The Productivity of Professions: Evidence from the Emergency Department

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Abstract

Professions play a key role in determining the division of labor and the returns to work. This paper studies the productivity difference between two distinct professions performing overlapping tasks—physicians and nurse practitioners (NPs)—but with stark differences in background, training, and pay. Using data from the Veterans Health Administration and quasi-experimental variation in patient probability of being treated by physicians versus NPs in the emergency department, we find that, compared to physicians, NPs significantly increase patient length of stay (by 11 percent) and medical costs (by 7 percent). Despite higher medical resource use, NPs achieve less favorable patient outcomes: They increase patient 30-day preventable hospitalization rate by 20 percent. We find evidence suggesting channels related to lower human capital among NPs relative to physicians. Our estimates suggest a net increase in medical costs with the use of NPs, even when accounting for NP salaries that are half as much as physician salaries. Despite large productivity differences between professions, we find even larger productivity differences within professions and substantial productivity overlap between professions. We find little overlap in wages between NPs and physicians and, within professions, no significant correlation between productivity and wages.

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1 Introduction

Professions occupy a dominant position in the economy, across sectors of health care, law, finance, and science. As described by sociologists, professional groups engage in activities that impact both the "jurisdictions" of tasks that each profession controls and the economic returns for this work (Abbott 2014). Because professional groups also control who may enter each profession, they could influence the relationship between work and social class and the distribution of income across classes.

Theoretically, it is ambiguous whether the designs by professional groups imply differences in worker productivity that are consistent with differences in pay (Shapiro 1986). On the one hand, selection into training, learning during the training, professional ethics, and ongoing assessments (e.g., by examination, licensing, and credentialing) may lead to higher worker quality. On the other hand, entry restrictions can reduce the supply of providers and create rents for existing providers, and professionals who are secure in their jobs may exert less effort, lowering their productivity (Freidson 1974; Berlant 1975). Stringent selection into highly sought-after professions may not reflect true differences in productivity but could nonetheless confer an advantage for the upper class to pass down privilege to the next generation (Markovits 2020).

Evidence comparing the productivity of distinct professions engaged in the same type of work remains scant. The very nature of professional groups acts to exclude outsiders from their jurisdictions (Abbott 2014). Yet, while the medical profession provides a well-documented case study of historical exclusion, it now provides an opportunity for study.[1] Recent decades have witnessed a dramatic growth in the demand for health care, outstripping the supply of doctors, and an emergence of a class of professionals from the nursing tradition—nurse practitioners (also referred to as NPs)—seeking to provide the same work that doctors do. On the basis of training, income, and social class, the comparison between physicians and NPs is stark. Physicians undergo a highly selective process and long periods of training to enter the profession. They comprise the single most common profession in the top percentile of the income distribution (Gottlieb et al. 2020). About half of medical students come from families in the top quintile of the income distribution, while only 5 percent of medical students come from families in the bottom quintile (Kahn and Sneed 2015). In contrast, the income of NPs is roughly half of that of physicians, and the number of years of training is also roughly half.[2] Admission rates to nursing programs are around ten times higher than admission rates to medical school, and nursing has been highlighted as realistic path to the middle class for women of

[1] Beginning in the early 20th century, doctors managed to enforce a well-documented scientific reorientation of the profession, excluding (and even jailing) providers without this training from the profession (Starr 1982; Brown 2018).

[2] According to the American Academy of Medical Colleges (2020), physicians must complete a 4-year undergraduate degree, a 4-year Doctor of Medicine (MD) degree, and 3-7 years of residency training. According to the American Association of Nurse Practitioners (2020), NPs are required to complete a 4-year Bachelor of Science in Nursing (BSN) degree and may choose between 1-2 years in a Master of Science in Nursing (MSN) degree or 3-4 years in a Doctor of Nursing Practice (DNP) degree.

working-class backgrounds (Friedman, Laurison and Macmillan 2017; Searcey, Porter and Gebeloff 2015).[3]

In this paper, we exploit a quasi-experiment in the Veterans Health Administration (VHA) to study the productivity differences between physicians and NPs. In December 2016, the VHA granted full practice authority to NPs, allowing them to practice without physician supervision. We leverage quasi-experimental variation in the availability of physician and NP providers in the emergency department (ED). In a sample of 1.1 million ED visits, our approach compares patients arriving at the same ED and during similar times (i.e., the year, month, day of the week, and hour of the day) that differ in the number of NPs available. We show that the number of available physicians declines with the number of available NPs, and NP availability strongly predicts whether an arriving patient will be assigned to an NP versus a physician. Under the plausible assumption that patients arrive quasi-randomly within cells of ED stations and time categories, this instrumental variables (IV) design allows us to study the effect of NPs on patient resource utilization and health outcomes.

Along a variety of measures, we find that NPs use more resources and achieve worse health outcomes than physicians. Our IV results show that NPs increase length of stay by 11 percent, about 18 minutes for each patient, and raise the cost of ED care by 7 percent, about $66 for each patient. While we can rule out large effects on inpatient admission and 30-day mortality in the overall sample, we find that NPs raise 30-day preventable hospitalizations by 20 percent. In contrast to our IV estimates, ordinary least squares (OLS) estimates for the benchmark outcomes of length of stay and ED costs are negative in sign, consistent with the descriptive evidence that NPs treat healthier patients. Our IV estimates for the benchmark outcomes are remarkably stable, regardless of the inclusion of a wide set of additional covariates; in contrast, OLS estimates under the full set of covariates are less than half in magnitude (though still wrong-signed relative to IV estimates) of those when only baseline covariates are included.

We undertake a range of analyses to assess the validity of our IV quasi-experiment. We show that, conditional on our baseline controls, a broad range of patient characteristics that predict outcomes are well balanced across values of our instrument, the number of available NPs. We assess the validity of the exclusion restriction, that the number of available NPs is not correlated with other factors that could drive care delivery or patient outcomes. Specifically, we show that our instrument is conditionally unrelated to a range of characteristics of available physicians and NPs. We also assess potential spillovers between NPs and physicians and find no evidence suggesting such spillovers.[4] Finally, we show that our results are robust

[3]We searched `https://www.petersons.com/graduate-schools.aspx` for admission rates to graduate nursing programs and to medical schools in the following universities: Columbia University, Duke University, Emory University, Johns Hopkins University, the University of North Carolina, the University of Pennsylvania, and the University of Washington. Rates ranged from 25 to 63 percent for graduate nursing programs and from 3 to 7 percent for medical schools.

[4]We examine whether NPs may impact physician performance by examining patients unlikely to ever be seen by NPs (i.e.,

to controlling for a series of other factors that may vary with the instrument, including the total level of available staff, patient volume, and patient wait time.

Next, we unpack the mechanisms behind the lower productivity of NPs versus physicians. Under several analytical lenses, the evidence suggests mechanisms related to lower human capital among NPs relative to physicians. We first find that the higher resource utilization by NPs grows with patient condition complexity and severity. The NP-physician gap in patient log length of stay, log ED cost, and the probability of inpatient admission grows for patients with more comorbidities and higher severity. Second, NPs are likelier to gather (costly) information from other sources than are physicians: They are more likely to order radiology tests and to order formal consults for their patients. Third, NPs exhibit differential prescription thresholds consistent with lower skill relative to physicians (Chan, Gentzkow and Yu 2022): NPs are *less* likely to prescribe opioids, which have higher health risks if incorrectly prescribed (i.e., type I errors), but they are *more* likely to prescribe antibiotics, which have higher health risks if incorrectly not prescribed (i.e., type II errors). Fourth, we find that NP experience matters: The NP-physician gap in medical resource use narrows when NPs have seen more prior patients in general and more prior patients with the same diagnosis in question.

To understand the policy implications of the productivity differences between NPs and physicians, we perform two counterfactual analyses. First, we conduct an accounting of the net costs implied by the lower productivity and the lower salaries of NPs. We consider the current allocation of approximately a quarter of the patients being treated by NPs in the sample of VHA EDs. This implies $160 million per year in additional costs due to increased ED costs, increased hospital admissions directly following the ED visit, and increased preventable hospitalizations in the next 30 days. To incorporate the implications of the lower salaries of NPs in our accounting, we consider rough assumptions on the number of NPs that would be equivalent to one physician, in terms of workflow.[5] Even under the conservative lower bound that one NP can perform the work of one physician, we arrive at a net cost of $74 million per year under the current allocation, compared to staffing the VHA EDs only with physicians. In other words, despite salaries that are half of physicians' salaries, NPs are more costly to employ, by a magnitude likely larger than their salary costs.

As a second counterfactual analysis, we consider the scenario in which it is impossible for hospitals to employ additional physicians and must increase provider labor solely by using additional NPs. The primary benefit of increasing the pool of providers in the ED is to increase throughput, decreasing wait times. Within

"never-takers," or patients treated by physicians even when the instrument is greater than its 90th percentile). We find that the outcomes of these patients are unrelated to the instrument. We also examine whether physician quality impacts NP performance by examining the outcomes of patients treated by NPs when only a single NP is available. We find that the outcomes of these patients are unrelated to the value-added of available physicians.

[5]We consider two facts to bound the number of NPs equivalent to a physician. First, we find that staffing three additional NPs on a day reduces the number of physicians by one. Second, we use the fact that NPs take longer to discharge patients but do not appear to handle more patients simultaneously. Both of these facts suggest that one physician is equivalent to more than one NP.

our set of baseline controls, we also hold fixed in these counterfactual scenarios the number of physicians working and the volume of patients arriving at the ED. Under the same conditional independence assumption that we consider in our benchmark quasi-experiment, we find that increasing the number of NPs on duty decreases wait time. However, decreasing wait time by 30 minutes increases length of stay—defined as the time after patient assignment until discharge—by 16 minutes. Decreasing the average wait time by 30 minutes by hiring additional NPs also increases total expected health care spending by 15 percent, or about $238 per visit, not including the costs of additional NP salaries.

Finally, we examine variation in productivity across providers *within* the professional classes of physicians and NPs. To arrive at provider-specific measures of productivity, we estimate a just-identified IV model, in which we instrument patient assignment to specific providers by indicators for provider availability. Using a method developed by Efron (2016) and adapted by Kline, Rose and Walters (2022), we deconvolve the estimates of provider-specific productivity into flexible underlying prior distributions for each of the two professional classes. We find wide variation in productivity within professions and substantial overlap between the productivity distributions of physicians and NPs. The probability that a randomly chosen NP is more productive than a randomly chosen physician can be as large as 38 percent. This statistic remains large when we adjust our productivity distributions to account for possible differences in treatment effects between the overall population and compliers. Exploiting detailed wage data in the VHA, we find little correlation between productivity and wages within professional class and little overlap in the wage distributions between physicians and NPs. In other words, despite our previous results, wages are highly predictive of professional class, while productivity is much less predictive.

Our findings contribute to several strands of literature. First, given the dramatic rise in the supply of NPs to meet the growing demand for health care, heated debates have arisen around the quality of NP-provided care and on whether NPs should be permitted to substitute for physicians. A recent body of research has investigated the state-level impacts of liberalizing "scope of practice laws" for NPs.[6] By design, these papers study general-equilibrium impacts of both allowing NPs greater scope to practice and increasing the supply of providers; results will depend on how labor is reallocated between professions. In contrast, our study sheds light on productivity differences between NPs and physicians as two distinct classes of professionals when quasi-experimentally assigned the same set of patients. An older medical literature has concerned itself with this question, but the small numbers of providers and other features of these earlier studies limit

[6]The findings of this literature are varied and somewhat mixed. Perry (2009) and Kleiner et al. (2016) find that these laws impact physician and NP earnings. Stange (2014) finds a minimal impact of greater NP supply on utilization, access, or prices but perhaps a moderate impact on primary care utilization. However, Traczynski and Udalova (2018) find increases in utilization and some evidence of increased quality, and Alexander and Schnell (2019) find evidence of better access and outcomes in mental health. McMichael and Markowitz (2020) note that these papers may adopt different definitions of scope of practice laws.

inference on systematic differences between the classes, let alone variation within the classes.[7]

Second, our research relates to the widespread practice of occupational licensing.[8] The existing literature suggests that occupational licensing increases the earnings of licensed workers (Kleiner and Krueger 2013; Kleiner et al. 2016; Farronato et al. 2020) but provides little evidence on whether higher earnings arise from restricting the supply of workers or from improving the quality of their work in modern settings (see Farronato et al. 2020 for a review). Two studies of an earlier, unregulated environment of midwifery, near the beginning of the 20th century, demonstrate meaningful reductions in maternal and infant mortality with the initial implementation of occupational licensing (Lazuka 2018; Anderson et al. 2020). To our knowledge, studies in this literature compare differences in quality within professions (along the margin of occupational licensing), while ours compares two competing professions. The nursing and medical professions differ meaningfully in their historical origins, social status, income, and selection and training processes, not to mention licensing.

A third related literature is concerned with worker human capital and productivity. These issues have received growing attention in the health care setting (e.g., Doyle, Ewer and Wagner 2010; Currie and MacLeod 2017, 2020; Chen 2021; Chan, Gentzkow and Yu 2022). But it also relates more broadly to measuring the productivity of agents more broadly (e.g., Chetty, Friedman and Rockoff 2014) and identifying the relationship between human capital and productivity (e.g., Moretti 2004; Gennaioli et al. 2013). Within this literature, our study is unique in that it compares the productivity of two distinct classes of professionals, with human capital differences that may stem from selection and training. In the rich ED setting, we also uncover key mechanisms by which human capital may lead to higher productivity along dimensions of case complexity, information-gathering, prior experience, and decision-making. Furthermore, consistent with prior literature on practice variation (e.g., Epstein and Nicholson 2009; Gowrisankaran, Joiner and Leger 2017), we also find wide variation in productivity across providers within professions, perhaps larger than the differences between professions.

Fourth, a broad set of questions is concerned with the distribution of wealth in society across occupations and strata of educational attainment. In recent decades, societies have witnessed a concentration of wealth in occupations associated with high human capital (Smith et al. 2019). Training to reach the highest levels

[7] See Laurant et al. (2005) for a systematic review of this literature. The literature features small randomized trials with null results, sometimes comparing a single-digit number of physicians with a single-digit number of NPs. The largest study contains 1,465 patients (Kinnersley et al. 2000); all studies involve at most a handful of clinic locations. The studies tend to focus on primary care settings but usually have short follow-up times that may be insufficient to detect meaningful effects. According to Laurant et al. (2005), the null findings of this literature "should be viewed with caution given that only one study was powered to assess equivalence of care, many studies had methodological limitations, and patient follow-up was generally 12 months or less."

[8] More than a thousand occupations and about a third of all jobs in the US require some form of licensing or certification (Kleiner and Krueger 2013).

of income has become increasingly competitive among the upper class, while the middle and lower classes are increasingly left behind, characterizing a "meritocracy trap" (Markovits 2020). Interestingly, our results suggest a productivity difference between professions that is in fact larger than wage differences, at least in our resource-intensive and information-dependent setting within health care. Yet, we find potentially even larger productivity variation within professions that may not be captured in worker-specific wages, consistent with frictions in observing productivity across similar workers from outside of the firm (e.g., Acemoglu and Pischke 1998). If worker-specific productivity is difficult to observe, then professional class and the actions of organized professional institutions, as richly documented by sociologists (e.g., Starr 1982; Abbott 2014), may matter more than a worker's individual productivity in setting her wage. Entering a profession may represent a costly and imperfect way for workers to distinguish themselves.

The remainder of this paper is organized as follows. Section 2 describes the institutional setting and the data. Section 3 describes our empirical approach and provides evidence for its validity. Section 4 provides our main results. Section 5 presents evidence on mechanisms. Section 6 presents analyses on policy-relevant counterfactual scenarios. Section 7 reports productivity and wage distributions within professions. Section 8 concludes.

2 Background and Data

2.1 Background

In December 2016, the VHA granted full practice authority to NPs. The policy enables NPs to practice without any requirement for physician supervision at VHA facilities. NPs can treat patients as independently as physicians, regardless of state restrictions that limit NPs' practice authority.[9]

Several features make the ED a setting well suited to study the productivity of providers. First, patients are not shared between providers in the ED; the responsibility for each patient visit is generally assigned to a single ED provider. This setup allows us to attribute patient outcomes to individual providers and compare productivity across providers. Second, patient flow in the ED is highly unpredictable, but provider schedules are typically set well in advance. We therefore can leverage variation in NP availability that is unrelated to the types of patients arriving. Third, patients present at the ED with a wide spectrum of conditions, ranging in complexity and severity. This provides an opportunity to investigate productivity across a range of tasks. Finally, the ED is a major setting that uses the NP workforce, understanding NPs' productivity in the ED is

[9] As of 2021, about half of the states in the US have not granted NPs full practice authority (American Association of Nurse Practitioners 2021).

thus important in itself. Across the nation, 8 percent of ED visits were seen by NPs between 2010 and 2017 (Wu and Darracq 2021). In the VHA, the share of ED visits seen by an NP has been steadily increasing and has reached 11 percent in 2019—close to the share of visits seen by NPs in primary care, which is around 20 percent (Morgan et al. 2012).[10]

2.2 Data

We use administrative health records from the VHA, the largest health care delivery system in the US, serving more than 9 million veterans. For each ED visit, the data allow us to identify the type of provider treating the patient (i.e., NP or physician) as well as resource use and patient outcomes (e.g., length of stay, 30-day preventable hospitalization). The data also contain detailed information on patient characteristics that include demographics, comorbidities, vital signs, and prior health care use, as well as information on provider characteristics such as birth date and gender.

