals, calculated using a wild bootstrap, from separate bivariate regressions of applicants of a given type per spot on $\bar{U}_y$. The left plot in each subfigure shows the effects in levels, whereas the right plot in each subfigure shows the effects as a percent of the group mean (i.e., the average number of U.S. applicants per spot from a given major cluster over the relevant time period). See Table A1 for the majors included in each cluster, and see Section II for details on the construction of $\bar{U}_y$. Data on applicants and matriculants are from the AAMC.

Figure A5: Entry conditions and average MCAT scores of medical school applicants

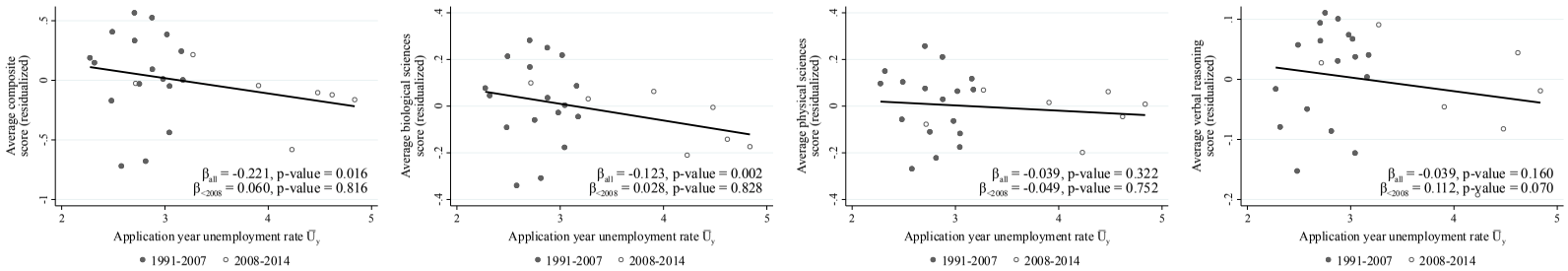
(a) Average MCAT scores (levels) and unemployment over time



(b) Average MCAT scores (residualized) and unemployment over time



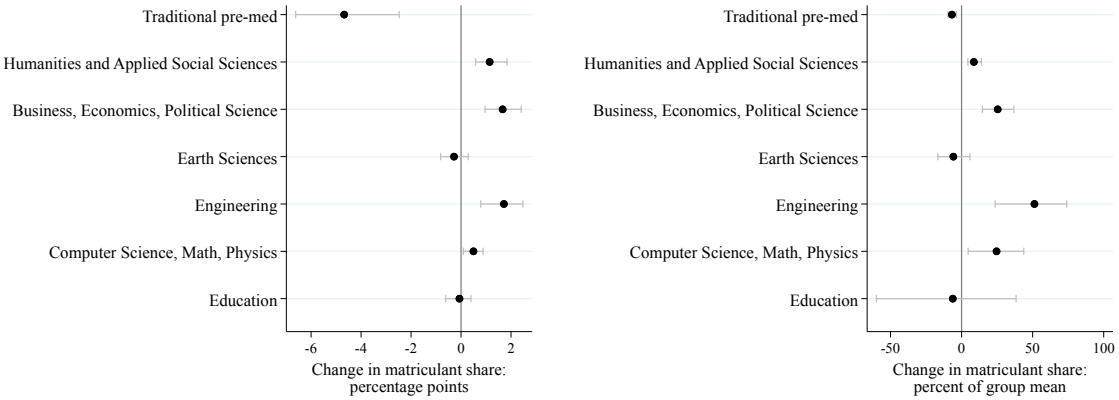
(c) Average MCAT scores (residualized) versus relevant unemployment



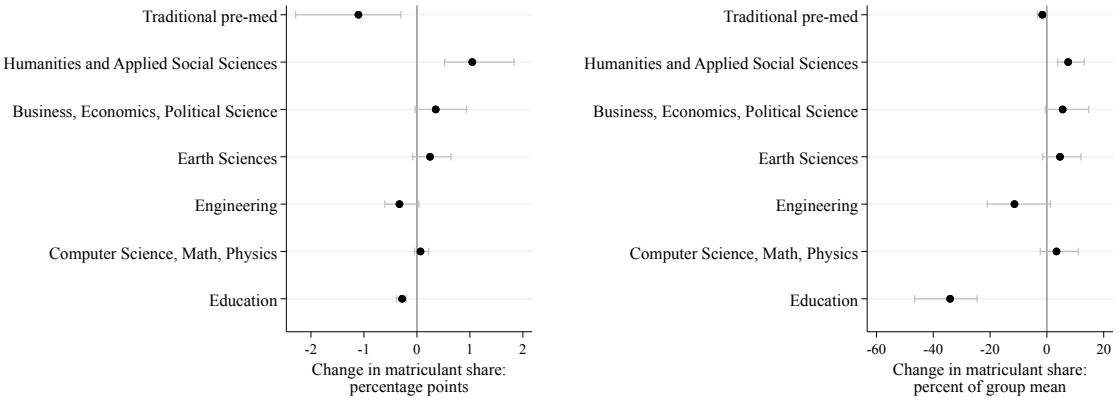
Notes: The above figures show how the average MCAT scores of applicants to U.S. medical schools covary with the medical-school-applicant-relevant business cycle ($\bar{U}_y$) from 1991 to 2014. Each column corresponds to a different score measure: the MCAT composite and, separately, its biological sciences, physical sciences, and verbal reasoning components. Subfigure (a) presents the raw time series of average scores in levels, while subfigure (b) presents the time series of average scores after residualizing from a linear time trend. Subfigure (c) shows a scatterplot of the residualized scores against $\bar{U}_y$. In subfigure (c), the relationship is shown separately for the period before the Great Recession (1991–2007; solid circles)—the period for which we can examine downstream outcomes on eventual practice styles—and the post–Great Recession period (2008–2014; hollow circles). The coefficients reported in subfigure (c) come from bivariate regressions of residualized scores on $\bar{U}_y$; corresponding p-values are calculated using a wild bootstrap. See Section II for details on the construction of $\bar{U}_y$. Data on MCAT scores are from the AAMC.

Figure A6: Entry conditions and medical school matriculant composition by major cluster

(a) 1989–2007



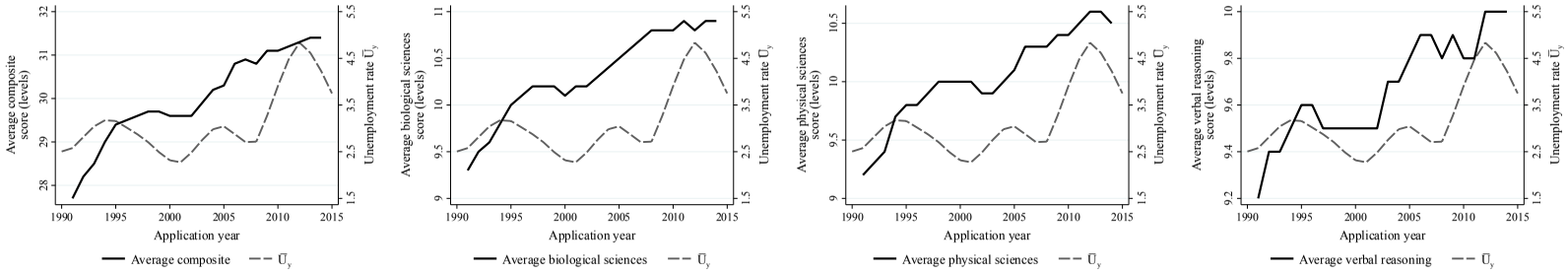
(b) 1989–2019



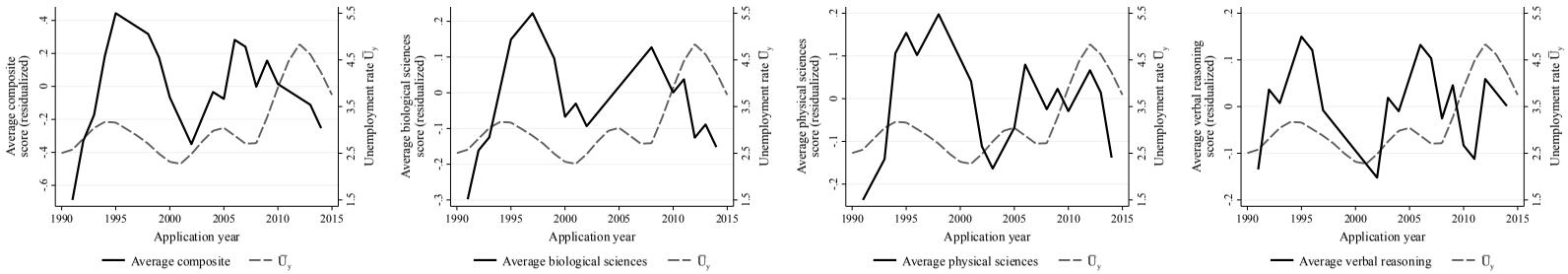
Notes: The above figures show how the annual percent of U.S. matriculants to U.S. medical schools from different undergraduate major clusters covaries with the medical-school-applicant-relevant business cycle ($\bar{U}_y$) from 1989 to 2007 (subfigure (a)) and from 1989 to 2019 (subfigure (b)). The figures plot coefficients and 95 percent confidence intervals, calculated using a wild bootstrap, from separate bivariate regressions of the percent of matriculants of a given type on $\bar{U}_y$. The left plot in each subfigure shows the effects in levels (i.e., percentage points), whereas the right plot in each subfigure shows the effects as a percent of the group mean (i.e., the average percent of U.S. matriculants from a given major cluster over the relevant time period). See Table A1 for the majors included in each cluster, and see Section II for details on the construction of $\bar{U}_y$. Data on applicants and matriculants are from the AAMC.

Figure A7: Entry conditions and average MCAT scores of medical school matriculants

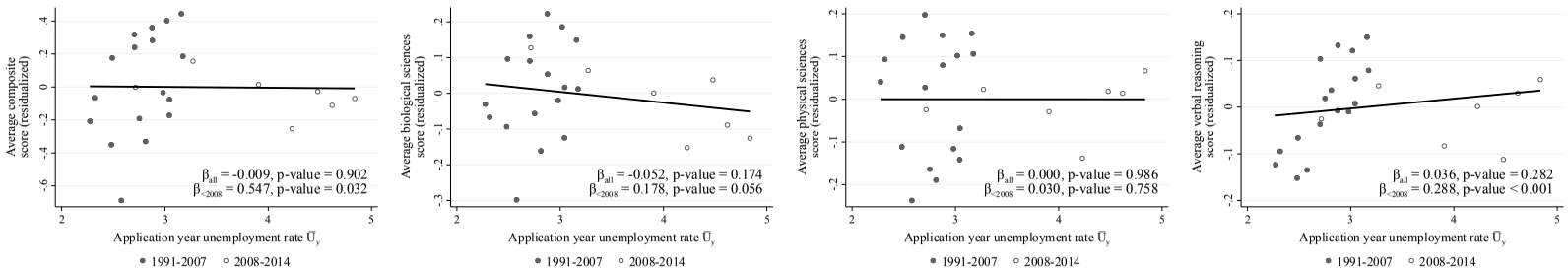
(a) Average MCAT scores (levels) and unemployment over time



(b) Average MCAT scores (residualized) and unemployment over time

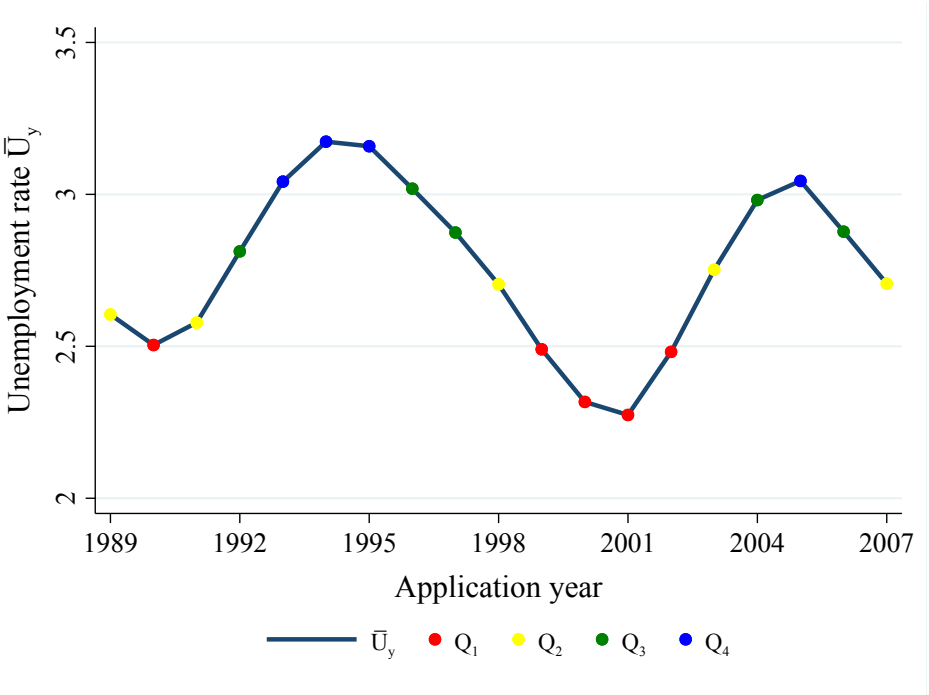


(c) Average MCAT scores (residualized) versus relevant unemployment



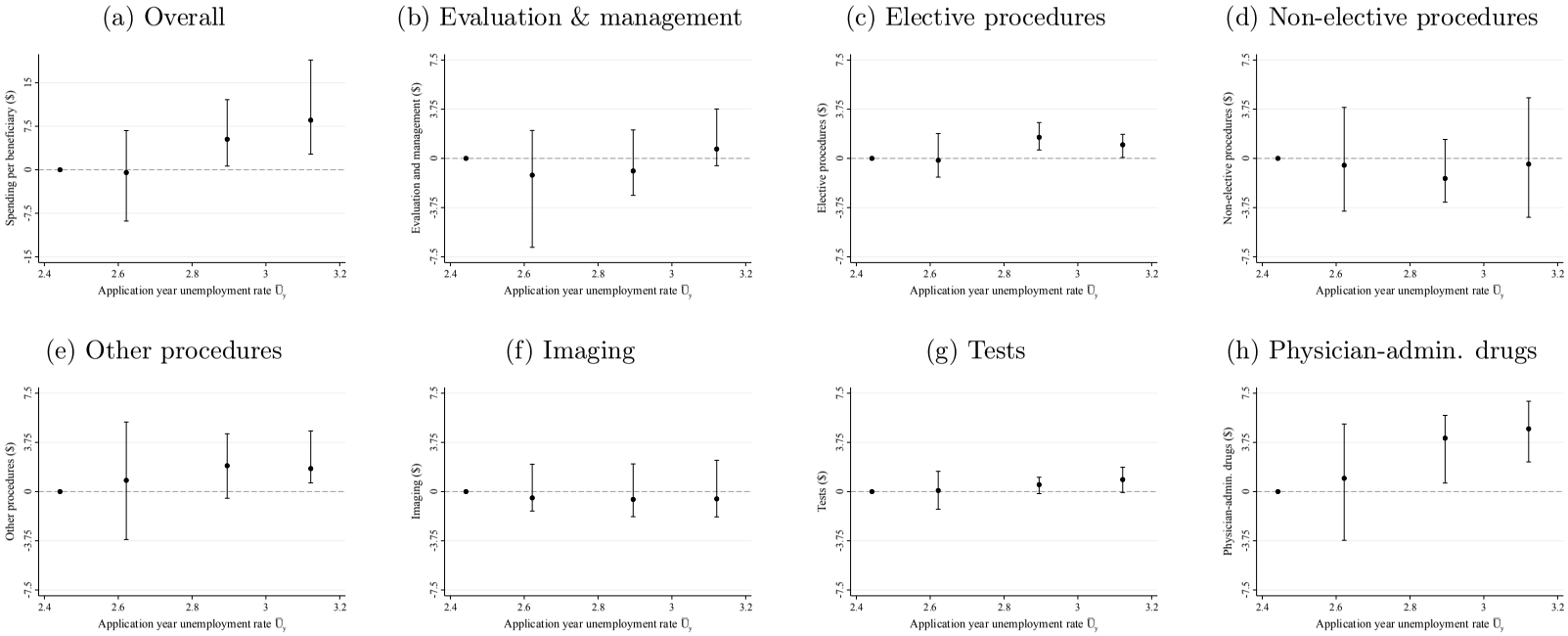
Notes: The above figures show how the average MCAT scores of matriculants to U.S. medical schools covary with the medical-school-applicant-relevant business cycle ($\bar{U}_y$) from 1991 to 2014. Each column corresponds to a different score measure: the MCAT composite and, separately, its biological sciences, physical sciences, and verbal reasoning components. Subfigure (a) presents the raw time series of average scores in levels, while subfigure (b) presents the time series of average scores after residualizing from a linear time trend. Subfigure (c) shows a scatterplot of the residualized scores against $\bar{U}_y$. In subfigure (c), the relationship is shown separately for the period before the Great Recession (1991–2007; solid circles)—the period for which we can examine downstream outcomes on eventual practice styles—and the post–Great Recession period (2008–2014; hollow circles). The coefficients reported in subfigure (c) come from bivariate regressions of residualized scores on $\bar{U}_y$; corresponding p-values are calculated using a wild bootstrap. See Section II for details on the construction of $\bar{U}_y$. Data on MCAT scores are from the AAMC.