Sample Construction. We restrict our analysis sample in the following ways, summarized in Appendix Table A.1. First, we restrict the sample to ED visits between January 2017 and January 2020, that is, after full practice authority was granted to NPs at the VHA and before the onset of the COVID pandemic in the US. Second, we include only cases arriving during the daytime (8 a.m. to 6 p.m.), because the data show that few NPs take evening or night shifts.[11] Third, we concentrate attention on visits to VHA EDs in the months the ED has adopted the full practice authority policy and has been using NPs for treating patients. Although the VHA granted full practice authority to NPs at all VHA facilities, local VHA facilities adopted the policy at different times and vary in whether to use NPs in the ED.[12] Fourth, we exclude cases treated in EDs that use physician extenders other than NPs (mainly physician assistants). This allows us to focus on a balanced set of cases assigned to either a physician or an NP across days with differing NP availability, which is important for the empirical identification described below. Finally, we drop a small number of cases with missing age or gender or whose age is below 20 or above 99 years. The final sample contains 1.1 million cases over 44 EDs, seen by a total of 156 NPs and 1,348 physicians.

Outcome Variables. To measure medical resource use, we include two primary outcomes that are frequently used in the ED setting: (i) the patient length of stay (i.e., the time between patient assignment to the provider

[10] We estimate the share of ED visits seen by an NP from the VHA data, which is described in the following section.

[11] One possible explanation is that, since patient volumes are on average much lower in the evening/night than in the daytime (e.g., the average number of cases arriving per hour is 3.6 between 8 a.m. and 6 p.m. versus only 0.9 outside of 8 a.m.-6 p.m.), EDs in our sample often staff only one provider for a shift in the evening/night, resulting in a low probability of having NPs in those times since NPs may not be able to handle very severe patients arriving.

[12] We define a VHA ED as having granted full practice authority to NPs and using NPs in a month if it has more than 15 cases treated by NPs in the month. The sample size changes only slightly when we use alternative thresholds: e.g., the sample size is 1.13 and 1.10 million when using a threshold of 10 and 20, respectively, compared to 1.12 million based on a threshold of 15.

and patient discharge) and (ii) the cost of care during the ED visit (excluding costs due to a resulting hospital admission, as described next).[13] We also include hospital admission, a resource-intensive option that indicates the provider's decision to admit the patient for inpatient care. To measure quality of care, we examine two prominent patient outcomes: We use linked death records to construct indicators of patient 30-day mortality, and we use linked inpatient data to construct indicators of 30-day preventable hospitalization, as defined by Agency for Healthcare Research and Quality (2021). We exclude from 30-day preventable hospitalization inpatient admissions immediately following the ED visit.

In examining mechanisms behind the effect of NPs, we include the following sets of outcomes: (i) whether the ED provider orders consults from other providers; (ii) whether the provider orders CT scans and X-rays, two primary diagnostics in the ED; and (iii) prescriptions of opioids and antibiotics—two major classes of drugs whose clinical indications for appropriate use are often unclear and may require skill to discern (e.g., Fleming-Dutra et al. 2016; Huang et al. 2018; Neuman, Bateman and Wunsch 2019).

2.3 Descriptive Statistics

Table 1 summarizes characteristics of the cases included in our analysis. Column 1 shows that the average age of the analysis sample is 62.1 years. About 42.4 percent of the sample are married, and 27.0 percent are Black. Cases are disproportionally male (90.5 percent), reflecting a feature of the veteran population. To measure patient comorbidities, we construct Elixhauser indices (Elixhauser et al. 1998), which are 31 indicators for comorbidities (e.g., cancer, diabetes) that are predictive of clinical outcomes, based on patient medical histories in the prior 365 days. The average case has 3.6 Elixhauser comorbidities. For outcomes, the average case has a length of stay of 162 minutes and ED spending of $939 (inflation adjusted to 2020 dollars). The 30-day preventable hospitalization rate is 1.2 percentage points.

Columns 2 and 3 of Table 1 compare cases treated by NPs with those treated by physicians. Along several dimensions, cases treated by NPs are on average healthier than those treated by physicians: Cases treated by NPs are younger (60.7 versus 62.5 years), have fewer Elixhauser comorbidities (3.2 versus 3.7), and have fewer outpatient visits and inpatient stays in the prior 365 days (5.7 versus 6.4 outpatient visits and 0.4 versus 0.7 inpatient stays). Cases treated by NPs appear to have better outcomes: They have a shorter average length of stay (120 versus 175 minutes), a lower average ED cost ($813 versus $978), and a lower 30-day preventable hospitalization rate (0.7 versus 1.4 percentage points). As discussed in detail below, OLS estimates of the NP effect on patient outcomes are highly sensitive to including additional controls

[13] For length of stay, we use detailed time-stamped data on patient assignment and discharge to estimate length of stay. For the cost of ED care, we rely on detailed accounting by the VHA that considers utilization during the ED visit.

for patient characteristics. The unbalanced patient characteristics and the sensitivity of OLS estimates to patient controls raise concerns about selection bias in OLS estimation of the NP effect. We therefore turn to a quasi-experimental approach that leverages plausibly exogenous variation in patient probability of being treated by an NP versus a physician.

3 Empirical Strategy

An ideal experiment to assess the effect of being treated by NPs would randomly assign cases to NPs and physicians. Lacking random assignment, we use a quasi-experimental approach: We leverage plausibly exogenous variation in the number of NPs on duty to instrument for whether a case is treated by an NP or a physician. In this section, we begin by describing our instrumental variables (IV) approach. We then discuss evidence that supports the validity of our identification strategy. We show that the number of NPs on duty is strongly predictive of whether a case is assigned to an NP versus a physician but is conditionally independent of a wide range of patient characteristics, as well as characteristics of available physicians and NPs that could affect patient treatments and outcomes.

3.1 Specification

Our empirical specification is a two-stage least squares (2SLS) model that takes the following form:

$$y_i = \delta \mathrm{NP}_i + \mathbf{T}_i \eta + \mathbf{X}_i \beta + \varepsilon_i, \tag{1}$$

$$\mathrm{NP}_i = \lambda Z_i + \mathbf{T}_i \zeta + \mathbf{X}_i \gamma + v_i, \tag{2}$$

where i denotes a case, y_i is the outcome of interest, and NP_i indicates whether case i is treated by an NP. We use Z_i to denote the instrument (i.e., the total number of NPs on duty between 8 a.m. and 6 p.m.) at the ED on the day that case i visits.[14] The parameter of interest is δ, which represents a local average treatment effect (LATE), i.e., the average causal effect among cases that would have been assigned to a different type of provider under a different number of NPs on duty.

[14] Since the data do not include direct information on provider scheduling, we measure Z_i as the number of NPs treating cases during the analysis time window in the ED-day cell of case i's visit. To mitigate the concern on measurement error, we count an NP as on duty if she is observed treating at least two cases between 8 a.m. and 6 p.m. in the ED-day cell (indeed, this only affects our estimates minimally because only 0.1 percent of cases in our sample are treated by NPs with only one case between 8 a.m.-6 p.m. of an ED-day cell). In the main analysis, we include the index case in calculating Z_i. Given that NPs generally see multiple cases in a shift, the results are virtually identical when using a leave-out measure of Z_i that excludes the index case. Nonetheless, in Section 4.5, we show the robustness of our estimates to an alternative Z_i that includes NPs with only one case in a shift as well as one that leaves out the index case in defining whether an NP is on duty.

The vector $\mathbf{T}_i$ encodes for interactions between indicators for the ED and indicators for time categories of the patient's arrival, specifically the year, the month, the day of the week, and the hour of the day of the patient's arrival. We condition on $\mathbf{T}_i$ to allow for sorting of NPs across shift types (e.g., weekdays versus weekends) and EDs, where patient characteristics and ED conditions may be systematically different. Controlling for ED-by-time-category indicators captures these potentially systematic differences.[15]

As robustness checks, our specification also includes a vector of patient covariates $\mathbf{X}_i$, including indicators for five-year age bins, marital status, gender, and race (white, Black, and Asian/Pacific Islander, with other racial categories omitted as the reference group); indicators for 31 Elixhauser comorbidities; prior health care use (the number of outpatient visits and the number of inpatient stays in any VHA facilities in the prior 365 days); vital signs (pulse, respiratory rate, blood oxygen level, pain level, body temperature, an indicator for fever, systolic blood pressure, and diastolic blood pressure); and indicators for 3-digit ICD-10 code of patient primary diagnosis of the visit.[16] For each patient covariate with missing values, we add an indicator for missing values and replace missing values with zero. Finally, ε_i and v_i are error terms. We cluster standard errors by provider. In robustness checks, we also show results under alternative clustering approaches, including clustering by ED-day (the level across which the instrument varies) and, more conservatively, two-way clustering by provider and ED-day, neither of which meaningfully affects our results.

3.2 Identification

To interpret δ as the LATE of being treated by NPs, our IV approach requires four identifying assumptions: first stage, conditional independence, exclusion, and monotonicity. In this section, we summarize empirical evidence that supports the validity of the identifying assumptions.

First Stage. Figure 1 shows the first stage of our IV model, controlling for the baseline controls, i.e., ED-by-time-category indicators, $\mathbf{T}_i$. Panel A shows that the number of physicians on duty declines linearly with the number of NPs on duty. Consequently, Panel B shows that patient probability of being treated by an NP increases with the number of NPs on duty: One more NP on duty increases patient probability of being treated by NPs by 18.6 percent. The increase is highly significant (with an F-statistic of 149.2, conditioning on ED-by-time-category indicators) and is close to linear.

[15]While fixed effects for the interactions between indicators for the ED and indicators for the hour of the day are not necessary to condition on to yield quasi-random variation in the instrument (since the instrument varies at the day instead of hour level), we include them for statistical precision of estimates.

[16]Since our study period is from January 2017 to January 2020, disease diagnoses are all coded in ICD-10 in the data. A potential question is whether the 3-digit diagnoses are endogenous to being treated by NPs. Yet as shown below in Section 4, our estimates are remarkably stable regardless of controlling for 3-digit diagnosis indicators or not. In Appendix A.1, we also show that NPs and physicians appear similar in 3-digit diagnosis coding.

To provide context, Appendix Figure A.1 presents a histogram of the number of NPs on duty across ED-day cells. The figure reveals a fair spread in the number of NPs on duty: 38.1, 47.2, and 11.3 percent of ED-days have zero, one, and two NP(s) on duty, respectively; 3.4 percent of ED-days have more than two NPs on duty. Perhaps a natural question is what drives the variation in the number of NPs on duty. NPs are less likely to work on weekends (77 percent of weekday ED-day observations versus 24 percent of weekend ED-day observations have NPs on duty). However, conditional on day-of-the-week and other time-category (year and month) indicators, we still observe substantial variation in the number of NPs on duty within EDs across days (standard deviation of 0.49).[17]

Conditional Independence. For our instrument to be valid, the number of NPs on duty must be uncorrelated with patient potential outcomes, conditional on our vector of baseline controls, $\mathbf{T}_i$. Two sets of empirical evidence provide strong support for this assumption. First, we show that patient observed characteristics are well balanced across the instrument, conditional on $\mathbf{T}_i$. As shown in Figure 2, patient average characteristics are remarkably uncorrelated with the instrument, conditioning on $\mathbf{T}_i$. For completeness, Appendix Figure A.2 reports similar coefficients for the instrument using each of the various patient characteristics included in $\mathbf{X}_i$ as the dependent variable. Despite the fact that these characteristics are strong predictors of patient outcomes (F-statistics around 100 for joint significance, even controlling for ED-time indicators, see Appendix Figure A.3), there is little significant relationship between our instrument and the broad range of patient characteristics, conditioning on $\mathbf{T}_i$.

As a second set of evidence, we examine the stability of our IV estimates under different sets of controls for patient covariates. Specifically, we divide patient observable characteristics into eight groups and estimate separate regressions that control for each of the $2^8 = 256$ different combinations of patient covariates.[18] We show in empirical results below that controlling for any combination of patient covariates results in virtually no change in our IV estimates of the NP effect. Following the logic of Altonji, Elder and Taber (2005), this evidence implies limited selection bias due to either observed or unobserved patient characteristics that are predictive of patient outcomes. In sum, conditional on $\mathbf{T}_i$, there appears to be little relationship between NP availability and patient characteristics.

Exclusion. While conditional independence supports a causal interpretation of the reduced-form effect, interpreting the IV estimates as identifying the causal effect of being treated by NPs requires an exclusion

[17] Anecdotes from ED providers to whom we spoke are that providers have differential preferences over shifts, e.g., more at the beginning versus the end of the month, leading to variation in NP availability even conditional on the time categories described above.

[18] We divide patient characteristics into the following eight groups: (i) five-year age-bin indicators; (ii) marital status; (iii) gender; (iv) race indicators; (v) dummies for 31 Elixhauser comorbidities; (vi) vital signs; (vii) prior health care use; and (viii) indicators for 3-digit patient primary diagnosis of the visit.

restriction. That is, the number of NPs on duty only impacts patient outcomes through patient probability of being treated by NPs, not through any other channels. After discussing our main results, we present in Section 4.4 empirical evidence to support this assumption in our setting. We show that both physicians' and NPs' characteristics are well balanced across the number of NPs on duty conditional on $\mathbf{T}_i$, and there appear to be little evidence of spillovers from NPs to physicians (e.g., NPs may ask physicians for help, which may slow down physicians). We also investigate a series of alternative explanations, finding little evidence indicating violation of the exclusion restriction.

Monotonicity. In the presence of heterogeneous treatment effects, we need to assume monotonicity to interpret IV estimates as a LATE, i.e., the average causal effect among cases induced by the instrument into being treated by NPs. In our setting, monotonicity requires that cases treated by NPs on days with fewer NPs on duty would also be treated by NPs on days with more NPs, and vice versa.

We examine one testable implication of the monotonicity assumption: The instrument and the probability of being treated by NPs should be positively correlated for any subsample defined by patient characteristics. We test this implication in Appendix Figure A.4, where we split the sample by patient characteristics and estimate the first-stage effect separately for each subsample. In particular, we divide the sample by patient age, marital status, gender, race, total number of Elixhauser comorbidities, and predicted 30-day mortality.[19] Appendix Figure A.4 shows that for all subsamples, the first-stage estimates are positive and statistically different from zero, consistent with the validity of the monotonicity assumption.[20]

4 Main Results

In this section, we present our main findings, showing the effect of NPs on resource use and patient outcomes. We find that, compared to physicians, NPs use more medical resources: They require longer lengths of stay and incur higher costs; however, they achieve less favorable patient outcomes, as measured by 30-day preventable hospitalizations. We also characterize compliers relative to the overall sample, present evidence supporting the exclusion restriction, and consider a series of additional robustness checks.

[19] Predicted 30-day mortality is generated from a linear regression of actual 30-day mortality on the full set of patient characteristics $\mathbf{X}_i$ included in Equations (1) and (2).

[20] An interesting pattern is that the first-stage coefficients appear to be larger for healthier patients. Appendix Figure A.4 shows that the first-stage coefficient is larger for patients who are younger, have a lower number of Elixhauser comorbidities, and have a lower predicted 30-day mortality. This pattern reflects that compliers are more heavily concentrated among healthy patients. In Section 4.3, we compute characteristics of compliers and never-takers. We find consistent evidence that compliers are healthier than the overall sample, while never-takers are riskier.

4.1 Length of Stay and Cost

As summary measures of medical resource use, we start by examining the effect of NPs on patient length of stay and cost of care during the ED visit. Figure 3 shows the reduced-form effect of the instrument (i.e., the number of NPs on duty) against patient log length of stay and log cost of the ED visit, controlling for our baseline controls, $\mathbf{T}_i$. Log length of stay and log cost increase significantly with the instrument. As a comparison, we also plot in the figure patient predicted log length of stay and predicted log cost, both of which are well balanced across the instrument.[21]

Table 2 reports the OLS and IV estimates of the effect of NPs on patient log length of stay and log cost of the ED visit, along with reduced-form coefficients on the instrument for these outcomes. All regressions control for the full set of controls described in Section 3.1. The OLS estimates (Columns 1 and 4) show that cases treated by NPs have significantly shorter lengths of stay and lower costs, which could reflect that NPs treat healthier and easier cases than do physicians, at least in terms of observable characteristics (Table 1). The IV estimates in Columns 3 and 6 improve upon OLS estimation by exploiting plausibly quasi-random variation in patient probability of being treated by NPs. The IV estimates suggest that NPs significantly raise patient medical resource use during the ED visit: On average, cases quasi-randomly assigned to NPs have lengths of stay that are 11 percent longer and ED costs that are 7 percent higher. Given the mean of the sample, the NP effect equals an 18-minute increase in length of stay and a $66 increase in cost per ED visit. Appendix Figure A.5 shows a visual IV plot of the NP effect on length of stay and cost.