Figure A8: Medical-school-applicant-relevant unemployment quartile groupings



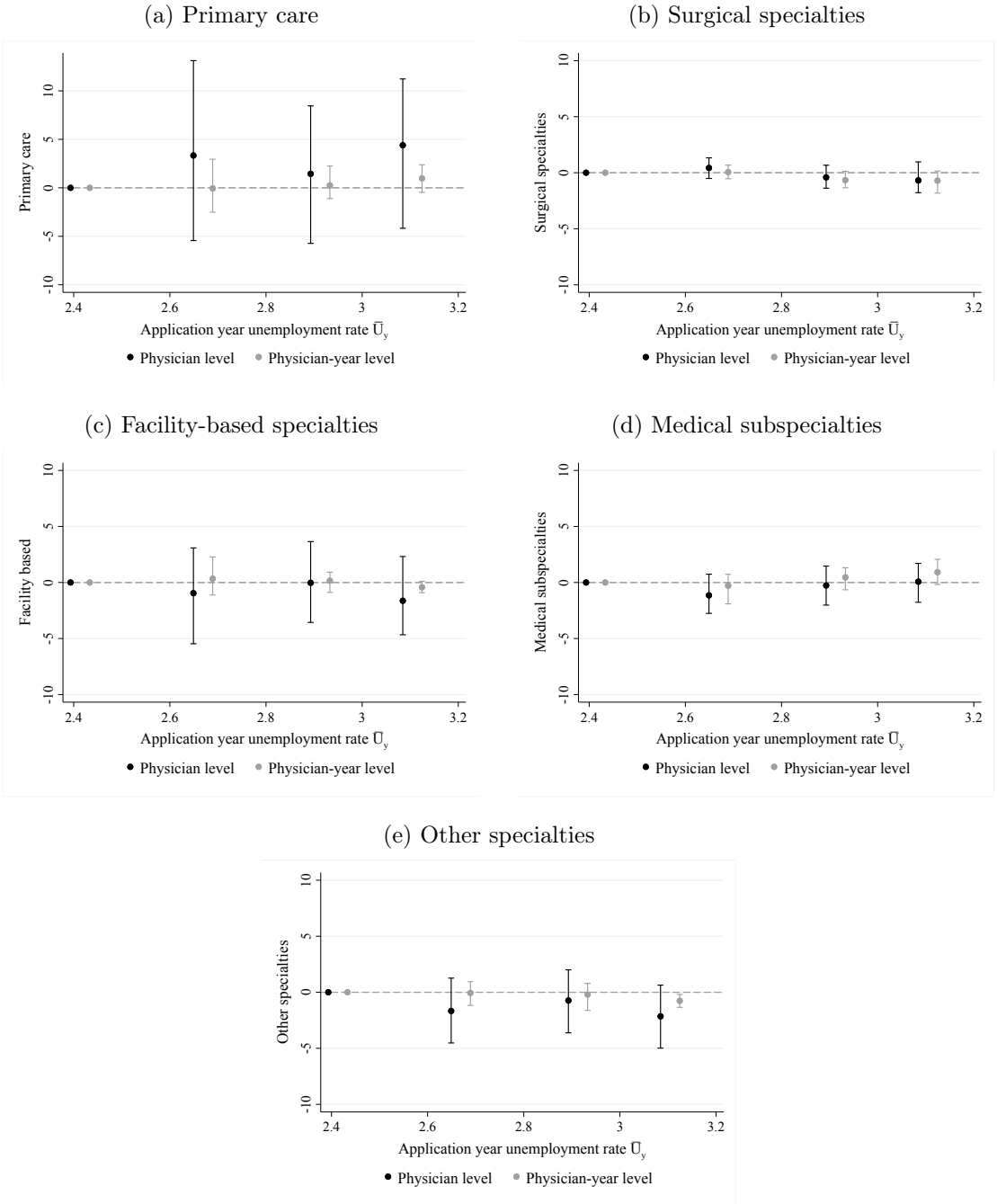
Notes: The above figures show how the medical-school-applicant-relevant business cycle ($\bar{U}_y$) is divided into quartiles. These groupings are subsequently used for analyses relying on estimation of equation (3). See Section II for details on the construction of $\bar{U}_y$.

Figure A9: Entry conditions and annual physician billing per beneficiary, overall and by service category (restricted Medicare)



Notes: The above figures present results from estimation of equation (3) using average annual revenue per beneficiary overall (subfigure (a)) and by service category (subfigures (b)–(h)) as outcomes. We divide all Medicare Part B physician billing into eight mutually exclusive and exhaustive service categories; all are shown except for the "other services" category. Confidence intervals are calculated using a wild cluster bootstrap clustered by application cohort (Cameron et al., 2008). The y-axis in subfigure (a) is scaled to span from –5 percent to +5 percent of the outcome mean; all other y-axes range from –$7.5 to +$7.5. All regressions include specialty, experience, and outcome year fixed effects. The sample is restricted to allopathic physicians who completed their initial medical education in the United States, applied to medical school in or after 1989, and have at least seven years of experience in a given outcome year. See Section II for details on the construction of $\bar{U}_y$. Outcome data are drawn from restricted Medicare data from 2001–2016.

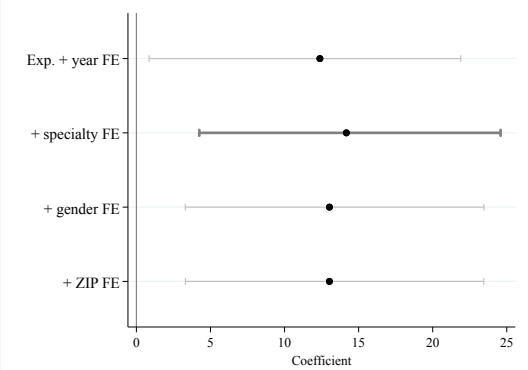
Figure A10: Entry conditions and physician specialty choice



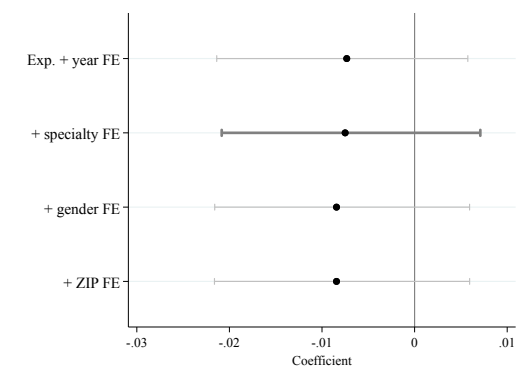
Notes: The above figures present results from estimation of analogs of equation (3) using data at the physician level (black dots and lines) and at the physician-year level from 2014–2019 (gray dots and lines). Confidence intervals are calculated using a wild cluster bootstrap clustered by application cohort (Cameron et al., 2008). The five specialty groupings are mutually exclusive and exhaustive. The panel regressions include experience and outcome year fixed effects. The sample is restricted to allopathic physicians who completed their initial medical education in the United States, applied to medical school in or after 1989, and have at least seven years of experience in a given outcome year. See Section II for details on the construction of $\bar{U}_y$. Outcome data are drawn from an extract of the AMA Masterfile from 2018 and the public-use Medicare Physician Compare files from 2014–2019.

Figure A11: Alternative controls: billing, patient mix, and practice size (public Medicare)

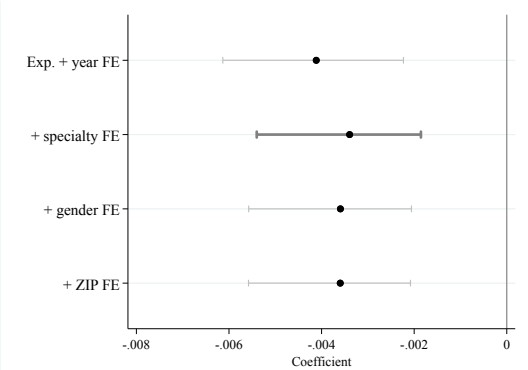
(a) Average revenue per beneficiary



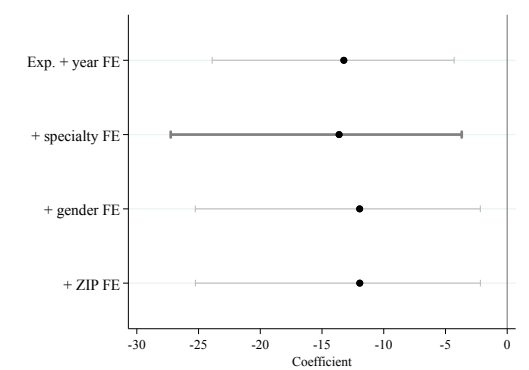
(b) Average patient risk scores



(c) Share Medicare beneficiaries dually eligible

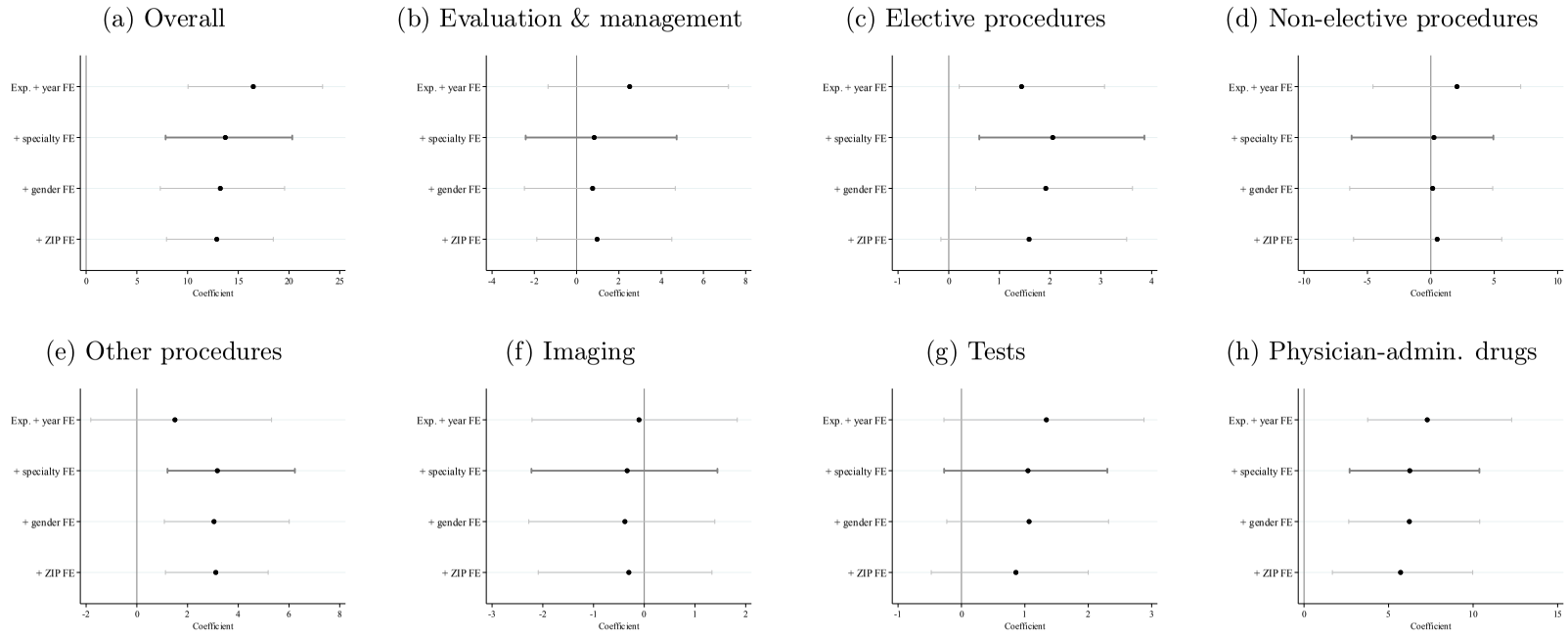


(d) Practice size



Notes: The above figures present results from estimation of analogs of equation (2) using average annual Medicare billing per beneficiary (subfigure (a)), average risk scores among the physician's Medicare beneficiaries (subfigure (b)), the share of a physician's Medicare beneficiaries that are dually eligible for Medicaid (subfigure (c)), and physician practice size (subfigure (d)) as outcomes. Confidence intervals are calculated using a wild cluster bootstrap clustered by application cohort (Cameron et al., 2008). All regressions include experience and outcome year fixed effects. The second row in each subfigure additionally includes specialty fixed effects; the third additionally includes physician sex fixed effects (male, female, and missing); and the final row additionally includes practice ZIP code fixed effects. The sample is restricted to allopathic physicians who completed their initial medical education in the United States, applied to medical school in or after 1989, and have at least seven years of experience in a given outcome year. See Section II for details on the construction of $\bar{U}_y$. Outcome data are drawn from the public-use Medicare Part B files from 2013–2019 and the public-use Medicare Physician Compare files from 2014–2019.

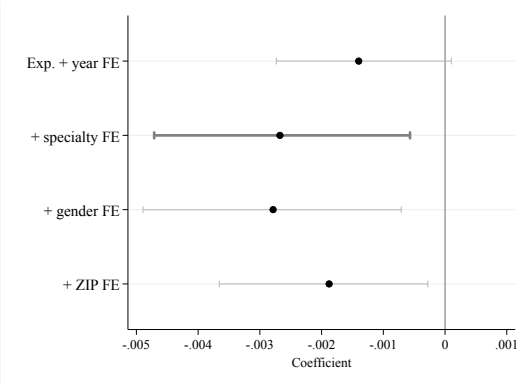
Figure A12: Alternative controls: billing per beneficiary, overall and by service category (restricted Medicare)



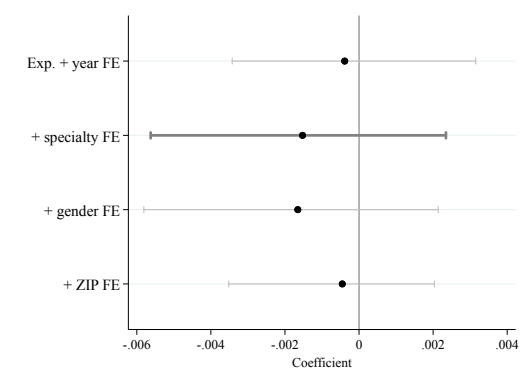
Notes: The above figures present results from estimation of analogs of equation (2) using average annual revenue per beneficiary overall (subfigure (a)) and by service category (subfigures (b)–(h)) as outcomes. We divide all Medicare Part B physician billing into eight mutually exclusive and exhaustive service categories; all are shown except for the "other services" category. Confidence intervals are calculated using a wild cluster bootstrap clustered by application cohort (Cameron et al., 2008). All regressions include experience and outcome year fixed effects. The second row in each subfigure additionally includes specialty fixed effects; the third additionally includes physician sex fixed effects (male, female, and missing); and the final row additionally includes practice ZIP code fixed effects. The sample is restricted to allopathic physicians who completed their initial medical education in the United States, applied to medical school in or after 1989, and have at least seven years of experience in a given outcome year. See Section II for details on the construction of $\bar{U}_y$. Outcome data are drawn from restricted Medicare data from 2001–2016.