Figure 4 examines the robustness of our OLS and IV estimates to the inclusion of different combinations of patient controls. Specifically, we divide patient covariates into eight subsets: (i) five-year age-bin indicators; (ii) marital status; (iii) gender; (iv) race indicators; (v) dummies for 31 Elixhauser comorbidities; (vi) vital signs; (vii) prior health care use; and (viii) indicators for 3-digit primary diagnosis of the visit. We then estimate separate regressions that control for each of the $2^8 = 256$ different combinations of patient covariates for each outcome for both OLS and IV estimations. Figure 4 shows the range of the coefficients across specifications with different patient controls. Each n on the x-axis reports the number of covariate subsets included. For each n, we plot the maximum, mean, and minimum of the estimated coefficients for the effect of NPs using all possible combinations with n (out of eight) subsets of patient covariates.

Figure 4 shows a stark divergence between the OLS and IV estimates. The OLS estimates are negative and decline sizably in magnitude with the addition of patient controls. For example, in the OLS results,

[21] We form these predictions using linear regressions of actual outcomes on the full set of patient controls that include five-year age-bin indicators, marital status, gender, race, indicators for 31 Elixhauser comorbidities, prior health care use, vital signs, and indicators for 3-digit ICD-10 code of patient primary diagnosis of the visit.

conditioning only on baseline controls (i.e., $\mathbf{T}_i$), we find that cases treated by NPs have 30 percent lower costs than cases treated by physicians. When we add the full set of patient controls, the difference attenuates to 10 percent. The lower health risks of cases treated by NPs (Table 1) and the sensitivity of the OLS estimates to patient controls indicate that OLS estimation is likely subject to selection bias due to patient unobservable characteristics. In contrast, the IV estimates are remarkably robust to controlling for any combination of patient covariates: Regardless of the additional controls, the IV estimates for the effect of NPs on length of stay and cost remain stable at 11 and 7 percent, respectively. Following the logic of Altonji, Elder and Taber (2005), the stability of the IV estimates implies limited scope for selection on either observable or unobservable patient characteristics that predict potential outcomes, further supporting the validity of our instrument.[22]

4.2 Hospital Admission and Patient Outcomes

Column 1 of Table 3 assesses another important margin of medical resource use: whether the provider admits the patient to the hospital. The results show that, overall, the admission rate does not significantly differ between NPs and physicians. Yet, as shown next in Section 5, we find that, for patients with high health risks, NPs induce a considerably higher admission rate.

Column 2 of Table 3 investigates the effect of NPs on 30-day mortality. Column 2 shows no significant difference in 30-day mortality between cases treated by NPs and those treated by physicians. In Section 5, we also examine heterogeneous effects by patient health risks. We finding no significant effect of NPs on 30-day mortality for most patients, but we note a marginally significant and clinically meaningful NP-driven increase in mortality for a highly severe type of patient: those with a sepsis diagnosis (point estimate: 24.5 percentage points, *p*-value: 0.106).[23] Perhaps compensated by higher medical inputs during the ED visit, NPs do not exhibit significant mortality difference from physicians, except among severe cases like sepsis, in which NPs nonetheless incur higher mortality.

Column 3 of Table 3 shows a significant NP effect of increasing patient 30-day preventable hospitalizations: Compared to physicians, NPs raise patient 30-day preventable hospitalization rate by 0.25 percentage

[22] Another potential explanation for the divergence between the OLS and IV estimates is heterogeneity in treatment effects, since OLS reports the average effect of NPs among the analysis sample, while IV reports the average effect among compliers (i.e., cases on the margin of being treated by NPs). To explore this possibility, we follow the procedure developed by Bhuller et al. (2020) and reweight the analysis sample to match the sample of compliers using predicted 30-day mortality, i.e., a composite index of all patient observables. The OLS estimates with the reweighting still differ in sign compared with the IV estimates, suggesting that the difference between the OLS and IV estimates cannot be accounted for by heterogeneity in effects, at least not by heterogeneous effects across observables.

[23] 30-day mortality among patients with a sepsis diagnosis is 11.5 percentage points, as against 1.25 percentage points for the average patient.

points, which is equivalent to a 20 percent increase compared to the mean of the sample. The higher 30-day preventable hospitalization rate of NPs may reflect two possibilities: (i) NPs have poorer decision-making over whom to admit to the hospital, resulting in under-admission of patients who should have been admitted and a net increase in return hospitalizations—despite the fact that NPs use longer lengths of stay to evaluating patients' need for hospital admission; and (ii) NPs produce lower quality of care conditional on admitting decisions, despite spending more resources on treating the patient (as measured by costs for the ED care). Both of these two possibilities imply lower skill of NPs relative to physicians.

Taken together, empirical evidence shows that NPs and physicians are operating on different production functions: NPs use more inputs (longer lengths of stay and higher costs), but achieve less favorable patient outcomes (as measured by 30-day preventable hospitalization). This evidence points to lower productivity of NPs. Nonetheless, it is worth noting that this evidence does not necessarily indicate that we can cut back on care for NPs without compromising patient outcomes: A lower production function may still exhibit positive returns to medical inputs (Silver 2021); that is, higher intensity of care may still be allocatively efficient for NPs.

4.3 Compliers and Patient Selection

Our IV estimates represent the LATE, i.e., the average causal effect among complier cases that are quasi-randomly assigned to NPs versus physicians due to the instrument. To better understand this LATE, we examine complier characteristics relative to the overall sample following the approach developed by Abadie (2003), as described in Appendix A.2. Appendix Table A.2 reports the results. Consistent with NPs treating less severe cases, we find that compliers are healthier than the average case. Compared to the average case, compliers are younger, have a lower number of Elixhauser comorbidities, have a lower number of inpatient stays in the prior year, and exhibit lower predicted mortality. Appendix Table A.2 also examines characteristics of never-takers and always-takers (of NPs), following an approach from Dahl, Kostøl and Mogstad (2014) that we detail in Appendix A.2.[24] In line with the notion that NPs treat healthier cases than physicians do, we find that never-takers are riskier than the average case and always-takers are healthier than both the average case and compliers.

In Appendix Figure A.6, we examine patient characteristics for those assigned to NPs and those assigned to physicians as the number of on-duty NPs increases. We find that the average health risks of cases seen by

[24] As described in Appendix A.2, we define never-takers as cases treated by physicians in ED-day cells with the residual share of cases treated by NPs at least as high as the 90th percentile of ED-days with at least one case treated by NPs. We define always-takers as cases treated by NPs in ED-day cells with the residual share of cases treated by NPs equal to or below the 10th percentile of ED-days with at least one case treated by NPs.

physicians and by NPs both increase (while the average risk of all cases remains constant). This pattern is consistent with the first cases treated by NPs having the lowest observable health risks; when more NPs are available, cases incrementally treated by NPs have higher health risks compared to those initially treated by NPs, but they are relatively healthy among the remaining cases. As a result, cases left for physicians also become riskier with more NPs, despite the average risk of all cases being constant.

4.4 Exclusion Restriction

As discussed in Section 3.2, interpreting the IV estimates as the causal effect of being treated by NPs requires the number of NPs on duty to affect patient outcomes only through the patient probability of being treated by NPs, not through any other channels. In this section, we present evidence supporting the validity of the exclusion restriction. We first show that the characteristics of physicians on duty are well balanced across the number of NPs on duty. Second, we show that NP characteristics remain stable regardless of the number of NPs on duty. Third, we assess and find little support for the possibility of productivity spillovers between NPs and physicians in our setting (e.g., NPs may ask physicians in the ED for help, which may slow down physicians, or NP peer identity affects physician productivity). Finally, we examine a range of factors that may vary across days and find no evidence to suggest these factors are driving our IV estimates.

Balance in Provider Characteristics. To start, we investigate whether physicians are similar across days with different numbers of NPs. If such a balance does not hold, our IV estimates could be driven by compositional changes of physicians. Figure 5 reports the balance for various physician characteristics. Specifically, we consider an ED-day level analysis that asks whether physician average characteristics (weighted by the number of cases treated by each physician) are independent of the number of NPs on duty, conditional on ED-year, ED-month, and ED-day-of-the-week indicators.[25]

We examine three sets of physician characteristics: (i) demographics of age and gender; (ii) measures of physician "value-added," reflecting physician risk-adjusted impact on 30-day mortality; and (iii) measures of physician "practice style," reflecting a physician's risk-adjusted average input choices in terms of length of stay and ED costs (see Appendix A.3 for details). Figure 5 shows that each of these physician average characteristics is well balanced across the instrument, conditional on the baseline controls. Panels A and

[25] Specifically, the empirical specification takes the form $\bar{y}_{jd} = \tilde{\lambda} Z_{jd} + \tilde{\mathbf{T}}_{jd}\tilde{\eta} + \varepsilon_{jd}$, where $\bar{y}_{jd}$ is the average characteristics of physicians on duty at ED j on day d (weighted by the number of cases treated by each physician), Z_{jd} is the number of NPs on duty, and $\tilde{\mathbf{T}}_{jd}$ includes ED-year, ED-month, and ED-day-of-the-week indicators. We cluster standard errors by ED. A potential question is, since we have only 44 EDs, whether the estimated standard errors are biased given the relatively small number of clusters. While there is currently no clear-cut definition of "small", if anything, such a potential issue would bias us toward rejecting the null hypothesis of physician balance across the instrument (Bertrand, Duflo and Mullainathan 2004; Cameron and Miller 2015). Nonetheless, as a robustness check, we apply the correction for the small number of clusters by using Wild cluster bootstrap as suggested by Cameron and Miller (2015); we find no meaningful change in the standard error estimates.

B show that physician average age and gender composition are remarkably stable across the instrument. Panels C-E show that, while there is large variation in physician value-added and practice style, the average value-added and practice style of physicians on duty is constant despite the instrument.

We similarly examine whether NP characteristics are systematically different across days with differing numbers of NPs on duty. If such systematic variation exists, our baseline IV strategy may not be able to disentangle the effect of being treated by NPs from that due to the potentially different quality of NPs across days.[26] Thus, as with physicians, we examine three sets of NP characteristics: (i) demographics that include age and gender; (ii) value-added constructed on the basis of patient 30-day mortality; and (iii) practice style constructed on the basis of patient log length of stay and log cost. Figure 6 shows that average NP characteristics are well balanced across days with differing numbers of NPs, conditional on ED-year, ED-month, and ED-day-of-the-week indicators.

Assessing Productivity Spillovers. We then consider the possibility of spillovers between NPs and physicians. If NPs ask physicians in the ED for assistance, this could slow down physicians. Alternatively, a change in peers from days without any NP to days with NPs may influence physicians' treatment decisions as, for example, physicians may come under different degrees of peer pressure that motivate them to work differently (Chan 2016; Silver 2021).

However, we find little empirical evidence to suggest meaningful spillovers between NPs and physicians. First, if NPs ask physicians for assistance, we might expect the outcomes of patients treated by NPs to depend on the quality of physicians on duty. Using the value-added measure described in Appendix A.3 as a proxy for physician quality, we find in Appendix Table A.3 no such relationship. Second, with spillovers (either through the assistance or peer pressure channel), outcomes for patients treated by physicians could change with the presence of NPs. A direct test would be regressing outcomes for patients treated by physicians on the number of NPs on duty. Yet such a test suffers from patient selection since physicians are allocated riskier cases on days with more NPs (Section 4.3). To circumvent this issue, we restrict the sample to never-takers of NPs, who, despite NP availability, are seen by physicians and thus remain a balanced pool regardless of the number of NPs on duty.[27] Appendix Table A.4 reports the results. Columns 1 and 2 confirm that the health risks of never-takers are balanced across days with different numbers of NPs. Columns 3-7 show that,

[26] This concern only applies to our baseline IV strategy, which utilizes variation in the number of NPs in addition to whether there is an NP available. In Section 4.5, we show that our results are robust to restricting to the extensive margin of whether there is any NP available and to restricting to days where there is either zero or one NP available.

[27] We define never-takers as cases seen by physicians in ED-day cells with the residual share of cases treated by NPs at least as high as the 90th percentile of ED-days with at least one case seen by NPs. Residual shares are constructed by first collapsing the data to ED-days and then residualizing the share of cases seen by NPs by indicators for ED-year, ED-month and ED-day-of-the-week. See Appendix A.2 for more details.

consistent with the hypothesis of no spillover, never-takers' outcomes are stable regardless of the number of NPs on duty. Relatedly, NPs may relieve the workload of physicians particularly on certain days with high workloads (e.g., a large patient volume or a very severe set of patients). Appendix Table A.5 therefore examines the relationship between never-takers' outcomes and the number of NPs on duty by whether the total number of cases or the sum of patient predicted 30-day mortality in the ED-day cell is among the top tercile of ED-days in our sample. The results continue to show no statistically significant change in never-takers' outcomes with the number of NPs on duty.

An alternative channel of productivity spillovers may operate through the patient reallocation described in Section 4.3.[28] Yet the finding that never-takers' outcomes remain constant with the number of NPs suggests such a channel does not appear to be operative.

Robustness to Additional Factors. Finally, we investigate factors that may vary across days, including the total number of cases arriving, the total number of physician equivalents on duty, and patient wait times (defined as the time between arrival at the ED and assignment to a treating provider). We control for the total number of physician equivalents on duty to mitigate the concern that the effective level of providers may vary across days with different numbers of NPs on duty.[29] Turning to wait time, since patient wait time is potentially endogenous (healthier cases could be assigned a lower priority and thus wait longer), we instrument for wait time using the average wait time of cases visiting on the same day at the same ED as the index case. While potentially important, the factors listed above do not affect our estimates: Appendix Table A.6 shows that our IV estimates are remarkably robust to controlling for these factors.

Another question is whether the estimated NP effect is driven by patient-provider gender mismatch, since the majority of patients are male (91 percent), while physicians are primarily male (74 percent) and NPs are mostly female (79 percent). Appendix Table A.7 explores this possibility by asking whether NPs treat patients of the opposite gender differently compared to the same gender. The results show little heterogeneity.

4.5 Additional Robustness Checks

Appendix Tables A.8-A.10 report additional robustness checks. Appendix Table A.8 shows that our findings are stable with alternative standard error clustering approaches: clustering by ED-day or two-way clustering

[28]For example, a riskier set of patients may adversely impact physicians' overall treatment quality.

[29]We calculate the number of physician equivalents on duty as the sum of the number of on-duty physicians and the number of on-duty NPs multiplied by 0.341, where 0.341 is the coefficient reported in Panel A of Figure 1 and assumed to be the extent of substitution between NPs and physicians. As a robustness check, we also apply a more conservative substitution rate of 0.5; the results are stable. We do not directly control for the total number of providers on duty because, conditional on the total number of providers, a higher number of NPs indicates lower staffing (since one NP appears unable to substitute for one physician given that NPs take longer to discharge patients but do not seem to handle more patients simultaneously); therefore, the 2SLS estimates for the NP effect conditional on the total number of on-duty providers would be confounded by lower staffing.

by ED-day and provider. The standard errors become smaller when clustering by ED-day compared to the baseline model that clusters by provider, but no conclusion on statistical significance is changed. The standard errors change only minimally with two-way clustering by ED-day and provider relative to the baseline model clustering by provider only. Panels A and B of Appendix Table A.9 show the robustness of our estimates to, respectively, an alternative instrument that also counts any NP with at least one case in the analysis time window of an ED-day cell and another instrument that leaves out the index case in defining whether an NP is on duty.[30] In Appendix Table A.9, Panels C-D, we construct two alternative instruments—the share of cases in the ED-day cell treated by NPs (leaving out the index case) and an indicator for any NP on duty; we show the results are stable. Appendix Table A.10 shows that our results remain similar when looking at the margin between when there are no NPs and when there is one NP on duty.

5 Mechanisms

The evidence in the previous section suggests that NPs have lower productivity than physicians: They use more medical resources and produce worse patient outcomes. In this section, we examine mechanisms related to human capital behind this productivity gap. First, we show that the NP effect is larger for more complex and more severe patients. Second, we show that NPs are likelier to call on external resources to gather information, namely from radiology exams and formal consults. Third, we examine prescription thresholds for two drug classes with likely asymmetric costs of type I and type II errors and show thresholds consistent with lower skill (Chan, Gentzkow and Yu 2022). Finally, we show that NP experience reduces the size of the productivity gap in some outcomes.

5.1 Case Complexity and Severity

We first examine heterogeneous effects of NPs by case complexity or severity. Following Imbens and Rubin (1997), we estimate complier potential outcomes under NPs and under physicians. Specifically, we estimate complier potential outcomes under NPs using the following IV regression:

$$y_i \cdot \mathrm{NP}_i = \sum_{g=1}^{G} \mathbf{1}(\mathrm{Group}_i = g)\left[\delta_g \mathrm{NP}_i + \lambda_g\right] + \mathbf{T}_i\eta + \mathbf{X}_i\beta + \varepsilon_i, \tag{3}$$

where $y_i \cdot \mathrm{NP}_i$ is the interaction between patient outcome and the indicator for being treated by an NP, $\mathbf{1}(\mathrm{Group}_i = g)$ is an indicator for case i belonging to group $g \in \{1, \ldots, G\}$ characterizing complexity or

[30] See Section 3.1 and footnote 14 for explanations for these two robustness checks.

severity. As a natural extension of our main IV model, we include additional instruments by interacting Z_i with $\{\mathbf{1}(\text{Group}_i = g)\}_{g=1}^{G}$, where Z_i is the number of NPs on duty. We estimate complier potential outcomes under physicians using an IV regression similar to Equation (3) but with a dependent variable of $y_i \cdot (\text{NP}_i - 1)$.