Figure A13: Alternative controls: patient mix and patient health outcomes (restricted Medicare)

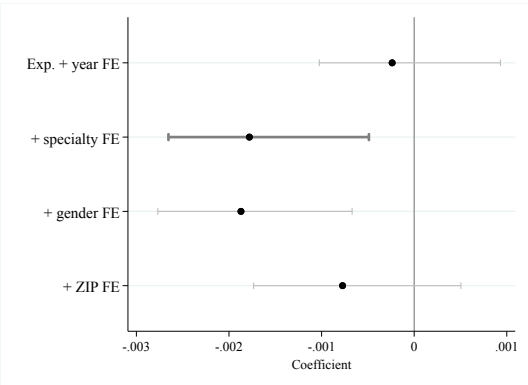
(a) Share any non-avoidable ED visits (1 year)



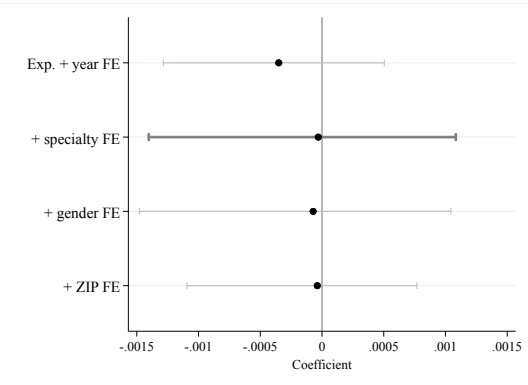
(b) Share any avoidable ED visits (1 year)



(c) Average predicted 1-year mortality



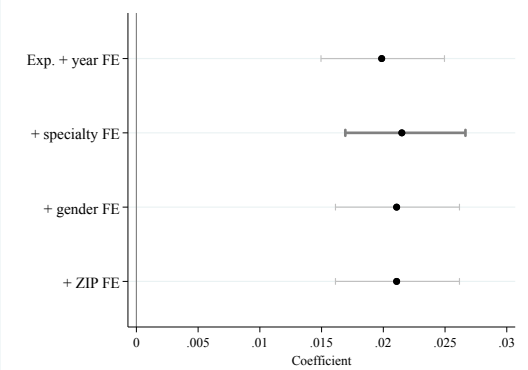
(d) Average actual 1-year mortality (conditional)



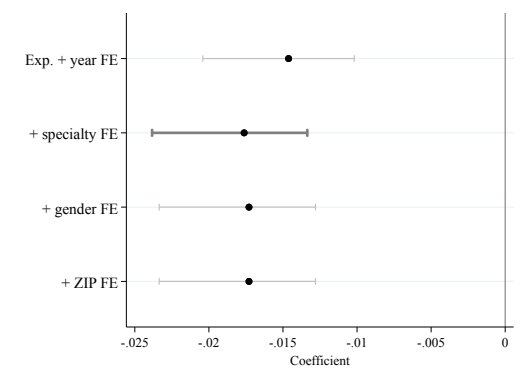
Notes: The above figures present results from estimation of analogs of equation (2) using the share of a physician's beneficiaries with an emergent, non-avoidable emergency department (ED) visit in the following year (subfigure (a)), the share with an avoidable ED visit in the following year (subfigure (b)), the average predicted one-year mortality rate among the physician's beneficiaries (subfigure (c)), and the average actual one-year mortality rate conditional on predicted (subfigure (d)) as outcomes. All four measures are visit-weighted, as described in Section IV.A. ED visits are classified using the algorithm of Billings et al. (2000); avoidable visits comprise the non-emergent, emergent/primary care treatable, and emergent/preventable or avoidable categories. Confidence intervals are calculated using a wild cluster bootstrap clustered by application cohort (Cameron et al., 2008). All regressions include experience and outcome year fixed effects. The second row in each subfigure additionally includes specialty fixed effects; the third additionally includes physician sex fixed effects (male, female, and missing); and the final row additionally includes practice ZIP code fixed effects. The sample is restricted to allopathic physicians who completed their initial medical education in the United States, applied to medical school in or after 1989, and have at least seven years of experience in a given outcome year. See Section II for details on the construction of $\bar{U}_y$. Outcome data are drawn from restricted Medicare data from 2001–2016.

Figure A14: Alternative controls: practice ownership and affiliation (SK&A)

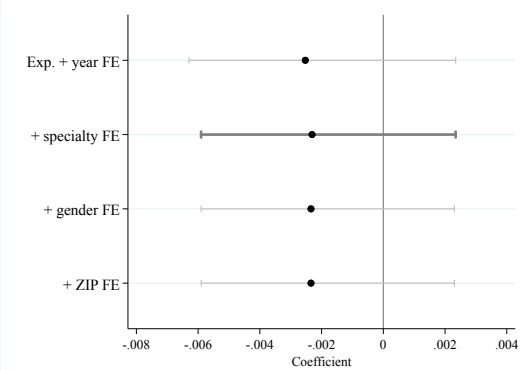
(a) Share hospital-affiliated practice



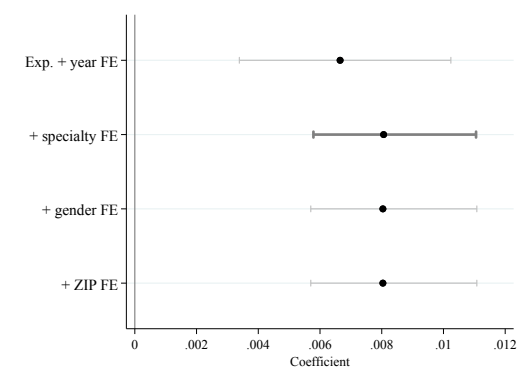
(b) Share hospital- or health-system-owned



(c) Share independent practice



(d) Share "other" practice type



Notes: The above figures present results from estimation of analogs of equation (2) using as outcomes the share of physicians in each practice category: independent but hospital affiliated (subfigure (a)), hospital- or health-system-owned (subfigure (b)), independent and unaffiliated (subfigure (c)), and "other" (subfigure (d)). Confidence intervals are calculated using a wild cluster bootstrap clustered by application cohort (Cameron et al., 2008). All regressions include experience and outcome year fixed effects. The second row in each subfigure additionally includes specialty fixed effects; the third additionally includes physician sex fixed effects (male, female, and missing); and the final row additionally includes practice ZIP code fixed effects. The sample is restricted to allopathic physicians who completed their initial medical education in the United States, applied to medical school in or after 1989, and have at least seven years of experience in a given outcome year. See Section II for details on the construction of $\bar{U}_y$. Outcome data are drawn from the SK&A data from 2008–2016.

Figure A15: Alternative sample definitions: public-use Medicare data (2013–2019)

(a) Main sample

Application year (cohort) \ Outcome year	2013	2014	2015	2016	2017	2018	2019
1989	19	20	21	22	23	24	25
1990	18	19	20	21	22	23	24
1991	17	18	19	20	21	22	23
1992	16	17	18	19	20	21	22
1993	15	16	17	18	19	20	21
1994	14	15	16	17	18	19	20
1995	13	14	15	16	17	18	19
1996	12	13	14	15	16	17	18
1997	11	12	13	14	15	16	17
1998	10	11	12	13	14	15	16
1999	9	10	11	12	13	14	15
2000	8	9	10	11	12	13	14
2001	7	8	9	10	11	12	13
2002	6	7	8	9	10	11	12
2003	5	6	7	8	9	10	11
2004	4	5	6	7	8	9	10
2005	3	4	5	6	7	8	9
2006	2	3	4	5	6	7	8
2007	1	2	3	4	5	6	7
2008	0	1	2	3	4	5	6

(b) 4+ observations per cell

Application year (cohort) \ Outcome year	2013	2014	2015	2016	2017	2018	2019
1989	19	20	21	22	23	24	25
1990	18	19	20	21	22	23	24
1991	17	18	19	20	21	22	23
1992	16	17	18	19	20	21	22
1993	15	16	17	18	19	20	21
1994	14	15	16	17	18	19	20
1995	13	14	15	16	17	18	19
1996	12	13	14	15	16	17	18
1997	11	12	13	14	15	16	17
1998	10	11	12	13	14	15	16
1999	9	10	11	12	13	14	15
2000	8	9	10	11	12	13	14
2001	7	8	9	10	11	12	13
2002	6	7	8	9	10	11	12
2003	5	6	7	8	9	10	11
2004	4	5	6	7	8	9	10
2005	3	4	5	6	7	8	9
2006	2	3	4	5	6	7	8
2007	1	2	3	4	5	6	7
2008	0	1	2	3	4	5	6

(c) Balance: outcome years and cohort

Application year (cohort) \ Outcome year	2013	2014	2015	2016	2017	2018	2019
1989	19	20	21	22	23	24	25
1990	18	19	20	21	22	23	24
1991	17	18	19	20	21	22	23
1992	16	17	18	19	20	21	22
1993	15	16	17	18	19	20	21
1994	14	15	16	17	18	19	20
1995	13	14	15	16	17	18	19
1996	12	13	14	15	16	17	18
1997	11	12	13	14	15	16	17
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1999	9	10	11	12	13	14	15
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2001	7	8	9	10	11	12	13
2002	6	7	8	9	10	11	12
2003	5	6	7	8	9	10	11
2004	4	5	6	7	8	9	10
2005	3	4	5	6	7	8	9
2006	2	3	4	5	6	7	8
2007	1	2	3	4	5	6	7
2008	0	1	2	3	4	5	6

(d) Balance: outcome years and experience

Application year (cohort) \ Outcome year	2013	2014	2015	2016	2017	2018	2019
1989	19	20	21	22	23	24	25
1990	18	19	20	21	22	23	24
1991	17	18	19	20	21	22	23
1992	16	17	18	19	20	21	22
1993	15	16	17	18	19	20	21
1994	14	15	16	17	18	19	20
1995	13	14	15	16	17	18	19
1996	12	13	14	15	16	17	18
1997	11	12	13	14	15	16	17
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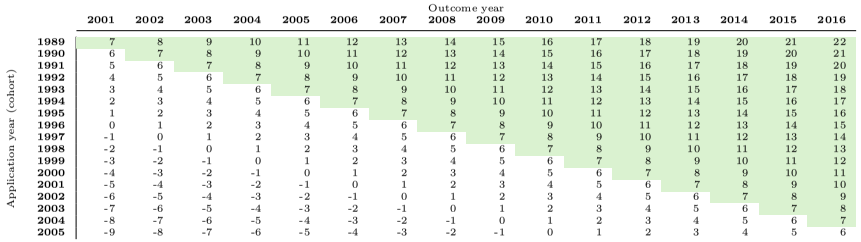
2001	7	8	9	10	11	12	13
2002	6	7	8	9	10	11	12
2003	5	6	7	8	9	10	11
2004	4	5	6	7	8	9	10
2005	3	4	5	6	7	8	9
2006	2	3	4	5	6	7	8
2007	1	2	3	4	5	6	7
2008	0	1	2	3	4	5	6

Notes: The figures present alternative samples used to estimate equation (3) using the public-use Medicare data from 2013–2019. Subfigure (a) shows the main sample, which limits the data to physicians who applied to medical school in or after 1989 and had at least seven years of experience following medical school in a given outcome year. Starting from this main sample, subfigure (b) additionally requires at least four cohort–experience–outcome year cells within each included cohort, experience profile, and outcome year; subfigure (c) requires balance across outcome years and application cohorts; and subfigure (d) requires balance across outcome years and experience profiles. Results for these alternative samples are reported in Table A16.

Figure A16: Alternative sample definitions: restricted Medicare data (2001–2016)

(a) Main sample



	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016
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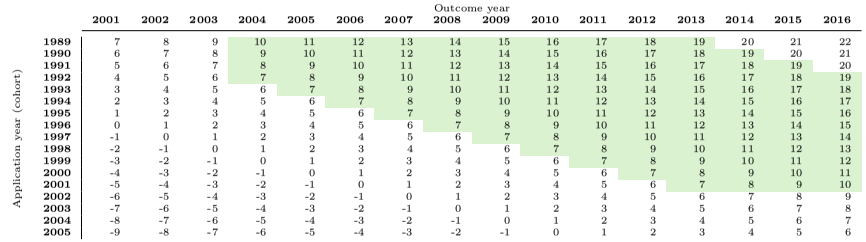
1990	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
1991	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
1992	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
1993	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
1994	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17
1995	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
1996	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
1997	-1	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14
1998	-2	-1	0	1	2	3	4	5	6	7	8	9	10	11	12	13
1999	-3	-2	-1	0	1	2	3	4	5	6	7	8	9	10	11	12
2000	-4	-3	-2	-1	0	1	2	3	4	5	6	7	8	9	10	11
2001	-5	-4	-3	-2	-1	0	1	2	3	4	5	6	7	8	9	10
2002	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5	6	7	8	9
2003	-7	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5	6	7	8
2004	-8	-7	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5	6	7
2005	-9	-8	-7	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5	6

(b) 4+ observations per cell



	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016
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1990	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
1991	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
1992	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
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2001	-5	-4	-3	-2	-1	0	1	2	3	4	5	6	7	8	9	10
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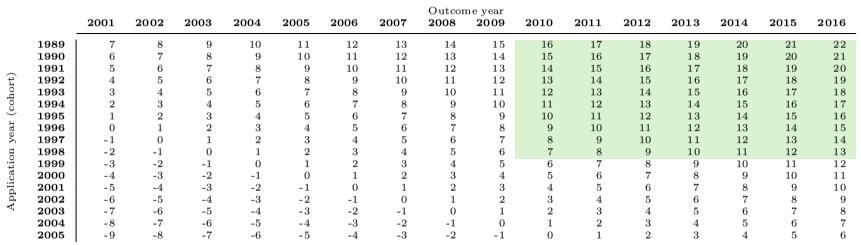
2003	-7	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5	6	7	8
2004	-8	-7	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5	6	7
2005	-9	-8	-7	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5	6

(c) Balance: outcome years and cohort



	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016
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1990	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
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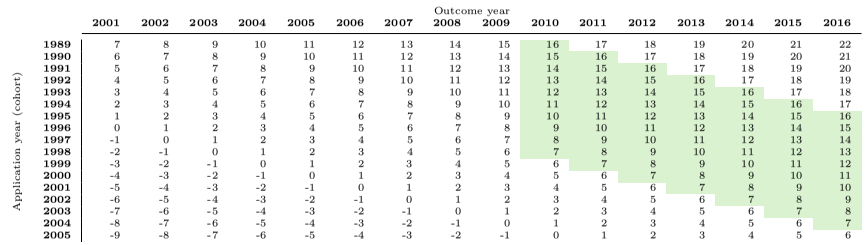
2005	-9	-8	-7	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5	6

(d) Balance: outcome years and experience



	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016
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1990	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
1991	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
1992	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
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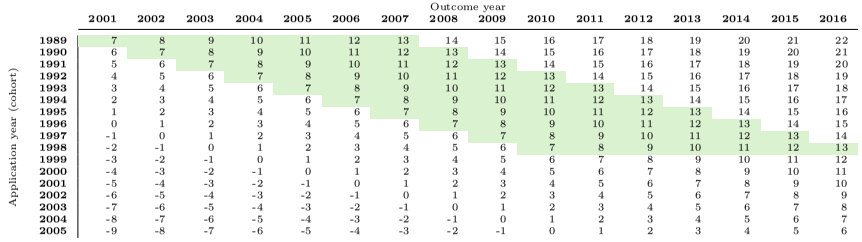
2002	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5	6	7	8	9
2003	-7	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5	6	7	8
2004	-8	-7	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5	6	7
2005	-9	-8	-7	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5	6

(e) Balance: experience and cohort



	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016
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1994	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17
1995	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
1996	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
1997	-1	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14
1998	-2	-1	0	1	2	3	4	5	6	7	8	9	10	11	12	13
1999	-3	-2	-1	0	1	2	3	4	5	6	7	8	9	10	11	12
2000	-4	-3	-2	-1	0	1	2	3	4	5	6	7	8	9	10	11
2001	-5	-4	-3	-2	-1	0	1	2	3	4	5	6	7	8	9	10
2002	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5	6	7	8	9
2003	-7	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5	6	7	8
2004	-8	-7	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5	6	7
2005	-9	-8	-7	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5	6

Notes: The figures present alternative samples used to estimate equation (3) using the restricted Medicare data from 2001–2016. Subfigure (a) shows the main sample, which limits the data to physicians who applied to medical school in or after 1989 and had at least seven years of experience following medical school in a given outcome year. Starting from this main sample, subfigure (b) additionally requires at least four cohort–experience–outcome year cells within each included cohort, experience profile, and outcome year; subfigure (c) requires balance across outcome years and application cohorts; subfigure (d) requires balance across outcome years and experience profiles; and subfigure (e) requires balance across experience profiles and cohorts. Results for these alternative samples are reported in Table A17–A19.