We consider two types of partitions of cases. First, we divide cases into quartiles by their number of Elixhauser comorbidities. Second, we divide cases by whether condition severity measured by 30-day mortality of the 3-digit diagnosis is equal to or above the 95th percentile of the sample. The 30-day mortality rate for this top-severity group is 8.6 percentage points, compared to 1.3 percentage points for the whole analysis sample. Included in this group are cases with relatively severe conditions such as heart failure and acute kidney failure, potentially requiring higher human capital to manage.[31]

As shown in Figure 7, the effect of NPs on raising lengths of stay and medical costs grows with cases with greater complexity or severity. For example, for cases in the lowest complexity quartile, NPs increase their length of stay by about 5 percent, while for cases in the highest complexity quartile, NPs increase their length of stay by around 25 percent. For cases with a condition severity at least as high as the 95th percentile, we find an NP effect that doubles their length of stay. For these cases, NPs also increase their hospital admission rate by about 30 percentage points, nearly a 100 percent increase from the potential admission rate under physicians. To summarize the heterogeneity, Appendix Table A.12 reports treatment effects of NPs (i.e., the difference between potential outcomes in Figure 7) by patient health risks, using the IV regression in Equation (3) but replacing the dependent variable with patient outcome y_i (i.e., the difference of the two dependent variables, or $y_i \cdot \text{NP}_i - y_i \cdot (\text{NP}_i - 1)$). The table further investigates the NP effect within four severe conditions with high mortality: stroke, acute myocardial infarction (AMI), sepsis, and heart failure. Consistent with other results, we find significantly larger NP effects among cases with these conditions.

Interestingly, we do not find increasing NP effects on 30-day preventable hospitalizations with case complexity or severity (Column 5 of Appendix Table A.12). If NPs are less skilled at treating more complex or more severe cases, the lower quality of skill-task matching could lead to worse patient outcomes. On the other hand, as NPs increase their intensity of care for these cases, the incremental care could mitigate the NP effect on raising preventable hospitalizations. The pattern that the NP effect on 30-day preventable hospitalizations does not increase with patient health risks may be suggestive of positive returns to medical

[31] Appendix Table A.11 summarizes the 10 most common 3-digit diagnosis codes in this group. The largest diagnosis category is heart failure, followed by acute kidney failure. The top 10 diagnoses also include acute myocardial infarction, a form of sepsis, and a form of respiratory failure. The 30-day mortality rate ranges between 5 and 17 percentage points.

Perhaps a question of interest is why cases with these severe conditions are treated by NPs. While the data show a declined probability of being seen by NPs when patient health risks increase, we still observe some cases with severe conditions seen by NPs. One potential explanation is that triage nurses may underevaluate condition severity (Chan and Gruber 2020) and assign to NPs cases that should have been assigned to physicians. Another potential explanation is that when physicians are fully occupied with existing cases, it could be more efficient to assign severe cases to NPs instead of keeping these cases waiting long for care.

inputs among NPs. As shown in Panel A of Appendix Table A.12, for cases in the highest risk quartile, NPs sizably increase their lengths of stay and medical costs, without leading to a significant change in 30-day preventable hospitalizations. In contrast, for cases in the lower risk quartiles, NPs reveal a smaller-magnitude and sometimes insignificant effect on lengths of stay and medical costs, but significantly raise 30-day preventable hospitalizations. While it remains an interesting area for future research, the negative relationship between medical resource use during the ED visit and post-discharge patient outcomes perhaps reflects positive returns to medical inputs among NPs, consistent with earlier research showing health benefits of incremental care intensity among physicians (e.g., Silver 2021).

5.2 Informational Resources

We next ask whether NPs and physicians differ in how they draw on formal informational resources. In the ED, providers face the complex task of treating patients with vague symptoms whose underlying cause and correct course of care are often unclear. Providers with less skill may call on external resources, such as (non-ED) physician consults and radiology exams, to help with diagnostic and treatment decisions.

Columns 1-3 of Table 4 report the results, using the 2SLS estimation specified in Equations (1) and (2). Column 1 shows that, relative to physicians, NPs are more likely to use consults: NPs increase consults by 2.6 percentage points, or 11 percent of the sample mean. Columns 2 and 3 show that, relative to physicians, NPs are more likely to order CT scans and X-rays, two primary diagnostics in the ED. NPs increase CT scan and X-ray ordering by 1.2 and 2.0 percentage points, respectively, or 8 and 7 percent of the respective sample means.

Taken together, NPs appear to be more likely to collect resource-intensive information from external sources than are physicians. This could directly increase lengths of stay as well as medical costs, since consults and diagnostics take time and resources to complete. On the other hand, consults and diagnostics allow lower-skilled providers to narrow their performance gap relative to higher-skilled providers by incorporating more information and interpretation from other experts.

5.3 Prescription Thresholds

Next, we evaluate skill from the lens of thresholds for prescriptions with asymmetric costs. Specifically, as shown by Chan, Gentzkow and Yu (2022), provider skill may correlate with treatment thresholds when the costs of false-positive (type I) and false-negative (type II) errors are asymmetric. Compared to higher-skilled providers, lower-skilled providers may (optimally) adjust their treatment thresholds in the face of less information. Specifically, providers with less information may more frequently opt for a treatment when false negatives (not treating when a case should have been treated) are costlier than false positives (treating when a case should not have been treated); conversely, lower-skilled providers may less frequently opt for a treatment when false positives are costlier than false negatives.

We choose two important prescriptions with different asymmetries in the costs of type I and type II errors: opioids and antibiotics. For both of these prescriptions, the clinical indications for appropriate use are often unclear and require clinical judgment.[32] We estimate the NP-physician difference in prescriptions using the 2SLS estimation specified in Equations (1) and (2). Column 4 of Table 4 shows that, for opioids, which have higher false-positive costs—e.g., addiction and overdose among patients who should not have received opioids compared to continued pain among patients who should have received them—NPs have a lower prescription likelihood relative to physicians: NPs lower opioid prescriptions by 1.8 percentage points, or 20 percent of the sample mean. In contrast, for antibiotics, which have higher false-negative costs—e.g., non-treatment of a potentially life-threatening infection compared to antibiotic resistance—NPs show a higher prescription likelihood relative to physicians: NPs increase antibiotic prescriptions by 4.0 percentage points, or 6 percent of the sample mean.[33] The joint evidence from prescription thresholds is consistent with lower skill among NPs.

5.4 Provider Experience

Finally, we ask whether experience impacts the magnitude of the performance difference between NPs and physicians. On the one hand, the NP-physician performance gap may narrow with experience if experience matters more to workers with lower human capital, at least as measured by formal education and training. On the other hand, lower human capital may restrict learning from informal experience, widening the NP-physician performance gap as experience grows.

To examine this, we form measures of both general and specific experience. We measure general experience as the number of cases the provider has treated since the start of the study period to the day before the current case's visit. We measure specific experience as the number of cases with a 3-digit primary diagnosis that is the same as the current case the provider has treated since the start of the study period to the day before the current case's visit. For ease of interpretation, we standardize both general and specific experience to have a mean of zero and a standard deviation of one for NPs and physicians separately.

[32] See, e.g., Fleming-Dutra et al. (2016), Huang et al. (2018), Butler et al. (2019), Neuman, Bateman and Wunsch (2019).

[33] Since opioids apply to a wide range of conditions, we include all patients in examining opioid prescriptions. For antibiotics, as they generally only apply to patients with infections, we restrict the sample to patients with respiratory or genitourinary system infections, i.e., two common types of infections.

Our empirical model takes the following form:

$$y_i = \delta_1 \text{NP}_i \times \text{Experience}_i + \delta_2 \text{NP}_i + \delta_3 \text{Experience}_i + \mathbf{T}_i \eta + \mathbf{X}_i \beta + \varepsilon_i. \tag{4}$$

Experience_i denotes standardized experience within each provider type.[34] We instrument for NP_i and $\text{NP}_i \times \text{Experience}_i$ using Z_i (i.e., the number of NPs on duty) and $Z_i \times \text{Experience}_i$.

We find that, for several outcomes, experience lowers the NP-physician performance gap. Panel A of Table 5 examines the impact of specific experience. For length of stay, Column 1 indicates that a one-standard-deviation increase in specific experience among both NPs and physicians lowers the performance gap by 5.8 percent, reducing the gap at the mean levels of experience (10.1 percent) by more than half. The NP-physician gaps in costs, CT scan ordering, and consult ordering similarly declines with specific experience. However, specific experience does not appear to reduce the NP-physician gap in 30-day preventable hospitalizations. Panel B shows a similar pattern for general experience in the outcomes of length of stay and consult ordering. Increasing general experience by one standard deviation in both NPs and physicians is associated with a 10 percent decline in the NP-physician performance gap in length of stay and a 2 percentage-point reduction in the gap in consult ordering. Overall, general experience is correlated with a reduced NP-physician performance gap in fewer dimensions than is specific experience, possibly reflecting task-specific human capital that accrues to a greater degree within tasks than across tasks (Gibbons and Waldman 2004).[35]

We examine several alternative measures of experience to address measurement concerns. One concern is that, because we do not observe cases treated by providers since the start of their careers, our measures of experience are imperfect representations of providers' true levels of experience. We mitigate this concern by restricting the sample to cases visiting in the second or later year of our analysis period, so that our measures of experience have at least a one-year look-back window and suffer less from measurement error. We also measure experience based on cases seen in the prior year (i.e., in the 365 days before the day of the current case's visit), so that the estimates precisely represent heterogeneity by prior-year experience.[36] As the effect of experience may decay with time (e.g., Benkard 2000), recent experience could be more important than

[34]A one-standard-deviation increase in specific experience equals 108 and 107 cases for NPs and physicians, respectively. A one-standard-deviation increase in general experience equals 1,855 and 1,258 cases for NPs and physicians, respectively.

[35]Although the magnitude of the decline in the NP-physician difference in length of stay and consult ordering is larger with a increase in general experience than with a increase in specific experience, it is not necessarily that general experience is more effective in reducing the NP-physician performance gap in these two outcomes. As described above, a increase in general experience equals 1,855 cases for NPs, while a increase in specific experience equals a smaller number, 108 cases, for NPs.

[36]Specifically, we measure general experience as the number of cases the provider treated in the proceeding 365 days; we measure specific experience as the number of cases with the same 3-digit ICD-10 primary diagnosis as the current case the provider treated in the proceeding 365 days. We exclude from this regression cases visiting in the first year of our analysis period, since we cannot fully observe their providers' experience in the proceeding 365 days.

experience gained in the relatively distant past. Appendix Tables A.13 and A.14 show that our estimates remain qualitatively similar under these alternative measurements. A second concern is that the number of cases a provider has seen may capture speed, which may have an effect on productivity independent of experience. For example, a faster provider may be observed treating a higher number of cases in the past and thus exhibit higher experience, while this provider could also generate a shorter length of stay for the current case. To examine this concern, we include an alternative (general) experience measure—the number of days a provider has worked since the start of our study period to the day before the current case's visit—which is independent of speed. Appendix Table A.15 shows that, though the heterogeneity estimates become marginally significant, they are similar to the baseline estimates for general experience (Panel B of Table 5).[37]

6 Counterfactual Scenarios

In this section, we consider two counterfactual policy scenarios. First, we use estimates from previous sections to consider overall cost implications of substituting physicians with NPs by assigning 25 percent of cases across VHA EDs to NPs, instead of physicians. Second, we perform auxiliary analyses to consider the policy of augmenting the existing supply of physicians with NPs. Overall, these analyses highlight that productivity differences between classes of workers may have even larger cost implications than the sizable differences in wages.

6.1 Substituting Physicians with NPs

We first perform a simple calculation of the cost of assigning 25 percent of all the cases in VHA EDs to NPs—approximately the share in our analysis sample, which consists of EDs that are early adopters of NPs under full practice authority. We assume that the average effect of NPs across all EDs would be similar to that in our sample and consider three components of extra costs due to NPs: the costs of ED care (Section 4.1), the costs of hospital admission following the ED visit for severe cases (Section 5.1), and the costs of 30-day preventable hospitalizations (Section 4.2). We find an extra cost of $160 million per year for the VHA.[38] This figure is approximately twice the yearly NP wage costs that the VHA would need in order to

[37]To an extent, we expect the heterogeneity estimates to be less precise when measuring experience by the number of days a provider has worked, since it is a relatively noisy representation of experience compared to the number of cases a provider has treated.

[38]To calculate the cost of increased 30-day preventable hospitalizations and hospital admissions in the ED visit, we apply the cost estimate of $19,220 per VHA hospital stay, on the basis of the average length of stay per hospitalization at the VHA and costs per VHA inpatient day reported by the VHA's Health Economics Resource Center (2021).

For extra costs as a result of greater 30-day preventable hospitalizations, we multiply the increased probability of having a 30-day assign 25 percent of its ED cases to NPs.[39]

The calculation ignores potential changes in provider wage costs, although the average NP wage is only half the average physician wage (Bureau of Labor Statistics 2021*a*,*b*). If two NPs substitute for one physician, which could be within the possible range given the coefficient reported in Panel A of Figure 1, there would be no wage saving when substituting physicians with NPs. For a conservative estimate, we consider the scenario in which one NP may substitute for one physician. Under this scenario, we nonetheless find net costs of $74 million per year for the VHA for the policy of assigning 25 percent of cases to NPs.

We finally consider the counterfactual scenario where, instead of using the observed patient assignments, EDs assign the least complex 25 percent of cases (by number of Elixhauser comorbidities) to NPs. We note that such an allocation may not be always feasible: For example, in hours when all arriving patients are relatively severe, EDs may have to assign some severe patients to NPs. Applying the estimates reported in Row 1 of Appendix Table A.12, we calculate extra costs of $81 million per year to the VHA. To overcome these extra costs through lower salaries, an NP would need to be able to substitute for about 0.85 physicians.

6.2 Augmenting Provider Supply with NPs

While our analysis up to this point has centered on substituting physicians with NPs, much of the policy motivation for hiring NPs has been to augment provider supply. When physician capacity is limited, it may nevertheless be efficient to hire additional NPs to improve throughput and reduce wait times. To examine this concept, we consider the trade-off between reducing wait times and worsening resource use—measured by ED length of stay and total cost per case—induced by additional NPs. As overcrowding is a significant issue in EDs, wait time has been an important object of attention for policymakers and ED management alike (e.g., Institute of Medicine 2006; American College of Emergency Physicians 2016).

In this auxiliary analysis, we hold fixed the number of cases arriving and the number of physicians on duty, and ask how additional NPs may affect patient wait time and downstream outcomes. We use the following empirical design:

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preventable hospitalization reported in Section 4 with the average number of 30-day preventable hospitalizations conditional on having any and the cost estimate of $19,220 per hospital stay. As this cost estimation focuses on preventable hospitalization effects within 30 days of the ED visit, the extra cost estimated can be viewed as that in the 30 days of the ED visit. To the extent that the cost-increasing effect of NPs may accrue over time and extend into other dimensions of post-ED care, the extra spending with using NPs may be larger than the estimate reported above.

[39]For this back-of-the-envelope estimation, we divide the total number of cases in the 25 percent set by the average caseload of NPs in our sample, finding that 827 NPs would be needed for treating 25 percent of VHA's ED cases annually. We then multiply the number of NPs needed with the average wage of NPs reported by Bureau of Labor Statistics (2021*a*), yielding a total wage estimate of $94.7 million per year.

$$y_i = \sum_{n=0}^{N} \delta_n \times \mathbf{1}(Z_i = n) + N_i^c \gamma_1 + N_i^p \gamma_2 + \mathbf{T}_i \eta + \mathbf{X}_i \beta + \varepsilon_i. \tag{5}$$

$\mathbf{1}(Z_i = n)$ is an indicator for $n \in \{0, \dots, 5\}$ NPs being on duty at the ED on the day case i visits.[40] N_i^c and N_i^p are, respectively, the number of cases arriving and the number of physicians on duty at the ED on the day case i visits. We apply Equation (5) to several outcomes of interest: (i) wait time (i.e., the time between patient arrival at the ED and assignment to a provider); (ii) ED length of stay (i.e., the time between assignment to a provider and discharge from the ED); (iii) cost of ED care; (iv) hospital admission; and (v) 30-day preventable hospitalization.[41]

Figure 8 presents the trade-off based on estimates of $\{\delta_n\}_{n=0}^{5}$ in Equation (5) across outcomes. Panel A shows the trade-off between wait time and ED length of stay. The first dot plots δ_0 (i.e., no NP on duty) for wait time on the y-axis and δ_0 for length of stay on the x-axis. Each of the subsequent dots plots a pair of estimates corresponding to $n \in \{1, \dots, 5\}$ with the estimate for wait time on the y-axis and the estimate for ED length of stay on the x-axis. Panel B similarly plots the trade-off between wait time and total cost per case. To arrive at the latter, we apply a cost estimate of \$19,220 per VHA hospital stay, as in Section 6.1, and calculate the sum of the coefficients for the three cost-related outcomes: cost of ED care, the cost of hospital admission (for high-severity patients), and the cost of 30-day preventable hospitalization.

As shown in Figure 8, decreasing wait time by 30 minutes per case by hiring additional NPs would increase length of stay by 16 minutes per case (Panel A), as well as increase total expected medical spending by 15 percent, or about \$238 per case, not including the cost of additional NP salaries (Panel B). Including the wage costs of additional NPs would bring this figure to about \$300 per case.[42] This suggests that roughly four fifths of the additional spending to reduce wait time by hiring NPs comes from the lower productivity of NPs, while only one fifth comes from additional NP wage costs.

7 Productivity and Wage Distributions

Up to this point, we have focused on the question of the productivity difference between the professional classes of NPs and physicians. We show that this difference is large, likely even larger than the difference

[40]The maximum of n is 6, but only a small share of ED-days has 5 or 6 NPs on duty (Appendix Figure A.1). We therefore group $n = 5$ and $n = 6$ together.

[41]As shown in Section 5.1, we only observe significant effects of NPs on increasing hospital admission in the ED visit among severe cases, i.e., cases with a 3-digit ICD-10 diagnosis whose 30-day mortality is equal to or above the 95th percentile of the sample; thus, in calculating the extra cost due to increased hospital admission in the ED visit, we include only that incurred by the severe cases.

[42]For this wage cost estimation, we divide the yearly NP wage by the average number of cases an NP treats per year, and then multiply this figure by the number of additional NPs required.

in average wages between the professions, despite the average physician wage being double the average NP wage. In this section, we turn to the distribution of productivity and wages within professions. Motivated by growing evidence of important variation in productivity across providers within profession, we ask how variation in productivity within professions compares to the difference in productivity between professions, in the case of NPs versus physicians.[43] Using the detailed data of the VHA, we also ask how this variation in productivity relates to variation in wages within profession.

We operationalize our examination of productivity variation by focusing on a measure of total cost per case. Specifically, for each case, we aggregate the three main components of resource utilization in which we find significant NP effects, the same components we previously considered in Section 6: the cost of care at the ED, whether admitted to the hospital from the ED visit, and preventable hospitalizations in the 30 days after the ED discharge, where we multiply the latter two components by the average cost of a hospital stay. We then estimate provider effects on this measure of total cost associated with each ED visit. To account for the possibility that the treating provider is endogenous, we use a just-identified IV model that instruments for indicators for treating providers with indicators for on-duty providers in the ED-day cell of the patient's visit. Appendix A.4.1 describes details of the estimation, and shows that these instruments are strongly predictive of the treating providers but are independent of arriving patients' characteristics conditional on our baseline controls, supporting the validity of these instruments.

Appendix Table A.16 reports estimates of the variance of provider effects on medical spending associated with the ED visit defined above. Using a split-sample approach to account for measurement error resulting from the fact that provider effects are estimated on a finite sample, we find a variance of 0.045 for physicians and 0.048 for NPs (see Appendix A.4.2 for details of the split-sample approach). These estimates suggest large variation in provider effects: A one-standard-deviation costlier physician and NP increases medical spending associated with the ED visit by 21 and 22 percent per case, respectively, which are about three times of the average NP effect of 6.7 percent from the 2SLS model in Equations (1) and (2).

We then investigate the full distributions of provider effects on total cost per case applying a non-parametric empirical Bayes deconvolution approach adapted by Kline, Rose and Walters (2022) from Efron (2016). This approach extracts an estimate of the empirical Bayes prior distribution of population provider effects, using the estimated provider effects and associated standard errors from the just-identified IV model that instruments for treating providers with indicators for on-duty providers. We apply this procedure

[43] Doyle, Ewer and Wagner (2010) show differences in resource utilization decisions among physician trainees, potentially driven by human capital. Gowrisankaran, Joiner and Leger (2017) provide evidence of variation in diagnostic and treatment skill, and Silver (2021) examines returns to time spent on patients by ED physicians and variation in the physicians' productivity. Chan, Gentzkow and Yu (2022) demonstrate important variation in diagnostic skill in the setting of radiology.

separately for NPs and physicians and ensure that the difference between the means of the deconvolved distributions for NPs and physicians equals the NP effect from the 2SLS model in Equations (1) and (2) (i.e., 6.7 percent). Panel A of Figure 9 displays the deconvolved density of provider effects for NPs and physicians. The figure shows large variation in provider effects both within and across professions (i.e., NPs versus physicians). The deconvolved distributions imply that the probability that a randomly drawn NP is costlier than a randomly drawn physician is 62 percent, equivalent to the c-statistic of a receiver operating characteristic (ROC) curve in which underlying provider effects from the deconvolved distributions, if observed, were to be used to predict professional class (Appendix Figure A.7). Appendices A.4.3 and A.4.4 describe details of these estimations.

Separately, we characterize the full distributions of annualized provider wages, for NPs and physicians. Since we observe actual wage payments to providers, we do not estimate them in a regression framework, nor do we apply deconvolution to obtain a population prior distribution. Rather, we simply describe the empirical distributions of actual annualized wages.[44] In contrast to the distributions of provider effects on total spending per case, Panel B of Figure 9 shows virtually no overlap between the wage distributions for NPs and physicians. That is, to a much larger degree than for provider effects, wages are extremely predictive of professional class.

Finally, we explore the relationship between wages and our measure of provider productivity (i.e., provider effects on total spending per case). In previous sections, we have demonstrated a large difference in productivity between NPs and physicians, with economic implications possibly larger than the difference in wages between the two professions. Here, we compute the empirical Bayes posterior mean for each provider's effect on total spending per case, using the previously described just-identified IV coefficients for each provider and the deconvolved empirical Bayes prior distributions for NPs and physicians. Appendix Figure A.8 shows the results with two binned scatterplots—one for NPs and one for physicians—in the space of wages on the y-axis and empirical Bayes posterior means on the x-axis, residualizing both by ED indicators. We find no significant correlation between wages and productivity within each of the professions. Appendix A.4.5 provides further details of the estimation.

[44] For each provider, we access detailed payment records of the full-time equivalents and wages associated for each pay period between the years 2011 to 2020, inclusive. We convert these data to annualized provider wages by (i) inflation-adjusting payments in any year to corresponding payments in 2020 dollars, (ii) computing a per-hour wage by dividing the sum of (inflation-adjusted) payments by the sum of hours in an 80-hour pay week, where the VHA considers 80 hours to be one full-time-equivalent pay period, and (iii) multiplying this figure by 26 pay periods and 80 hours per pay period.

8 Conclusion

Professionals perform some of the most important tasks across a variety of economic sectors. In turn, professional groups play a central role in determining the division of professional labor, the process of selecting and training members of the profession from society at large, and the economic returns to working as a professional. However, very little is known empirically about the impact of qualitatively distinct professions on productivity. This is because, by their very nature, professions dominate areas of work and exclude other groups from providing the same types of work within their "jurisdictions" (Abbott 2014).

In this paper, we exploit a unique opportunity to study two starkly different classes of professionals—nurse practitioners (NPs) and physicians. While physicians have occupied a dominant position in society since the turn of the 20th century (Starr 1982), the rising demand for health care in an aging population and the limited supply of physicians have set the stage for the rise of NPs to challenge the monopoly of physicians for the independent provision of medical care. The professions of NPs and physicians differ widely in their incomes, years of training, selectivity, and social standing.

Yet the coexistence of these two classes of professionals for the provision of medical care raises questions about whether professional class matters for productivity. Equal productivity between NPs and physicians would imply that the process to become a physician is unnecessarily selective, that the additional years of medical training is wasteful from society's perspective, and that the two-fold higher salary of physicians reflects monopoly rents and perhaps institutions based on the mistaken concept that physicians are worth the higher price of their labor. On the other hand, lower NP productivity would suggest additional productivity costs in expanding the supply of labor to meet rising demand for health care. Productivity differences may also provide an opportunity to understand behavioral foundations of productivity, particularly in a complex environment such as the emergency department (ED), and implications for the optimal allocation of different classes of labor.

Our empirical setting allows us to study the quasi-experimental assignment of ED patients to NPs versus physicians in the Veterans Health Administration (VHA). Beginning in December 2016, the VHA directed its stations to allow full practice authority to NPs. We use the quasi-random arrival of patients at the ED between times that may differ in the availability of NPs on shift, which drives the probability of being treated by an NP versus a physician. Compared to physicians, NPs require greater lengths of stay to treat and incur higher ED costs in treating their patients. Despite higher resource utilization, NPs achieve worse patient outcomes, as measured by 30-day preventable hospitalizations. The productivity gap between NPs and physicians is higher for more complex patients; in the direction of optimal allocation, NPs are less likely to select riskier and more complex patients. We also shed light on behavioral foundations of lower productivity that are suggestive of human capital: NPs are more likely to gather external information from radiology studies and physician consults, suggesting compensation for less information that they can perceive on their own. Consistent with lower skill, NPs are less likely to prescribe drugs with potentially high errors of commission (i.e., opioids), while they are more likely to prescribe drugs with potentially high errors of omission (i.e., antibiotics). Finally, the outcomes gap between NPs and physicians seems to narrow as NPs gain more experience.

Even under the most conservative assumptions, we find that the resource costs implied by lower productivity outweigh any salary savings from hiring NPs, despite NP salaries that are half as much as physician salaries. This reflects the outsize importance of productivity in modern health care, in which the utilization of considerable resources rests on the judgment of workers. Nonetheless, our results suggest productivity differences within each of the professions that are even larger than the difference between professions. In a just-identified IV model of provider-specific effects on total spending per case, we estimate standard deviations of the population distributions of NP and physician effects that are both roughly three times that of the difference between professions. The distributions of NP and physician productivity—at least according to our measures of provider effects on total spending per case—substantially overlap, while there is almost no overlap in the wages of NPs and physicians. Furthermore, there is no significant correlation between wages and productivity within professions.

Considered together, our findings paint a nuanced picture of the role of professions in determining the productivity and wages of workers. While the intensive processes of professional selection and training may imply important productivity differences between professional classes, perhaps justifying sizable differences in wages, it appears likely that professional institutions are more effective at compressing wages rather than at standardizing productivity within professions. These relationships may derive from frictions in observing productivity in the labor marketplace (Acemoglu and Pischke 1998), where professions may provide an important function of certifying membership. Professional membership may sometimes be exclusive, as in the case of physicians, but it may nonetheless be a highly imperfect proxy for productivity.

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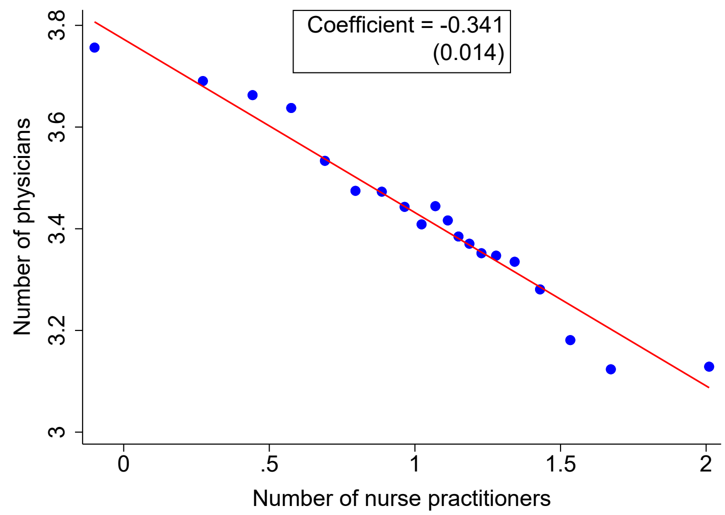
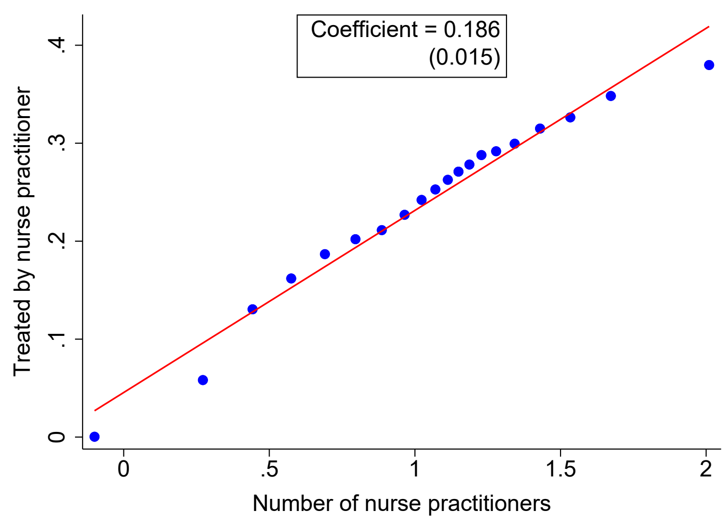
Figure 1: First Stage

A. Number of Physicians

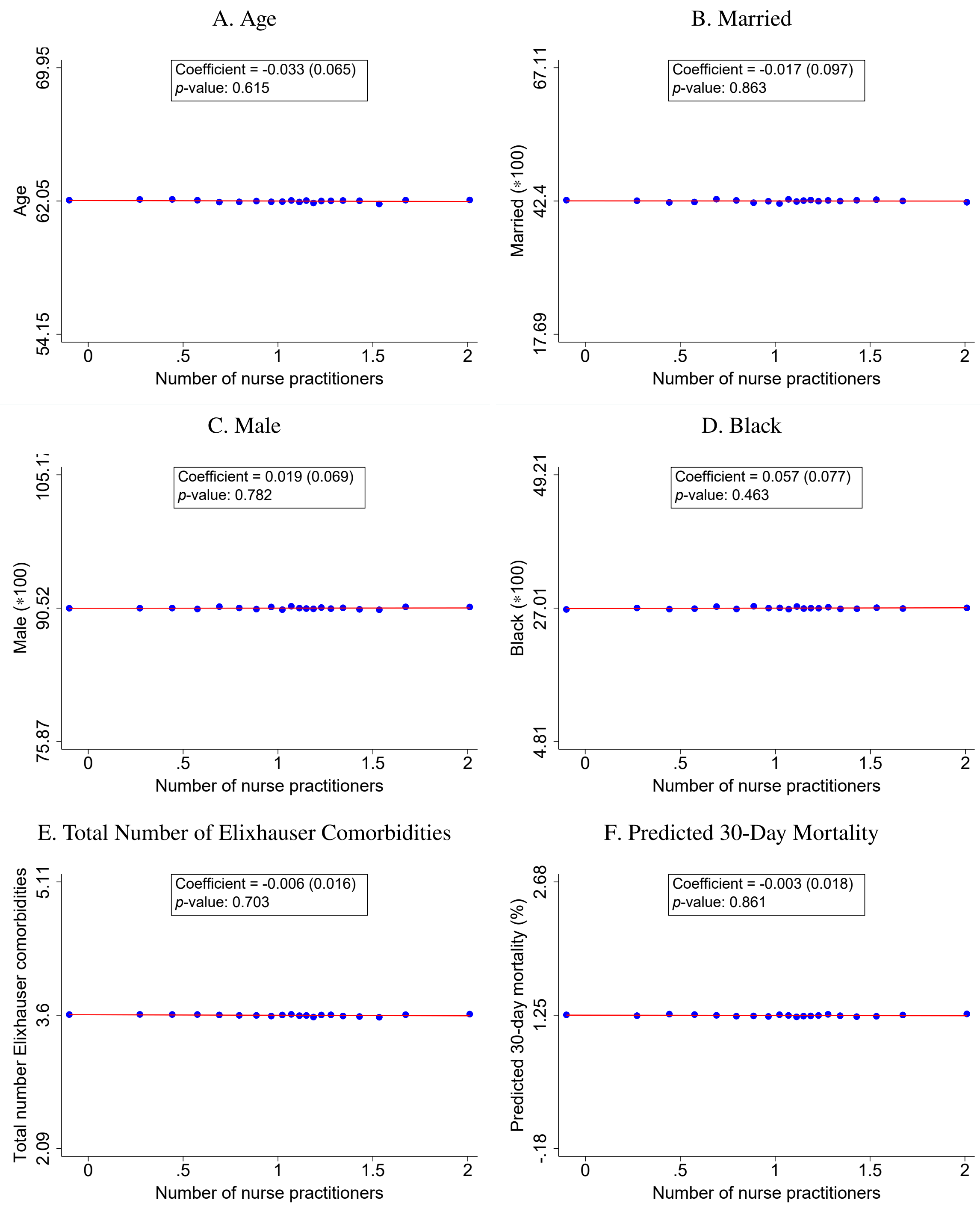


B. Treated by Nurse Practitioner



Notes: This figure represents a graphical illustration of the first-stage estimation. Panel A shows a binned scatterplot of the number of physicians on duty versus the number of NPs on duty. Panel B shows a binned scatterplot of whether the case is treated by an NP versus the number of NPs on duty. To construct these binned scatterplots, we first residualize both the *y*-axis and *x*-axis variable with respect to the baseline control vector (i.e., indicators for ED-year, ED-month, ED-day-of-the-week, and ED-hour-of-the-day) and then add means back for ease of interpretation. The coefficients report the estimated slope of the best-fit line between the *y*-axis and *x*-axis variable (conditional on the baseline control vector), with standard errors clustered by provider reported in parentheses.