Figure A17: Alternative sample definitions: SK&A data (2008–2016)

(a) Main sample

Application year (cohort) \ Outcome year	2008	2009	2010	2011	2012	2013	2014	2015	2016
1989	14	15	16	17	18	19	20	21	22
1990	13	14	15	16	17	18	19	20	21
1991	12	13	14	15	16	17	18	19	20
1992	11	12	13	14	15	16	17	18	19
1993	10	11	12	13	14	15	16	17	18
1994	9	10	11	12	13	14	15	16	17
1995	8	9	10	11	12	13	14	15	16
1996	7	8	9	10	11	12	13	14	15
1997	6	7	8	9	10	11	12	13	14
1998	5	6	7	8	9	10	11	12	13
1999	4	5	6	7	8	9	10	11	12
2000	3	4	5	6	7	8	9	10	11
2001	2	3	4	5	6	7	8	9	10
2002	1	2	3	4	5	6	7	8	9
2003	0	1	2	3	4	5	6	7	8
2004	-1	0	1	2	3	4	5	6	7
2005	-2	-1	0	1	2	3	4	5	6

(b) 4+ observations per cell

Application year (cohort) \ Outcome year	2008	2009	2010	2011	2012	2013	2014	2015	2016
1989	14	15	16	17	18	19	20	21	22
1990	13	14	15	16	17	18	19	20	21
1991	12	13	14	15	16	17	18	19	20
1992	11	12	13	14	15	16	17	18	19
1993	10	11	12	13	14	15	16	17	18
1994	9	10	11	12	13	14	15	16	17
1995	8	9	10	11	12	13	14	15	16
1996	7	8	9	10	11	12	13	14	15
1997	6	7	8	9	10	11	12	13	14
1998	5	6	7	8	9	10	11	12	13
1999	4	5	6	7	8	9	10	11	12
2000	3	4	5	6	7	8	9	10	11
2001	2	3	4	5	6	7	8	9	10
2002	1	2	3	4	5	6	7	8	9
2003	0	1	2	3	4	5	6	7	8
2004	-1	0	1	2	3	4	5	6	7
2005	-2	-1	0	1	2	3	4	5	6

(c) Balance: outcome years and cohort

Application year (cohort) \ Outcome year	2008	2009	2010	2011	2012	2013	2014	2015	2016
1989	14	15	16	17	18	19	20	21	22
1990	13	14	15	16	17	18	19	20	21
1991	12	13	14	15	16	17	18	19	20
1992	11	12	13	14	15	16	17	18	19
1993	10	11	12	13	14	15	16	17	18
1994	9	10	11	12	13	14	15	16	17
1995	8	9	10	11	12	13	14	15	16
1996	7	8	9	10	11	12	13	14	15
1997	6	7	8	9	10	11	12	13	14
1998	5	6	7	8	9	10	11	12	13
1999	4	5	6	7	8	9	10	11	12
2000	3	4	5	6	7	8	9	10	11
2001	2	3	4	5	6	7	8	9	10
2002	1	2	3	4	5	6	7	8	9
2003	0	1	2	3	4	5	6	7	8
2004	-1	0	1	2	3	4	5	6	7
2005	-2	-1	0	1	2	3	4	5	6

(d) Balance: outcome years and experience

Application year (cohort) \ Outcome year	2008	2009	2010	2011	2012	2013	2014	2015	2016
1989	14	15	16	17	18	19	20	21	22
1990	13	14	15	16	17	18	19	20	21
1991	12	13	14	15	16	17	18	19	20
1992	11	12	13	14	15	16	17	18	19
1993	10	11	12	13	14	15	16	17	18
1994	9	10	11	12	13	14	15	16	17
1995	8	9	10	11	12	13	14	15	16
1996	7	8	9	10	11	12	13	14	15
1997	6	7	8	9	10	11	12	13	14
1998	5	6	7	8	9	10	11	12	13
1999	4	5	6	7	8	9	10	11	12
2000	3	4	5	6	7	8	9	10	11
2001	2	3	4	5	6	7	8	9	10
2002	1	2	3	4	5	6	7	8	9
2003	0	1	2	3	4	5	6	7	8
2004	-1	0	1	2	3	4	5	6	7
2005	-2	-1	0	1	2	3	4	5	6

Notes: The figures present alternative samples used to estimate equation (3) using the SK&A data from 2008–2016. Subfigure (a) shows the main sample, which limits the data to physicians who applied to medical school in or after 1989 and had at least seven years of experience following medical school in a given outcome year. Starting from this main sample, subfigure (b) additionally requires at least four cohort–experience–outcome year cells within each included cohort, experience profile, and outcome year; subfigure (c) requires balance across outcome years and application cohorts; and subfigure (d) requires balance across outcome years and experience profiles. Results for these alternative samples are reported in Table A20.

B Supplementary tables

Table A1: Clusters of undergraduate majors from k-means clustering

Cluster 1: Traditional Pre-Medical Majors	
Biochemical Sciences	Molecular Biology
Biology	Neuroscience
Biomedical Engineering	Nursing
Chemistry	Pharmacology
Genetics	Pharmacy, Pharmaceutical Sciences
Health and Medical Preparatory Programs	Physiology
Microbiology	Zoology
Cluster 2: Engineering	
Aerospace Engineering	Electrical Engineering
Biological Engineering	General Engineering
Chemical Engineering	Mechanical Engineering
Civil Engineering	
Cluster 3: Computer Science, Math, Physics	
Astronomy and Astrophysics	Mathematics
Computer Science	Physics
Management Information Systems and Statistics	
Cluster 4: Earth Sciences	
Animal Sciences	Geography
Architecture	Geology and Earth Science
Atmospheric Sciences and Meteorology	Geosciences
Botany	Medical Technologies Technicians
Environmental Science	Military Technologies
Forestry	Miscellaneous Biology
General Agriculture	Oceanography
Cluster 5: Humanities and Applied Social Sciences	
Anthropology and Archeology	Library Science
Communications	Linguistics and Comparative Language
Community and Public Health	Miscellaneous Business and Medical Admin.
Composition and Speech	Miscellaneous Health Medical Professions
Drama and Theater Arts	Miscellaneous Social Sciences
English Language and Literature	Music
Fine Arts	Nutrition Sciences
French, German, Latin, and Other Common Languages	Psychology
General Social Sciences	Social Work
Humanities	Sociology
Journalism	Treatment Therapy Professions
Liberal Arts	
Cluster 6: Business, Economics, Political Science	
Accounting	International Relations
Economics	Philosophy and Religious Studies
General Business	Political Science and Government
History	Pre-Law and Legal Studies
Cluster 7: Education	
General Education	Physical and Health Education Teaching
Interdisciplinary and Multi-Disciplinary Studies	

Notes: The above table shows the grouping of undergraduate majors into seven clusters. Groupings are obtained via k-means clustering based on the similarity in the occupational distributions of graduates across majors as reported in one-year ACS from 2009–2019. See Section II for additional details.

Table A2: Unemployment and medical school applicants and matriculants nationally

a. Overall	U.S. applicants per spot		Percent matriculants from U.S.	
	1989–2019	1989–2007	1989–2019	1989–2007
	(1)	(2)	(3)	(4)
Unemployment rate $\bar{U}_y$	0.09**	0.81***	-0.12**	0.27
	(0.04)	(0.16)	(0.03)	(0.18)
	[0.044]	[0.006]	[0.028]	[0.192]
Mean dependent variable	2.3	2.3	97.8	97.9
Percent of mean	4.1	35.8	-0.1	0.3
b. U.S. applicants by major	Pre-medical applicants per spot		Other applicants per spot	
	1989–2019	1989–2007	1989–2019	1989–2007
	(1)	(2)	(3)	(4)
Unemployment rate $\bar{U}_y$	-0.00	0.43**	0.02	0.31**
	(0.02)	(0.09)	(0.01)	(0.06)
	[0.870]	[0.010]	[0.106]	[0.010]
Mean dependent variable	1.4	1.4	0.6	0.6
Percent of mean	-0.3	30.2	4.0	51.1
c. U.S. applicants by sex	Male applicants per spot		Female applicants per spot	
	1989–2019	1989–2007	1989–2019	1989–2007
	(1)	(2)	(3)	(4)
Unemployment rate $\bar{U}_y$	0.06**	0.53**	0.04	0.28***
	(0.03)	(0.12)	(0.02)	(0.07)
	[0.036]	[0.016]	[0.148]	[<0.001]
Mean dependent variable	1.2	1.2	1.1	1.0
Percent of mean	4.6	42.8	3.5	27.3
d. U.S. matriculant composition	Percent non-pre-medical		Percent female	
	1989–2019	1989–2007	1989–2019	1989–2007
	(1)	(2)	(3)	(4)
Unemployment rate $\bar{U}_y$	1.10***	4.68***	0.45	-2.35
	(0.35)	(0.94)	(0.67)	(2.63)
	[0.002]	[<0.001]	[0.558]	[0.404]
Mean dependent variable	31.4	31.0	46.6	45.0
Percent of mean	3.5	15.1	1.0	-5.2
Observations	31	19	31	19

Notes: The above table shows output from estimation of an analog of equation (1) using national data at the annual level. Columns (1) and (3) in each panel use data from 1989–2019, while columns (2) and (4) restrict the sample to 1989–2007. Robust standard errors are shown in parentheses; p-values, calculated using a wild bootstrap, are shown in brackets. See Section II for details on the construction of $\bar{U}_y$. Data on applicants and matriculants are from the AAMC. Significance stars are based on the wild bootstrap p-values: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

Table A3: Unemployment and MCAT scores of medical school applicants and matriculants

a. Composite score	Average applicant		Average matriculant	
	1991–2014	1991–2007	1991–2014	1991–2007
	(1)	(2)	(3)	(4)
Unemployment rate $\bar{U}_y$	-0.22**	0.06	-0.01	0.55**
	(0.08)	(0.26)	(0.08)	(0.24)
	[0.016]	[0.816]	[0.902]	[0.032]
Within-cohort standard deviation	5.7	5.7	4.3	4.4
Effect in standard deviations	-0.04	0.01	-0.00	0.12
b. Biological sciences	Average applicant		Average matriculant	
	1991–2014	1991–2007	1991–2014	1991–2007
	(1)	(2)	(3)	(4)
Unemployment rate $\bar{U}_y$	-0.12***	0.03	-0.05	0.18*
	(0.04)	(0.12)	(0.04)	(0.10)
	[0.002]	[0.828]	[0.174]	[0.056]
Within-cohort standard deviation	2.1	2.1	1.7	1.7
Effect in standard deviations	-0.06	0.01	-0.03	0.10
c. Physical sciences	Average applicant		Average matriculant	
	1991–2014	1991–2007	1991–2014	1991–2007
	(1)	(2)	(3)	(4)
Unemployment rate $\bar{U}_y$	-0.04	-0.05	-0.00	0.03
	(0.04)	(0.12)	(0.04)	(0.12)
	[0.322]	[0.752]	[0.986]	[0.758]
Within-cohort standard deviation	2.3	2.2	2.0	2.0
Effect in standard deviations	-0.02	-0.02	0.00	0.02
d. Verbal reasoning	Average applicant		Average matriculant	
	1991–2014	1991–2007	1991–2014	1991–2007
	(1)	(2)	(3)	(4)
Unemployment rate $\bar{U}_y$	-0.04	0.11*	0.04	0.29***
	(0.03)	(0.06)	(0.03)	(0.05)
	[0.160]	[0.070]	[0.282]	[<0.001]
Within-cohort standard deviation	2.2	2.2	1.7	1.8
Effect in standard deviations	-0.02	0.05	0.02	0.16
Observations	24	17	24	17

Notes: The above table shows output from estimation of an analog of equation (1) using national data at the annual level, where the dependent variable is the average MCAT score among applicants (columns 1–2) or matriculants (columns 3–4). Each panel corresponds to a different score measure: the total composite score (panel (a)) and, separately, its biological sciences (panel (b)), physical sciences (panel (c)), and verbal reasoning (panel (d)) components. All regressions include a linear time trend to account for the secular upward trend in scores over the sample period. Columns (1) and (3) in each panel use data from 1991–2014, while columns (2) and (4) restrict the sample to 1991–2007. Robust standard errors are shown in parentheses; p-values, calculated using a wild bootstrap, are shown in brackets. See Section II for details on the construction of $\bar{U}_y$. Data on MCAT scores are from the AAMC. Significance stars are based on the wild bootstrap p-values: $^{*}\, p < 0.10$, $^{**}\, p < 0.05$, $^{***}\, p < 0.01$.

Table A4: BETOS procedure classifications: elective, non-elective, and ambiguous

BETOS code	Description
Elective procedures: physicians have meaningful discretion over whether and when to intervene	
P1D	Major procedure – TURP
P1E	Major procedure – hysterectomy
P2E	Major procedure, cardiovascular – pacemaker insertion
P3B	Major procedure, orthopedic – hip replacement
P3C	Major procedure, orthopedic – knee replacement
P4B	Eye procedure – cataract removal/lens insertion
P5A	Ambulatory procedures – skin
P5B	Ambulatory procedures – musculoskeletal
P5C	Ambulatory procedures – inguinal hernia repair
P5D	Ambulatory procedures – lithotripsy
P5E	Ambulatory procedures – other
P6A	Minor procedures – skin
P6B	Minor procedures – musculoskeletal
P6C	Minor procedures – other (Medicare fee schedule)
P6D	Minor procedures – other (non-Medicare fee schedule)
P8A	Endoscopy – arthroscopy
P8C	Endoscopy – sigmoidoscopy
P8D	Endoscopy – colonoscopy
Non-elective procedures: clinically indicated and time-sensitive, with limited scope for delay	
P2A	Major procedure, cardiovascular – CABG
P2B	Major procedure, cardiovascular – aneurysm repair
P2C	Major procedure, cardiovascular – thromboendarterectomy
P2D	Major procedure, cardiovascular – coronary angioplasty (PTCA)
P3A	Major procedure, orthopedic – hip fracture repair
P4C	Eye procedure – retinal detachment
P7A	Oncology – radiation therapy
P7B	Oncology – other
P9A	Dialysis services (Medicare fee schedule)
P9B	Dialysis services (non-Medicare fee schedule)
Ambiguous procedures: discretion varies meaningfully across patients and clinical indications	
P0	Anesthesia
P1A	Major procedure – breast
P1B	Major procedure – colectomy
P1C	Major procedure – cholecystectomy
P1F	Major procedure – explor/decompr/excis disc
P1G	Major procedure – other
P2F	Major procedure, cardiovascular – other
P3D	Major procedure, orthopedic – other
P4A	Eye procedure – corneal transplant
P4D	Eye procedure – treatment of retinal lesions
P4E	Eye procedure – other
P8B	Endoscopy – upper gastrointestinal
P8E	Endoscopy – cystoscopy
P8F	Endoscopy – bronchoscopy
P8G	Endoscopy – laparoscopic cholecystectomy
P8H	Endoscopy – laryngoscopy
P8I	Endoscopy – other

Notes: The table reports the classification of procedure-level BETOS subcategories (codes beginning with "P") as elective, non-elective, or ambiguous, based on the degree of physician discretion over whether and when to intervene. Endoscopic procedures are classified as elective when they are predominantly used in screening or routine diagnostic settings (e.g., colonoscopy or sigmoidoscopy), and as ambiguous when they are frequently performed in response to acute or serious symptoms (e.g., bronchoscopy or upper gastrointestinal endoscopy). Cholecystectomy (P1C) is classified as ambiguous because it may be performed urgently for acute cholecystitis or electively for chronic gallstones; anesthesia (P0) is classified as ambiguous because it is tied to other procedures whose discretion varies. BETOS subcategories outside of procedures are assigned to the eight-category aggregation as described in the main text and are not further subdivided.

Table A5: Impacts of entry conditions on ED visits, overall and by type: restricted Medicare data (2001–2016)

a. Summary measures	Any ED visit (1 year) (1)	Any non-avoidable ED visit (2)	Any avoidable ED visit (3)
Unemployment rate $\bar{U}_y$	–0.36	–0.27**	–0.15
	(0.18)	(0.08)	(0.14)
	[0.108]	[0.016]	[0.388]
Observations	1,322,086	1,322,086	1,322,086
R^2	0.13	0.09	0.09
Mean dependent variable	39.9	15.1	25.5
Percent of mean	–0.9	–1.8	–0.6
b. Avoidable categories	Any non-emergent ED visit (1)	Any primary care treatable visit (2)	Any preventable ED visit (3)
Unemployment rate $\bar{U}_y$	–0.09	–0.06	–0.08
	(0.08)	(0.06)	(0.07)
	[0.398]	[0.358]	[0.336]
Observations	1,322,086	1,322,086	1,322,086
R^2	0.05	0.04	0.06
Mean dependent variable	10.9	10.6	7.9
Percent of mean	–0.8	–0.6	–1.0
Specialty fixed effects	✓	✓	✓
Experience fixed effects	✓	✓	✓
Outcome year fixed effects	✓	✓	✓

Notes: The above table shows output from estimation of equation (2) using data at the physician-year level. In panel (a), outcomes are the percent of a physician's beneficiaries with any emergency department (ED) visit in the following year (column (1)), the percent with an emergent, non-avoidable ED visit (column (2)), and the percent with an avoidable ED visit (column (3)). In panel (b), outcomes are the subcomponents of "avoidable" visits: the percent of a physician's beneficiaries with a non-emergent ED visit in the following year (column (1)), the percent with an emergent, primary care treatable ED visit (column (2)), and the percent with an emergent, preventable ED visit (column (3)). Columns do not sum, as beneficiaries with visits in multiple categories appear in each but are counted only once in any broader total; this applies both to columns (2) and (3) relative to column (1) in panel (a), and to the panel (b) subcomponents relative to column (3) in panel (a). ED visits are classified using the algorithm of Billings et al. (2000). Standard errors, clustered by application cohort, are shown in parentheses; p-values, calculated using a wild cluster bootstrap (Cameron et al., 2008) clustered by cohort, are shown in brackets. See Section II for details on the construction of $\bar{U}_y$. Outcome data are drawn from restricted Medicare files from 2001–2016. Significance stars are based on the wild cluster bootstrap p-values: $^{*}\, p < 0.10$, $^{**}\, p < 0.05$, $^{***}\, p < 0.01$.

Table A6: Heterogeneity by physician sex: billing, patient mix, and practice size (public Medicare)

a. Overall	Revenue per beneficiary (1)	Average risk score (x100) (2)	Dual eligible benes. (%) (3)	Practice size (4)
Unemployment rate $\bar{U}_y$	14.17***	-0.75	-0.34***	-13.62**
	(3.43)	(0.59)	(0.07)	(4.67)
	[0.006]	[0.244]	[0.002]	[0.010]
Observations	1,178,466	1,178,466	1,178,466	927,400
Mean dependent variable	301.4	174.1	24.1	518.9
Percent of mean	4.7	-0.4	-1.4	-2.6
b. By physician sex	Revenue per beneficiary (1)	Average risk score (x100) (2)	Dual eligible benes. (%) (3)	Practice size (4)
$\bar{U}_y$: male physicians	17.17*	-0.70	-0.40**	-14.76
	(5.92)	(0.70)	(0.13)	(6.66)
	[0.068]	[0.384]	[0.022]	[0.104]
$\bar{U}_y$: female physicians	8.25**	-0.77	-0.21	-6.40
	(2.71)	(0.94)	(0.17)	(9.41)
	[0.010]	[0.504]	[0.300]	[0.516]
Observations	1,178,317	1,178,317	1,178,317	927,278
Mean outcome: males	331.7	178.9	24.6	488.7
Mean outcome: females	251.9	166.4	23.4	567.4
Percent of mean: males	5.2	-0.4	-1.6	-3.0
Percent of mean: females	3.3	-0.5	-0.9	-1.1
P-value on difference	[0.342]	[0.996]	[0.558]	[0.580]
Specialty fixed effects	✓	✓	✓	✓
Experience fixed effects	✓	✓	✓	✓
Outcome year fixed effects	✓	✓	✓	✓

Notes: The above table shows output from estimation of equation (2) using data at the physician-year level. Outcomes are the average annual Medicare revenue per beneficiary (column (1)), average risk scores among the physician's Medicare beneficiaries times 100 (column (2)), the percent of a physician's Medicare beneficiaries that are dually eligible for Medicaid (column (3)), and physician practice size (column (4)). Regressions in panel (b) are estimated separately for male and female physicians; physicians with missing gender information are excluded from this analysis. Panel (a) reproduces the overall results in Table 3 for comparison. Standard errors, clustered by application cohort, are shown in parentheses; p-values, calculated using a wild cluster bootstrap (Cameron et al., 2008) clustered by cohort, are shown in brackets. See Section II for details on the construction of $\bar{U}_y$. Outcome data are drawn from the public-use Medicare Part B files from 2013–2019 and the public-use Medicare Physician Compare files from 2014–2019. Significance stars are based on the wild cluster bootstrap p-values: $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

Table A7: Heterogeneity by physician sex: billing per beneficiary (restricted Medicare)

	Revenue per beneficiary (1)	Evaluation & management (2)	Elective procedures (3)	Non-elective procedures (4)
$\bar{U}_y$: male physicians	19.69***	1.96	2.35*	1.65
	(2.43)	(1.49)	(0.90)	(2.56)
	[<0.001]	[0.228]	[0.066]	[0.566]
$\bar{U}_y$: female physicians	6.70	0.15	1.56	–1.19
	(3.61)	(1.23)	(1.09)	(2.35)
	[0.126]	[0.980]	[0.214]	[0.630]
Observations	1,505,752	1,505,752	1,505,752	1,505,752
Mean outcome: males	320.7	104.8	47.5	22.6
Mean outcome: females	244.3	100.8	27.1	11.0
Percent of mean: males	6.1	1.9	5.0	7.3
Percent of mean: females	2.7	0.1	5.8	–10.9
P-value on difference	[0.014]	[0.022]	[0.652]	[0.476]

	Other procedures (5)	Imaging (6)	Tests (7)	Phys.-admin. drugs (8)
$\bar{U}_y$: male physicians	4.39***	–0.54	1.15	8.28***
	(1.33)	(0.87)	(0.68)	(1.67)
	[0.004]	[0.632]	[0.134]	[<0.001]
$\bar{U}_y$: female physicians	0.70	0.02	1.33	3.57
	(1.00)	(0.56)	(0.62)	(2.55)
	[0.510]	[0.982]	[0.128]	[0.212]
Observations	1,505,752	1,505,752	1,505,752	1,505,752
Mean outcome: males	57.5	33.0	25.0	26.4
Mean outcome: females	23.9	28.1	30.4	18.3
Percent of mean: males	7.6	–1.6	4.6	31.4
Percent of mean: females	2.9	0.09	4.4	19.5
P-value on difference	[0.050]	[0.538]	[0.894]	[0.142]
Specialty fixed effects	✓	✓	✓	✓
Experience fixed effects	✓	✓	✓	✓
Outcome year fixed effects	✓	✓	✓	✓

Notes: The above table shows output from estimation of equation (2) using data at the physician-year level. Outcomes are the average annual Medicare revenue per beneficiary overall (column (1)) and by service category (columns (2)–(8)). We divide all Medicare Part B physician billing into eight mutually exclusive and exhaustive service categories; all are shown except for the "other services" category. Regressions are estimated separately for male and female physicians; physicians with missing gender information are excluded from this analysis. Standard errors, clustered by application cohort, are shown in parentheses; p-values, calculated using a wild cluster bootstrap (Cameron et al., 2008) clustered by cohort, are shown in brackets. See Section II for details on the construction of $\bar{U}_y$. Outcome data are drawn from restricted Medicare files from 2001–2016. Significance stars are based on the wild cluster bootstrap p-values: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

Table A8: Heterogeneity by physician sex: practice ownership and affiliation (SK&A)

a. Overall	Hospital affiliated (%) (1)	Hospital or system owned (%) (2)	Unaffiliated practice (%) (3)	Other practice type (%) (4)
Unemployment rate $\bar{U}_y$	2.15***	-1.76***	-0.23	0.81***
	(0.22)	(0.24)	(0.18)	(0.10)
	[<0.001]	[<0.001]	[0.270]	[<0.001]
Observations	932,173	932,173	932,173	932,173
Mean dependent variable	49.7	43.5	6.7	7.7
Percent of mean	4.3	-4.0	-3.4	10.5
b. By physician sex	Hospital affiliated (%) (1)	Hospital or system owned (%) (2)	Unaffiliated practice (%) (3)	Other practice type (%) (4)
$\bar{U}_y$: male physicians	2.88***	-2.50***	-0.10	0.92***
	(0.42)	(0.47)	(0.29)	(0.14)
	[<0.001]	[<0.001]	[0.796]	[<0.001]
$\bar{U}_y$: female physicians	0.95*	-0.53	-0.51*	0.65*
	(0.53)	(0.40)	(0.24)	(0.26)
	[0.098]	[0.266]	[0.058]	[0.066]
Observations	932,085	932,085	932,085	932,085
Mean outcome: males	53.2	41.4	6.9	7.6
Mean outcome: females	44.6	46.6	6.4	7.7
Percent of mean: males	5.4	-6.0	-1.4	12.1
Percent of mean: females	2.1	-1.1	-8.0	8.4
P-value on difference	[0.068]	[0.014]	[0.384]	[0.496]
Specialty fixed effects	✓	✓	✓	✓
Experience fixed effects	✓	✓	✓	✓
Outcome year fixed effects	✓	✓	✓	✓

Notes: The above table shows output from estimation of equation (2) using data at the physician-year level. Outcomes are the percent of physicians in each practice category: independent but hospital affiliated (column (1)), hospital- or health-system-owned (column (2)), independent and unaffiliated (column (3)), and other (column (4)). Because physicians can report more than one practice location in a given year, the four shares sum to more than 100 percent. Regressions in panel (b) are estimated separately for male and female physicians; physicians with missing gender information are excluded from this analysis. Panel (a) reproduces the overall results in Table 6 for comparison. Standard errors, clustered by application cohort, are shown in parentheses; p-values, calculated using a wild cluster bootstrap (Cameron et al., 2008) clustered by cohort, are shown in brackets. See Section II for details on the construction of $\bar{U}_y$. Outcome data are drawn from the SK&A data from 2008–2016. Significance stars are based on the wild cluster bootstrap p-values: $^{*}\, p < 0.10$, $^{**}\, p < 0.05$, $^{***}\, p < 0.01$.

Table A9: Heterogeneity by physician sex: patient mix and patient health outcomes (restricted Medicare)

a. Overall	Any non-avoidable ED visit (1 year) (1)	Any avoidable ED visit (1 year) (2)	Predicted 1-year mortality (3)	Actual 1-year mortality (cond.) (4)
Unemployment rate $\bar{U}_y$	-0.27**	-0.15	-0.18**	-0.003
	(0.08)	(0.14)	(0.04)	(0.05)
	[0.016]	[0.388]	[0.014]	[0.956]
Observations	1,322,086	1,322,086	1,320,747	1,320,747
Mean dependent variable	15.1	25.5	10.9	12.3
Percent of mean	–1.8	–0.6	–1.6	–0.02
b. By physician sex	Any non-avoidable ED visit (1 year) (1)	Any avoidable ED visit (1 year) (2)	Predicted 1-year mortality (3)	Actual 1-year mortality (cond.) (4)
$\bar{U}_y$: male physicians	–0.21*	–0.10	–0.17**	–0.08
	(0.10)	(0.18)	(0.06)	(0.06)
	[0.090]	[0.652]	[0.042]	[0.218]
$\bar{U}_y$: female physicians	–0.34***	–0.13	–0.18***	0.07
	(0.07)	(0.13)	(0.04)	(0.06)
	[0.002]	[0.346]	[0.008]	[0.360]
Observations	1,321,948	1,321,948	1,320,609	1,320,609
Mean outcome: males	15.8	26.2	11.6	13.3
Mean outcome: females	13.9	24.3	9.8	10.6
Percent of mean: males	–1.4	–0.4	–1.4	–0.6
Percent of mean: females	–2.4	–0.5	–1.9	0.7
P-value on difference	[0.336]	[0.836]	[0.804]	[0.044]
Specialty fixed effects	✓	✓	✓	✓
Experience fixed effects	✓	✓	✓	✓
Outcome year fixed effects	✓	✓	✓	✓

Notes: The above table shows output from estimation of equation (2) using data at the physician-year level. Outcomes are the percent of a physician's beneficiaries with an emergent, non-avoidable emergency department (ED) visit in the following year (column (1)), the percent with an avoidable ED visit in the following year (column (2)), the average predicted one-year mortality rate (in percent) among the physician's beneficiaries (column (3)), and the average actual one-year mortality rate (in percent) conditional on predicted (column (4)). All four measures are visit-weighted, as described in Section IV.A. Regressions in panel (b) are estimated separately for male and female physicians; physicians with missing gender information are excluded from this analysis. Panel (a) reproduces the overall results in Table 5 for comparison. Standard errors, clustered by application cohort, are shown in parentheses; p-values, calculated using a wild cluster bootstrap (Cameron et al., 2008) clustered by cohort, are shown in brackets. See Section II for details on the construction of $\bar{U}_y$. Outcome data are drawn from restricted Medicare files from 2001–2016. Significance stars are based on the wild cluster bootstrap p-values: $^{*}\, p < 0.10$, $^{**}\, p < 0.05$, $^{***}\, p < 0.01$.

Table A10: Impacts of entry conditions: alternative specs. and samples (public Medicare)

a. Revenue per beneficiary	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	14.17***	14.41***	6.15**	14.98**	12.07***	6.49**
	(3.43)	(3.47)	(2.16)	(4.30)	(3.61)	(2.74)
	[0.006]	[0.004]	[0.016]	[0.010]	[0.002]	[0.048]
Observations	1,178,466	1,178,466	1,178,466	950,328	1,954,415	1,729,560
Mean dependent variable	301.4	301.4	269.2	309	299.6	306.6
Percent of mean	4.7	4.8	2.3	4.8	4.0	2.1
b. Average risk score (x100)	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	-0.75	—	0.63	-1.99***	-0.44	-0.84
	(0.59)	—	(0.57)	(0.47)	(0.47)	(0.56)
	[0.244]	—	[0.330]	[<0.001]	[0.378]	[0.196]
Observations	1,178,466	—	1,178,466	950,328	1,954,415	1,729,560
Mean dependent variable	174.1	—	174.6	171.6	172.1	167.7
Percent of mean	-0.4	—	0.4	-1.2	-0.3	-0.5
c. Percent dual eligible	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	-0.34***	-0.34***	-0.31	-0.37***	-0.32***	-0.21**
	(0.07)	(0.07)	(0.17)	(0.09)	(0.07)	(0.08)
	[0.002]	[0.002]	[0.144]	[<0.001]	[0.002]	[0.032]
Observations	1,178,466	1,178,466	1,178,466	950,328	1,954,415	1,729,560
Mean dependent variable	24.1	24.1	23.7	23.5	22.1	23.2
Percent of mean	-1.4	-1.4	-1.3	-1.6	-1.4	-0.9
d. Practice size	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	-13.62**	-11.84**	-12.75	-14.73***	-15.63***	-14.60**
	(4.67)	(4.34)	(6.79)	(5.01)	(5.14)	(6.21)
	[0.010]	[0.014]	[0.116]	[0.006]	[<0.001]	[0.024]
Observations	927,400	927,400	927,400	751,812	1,638,337	1,325,054
Mean dependent variable	518.9	518.9	343.8	504.4	618.9	469.1
Percent of mean	-2.6	-2.3	-3.7	-2.9	-2.5	-3.1
Outcome years	2013–2019	2013–2019	2013–2019	2013–2019	2013–2023	2013–2019
Experience restriction	≥ 7	≥ 7	≥ 7	≥ 10	≥ 7	7–30
Application years	1989–2007	1989–2007	1989–2007	1989–2004	1989–2007	1978–2007
Control for avg. risk score		✓				
Weight by beneficiaries			✓			

Notes: The above table shows output from estimation of analogs of equation (2) using data at the physician-year level. Outcomes are the average annual Medicare revenue per beneficiary (panel (a)), average risk scores among the physician's Medicare beneficiaries times 100 (panel (b)), the percent of a physician's Medicare beneficiaries that are dually eligible for Medicaid (panel (c)), and physician practice size (panel (d)). Standard errors, clustered by application cohort, are shown in parentheses; p-values, calculated using a wild cluster bootstrap (Cameron et al., 2008) clustered by cohort, are shown in brackets. Results from the baseline specification—in which outcome data cover 2013–2019, physician-year observations occur at least seven years after medical school graduation, and all observations receive equal weight—are shown in column (1). Column (2) additionally controls for the average patient risk score at the physician-year level, and column (3) weights observations by the total number of beneficiaries. Column (4) restricts the sample to physician-year observations at least ten years after medical school graduation, while column (5) extends the outcome data to include years 2020–2023. Column (6) extends the sample to include application cohorts predating the start of the AAMC data in 1989, restricting to physician-year observations within 30 years of medical school graduation. Wild cluster bootstrap p-values: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