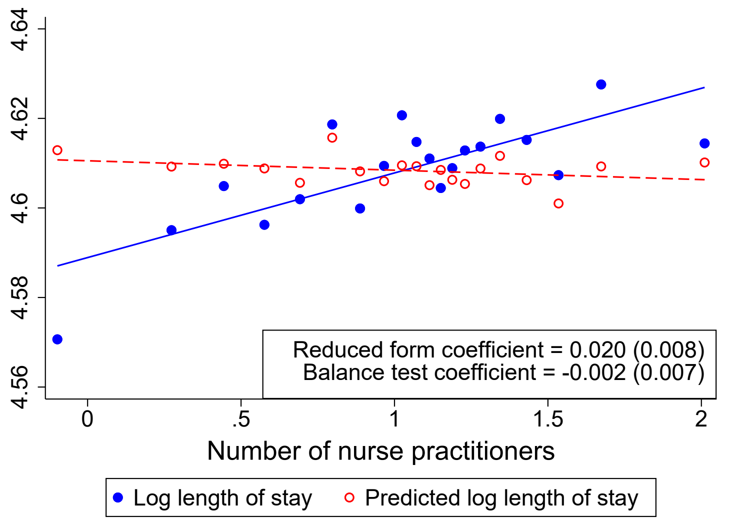
Figure 2: Balance in Patient Characteristics



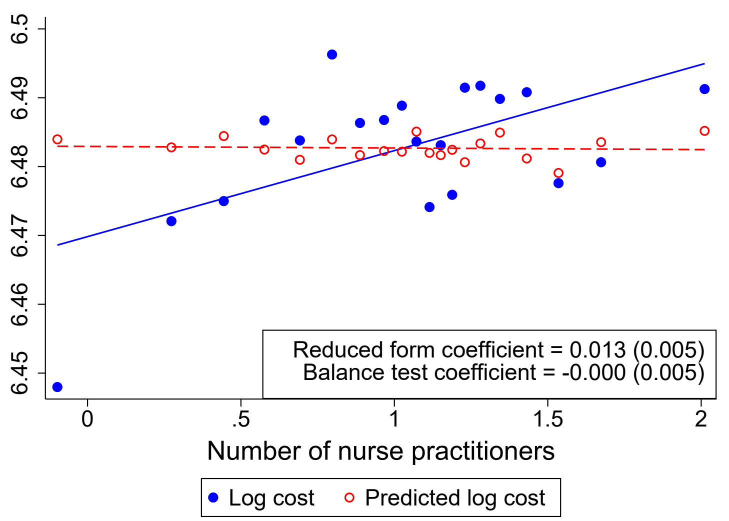
Notes: This figure shows balance in patient characteristics across the number of NPs on duty, conditional on the baseline control vector (i.e., indicators for ED-year, ED-month, ED-day-of-the-week, and ED-hour-of-the-day). To construct these binned scatterplots, we first residualize both the y-axis and x-axis variable with respect to the baseline control vector and then add means back for ease of interpretation. The middle number on the y-axis of each panel reports the mean of the sample; the top and bottom number report the mean plus and minus a half standard deviations, respectively. The coefficients report the estimated slope of the best-fit line between the y-axis and x-axis variable (conditional on the baseline control vector), with standard errors clustered by provider reported in parentheses. Each panel also reports p-values for the coefficient estimates. For readability of the coefficients, Panels B, C, and D scale up the dependent variable by 100. Predicted 30-day mortality is generated from a linear regression of actual 30-day mortality on patient characteristics $\mathbf{X}_i$ included in Equations (1) and (2), including demographics, comorbidities, prior health care use, vital signs, and 3-digit diagnosis indicators.

Figure 3: Reduced-Form and Balance

A. Log Length of Stay



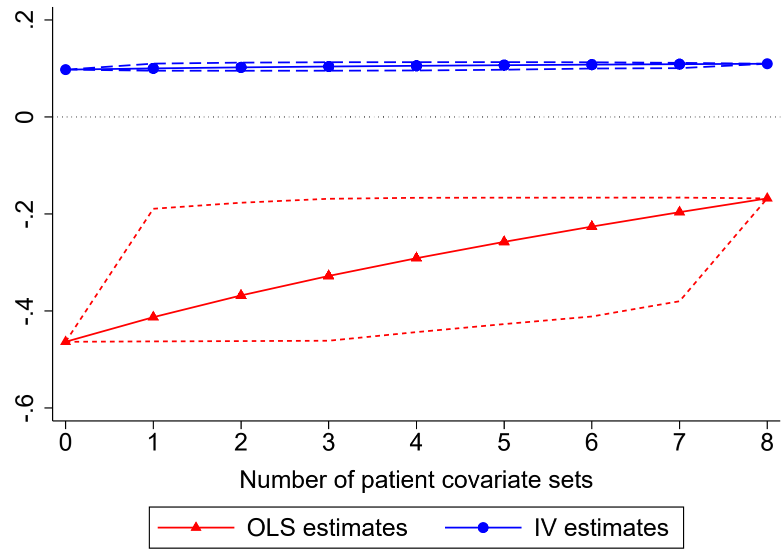
B. Log Cost



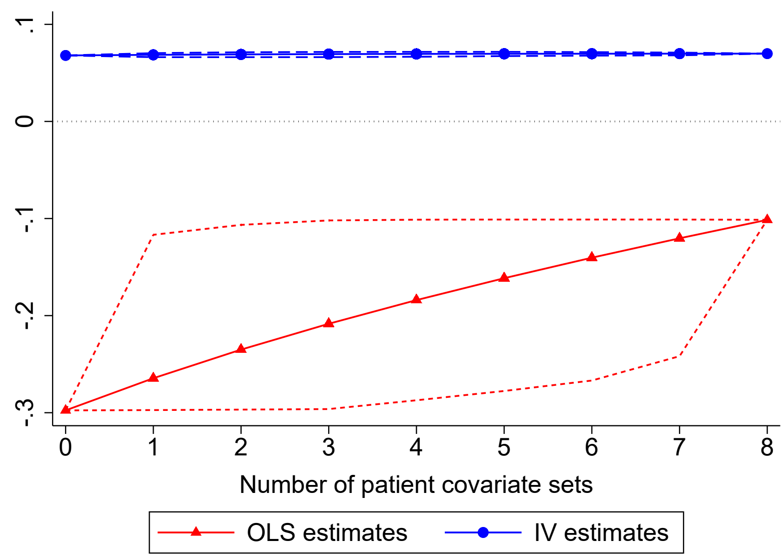
Notes: This figure shows binned scatterplots of patient actual and predicted outcomes on the *y*-axis versus the number of NPs on duty on the *x*-axis, controlling for the baseline control vector (i.e., indicators for ED-year, ED-month, ED-day-of-the-week, and ED-hour-of-the-day). Panel A reports results for log length of stay; Panel B reports results for log cost of the ED visit. The solid circles and lines represent patient actual outcomes. The hollow circles and dashed lines represent patient predicted outcomes based on patient characteristics $\mathbf{X}_i$ included in Equations (1) and (2), including patient demographics, comorbidities, prior health care use, vital signs, and 3-digit diagnosis indicators. The reduced-form coefficients are estimated using Equation (2), with patient actual outcomes as the dependent variable; the balance-test coefficients are estimated by regressing patient predicted outcomes on the number of NPs on duty, conditional on the baseline control vector. Standard errors clustered by provider are reported in parentheses.

Figure 4: Stability of OLS and IV Estimates

A. Log Length of Stay

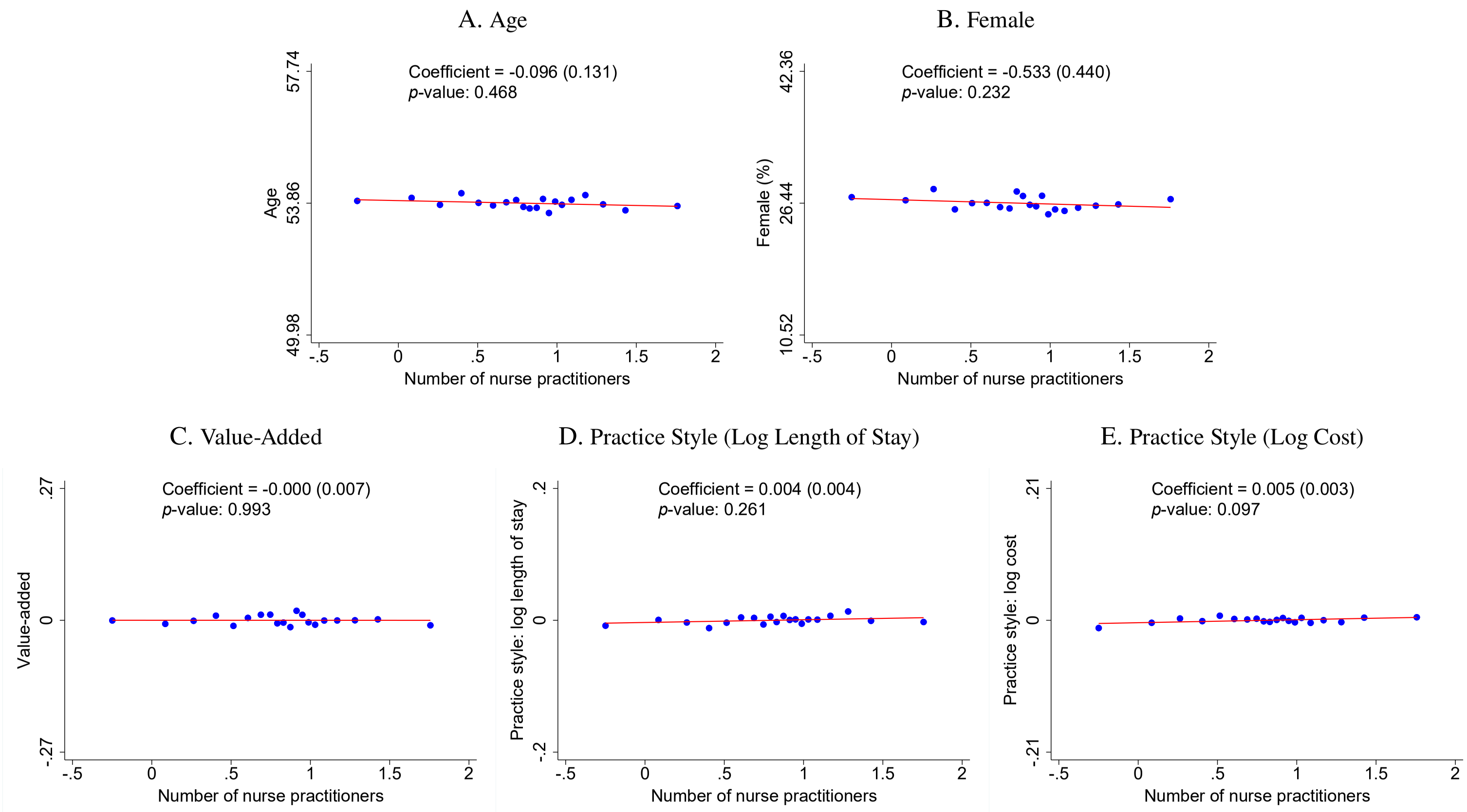


B. Log Cost



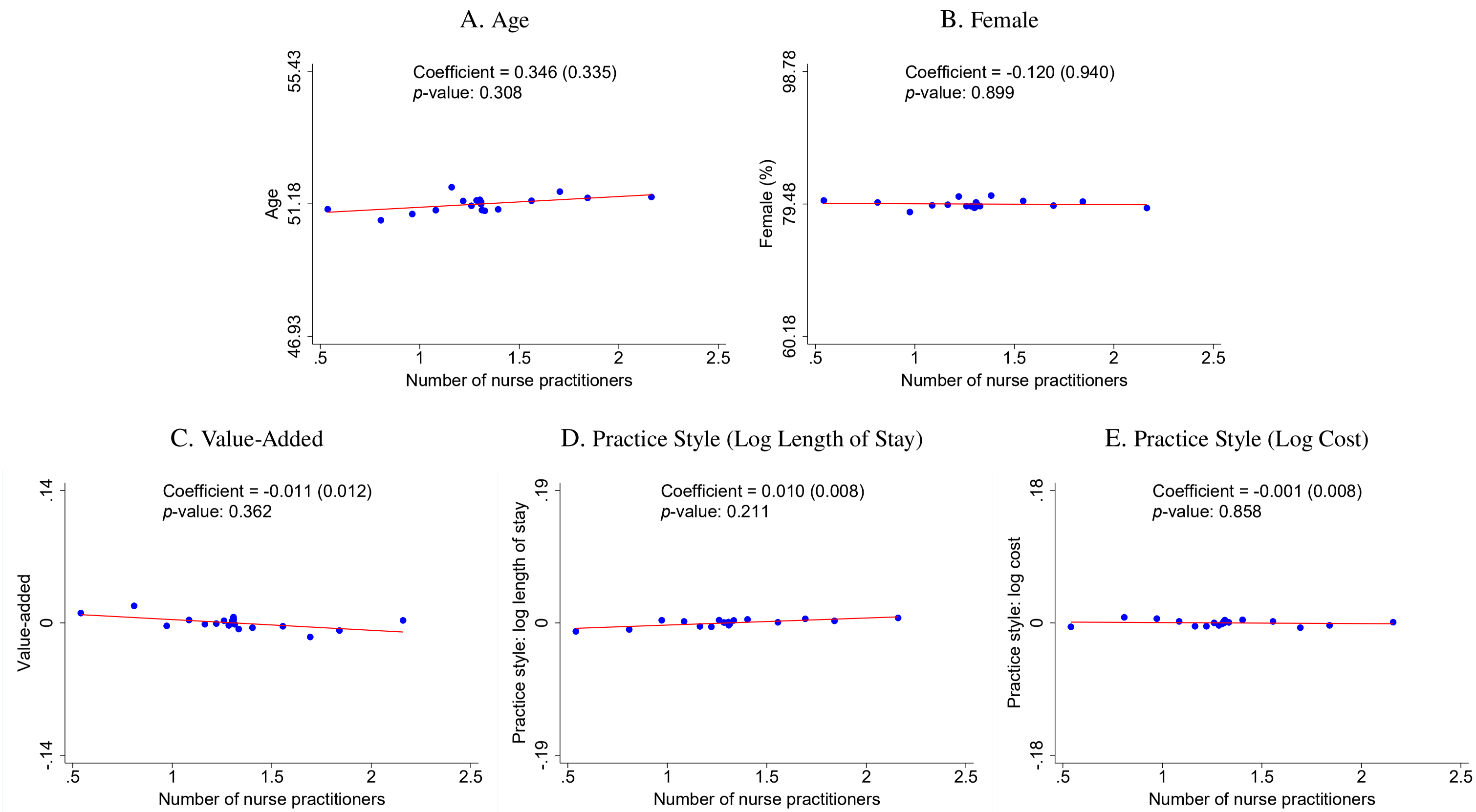
Notes: This figure shows the robustness of our OLS and IV estimates to the inclusion of different sets of patient controls. We divide patient observable characteristics into eight subsets: (i) five-year age-bin indicators; (ii) marital status; (iii) gender; (iv) race indicators; (v) indicators for 31 Elixhauser comorbidities; (vi) vital signs; (vii) prior health care use; and (viii) indicators for 3-digit patient primary diagnosis of the visit. We then run separate regressions that control for each of the $2^8 = 256$ different combinations of patient covariates for each outcome for both OLS and IV estimations. Each n on the x-axis indicates the number of covariate subsets included. For each n, we plot the maximum, mean, and minimum of the estimated coefficients for the effect of NPs using all C_8^n (i.e., selecting n distinct elements from a total of eight) possible combinations of patient controls. The connected triangles and circles show the mean of the estimated coefficients from OLS and IV regressions, respectively. The dashed lines connect the maximum and minimum of the estimated IV coefficients. The dotted lines connect the maximum and minimum of the estimated OLS coefficients. Panel A reports results for log length of stay. Panel B reports results for log cost of the ED visit.

Figure 5: Balance in Physician Characteristics



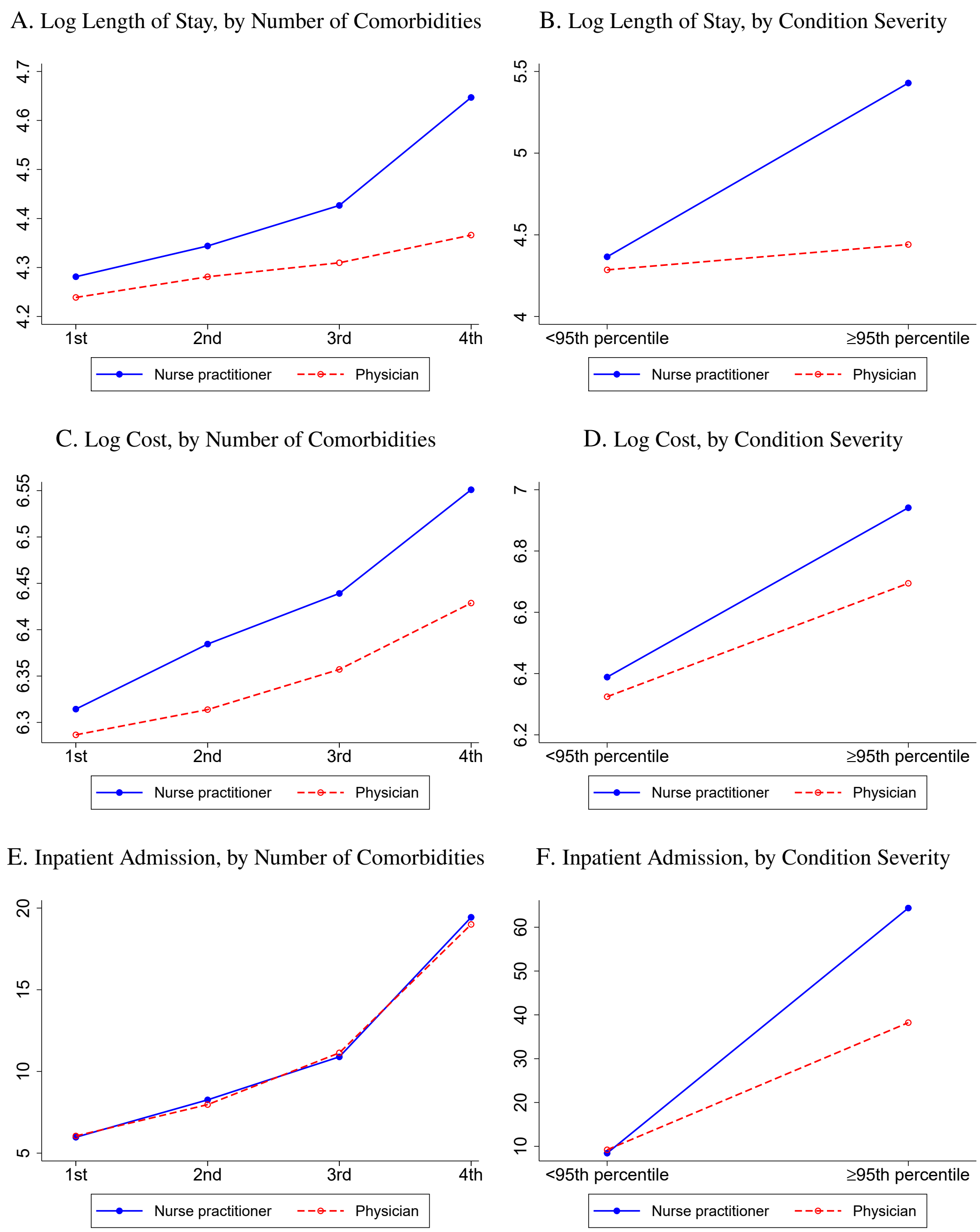
Notes: These panels are graphical representations of the balance-test regression at the ED-day level of physician average characteristics (weighted by the number of cases treated by each physician) on the number of NPs on duty, conditional on ED-year, ED-month, and ED-day-of-the-week indicators. Coefficients from the regressions are reported in each panel, along with standard errors (shown in parentheses) and p-values. To construct the binned scatterplots, we first residualize both the y-axis variable (average characteristics of physicians on duty) and the x-axis variable (the number of NPs on duty) with respect to indicators for ED-year, ED-month, and ED-day-of-the-week, and then add means back to aid in interpretation. The middle number on the y-axis of each panel reports the mean of the sample; the top and bottom number report the mean plus and minus a half standard deviations, respectively. The physician characteristics reported in Panels A-E are, respectively, age, gender, valued added, practice style based on patient log length of stay, practice style based on patient log cost of the ED visit. Construction details of value-added and practice style are described in Appendix A.3.

Figure 6: Balance in Nurse Practitioner Characteristics



Notes: These panels are graphical representations of the balance-test regression at the ED-day level of NP average characteristics (weighted by the number of cases treated by each NP) on the number of NPs on duty, conditional on ED-year, ED-month, and ED-day-of-the-week indicators. Coefficients from the regressions are reported in each panel, along with standard errors (shown in parentheses) and p-values. To construct the binned scatterplots, we first residualize both the y-axis variable (average characteristics of NPs on duty) and the x-axis variable (the number of NPs on duty) with respect to indicators for ED-year, ED-month, and ED-day-of-the-week, and then add means back to aid in interpretation. The middle number on the y-axis of each panel reports the mean of the sample; the top and bottom number report the mean plus and minus a half standard deviations, respectively. The NP characteristics reported in Panels A-E are, respectively, age, gender, valued added, practice style based on patient log length of stay, practice style based on patient log cost of the ED visit. Construction details of value-added and practice style are described in Appendix A.3.

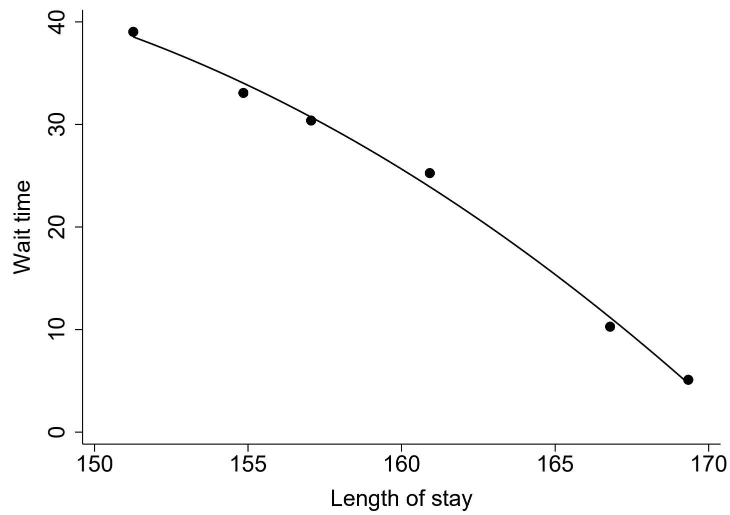
Figure 7: Heterogeneous Effects by Patient Health Risks



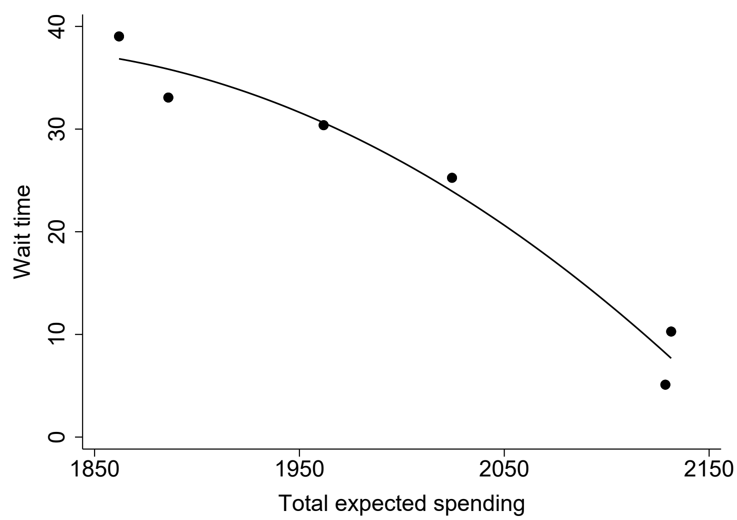
Notes: This figure shows heterogeneous effects of NPs by patient health risks. Panels A, C, and E divide cases into quartiles by their total number of Elixhauser comorbidities, with the highest quartile indicating the riskiest cases. Panels B, D, and F further divide cases by whether condition severity measured by 30-day mortality of cases with the same 3-digit ICD-10 primary diagnosis is equal to or above the 95th percentile of the sample. The solid and dashed lines show complier potential outcomes if they were treated by NPs and physicians, respectively. We estimate complier potential outcomes under NPs by the IV regression in Equation (1) replacing the dependent variable y_i with $y_i \times \text{NP}_i$, i.e., the interaction between patient outcome and the indicator for being treated by an NP. We estimate complier potential outcome under physicians by a similar IV regression with a dependent variable of $y_i \times (\text{NP}_i - 1)$, i.e., the interaction between patient outcome and the indicator for being treated by an NP minus one. Panels A-B, C-D, and E-F report results for log length of stay, log cost, and inpatient admission in the ED visit, respectively.

Figure 8: Trade-Off: Wait Time versus Length of Stay and Total Expected Spending

A. Wait Time versus Length of Stay



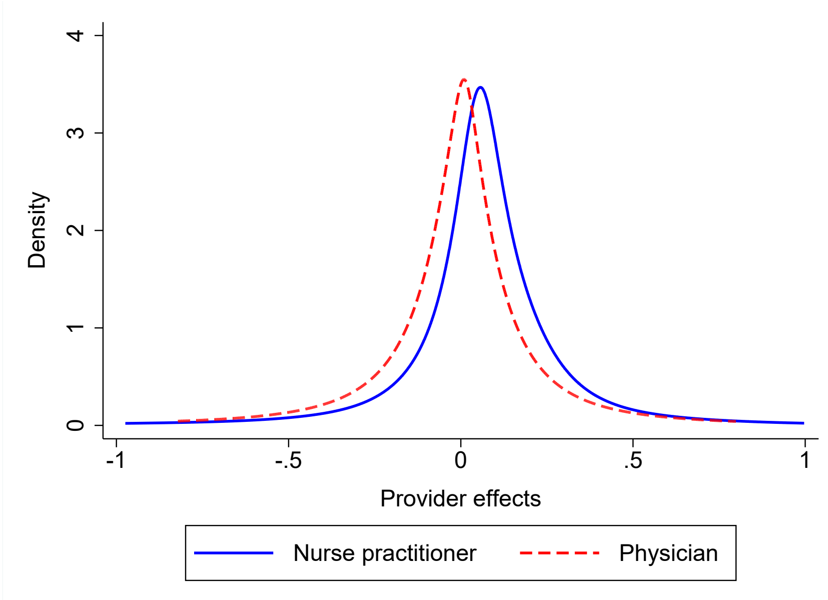
B. Wait Time versus Total Expected Spending



Notes: This figure shows changes in patient average wait time and other outcomes with incremental numbers of NPs on duty, holding fixed the number of physicians on duty and the number of cases arriving. Panel A presents the trade-off between wait time and length of stay. Panel B presents the trade-off between wait time and total expected spending. The estimates are generated using Equation (5) and represent average outcomes per ED visit. Total expected spending is the sum of the cost of care at the ED and expected spending as a result of hospital admission in the ED visit and preventable hospitalizations in the 30 days after the ED visit. The solid line shows the quadratic fit estimated on the plotted points.

Figure 9: Distribution of Provider Effects on Medical Spending and Provider Wages

A. Provider Effects on Medical Spending



B. Wages

Notes: Panel A reports the deconvolved distributions of provider effects on total spending associated with the ED visit, i.e., the sum of the cost of care at the ED, hospital admission in the ED visit, and preventable hospitalizations in the 30 days after the ED visit. See Appendix A.4.3 for details of the deconvolution estimator. The solid and dashed lines show the deconvolved distributions of NPs and physicians, respectively. Panel B plots histograms of provider wages observed in the VHA data. The white and gray bins show NPs' and physicians' wages, respectively. Wages are winsorized at the value of $450,000.

Table 1: Characteristics of Analysis Sample

	All	Treated by NPs	Treated by physicians	p-value
Age	62.05	60.72	62.46	0.00
	[15.80]	[15.87]	[15.75]	
Married	0.424	0.424	0.424	0.80
	[0.494]	[0.494]	[0.494]	
Male	0.905	0.904	0.906	0.00
	[0.293]	[0.295]	[0.292]	
Black	0.270	0.271	0.270	0.12
	[0.444]	[0.445]	[0.444]	
White	0.708	0.705	0.709	0.00
	[0.455]	[0.456]	[0.454]	
Asian/Pacific Islander	0.021	0.021	0.020	0.04
	[0.142]	[0.144]	[0.142]	
# outpatient visits in prior year	6.242	5.658	6.423	0.00
	[7.284]	[6.361]	[7.538]	
# inpatient stays in prior year	0.612	0.431	0.668	0.00
	[1.543]	[1.249]	[1.620]	
# Elixhauser comorbidities	3.599	3.190	3.726	0.00
	[3.018]	[2.772]	[3.079]	
Length of stay (minutes)	162.09	119.53	175.29	0.00
	[172.48]	[131.28]	[181.38]	
Cost ($, inflation adjusted to 2020)	938.99	813.16	978.12	0.00
	[1330.54]	[1009.67]	[1413.30]	
Inpatient admission (%)	16.625	7.866	19.340	0.00
	[37.230]	[26.921]	[39.497]	
30-day preventable hospitalization (%)	1.234	0.745	1.386	0.00
	[11.041]	[8.600]	[11.691]	
30-day mortality (%)	1.247	0.630	1.439	0.00
	[11.099]	[7.909]	[11.909]	
Observations	1,118,836	264,789	854,047	

Notes: Column 1 shows average characteristics of all cases in the analysis sample. Columns 2 and 3 show average characteristics of cases treated by NPs and physicians in the sample, respectively. Standard deviations are reported in brackets; p-values of t-tests for the equivalence of means between cases treated by NPs and by physicians are shown in the last column.

Table 2: Effect of Nurse Practitioners on Log Length of Stay and Log Cost

	Log length of stay			Log cost		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	Reduced form	IV	OLS	Reduced form	IV
Nurse practitioner	-0.168***		0.110**	-0.101***		0.070**
	(0.034)		(0.045)	(0.023)		(0.030)
Number of nurse practitioners		0.020***			0.013**	
		(0.008)			(0.005)	
Full control	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	4.61	4.61	4.61	6.48	6.48	6.48
S.D. dep. var.	1.16	1.16	1.16	0.88	0.88	0.88
Observations	1,110,798	1,110,798	1,110,798	1,108,961	1,108,961	1,108,961

Notes: This table shows OLS, reduced-form, and IV estimates of the effect of NPs on patient log length of stay and log cost of the ED visit. Columns 1 and 4 report the OLS estimates; Columns 2 and 5 report the reduced-form estimates; Columns 3 and 6 report the IV estimates. Sample sizes are smaller than that reported in Column 1 of Table 1 due to missing outcomes for a small number of cases. The set of full controls includes indicators for ED-year, ED-month, ED-day-of-the-week, and ED-hour-of-the-day, and patient characteristics that include five-year age-bin indicators, marital status, gender, race indicators (white, Black, and Asian/Pacific Islanders, with other racial categories omitted as the reference group), indicators for 31 Elixhauser comorbidities, prior health care use (the number of outpatient visits and the number of inpatient stays in any VHA facilities in the prior 365 days), vital signs, and indicators for 3-digit ICD-10 code of patient primary diagnosis of the visit. Standard errors clustered by provider are shown in parentheses. ***: $p \leq 0.01$; **: $p \leq 0.05$; *: $p \leq 0.1$.

Table 3: Effect of Nurse Practitioners on Additional Outcomes

	(1) Inpatient admission	(2) 30-day mortality	(3) 30-day preventable hospitalization
Reduced form	0.019	-0.021	0.047**
	(0.108)	(0.021)	(0.021)
IV estimate	0.103	-0.116	0.252**
	(0.585)	(0.115)	(0.120)
Full control	Yes	Yes	Yes
Mean dep. var.	16.62	1.25	1.23
S.D. dep. var.	37.23	11.10	11.04
Observations	1,118,836	1,118,836	1,118,836

Notes: This table shows reduced-form and IV estimates of the effect of NPs on various outcomes. Inpatient admission is an indicator for whether the patient is admitted to the hospital in the ED visit; 30-day mortality indicates whether the patient dies within 30 days of the ED visit; 30-day preventable hospitalization is defined as having any preventable hospitalization in the 30 days after the ED visit. All estimations include the full set of controls described in the notes to Table 2. Standard errors clustered by provider are shown in parentheses. ***: p≤0.01; **: p≤0.05; *: p≤0.1.

Table 4: Consults, Tests, and Prescriptions

	(1) Consult	(2) CT	(3) X-ray	(4) Opioids	(5) Antibiotics
Nurse practitioner	0.026***	0.012*	0.020**	-0.018***	0.040*
	(0.009)	(0.007)	(0.009)	(0.006)	(0.022)
Full control	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.23	0.15	0.29	0.09	0.64
S.D. dep. var.	0.42	0.35	0.45	0.28	0.48
Observations	1,118,836	1,118,836	1,118,836	1,118,836	123,395

Notes: This table shows IV estimates of the effect of NPs on various outcomes. The outcomes in Columns 1-5 are whether the patient receives in the ED visit, respectively, formal consults, CT scans, X-rays, opioid prescriptions, and antibiotic prescriptions. Since antibiotics generally only apply to patients with infections, Column 5 restricts the sample to patients with respiratory or genitourinary system infections, which are two common types of infections. All estimations include the full set of controls described in the notes to Table 2. Standard errors clustered by provider are shown in parentheses. ***: $p \leq 0.01$; **: $p \leq 0.05$; *: $p \leq 0.1$.