Table A11: Impacts of entry conditions: alternative unemployment (public Medicare)

a. Revenue per beneficiary	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	14.17***	7.08***	13.96***	7.27***	12.70***	13.88**
	(3.43)	(1.43)	(3.42)	(1.95)	(2.72)	(4.37)
	[0.006]	[0.002]	[0.004]	[<0.001]	[<0.001]	[0.040]
Observations	1,178,466	1,178,466	1,178,466	1,178,466	1,178,466	1,178,466
Mean dependent variable	301.4	301.4	301.4	301.4	301.4	301.4
Percent of mean	4.7	2.3	4.6	2.4	4.2	4.6
b. Average risk score (x100)	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	-0.75	-0.18	-0.69	-0.05	-0.49	-1.08
	(0.59)	(0.24)	(0.57)	(0.28)	(0.45)	(0.76)
	[0.244]	[0.494]	[0.284]	[0.860]	[0.354]	[0.196]
Observations	1,178,466	1,178,466	1,178,466	1,178,466	1,178,466	1,178,466
Mean dependent variable	174.1	174.1	174.1	174.1	174.1	174.1
Percent of mean	-0.4	-0.1	-0.4	-0.0	-0.3	-0.6
c. Percent dual eligible	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	-0.34***	-0.15***	-0.34***	-0.13*	-0.26***	-0.43***
	(0.07)	(0.03)	(0.07)	(0.05)	(0.07)	(0.09)
	[0.002]	[0.002]	[0.002]	[0.078]	[0.002]	[<0.001]
Observations	1,178,466	1,178,466	1,178,466	1,178,466	1,178,466	1,178,466
Mean dependent variable	24.1	24.1	24.1	24.1	24.1	24.1
Percent of mean	-1.4	-0.6	-1.4	-0.5	-1.1	-1.8
d. Practice size	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	-13.62**	-7.85***	-14.08***	-14.54***	-15.88***	-10.70**
	(4.67)	(2.54)	(4.77)	(3.62)	(4.79)	(4.67)
	[0.010]	[0.006]	[0.006]	[0.002]	[0.006]	[0.042]
Observations	927,400	927,400	927,400	927,400	927,400	927,400
Mean dependent variable	518.9	518.9	518.9	518.9	518.9	518.9
Percent of mean	-2.6	-1.5	-2.7	-2.8	-3.1	-2.1
Unemployment measure	Med-specific	Overall	Exclude physicians	Med-specific	Med-specific	Med-specific
Number of included lags	0–3	0–3	0–3	0	0–2	0–4

Notes: The above table shows output from estimation of analogs of equation (2) using physician-year data from 2013–2019. Outcomes are the average annual Medicare revenue per beneficiary (panel (a)), average risk scores among the physician's Medicare beneficiaries times 100 (panel (b)), the percent of a physician's Medicare beneficiaries that are dually eligible for Medicaid (panel (c)), and physician practice size (panel (d)). Standard errors, clustered by application cohort, are shown in parentheses; p-values, calculated using a wild cluster bootstrap (Cameron et al., 2008) clustered by cohort, are shown in brackets. Results from the baseline specification—which uses the medical-school-applicant-relevant business cycle averaged over the year of application to medical school and the three preceding years (years 0–3)—are shown in column (1). Column (2) instead uses the overall business cycle, while column (3) uses the medical-school-applicant-relevant business cycle holding physician employment fixed; both measures are averaged over year 0–3. Columns (4)–(6) use the baseline medical-school-applicant-relevant unemployment rate but vary the averaging window. Column (4) considers only the year of application to medical school (year 0), column (5) averages over the year of application and the two preceding years (years 0–2), and column (6) averages over the year of application and the four preceding years (years 0–4). Wild cluster bootstrap p-values: $^{*}\, p < 0.10$, $^{**}\, p < 0.05$, $^{***}\, p < 0.01$.

Table A12: Impacts of entry conditions: alternative unemployment measures (restricted Medicare, billing [1/2])

a. Revenue per beneficiary	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	13.72***	6.56***	13.77***	8.29***	11.97***	14.48***
	(2.38)	(1.16)	(2.32)	(1.86)	(2.31)	(3.13)
	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[0.004]
Mean dependent variable	291.7	291.7	291.7	291.7	291.7	291.7
Percent of mean	4.7	2.2	4.7	2.8	4.1	5.0
b. Evaluation & management	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	0.83	0.76	0.87	2.75*	1.66	–0.33
	(1.36)	(0.68)	(1.34)	(1.04)	(1.32)	(1.39)
	[0.606]	[0.294]	[0.582]	[0.068]	[0.288]	[0.774]
Mean dependent variable	103.3	103.3	103.3	103.3	103.3	103.3
Percent of mean	0.8	0.7	0.9	2.7	1.6	–0.3
c. Elective procedures	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	2.05***	0.88**	2.05***	1.52***	1.86***	2.55**
	(0.64)	(0.31)	(0.64)	(0.28)	(0.47)	(0.70)
	[0.008]	[0.012]	[0.008]	[0.002]	[0.006]	[0.012]
Mean dependent variable	39.7	39.7	39.7	39.7	39.7	39.7
Percent of mean	5.2	2.2	5.2	3.8	4.7	6.4
d. Non-elective procedures	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	0.25	0.06	0.33	–1.15	–0.44	0.23
	(1.89)	(0.95)	(1.88)	(0.99)	(1.65)	(2.03)
	[0.928]	[0.968]	[0.906]	[0.350]	[0.794]	[0.954]
Mean dependent variable	18.2	18.2	18.2	18.2	18.2	18.2
Percent of mean	1.4	0.4	1.8	–6.4	–2.4	1.3
Observations	1,505,912	1,505,912	1,505,912	1,505,912	1,505,912	1,505,912
Unemployment measure	Med-specific	Overall	Exclude physicians	Med-specific	Med-specific	Med-specific
Number of included lags	0–3	0–3	0–3	0	0–2	0–4

Notes: The above table shows output from estimation of equation (2) using data at the physician-year level from 2001–2016. Outcomes are the average annual Medicare revenue per beneficiary overall (panel (a)) and by service category (panels (b)–(d)). We divide all Medicare Part B physician billing into eight mutually exclusive and exhaustive service categories; all are shown in this table and Table A13 except for the "other services" category. Standard errors, clustered by application cohort, are shown in parentheses; p-values, calculated using a wild cluster bootstrap (Cameron et al., 2008) clustered by cohort, are shown in brackets. Results from the baseline specification—which uses the medical-school-applicant-relevant business cycle averaged over the year of application to medical school and the three preceding years (years 0–3)—are shown in column (1). Column (2) instead uses the overall business cycle, while column (3) uses the medical-school-applicant-relevant business cycle holding physician employment fixed; both measures are averaged over year 0–3. Columns (4)–(6) use the baseline medical-school-applicant-relevant unemployment rate but vary the averaging window. Column (4) considers only the year of application to medical school (year 0), column (5) averages over the year of application and the two preceding years (years 0–2), and column (6) averages over the year of application and the four preceding years (years 0–4). Wild cluster bootstrap p-values: $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

Table A13: Impacts of entry conditions: alternative unemployment measures (restricted Medicare, billing [2/2])

e. Other procedures	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	3.18***	1.00**	3.05***	–1.06	1.60**	4.82***
	(0.94)	(0.44)	(0.93)	(0.77)	(0.79)	(1.26)
	[0.004]	[0.046]	[0.004]	[0.214]	[0.044]	[<0.001]
Mean dependent variable	44.7	44.7	44.7	44.7	44.7	44.7
Percent of mean	7.1	2.2	6.8	–2.4	3.6	10.8
f. Imaging	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	–0.34	–0.10	–0.27	0.05	–0.19	–0.75
	(0.69)	(0.34)	(0.69)	(0.45)	(0.63)	(0.77)
	[0.716]	[0.852]	[0.770]	[0.926]	[0.872]	[0.444]
Mean dependent variable	31.2	31.2	31.2	31.2	31.2	31.2
Percent of mean	–1.1	–0.3	–0.9	0.2	–0.6	–2.4
g. Tests	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	1.05*	0.57*	1.05*	1.13***	1.14**	0.92
	(0.51)	(0.24)	(0.51)	(0.29)	(0.41)	(0.54)
	[0.094]	[0.054]	[0.096]	[0.002]	[0.048]	[0.164]
Mean dependent variable	27.1	27.1	27.1	27.1	27.1	27.1
Percent of mean	3.9	2.1	3.9	4.2	4.2	3.4
h. Physician-admin. drugs	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	6.26***	3.20***	6.25***	4.95***	6.02***	6.40**
	(1.58)	(0.63)	(1.57)	(0.79)	(1.18)	(1.98)
	[0.004]	[0.002]	[0.002]	[<0.001]	[<0.001]	[0.014]
Mean dependent variable	23.3	23.3	23.3	23.3	23.3	23.3
Percent of mean	26.8	13.7	26.8	21.2	25.8	27.5
Observations	1,505,912	1,505,912	1,505,912	1,505,912	1,505,912	1,505,912
Unemployment measure	Med-specific	Overall	Exclude physicians	Med-specific	Med-specific	Med-specific
Number of included lags	0–3	0–3	0–3	0	0–2	0–4

Notes: The above table shows output from estimation of equation (2) using data at the physician-year level from 2001–2016. Outcomes are the average annual Medicare revenue per beneficiary by service category. We divide all Medicare Part B physician billing into eight mutually exclusive and exhaustive service categories; all are shown in this table and Table A12 except for the "other services" category. Standard errors, clustered by application cohort, are shown in parentheses; p-values, calculated using a wild cluster bootstrap (Cameron et al., 2008) clustered by cohort, are shown in brackets. Results from the baseline specification—which uses the medical-school-applicant-relevant business cycle averaged over the year of application to medical school and the three preceding years (years 0–3)—are shown in column (1). Column (2) instead uses the overall business cycle, while column (3) uses the medical-school-applicant-relevant business cycle holding physician employment fixed; both measures are averaged over year 0–3. Columns (4)–(6) use the baseline medical-school-applicant-relevant unemployment rate but vary the averaging window. Column (4) considers only the year of application to medical school (year 0), column (5) averages over the year of application and the two preceding years (years 0–2), and column (6) averages over the year of application and the four preceding years (years 0–4). Wild cluster bootstrap p-values: $^{*}\, p < 0.10$, $^{**}\, p < 0.05$, $^{***}\, p < 0.01$.

Table A14: Impacts of entry conditions: alternative unemployment measures (restricted Medicare, patient health)

a. Any non-avoidable ED	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	–0.27**	–0.10*	–0.26**	0.01	–0.15*	–0.40***
	(0.08)	(0.04)	(0.08)	(0.04)	(0.07)	(0.08)
	[0.016]	[0.062]	[0.018]	[0.740]	[0.056]	[0.006]
Observations	1,322,086	1,322,086	1,322,086	1,322,086	1,322,086	1,322,086
Mean dependent variable	15.1	15.1	15.1	15.1	15.1	15.1
Percent of mean	–1.8	–0.6	–1.7	0.08	–1.0	–2.6
b. Any avoidable ED	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	–0.15	–0.04	–0.15	0.11	–0.03	–0.27
	(0.14)	(0.07)	(0.14)	(0.08)	(0.11)	(0.16)
	[0.388]	[0.592]	[0.388]	[0.282]	[0.782]	[0.170]
Observations	1,322,086	1,322,086	1,322,086	1,322,086	1,322,086	1,322,086
Mean dependent variable	25.5	25.5	25.5	25.5	25.5	25.5
Percent of mean	–0.6	–0.2	–0.6	0.4	–0.1	–1.1
c. Predicted mortality	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	–0.18**	–0.07*	–0.18**	0.00	–0.11*	–0.25***
	(0.04)	(0.02)	(0.04)	(0.04)	(0.04)	(0.04)
	[0.014]	[0.056]	[0.016]	[0.938]	[0.052]	[0.006]
Observations	1,320,747	1,320,747	1,320,747	1,320,747	1,320,747	1,320,747
Mean dependent variable	10.9	10.9	10.9	10.9	10.9	10.9
Percent of mean	–1.6	–0.7	–1.6	–0.01	–1.0	–2.2
d. Actual mortality (cond.)	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	–0.003	0.01	–0.002	0.06	0.03	–0.05
	(0.05)	(0.02)	(0.05)	(0.03)	(0.04)	(0.05)
	[0.956]	[0.546]	[0.974]	[0.100]	[0.474]	[0.404]
Observations	1,320,747	1,320,747	1,320,747	1,320,747	1,320,747	1,320,747
Mean dependent variable	12.3	12.3	12.3	12.3	12.3	12.3
Percent of mean	–0.02	0.1	–0.02	0.5	0.3	–0.4
Unemployment measure	Med-specific	Overall	Exclude physicians	Med-specific	Med-specific	Med-specific
Number of included lags	0–3	0–3	0–3	0	0–2	0–4

Notes: The above table shows output from estimation of equation (2) using data at the physician-year level from 2001–2016. Outcomes are the percent of a physician's beneficiaries with an emergent, non-avoidable emergency department (ED) visit in the following year (panel (a)), the percent with an avoidable ED visit in the following year (panel (b)), the average predicted one-year mortality rate (in percent) among the physician's beneficiaries (panel (c)), and the average actual one-year mortality rate (in percent) conditional on predicted (panel (d)). Standard errors, clustered by application cohort, are shown in parentheses; p-values, calculated using a wild cluster bootstrap (Cameron et al., 2008) clustered by cohort, are shown in brackets. Results from the baseline specification—which uses the medical-school-applicant-relevant business cycle averaged over the year of application to medical school and the three preceding years (years 0–3)—are shown in column (1). Column (2) instead uses the overall business cycle, while column (3) uses the medical-school-applicant-relevant business cycle holding physician employment fixed; both measures are averaged over year 0–3. Columns (4)–(6) use the baseline medical-school-applicant-relevant unemployment rate but vary the averaging window. Column (4) considers only the year of application to medical school (year 0), column (5) averages over the year of application and the two preceding years (years 0–2), and column (6) averages over the year of application and the four preceding years (years 0–4). Wild cluster bootstrap p-values: $^{*}\, p < 0.10$, $^{**}\, p < 0.05$, $^{***}\, p < 0.01$.

Table A15: Impacts of entry conditions: alternative unemployment measures (SK&A)

a. Hospital affiliated	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	2.15***	0.98***	2.16***	1.16***	1.84***	2.43***
	(0.22)	(0.10)	(0.22)	(0.25)	(0.21)	(0.38)
	[<0.001]	[<0.001]	[<0.001]	[0.004]	[<0.001]	[<0.001]
Mean dependent variable	49.7	49.7	49.7	49.7	49.7	49.7
Percent of mean	4.3	2.0	4.3	2.3	3.7	4.9
b. Hospital owned	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	-1.76***	-0.87***	-1.79***	-1.31***	-1.71***	-1.79***
	(0.24)	(0.13)	(0.25)	(0.22)	(0.24)	(0.35)
	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]
Mean dependent variable	43.5	43.5	43.5	43.5	43.5	43.5
Percent of mean	-4.0	-2.0	-4.1	-3.0	-3.9	-4.1
c. Unaffiliated	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	-0.23	-0.07	-0.21	0.26	-0.06	-0.35
	(0.18)	(0.09)	(0.18)	(0.16)	(0.19)	(0.21)
	[0.270]	[0.496]	[0.294]	[0.258]	[0.790]	[0.132]
Mean dependent variable	6.7	6.7	6.7	6.7	6.7	6.7
Percent of mean	-3.4	-1.0	-3.1	3.9	-0.9	-5.2
d. Other practice type	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	0.81***	0.36***	0.79***	0.38*	0.68***	0.96***
	(0.10)	(0.05)	(0.10)	(0.12)	(0.10)	(0.15)
	[<0.001]	[0.002]	[<0.001]	[0.070]	[<0.001]	[<0.001]
Mean dependent variable	7.7	7.7	7.7	7.7	7.7	7.7
Percent of mean	10.5	4.7	10.3	4.9	8.8	12.5
Observations	932,173	932,173	932,173	932,173	932,173	932,173
Unemployment measure	Med-specific	Overall	Exclude physicians	Med-specific	Med-specific	Med-specific
Number of included lags	0–3	0–3	0–3	0	0–2	0–4

Notes: The above table shows output from estimation of analogs of equation (2) using physician-year data from 2008–2016. Outcomes are the percent of physicians in each practice category: independent but hospital affiliated (panel (a)), hospital- or health-system-owned (panel (b)), independent and unaffiliated (panel (c)), and other (panel (d)). Standard errors, clustered by application cohort, are shown in parentheses; p-values, calculated using a wild cluster bootstrap (Cameron et al., 2008) clustered by cohort, are shown in brackets. Results from the baseline specification—which uses the medical-school-applicant-relevant business cycle averaged over the year of application to medical school and the three preceding years (years 0–3)—are shown in column (1). Column (2) instead uses the overall business cycle, while column (3) uses the medical-school-applicant-relevant business cycle holding physician employment fixed; both measures are averaged over year 0–3. Columns (4)–(6) use the baseline medical-school-applicant-relevant unemployment rate but vary the averaging window. Column (4) considers only the year of application to medical school (year 0), column (5) averages over the year of application and the two preceding years (years 0–2), and column (6) averages over the year of application and the four preceding years (years 0–4). Wild cluster bootstrap p-values: $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

Table A16: Impacts of entry conditions: alternative support and balance (public Medicare)

a. Revenue per beneficiary	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	14.17***	14.62***	15.45***	12.64***	14.63***	14.25***
	(3.43)	(3.56)	(4.43)	(3.26)	(5.06)	(3.45)
	[0.006]	[0.006]	[0.008]	[0.004]	[0.008]	[0.006]
Observations	1,178,466	1,050,659	947,635	965,997	1,031,949	1,178,466
Mean dependent variable	301.4	303.3	308.4	298.5	301.4	301.4
Percent of mean	4.7	4.8	5.0	4.2	4.9	4.7
b. Average risk score (x100)	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	-0.75	-1.01	-2.52***	-0.55	-1.13**	-0.78
	(0.59)	(0.74)	(0.41)	(0.57)	(0.42)	(0.59)
	[0.244]	[0.268]	[<0.001]	[0.410]	[0.032]	[0.234]
Observations	1,178,466	1,050,659	947,635	965,997	1,031,949	1,178,466
Mean dependent variable	174.1	173.5	170.6	176.4	174.0	174.1
Percent of mean	-0.4	-0.6	-1.5	-0.3	-0.6	-0.4
c. Percent dual eligible	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	-0.34***	-0.33***	-0.39***	-0.36***	-0.37***	-0.34***
	(0.07)	(0.08)	(0.10)	(0.08)	(0.10)	(0.07)
	[0.002]	[<0.001]	[<0.001]	[<0.001]	[0.008]	[0.002]
Observations	1,178,466	1,050,659	947,635	965,997	1,031,949	1,178,466
Mean dependent variable	24.1	24.2	23.7	24.7	24.1	24.1
Percent of mean	-1.4	-1.4	-1.6	-1.5	-1.5	-1.4
d. Practice size	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	-13.62**	-11.98**	-12.55**	-12.67***	-19.35***	-13.66**
	(4.67)	(5.16)	(5.53)	(4.18)	(5.60)	(4.69)
	[0.010]	[0.022]	[0.032]	[0.006]	[0.002]	[0.010]
Observations	927,400	815,108	720,573	742,664	814,826	927,400
Mean dependent variable	518.9	505.4	487.0	530.9	519.1	518.9
Percent of mean	-2.6	-2.4	-2.6	-2.4	-3.7	-2.6
Outcome years	2013–2019	2013–2019	2013–2019	2013–2019	2013–2019	2013–2019
Experience restriction	≥ 7	7–19	≥ 7	7–19	≥ 7	≥ 7
Application years	1989–2007	1992–2004	1989–2001	1989–2007	Not 1994, 2001	1989–2007
Inverse weighting						✓

Notes: The above table shows output from estimation of equation (2) using physician-year data. Outcomes are the average annual Medicare revenue per beneficiary (panel (a)), average risk scores among the physician's Medicare beneficiaries times 100 (panel (b)), the percent of a physician's Medicare beneficiaries that are dually eligible for Medicaid (panel (c)), and physician practice size (panel (d)). Standard errors, clustered by application cohort, are shown in parentheses; p-values, calculated using a wild cluster bootstrap (Cameron et al., 2008) clustered by cohort, are shown in brackets. Results from the baseline sample, which limits the data to physicians who applied to medical school in or after 1989 and had at least seven years of experience following medical school in a given outcome year (see Figure A15(a)), are shown in column (1). Starting from this baseline sample, column (2) additionally requires that each cohort, outcome year, and experience level be observed with at least four values along the other two dimensions, as shown in Figure A15(b); column (3) requires balance across outcome years and application cohorts, as shown in Figure A15(c); and column (4) requires balance across outcome years and experience profiles, as shown in Figure A15(d). Column (5) drops the application cohorts with the highest and lowest values of $\bar{U}y$ (1994 and 2001, respectively), while column (6) weights observations by the inverse of the share of physician-year observations accounted for by each experience–outcome year cell. Wild cluster bootstrap p-values: $^{*}\, p < 0.10$, $^{**}\, p < 0.05$, $^{***}\, p < 0.01$.

Table A17: Impacts of entry conditions: alternative support and balance restrictions (restricted Medicare, billing [1/2])

a. Revenue per beneficiary	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	13.72***	14.33***	12.27**	13.11***	11.67**	13.78***
	(2.38)	(2.39)	(4.23)	(3.02)	(2.51)	(2.37)
	[<0.001]	[<0.001]	[0.034]	[0.002]	[0.026]	[<0.001]
Mean dependent variable	291.7	294.5	312.5	299.2	280.0	291.7
Percent of mean	4.7	4.9	3.9	4.4	4.2	4.7
b. Evaluation & management	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	0.83	1.02	0.21	–0.68	2.85*	0.94
	(1.36)	(1.39)	(1.57)	(0.85)	(1.75)	(1.38)
	[0.606]	[0.534]	[0.934]	[0.446]	[0.090]	[0.564]
Mean dependent variable	103.3	103.7	107.9	104.9	99.7	103.3
Percent of mean	0.8	1.0	0.2	–0.7	2.9	0.9
c. Elective procedures	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	2.05***	2.17***	0.66	1.58**	2.74	2.11***
	(0.64)	(0.68)	(1.46)	(0.62)	(1.59)	(0.64)
	[0.008]	[0.006]	[0.784]	[0.014]	[0.460]	[0.008]
Mean dependent variable	39.7	40.3	42.5	40.8	38.5	39.7
Percent of mean	5.2	5.4	1.6	3.9	7.1	5.3
d. Non-elective procedures	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	0.25	0.50	1.49	2.86	0.40	0.18
	(1.89)	(1.80)	(3.08)	(1.57)	(2.41)	(1.89)
	[0.928]	[0.838]	[0.716]	[0.158]	[0.904]	[0.954]
Mean dependent variable	18.2	18.4	18.5	18.5	18.7	18.2
Percent of mean	1.4	2.7	8.0	15.4	2.2	1.0
Observations	1,505,912	1,314,820	798,288	800,693	750,655	1,505,912
Outcome years	2001–2016	2004–2016	2010–2016	2010–2016	2001–2016	2001–2016
Experience restriction	7–22	7–19	7–22	7–16	7–13	7–22
Application years	1989–2004	1989–2001	1989–1998	1989–2004	1989–1998	1989–2004
Inverse weighting						✓

Notes: The above table shows output from estimation of equation (2) using data at the physician-year level. Outcomes are the average annual Medicare revenue per beneficiary overall (panel (a)) and by service category (panels (b)–(d)). We divide all Medicare Part B physician billing into eight mutually exclusive and exhaustive service categories; all are shown in this table and Table A18 except for the "other services" category. Standard errors, clustered by application cohort, are shown in parentheses; p-values, calculated using a wild cluster bootstrap (Cameron et al., 2008) clustered by cohort, are shown in brackets. Results from the baseline sample, which limits the data to physicians who applied to medical school in or after 1989 and had at least seven years of experience following medical school in a given outcome year (see Figure A16(a)), are shown in column (1). Starting from this baseline sample, column (2) additionally requires that each cohort, outcome year, and experience level be observed with at least four values along the other two dimensions, as shown in Figure A16(b); column (3) requires balance across outcome years and application cohorts, as shown in Figure A16(c); column (4) requires balance across outcome years and experience profiles, as shown in Figure A16(d); and column (5) requires balance across experience profiles and application cohorts, as shown in Figure A16(e). Column (6) weights observations by the inverse of the share of physician-year observations accounted for by each experience–outcome year cell. Wild cluster bootstrap p-values: $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

Table A18: Impacts of entry conditions: alternative support and balance restrictions (restricted Medicare, billing [2/2])

e. Other procedures	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	3.18***	3.15**	0.38	3.03**	–0.46	3.19***
	(0.94)	(1.08)	(1.64)	(1.17)	(1.14)	(0.94)
	[0.004]	[0.038]	[0.884]	[0.034]	[0.678]	[0.004]
Mean dependent variable	44.7	45.0	49.1	48.9	41.5	44.7
Percent of mean	7.1	7.0	0.8	6.2	–1.1	7.1
f. Imaging	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	–0.34	–0.26	–0.54	0.82	0.01	–0.39
	(0.69)	(0.57)	(0.93)	(0.64)	(0.71)	(0.69)
	[0.716]	[0.734]	[0.632]	[0.284]	[0.990]	[0.686]
Mean dependent variable	31.2	31.8	31.9	29.8	32.1	31.2
Percent of mean	–1.1	–0.8	–1.7	2.7	0.02	–1.3
g. Tests	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	1.05*	1.07*	0.85	0.75	1.36	1.07*
	(0.51)	(0.53)	(0.82)	(0.59)	(0.59)	(0.50)
	[0.094]	[0.084]	[0.424]	[0.272]	[0.126]	[0.092]
Mean dependent variable	27.1	27.5	30.6	27.2	24.8	27.1
Percent of mean	3.9	3.9	2.8	2.8	5.5	4.0
h. Physician-admin. drugs	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	6.26***	6.23***	8.49***	4.26*	4.95	6.24***
	(1.58)	(1.60)	(1.65)	(1.98)	(2.59)	(1.58)
	[0.004]	[0.002]	[<0.001]	[0.064]	[0.196]	[0.002]
Mean dependent variable	23.3	23.8	27.1	25.2	21.3	23.3
Percent of mean	26.8	26.2	31.4	16.9	23.2	26.8
Observations	1,505,912	1,314,820	798,288	800,693	750,655	1,505,912
Outcome years	2001–2016	2004–2016	2010–2016	2010–2016	2001–2016	2001–2016
Experience restriction	7–22	7–19	7–22	7–16	7–13	7–22
Application years	1989–2004	1989–2001	1989–1998	1989–2004	1989–1998	1989–2004
Inverse weighting						✓

Notes: The above table shows output from estimation of equation (2) using data at the physician-year level. Outcomes are the average annual Medicare revenue per beneficiary by service category. We divide all Medicare Part B physician billing into eight mutually exclusive and exhaustive service categories; all are shown in this table and Table A17 except for the "other services" category. Standard errors, clustered by application cohort, are shown in parentheses; p-values, calculated using a wild cluster bootstrap (Cameron et al., 2008) clustered by cohort, are shown in brackets. Results from the baseline sample, which limits the data to physicians who applied to medical school in or after 1989 and had at least seven years of experience following medical school in a given outcome year (see Figure A16(a)), are shown in column (1). Starting from this baseline sample, column (2) additionally requires that each cohort, outcome year, and experience level be observed with at least four values along the other two dimensions, as shown in Figure A16(b); column (3) requires balance across outcome years and application cohorts, as shown in Figure A16(c); column (4) requires balance across outcome years and experience profiles, as shown in Figure A16(d); and column (5) requires balance across experience profiles and application cohorts, as shown in Figure A16(e). Column (6) weights observations by the inverse of the share of physician-year observations accounted for by each experience–outcome year cell. Wild cluster bootstrap p-values: $^{*}\, p < 0.10$, $^{**}\, p < 0.05$, $^{***}\, p < 0.01$.

Table A19: Impacts of entry conditions: alternative support and balance restrictions (restricted Medicare, patient health)

a. Any non-avoidable ED	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	–0.27**	–0.23**	–0.11	–0.26*	–0.06	–0.27***
	(0.08)	(0.08)	(0.17)	(0.10)	(0.08)	(0.08)
	[0.016]	[0.048]	[0.638]	[0.070]	[0.644]	[0.008]
Observations	1,322,086	1,131,755	684,952	684,845	671,505	1,322,086
Mean dependent variable	15.1	15.0	15.9	16.5	14.1	15.1
Percent of mean	–1.8	–1.6	–0.7	–1.6	–0.4	–1.8
b. Any avoidable ED	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	–0.15	–0.16	–0.05	–0.06	0.02	–0.15
	(0.14)	(0.16)	(0.22)	(0.15)	(0.21)	(0.14)
	[0.388]	[0.372]	[0.888]	[0.800]	[0.954]	[0.386]
Observations	1,322,086	1,131,755	684,952	684,845	671,505	1,322,086
Mean dependent variable	25.5	25.5	26.0	26.9	24.9	25.5
Percent of mean	–0.6	–0.6	–0.2	–0.2	0.07	–0.6
c. Predicted mortality	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	–0.18**	–0.16**	–0.17*	–0.17**	–0.11	–0.18**
	(0.04)	(0.04)	(0.06)	(0.04)	(0.08)	(0.04)
	[0.014]	[0.016]	[0.088]	[0.016]	[0.254]	[0.012]
Observations	1,320,747	1,130,441	684,951	684,843	670,272	1,320,747
Mean dependent variable	10.9	10.9	11.0	11.4	10.8	10.9
Percent of mean	–1.6	–1.5	–1.6	–1.5	–1.0	–1.6
d. Actual mortality (cond.)	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	–0.003	0.00	0.12	0.02	0.06	–0.01
	(0.05)	(0.06)	(0.10)	(0.07)	(0.05)	(0.05)
	[0.956]	[0.996]	[0.304]	[0.834]	[0.300]	[0.882]
Observations	1,320,747	1,130,441	684,951	684,843	670,272	1,320,747
Mean dependent variable	12.3	12.2	12.1	12.8	12.3	12.3
Percent of mean	–0.02	0.00	1.0	0.2	0.5	–0.06
Outcome years	2001–2016	2004–2016	2010–2016	2010–2016	2001–2016	2001–2016
Experience restriction	7–21	7–18	7–21	7–16	7–13	7–21
Application years	1989–2003	1989–2000	1989–1998	1989–2003	1989–1997	1989–2003
Inverse weighting						✓

Notes: The above table shows output from estimation of equation (2) using data at the physician-year level. Outcomes are the same as in Table A14. Standard errors, clustered by application cohort, are shown in parentheses; p-values, calculated using a wild cluster bootstrap (Cameron et al., 2008) clustered by cohort, are shown in brackets. Results from the baseline sample, which limits the data to physicians who applied to medical school in or after 1989 and had at least seven years of experience following medical school in a given outcome year (see Figure A16(a)), are shown in column (1). Starting from this baseline sample, column (2) additionally requires that each cohort, outcome year, and experience level be observed with at least four values along the other two dimensions, as shown in Figure A16(b); column (3) requires balance across outcome years and application cohorts, as shown in Figure A16(c); column (4) requires balance across outcome years and experience profiles, as shown in Figure A16(d); and column (5) requires balance across experience profiles and application cohorts, as shown in Figure A16(e). Column (6) weights observations by the inverse of the share of physician-year observations accounted for by each experience–outcome year cell. Wild cluster bootstrap p-values: $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

Table A20: Impacts of entry conditions: alternative support and balance restrictions (SK&A)

a. Hospital affiliated	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	2.15***	2.20***	2.32*	2.08***	2.18***	2.17***
	(0.22)	(0.23)	(0.67)	(0.24)	(0.25)	(0.23)
	[<0.001]	[<0.001]	[0.098]	[<0.001]	[<0.001]	[<0.001]
Mean dependent variable	49.7	50.5	49.8	48.2	49.9	49.7
Percent of mean	4.3	4.4	4.7	4.3	4.4	4.4
b. Hospital owned	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	-1.76***	-1.76***	-2.30*	-1.72***	-1.88***	-1.71***
	(0.24)	(0.26)	(0.70)	(0.29)	(0.33)	(0.25)
	[<0.001]	[<0.001]	[0.098]	[<0.001]	[<0.001]	[<0.001]
Mean dependent variable	43.5	42.7	43.6	45.3	43.3	43.5
Percent of mean	-4.0	-4.1	-5.3	-3.8	-4.3	-3.9
c. Unaffiliated	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	-0.23	-0.22	-0.24	-0.24	-0.13	-0.28
	(0.18)	(0.18)	(0.29)	(0.18)	(0.17)	(0.18)
	[0.270]	[0.302]	[0.712]	[0.260]	[0.454]	[0.206]
Mean dependent variable	6.7	6.7	6.4	6.2	6.7	6.7
Percent of mean	-3.4	-3.3	-3.8	-3.9	-1.9	-4.2
d. Other practice type	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate $\bar{U}_y$	0.81***	0.79***	0.86**	0.73***	0.81***	0.77***
	(0.10)	(0.10)	(0.23)	(0.15)	(0.14)	(0.11)
	[<0.001]	[<0.001]	[0.034]	[0.002]	[0.004]	[<0.001]
Mean dependent variable	7.7	7.7	8.1	7.6	7.7	7.7
Percent of mean	10.5	10.3	10.6	9.6	10.5	10.0
Observations	932,173	833,758	677,105	604,249	823,437	932,173
Outcome years	2008–2016	2008–2016	2010–2016	2010–2016	2008–2016	2008–2016
Experience restriction	≥ 7	7–19	≥ 7	7–16	≥ 7	≥ 7
Application years	1989–2004	1989–2001	1989–1998	1989–2004	Not 1994, 2001	1989–2004
Inverse weighting						✓

Notes: The above table shows output from estimation of equation (2) using physician-year data. Outcomes are the percent of physicians in each practice category: independent but hospital affiliated (panel (a)), hospital- or health-system-owned (panel (b)), independent and unaffiliated (panel (c)), and other (panel (d)). Standard errors, clustered by application cohort, are shown in parentheses; p-values, calculated using a wild cluster bootstrap (Cameron et al., 2008) clustered by cohort, are shown in brackets. Results from the baseline sample, which limits the data to physicians who applied to medical school in or after 1989 and had at least seven years of experience following medical school in a given outcome year (see Figure A17(a)), are shown in column (1). Starting from this baseline sample, column (2) additionally requires that each cohort, outcome year, and experience level be observed with at least four values along the other two dimensions, as shown in Figure A17(b); column (3) requires balance across outcome years and application cohorts, as shown in Figure A17(c); and column (4) requires balance across outcome years and experience profiles, as shown in Figure A17(d). Column (5) drops the application cohorts with the highest and lowest values of $\bar{U}y$ (1994 and 2001, respectively), while column (6) weights observations by the inverse of the share of physician-year observations accounted for by each experience–outcome year cell. Wild cluster bootstrap p-values: $^{*}\, p < 0.10$, $^{**}\, p < 0.05$, $^{***}\, p < 0.01$.

C Mediation analysis

This appendix provides the regression-based mediation decomposition referenced in Section V. The decomposition quantifies how much of the total reduced-form effect of applicant-relevant unemployment on physician practice outcomes is statistically accounted for by observed changes in the skill versus the preferences of entering cohorts, proxied respectively by the medical school acceptance rate and the share of matriculants from non-traditional pre-medical backgrounds. Similar regression-based decompositions have long been used in sociology and related fields and, more recently, in empirical economics. For example, they have been used to understand the channels through which an early childhood program affected adult outcomes (Heckman et al., 2013), to quantify the drivers of intergenerational wealth persistence (Fagereng et al., 2021), and to decompose changes in victimization following criminal justice reforms (Gonçalves et al., forthcoming).

We emphasize that this exercise provides a regression-based decomposition of the reduced-form relationship rather than a fully causal mediation analysis. A causal interpretation of the indirect components would require strong assumptions about the absence of unobserved cohort-level confounders affecting both the mediators and physician outcomes. These assumptions would be violated if, for example, contemporaneous changes in admissions policies, class sizes, or federal financing of medical education independently influenced cohort selectivity and composition, as well as downstream physician outcomes, even conditional on applicant-relevant unemployment rates. Our goal is therefore more modest: to quantify the extent to which observable changes in selectivity and cohort composition statistically account for the reduced-form relationship between unemployment at application and downstream practice outcomes. We consider two cohort-level mediators: the medical school acceptance rate, M_y^{acc}, our proxy for selectivity and thus the skill composition of a cohort, and the share of matriculants from non-traditional pre-medical backgrounds, M_y^{nontrad}, our proxy for the preference composition of a cohort.

Recall our primary estimating equation, displayed as equation (2) in Section IV:

$$Outcome_{it} = \beta \cdot \bar{U}_{y(i)} + \lambda_{specialty} + \lambda_{experience} + \lambda_t + \epsilon_{it}$$

where i indexes physicians, $y(i)$ indexes application cohorts, and t indexes outcome years. The coefficient β captures the reduced-form effect of the applicant-relevant unemployment

rate on downstream physician outcomes.

To implement the mediation analysis, we begin by estimating first-stage regressions of each mediator on the applicant-relevant unemployment rate:

$$M^k_{y(i)} = \delta_k \cdot \bar{U}_{y(i)} + \lambda^k_{specialty} + \lambda^k_{experience} + \lambda^k_t + \eta_{ikt}, \quad k \in \{acc, nontrad\} \tag{A1}$$

We estimate these first stages on the same physician-year observations and with the same specialty, experience, and outcome year fixed effects as in the outcome equation. Although the identifying variation lies entirely across cohorts, estimating at the physician-year level recovers the physician-year-weighted slope and ensures a common residual structure across the first-stage and outcome equations in the bootstrap below; including the same fixed effects is what makes the components sum exactly to the total reduced-form effect.

Next, we estimate the outcome equation including both mediators:

$$\begin{aligned} Outcome_{it} =& \gamma \cdot \bar{U}_{y(i)} + \theta_{acc} \cdot M^{acc}_{y(i)} + \theta_{nontrad} \cdot M^{nontrad}_{y(i)} \\ &+ \lambda_{specialty} + \lambda_{experience} + \lambda_t + \varepsilon_{it} \end{aligned} \tag{A2}$$

The contribution of mediator k to the total reduced-form effect is given by the product $\delta_k\theta_k$. The sum $\delta_{\text{acc}}\theta_{\text{acc}} + \delta_{\text{nontrad}}\theta_{\text{nontrad}}$ represents the portion of the overall unemployment effect that is statistically accounted for by observed changes in selectivity and cohort composition. The remaining portion (i.e., the "residual" component) is given by γ, the coefficient on unemployment in the outcome equation that conditions on both mediators. Intuitively, this captures the portion of the reduced-form effect that is not statistically mediated by our two measured channels. By construction, the total reduced-form effect equals $\beta = \delta_{acc}\theta_{acc} + \delta_{nontrad}\theta_{nontrad} + \gamma$.

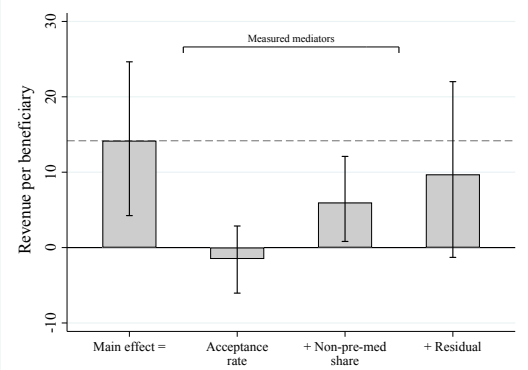
Inference in the main specification uses a wild cluster bootstrap at the cohort level with Rademacher weights. For the direct components—the total effect β and the residual γ, each a single regression coefficient—we use the same restricted wild cluster bootstrap as in our main specification (Cameron et al., 2008). The indirect components, however, are products of first-stage and second-stage estimates, which combine coefficients from two separate regressions. Because no single null restriction applies across both equations, standard wild bootstrap routines do not apply directly, and we therefore implement an unrestricted pro-

cedure manually. In each replication, we draw a single set of Rademacher weights at the cohort level, apply that set to the residuals of both the first stages (equation (A1)) and the mediation regression (equation (A2)), and recompute each indirect component. We report confidence intervals for these components from the resulting bootstrap distribution, recentered on the corresponding point estimate.

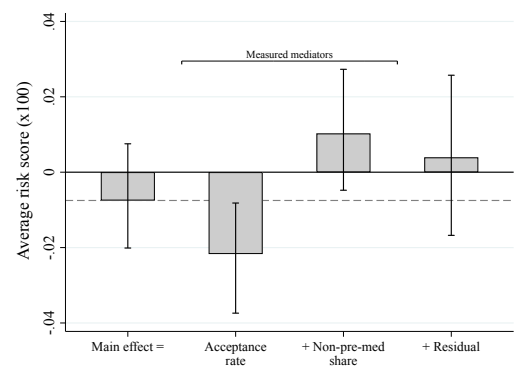
Figures A18 and A19 report results from the mediation analysis for outcomes in the public-use Medicare data and SK&A data, respectively. The relative importance of the two mediators differs across outcomes. For measures most directly tied to physician income, such as Medicare revenue per beneficiary and choice of practice ownership, the share of matriculants from non-traditional pre-medical backgrounds accounts for a meaningful portion of the reduced-form effect, consistent with a preference-based channel. The acceptance rate, by contrast, plays a more prominent role in explaining differences in patient risk: although imprecise, the point estimates suggest that cohorts entering during periods of higher selectivity treat patients with lower average risk scores and who are less likely to be dually eligible for Medicaid. This pattern is somewhat surprising, as one might expect more selective cohorts to manage more complex patients. Rather, it suggests that an increase in applicants for a fixed number of spots does not translate into behavior consistent with higher clinical aptitude, and that the screening process for medical school admissions may be imperfect at identifying such potential clinicians. As emphasized in the main text, however, the channel-specific components are estimated imprecisely, and thus the relative contributions of the two channels should be interpreted with caution.

Figure A18: Mediation analysis: practice styles and patient composition

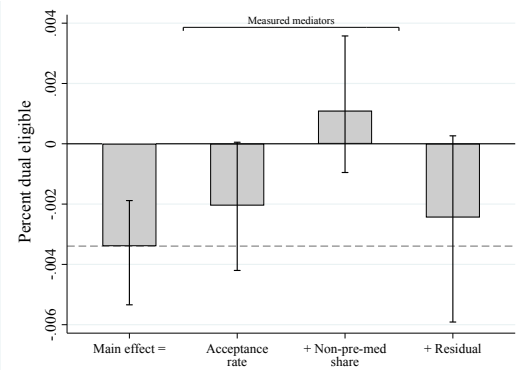
(a) Average revenue per beneficiary



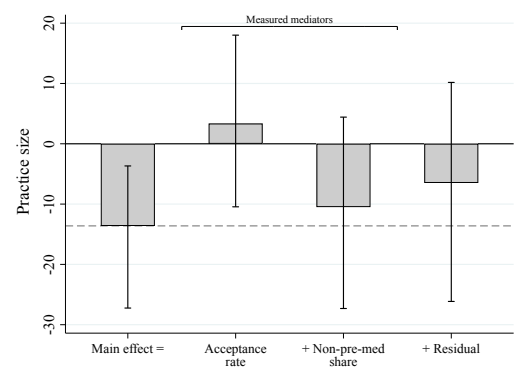
(b) Average patient risk scores



(c) Percent Medicare beneficiaries dually eligible



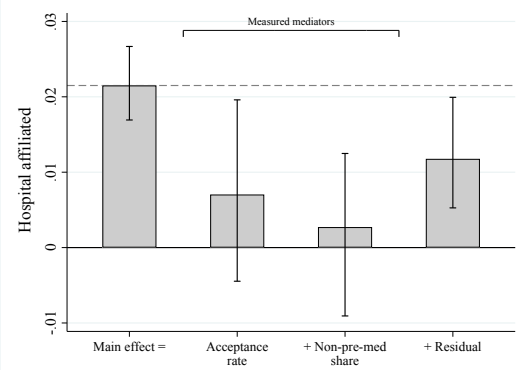
(d) Practice size



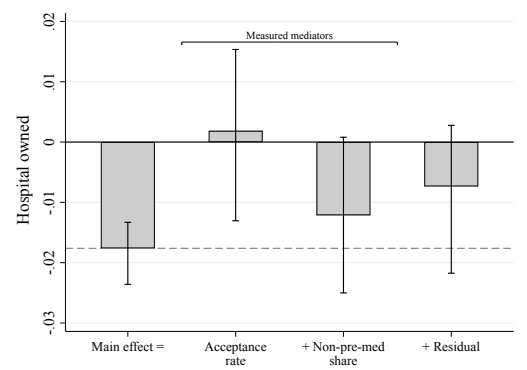
Notes: The above figures present regression-based decompositions of the reduced-form effect of the applicant-relevant unemployment rate at the time of medical school application on average annual Medicare revenue per beneficiary (subfigure (a)), average risk scores among the physician's Medicare beneficiaries times 100 (subfigure (b)), the percent of a physician's Medicare beneficiaries that are dually eligible for Medicaid (subfigure (c)), and physician practice size (subfigure (d)). The total reduced-form effect ("Main effect") corresponds to the coefficient β from equation (2). The mediator components are computed using the first-stage and mediation regressions in equations (A1) and (A2). Specifically, the "Acceptance rate" and "Non-pre-med share" bars equal $\delta_k\theta_k$, where δ_k is the coefficient from the first-stage regression (equation (A1)) and θ_k is the corresponding coefficient from the outcome equation including both mediators (equation (A2)). The "Residual" component corresponds to the coefficient γ from equation (A2). By construction, the sum of the mediator components and the residual equals the total reduced-form effect. Confidence intervals for the main effect and the residual are computed using a restricted wild cluster bootstrap at the cohort level, as in our main specification; because the mediator components are products of coefficients from two separate regressions, their confidence intervals are instead computed from an unrestricted cohort-level wild cluster bootstrap that perturbs the residuals of equations (A1) and (A2) in each replication. See Section II for details on the construction of $\bar{U}_y$. Outcome data are drawn from the public-use Medicare Part B files from 2013–2019 and the public-use Medicare Physician Compare files from 2014–2019; data on applicants and matriculants are from the AAMC.

Figure A19: Mediation analysis: practice ownership and affiliation

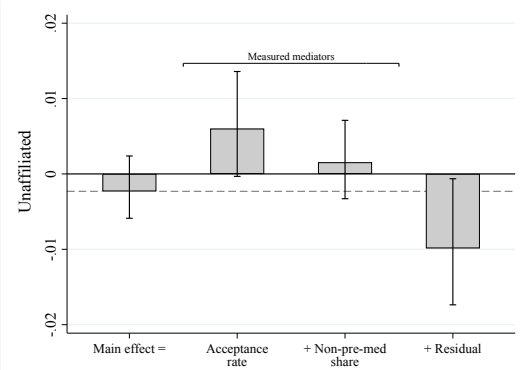
(a) Share hospital-affiliated practice



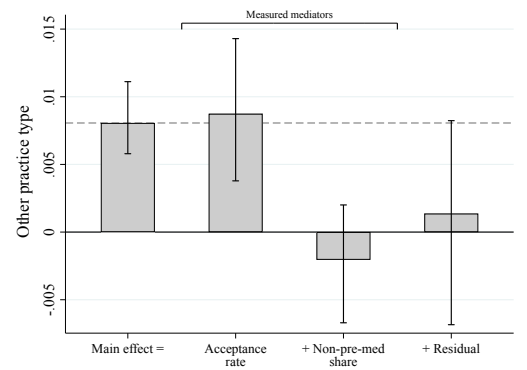
(b) Share hospital- or health-system-owned



(c) Share independent practice



(d) Share "other" practice type



Notes: The above figures present regression-based decompositions of the reduced-form effect of the applicant-relevant unemployment rate at the time of medical school application on the share of physicians in each practice category: independent but hospital affiliated (subfigure (a)), hospital- or health-system-owned (subfigure (b)), independent and unaffiliated (subfigure (c)), and "other" (subfigure (d)). The total reduced-form effect ("Main effect") corresponds to the coefficient β from equation (2). The mediator components are computed using the first-stage and mediation regressions in equations (A1) and (A2). Specifically, the "Acceptance rate" and "Non-pre-med share" bars equal $\delta_k\theta_k$, where δ_k is the coefficient from the first-stage regression (equation (A1)) and θ_k is the corresponding coefficient from the outcome equation including both mediators (equation (A2)). The "Residual" component corresponds to the coefficient γ from equation (A2). By construction, the sum of the mediator components and the residual equals the total reduced-form effect. Confidence intervals for the main effect and the residual are computed using a restricted wild cluster bootstrap at the cohort level, as in our main specification; because the mediator components are products of coefficients from two separate regressions, their confidence intervals are instead computed from an unrestricted cohort-level wild cluster bootstrap that perturbs the residuals of equations (A1) and (A2) in each replication. See Section II for details on the construction of $\bar{U}_y$. Outcome data are drawn from the SK&A data from 2008–2016; data on applicants and matriculants are from the AAMC.