Table 5: Heterogeneous Effects by Provider Experience

	(1) Log LOS	(2) Log cost	(3) Inpatient admission	(4) 30-day mortality	(5) 30-day preventable hospitalization	(6) Consult	(7) CT	(8) X-ray
Panel A. By specific experience in the case's condition								
Nurse practitioner	0.101**	0.072**	0.092	-0.115	0.255**	0.025***	0.011	0.019**
	(0.044)	(0.030)	(0.579)	(0.116)	(0.121)	(0.009)	(0.007)	(0.009)
Nurse practitioner*specific experience	-0.058**	-0.042**	-0.504	-0.001	0.016	-0.014*	-0.010***	-0.003
	(0.025)	(0.019)	(0.331)	(0.041)	(0.030)	(0.008)	(0.003)	(0.008)
Panel B. By general experience in all conditions								
Nurse practitioner	0.086**	0.062**	0.089	-0.100	0.255**	0.022**	0.010	0.018*
	(0.043)	(0.029)	(0.608)	(0.116)	(0.121)	(0.009)	(0.007)	(0.009)
Nurse practitioner*general experience	-0.103*	-0.035	0.340	0.088	0.043	-0.020*	-0.008	-0.012
	(0.056)	(0.033)	(1.048)	(0.093)	(0.068)	(0.011)	(0.010)	(0.010)
Full control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	4.61	6.48	16.62	1.25	1.23	0.23	0.15	0.29
S.D. dep. var.	1.16	0.88	37.23	11.10	11.04	0.42	0.35	0.45
Observations	1,110,798	1,108,961	1,118,836	1,118,836	1,118,836	1,118,836	1,118,836	1,118,836

Notes: Panel A shows heterogeneous effects of NPs by provider specific experience in the case's condition, measured as the number of cases with the same 3-digit primary diagnosis as the current case the provider has treated since the start of the study period to the day before the current case's visit. Panel B shows heterogeneous effects of NPs by provider general experience, measured as the number of cases the provider has treated since the start of the study period to the day before the current case's visit. For ease of interpretation, both specific and general experience are standardized to have a mean of zero and a standard deviation of one for NPs and physicians separately. The outcome variables in Columns 1-8 are, respectively, log length of stay, log cost of the ED visit, hospital admission in the ED visit, 30-day mortality, 30-day preventable hospitalization, whether the patient receives consults in the ED visit, whether the patient receives CT scans in the ED visit, and whether the patient receives X-rays in the ED visit. All estimations include the full set of controls described in the notes to Table 2. Standard errors clustered by provider are shown in parentheses. ***: p≤0.01; **: p≤0.05; *: p≤0.1.

Online Appendix

A.1 Diagnosis Coding: NPs versus Physicians

In this appendix, we explore whether NPs and physicians are significantly different in reporting 3-digit ICD-10 diagnoses. All diagnoses in our data are coded in ICD-10 within our study period from January 2017 to January 2020. As OLS estimation is likely to be confounded by patient selection, we leverage IV regressions that instrument for whether a case is treated by an NP using the number of NPs on duty. Specifically, we first create indicators for each of the 836 different 3-digit ICD-10 primary diagnoses in our data (including one for the missing category). Then for each diagnosis indicator, we run a separate 2SLS regression as follows to estimate whether NPs and physicians are significantly different in reporting the diagnosis:

$$y_i = \delta \mathrm{NP}_i + \mathbf{T}_i \eta + \varepsilon_i, \tag{A.1}$$

$$\mathrm{NP}_i = \lambda Z_i + \mathbf{T}_i \zeta + \nu_i, \tag{A.2}$$

where, similar to Equations (1) and (2), NP_i indicates whether case i is treated by an NP and Z_i denotes the instrument (i.e., the total number of NPs on duty between 8 a.m. and 6 p.m., our analysis time window, at the ED on the day case i visits). $\mathbf{T}_i$ are indicators for ED-by-year, ED-by-month, ED-by-day-of-the-week, and ED-by-hour-of-the-day. The coefficient of interest is δ. As with the main specification, we cluster standard errors by provider.

Panel A of Appendix Figure A.9 plots the distribution of t-statistics for the estimated δ coefficients from the 836 separate regressions that use each 3-digit diagnosis indicator as the outcome variable. The share of t-statistics indicating a p-value below or equal to 0.05 is only 0.07, close to the null hypothesis of no differential 3-digit diagnosis coding between NPs and physicians (i.e., share 0.05 of t-statistics indicating a p-value below or equal to 0.05). Both the Shapiro-Wilk normality test and the test for normality on the basis of skewness and kurtosis suggest that we cannot reject the null hypothesis that the t-statistics are normally distributed, at least at the 10% level. Panel B of Appendix Figure A.9 further plots t-statistics against the prevalence of the 3-digit diagnosis among physicians, showing that NPs are not more likely to report diagnoses that are more (or less) common.[1]

The pattern of similar 3-digit diagnosis coding between NPs and physicians may arise, despite the possibility that NPs may be less capable of diagnosing and treating patients, for the relatively more straightforward patients who are compliers. For these patients, with the aid of additional consults and diagnostics (Section 5.2), NPs may be able to reach the same 3-digit diagnosis as physicians. Perhaps also worth noting, VHA ED provider reimbursements are independent of patient diagnoses, and NPs and physicians are unlikely to have differential financial incentives in diagnosis coding.

[1] We measure the prevalence of the diagnosis as the share of cases with the diagnosis among cases treated by physicians on days without any NP, to restrict potential influences of patient sorting between NPs and physicians.

A.2 Characterizing Compliers, Never-Takers, and Always-Takers

This appendix describes estimation of characteristics of compliers, never-takers, and always-takers. Following the approach developed by Abadie (2003), we characterize compliers by δ estimated through the 2SLS model specified in Equations (A.1) and (A.2), replacing the outcome variable y_i with $x_i \times \text{NP}_i$, i.e., the interaction between each patient characteristic x_i and the indicator for being treated by an NP. Results are discussed in Section 4.3 and shown in Columns 2-3 of Appendix Table A.2.

To estimate characteristics of never-takers and always-takers, we follow a method by Dahl, Kostøl and Mogstad (2014). We first collapse the data to the ED-day level. We then residualize the share of cases treated by NPs by indicators for ED-by-year, ED-by-month, and ED-by-day-of-the-week. We define never-takers as cases treated by physicians in ED-day cells with the residual share of cases treated by NPs at least as high as the 90th percentile of ED-days with at least one case treated by NPs. We define always-takers as cases treated by NPs in ED-day cells with the residual share of cases treated by NPs equal to or below the 10th percentile of ED-days with at least one case treated by NPs.

Columns 4 and 6 of Appendix Table A.2 report the average characteristics of never-takers and always-takers. For each characteristic, we compute the mean of never-takers and the mean of always-takers. As with compliers, we report the ratio between these means and the overall sample mean. We estimate standard errors for these means by bootstrap, blocking observations by provider with 500 replications. In line with the notion that NPs treat healthier cases than physicians do, Appendix Table A.2 shows that never-takers are the riskiest, followed by the overall sample, compliers, and finally, always-takers. For example, the total number of Elixhauser comorbidities among never-takers, the overall sample, compliers, and always-takers are, respectively, 3.9, 3.6, 3.3, and 3.2; the average predicted 30-day mortality among these four types of cases are, respectively, 1.6, 1.2, 0.9, and 0.6 percentage points.

A.3 Provider Value-Added and Practice-Style Measures

This appendix describes our construction of measures of provider value-added and practice styles, used to examine the exclusion restriction in Section 4.4. We consider physician value-added as a measure of risk-adjusted mortality outcomes and form these measures using leave-out data. Specifically, for physician p on day d, we measure

$$A_d^p = \frac{\sum_{i \in \mathcal{I}_p} \mathbf{1}(d(i) \neq d, Z_i = 0)\tilde{Y}_i}{\sum_{i \in \mathcal{I}_p} \mathbf{1}(d(i) \neq d, Z_i = 0)}, \tag{A.3}$$

where $\tilde{Y}_i$ is risk-adjusted 30-day mortality, or the difference between patient actual and predicted 30-day mortality. To deal with potential finite-sample bias, we leave out cases visiting on day d.[2] We also leave out cases visiting on days with any NPs on duty, to mitigate the concern on patient sorting between NPs and physicians.

[2]Specifically, there may be ED-day level shocks that are correlated with both the number of NPs on duty and the set of patients treated by a specific physician; these shocks can be influential in estimations with a finite sample.

Still, since cases are not experimentally assigned to physicians, A_d^p may reflect both a provider's effect on patient outcomes and systematic patient-physician sorting under imperfect risk adjustment. As one way to assess the degree of such potential biases, we investigate the robustness of physician value-added estimates to patient predicted mortality constructed on the basis of different risk adjusters, analogous to the test of student sorting biases in the teacher value-added literature (e.g., Kane and Staiger 2008; Chetty, Friedman and Rockoff 2014). If patient sorting is important, the estimated physician value-added will change meaningfully with the addition of risk adjusters. Alternatively, our estimates would remain stable. Appendix Figure A.10 shows that physician value-added estimates are stable regardless of patient risk adjusters. We compare physician value-added measures constructed using (i) the most parsimonious set of risk adjusters that includes only age-bin and 3-digit primary-diagnosis indicators, (ii) the less parsimonious set that adds non-age demographics (gender, race, and marital status), and (iii) the set that further adds dummies for 31 Elixhauser comorbidities, with the baseline physician value-added constructed using the full set of patient covariates (i.e., demographics, Elixhauser comorbidities, prior health care use, vital signs, and 3-digit diagnosis indicators). The correlations between measures (i)–(iii) and the baseline measure are all above 0.99. Note that, these risk adjusters are important predictors of patient 30-day mortality: They alone explain 7 percent of the variation in 30-day mortality, with an F-statistic of 88 for joint significance. Note that the robustness of physician value-added implies limited patient sorting within the physician group, not necessarily limited patient sorting between the physician and the NP group, which we show evidence against in Table 1 and Figure 4.

We consider physician practice styles as measures of physician-chosen inputs to care. Specifically, we define practice style measures by Equation (A.3), but instead set $\tilde{Y}_i$ as the difference between patient actual and predicted log length of stay or log cost of the ED visit. As with value-added, we show the robustness of practice style estimates to different patient risk adjusters in Appendix Figure A.10.

We construct similar measures of value added and practice style for NPs. As with physicians, we show the robustness of these estimates to different patient risk adjusters. Appendix Figure A.10 shows that NP valued-added and practice-style estimates are highly stable among those constructed using (i) the most parsimonious set of risk adjusters that includes only age-bin and 3-digit primary-diagnosis indicators, (ii) the less parsimonious set that adds non-age demographics (gender, race, and marital status), (iii) the set that additionally includes dummies for 31 Elixhauser comorbidities, and (iv) the full set that further adds detailed controls for prior health care use and vital signs upon arrival at the ED.

A.4 Distribution of Provider Effects on Total Spending

In this appendix, we estimate the distribution of provider effects on log total spending associated with the ED visit. We start by identifying provider effects using a just-identified IV model. Next, we estimate the variance of provider effects, using a split-sample approach to account for the bias due to sampling error in the estimated provider effects. We then apply an Empirical Bayes deconvolution method, following Efron (2016) and Kline, Rose and Walters (2022), to recover the underlying population distribution of provider effects.

A.4.1 Estimating Provider Effects

We generate a measure of total spending associated with the ED visit: the sum of the three main components of costs that we find significant NP effects—the cost of care at the ED, whether admitted to the hospital during the ED visit multiplied by the average cost per VHA hospitalization \$19,220, and preventable hospitalizations in the 30 days after the ED discharge multiplied by \$19,220.

We then estimate provider effects on total spending associated with the ED visit. To mitigate the effect of extreme values, we take the log of the medical spending. To account for the possibility that the treating provider is endogenous, we instrument for indicators for treating providers with indicators for on-duty providers in the ED-day cell of the patient's visit. The empirical specification is a just-identified 2SLS model as follows:

$$y_i = \sum_j \theta_j \mathbf{1}_{\{j(i)=j\}} + \mathbf{T}_i \eta + \mathbf{X}_i \beta + \varepsilon_i, \tag{A.4}$$

$$\mathbf{1}_{\{j(i)=j\}} = \sum_j \lambda_j \mathbf{1}_{\{j \in \mathcal{I}_i\}} + \mathbf{T}_i \zeta + \mathbf{X}_i \gamma + \nu_i. \tag{A.5}$$

$\mathbf{1}_{\{j(i)=j\}}$ is an indicator for whether case i is treated by provider j, and $\mathcal{I}_i$ is the set of providers on duty in the ED-day cell of case i's visit. The coefficients of interest are θ_j, representing provider effects. Since θ_j is only identified relative to one another for providers within the same ED, we make the natural normalization that the case-weighted mean of θ_j is 0 within each ED, using linear constraints in the 2SLS estimation to yield valid standard errors.

The F-statistics for the joint significance of on-duty provider indicators in the first-stage regressions, i.e., Equation (A.5), have a share of 0.99 and 0.68 above 10 and 100, respectively, suggesting that provider availability is strongly predictive of the treating provider.[3] Appendix Figure A.11 shows that patient characteristics are well balanced across the average characteristics (age, gender, and practice style) of on-duty providers, conditional on the baseline controls, i.e., ED-by-time-category indicators.[4] In addition, the F-statistics for the joint significance of on-duty provider indicators from regressions of patient predicted log length of stay and predicted log cost of the ED visit on on-duty provider indicators conditioning on ED-by-time-category indicators, are 2.4 and 2.1, respectively. These are much smaller than the corresponding F-statistics using the actual log length of stay and log cost of the ED visit as the outcome—which are 10.3 and 9.9, respectively. These results make plausible the assumption that the set of on-duty providers is conditionally independent of the set of patients arriving, supporting the validity of our instruments.

A.4.2 Estimating Variance of Provider Effects

We estimate the variance of provider effects, within each professional class, on log total spending associated with the ED visit. The estimated provider effects $\hat{\theta}_j$ from Appendix A.4.1 yields a case-weighted variance of

[3] Since $\mathbf{1}_{\{j(i)=j\}}$ is always zero for patients outside of the ED a provider practices, we report F-statistics from the first stage regression in Equation (A.5) using observations in each ED separately.

[4] We compute case-weighted average characteristics of on-duty providers, with the index case left out. For practice style examined in this balance test, to deal with the concern on patient sorting between NPs and physicians, we use provider effects on patient log length of stay and log cost of the visit estimated by the 2SLS model in Equations (A.4) and (A.5).

0.054 for NPs, and 0.064 for physicians (see Appendix Table A.16).[5] However, these estimates are upward biased, due to sampling error resulting from the fact that provider effects are estimated on a finite sample. To account for such biases, we leverage a split-sample approach, resembling that employed in earlier studies (e.g., Chetty, Friedman and Rockoff 2014; Silver 2021). Specifically, we randomly split a provider's patients within each day to two approximately equal-sized partitions. We then estimate the 2SLS model in Equations (A.4) and (A.5) using each partition separately, yielding two fixed effect estimates for each provider $\hat{\theta}_{j,a}$ and $\hat{\theta}_{j,b}$. Suppressing the j subscript for simplicity, we have

$$\hat{\theta}_q = \theta + e_q, q \in \{a,\ b\},$$

where q indicates partitions, and e_q is partition-specific sampling error, such that $\text{Cov}(\theta, e_b) = \text{Cov}(e_a, \theta) = 0$. The random split of patients for each provider-day makes plausible the assumption that e_a and e_b are uncorrelated, i.e., $\text{Cov}(e_a, e_b) = 0$. We therefore can compute the variance of provider effects as the covariance of $\hat{\theta}_a$ and $\hat{\theta}_b$:

$$\begin{aligned}
\text{Cov}(\hat{\theta}_a, \hat{\theta}_b) &= \text{Cov}(\theta + e_a, \theta + e_b) \\
&= \text{Cov}(\theta, \theta) + \text{Cov}(\theta, e_b) + \text{Cov}(e_a, \theta) + \text{Cov}(e_a, e_b) \\
&= \text{Var}(\theta).
\end{aligned}$$

We perform this calculation for NPs and physicians separately.

Appendix Table A.16 reports the case-weighted variance of provider effects from the split-sample approach. The variance for physicians is estimated to be 0.045, which is about 70 percent of the calculated variance without accounting for the bias due to sampling error. The variance from the split-sample approach suggests that on average, a one-standard-deviation costlier physician increases medical spending associated with the ED visit by 21 percent per case. For NPs, the split-sample variance estimate is 0.048, suggesting that a one-standard-deviation costlier NP raises spending by 22 percent per case.

A.4.3 The Population Distribution of Provider Effects

We now estimate the distribution of provider effects by applying a non-parametric empirical Bayes deconvolution approach adapted by Kline, Rose and Walters (2022) from Efron (2016). This approach extracts a flexible estimate of the distribution of population provider effects using provider effects $\hat{\theta}_j$ and their standard errors s_j estimated in Equations (A.4) and (A.5). Assuming provider z-scores $z_j = \hat{\theta}_j / s_j$ are distributed as

$$z_j | c_j \sim \mathcal{N}(c_j, 1),\ c_j \sim G_c,$$

where $c_j = \theta_j / s_j$ (i.e., the population analogue of z_j), the procedure first applies the Efron (2016) deconvolution procedure to yield a distribution of provider z-scores $\hat{G}_c$ with density function $\hat{g}_c(\cdot)$. The Efron (2016) procedure estimates $\hat{G}_c$ by maximum likelihood of parameters that represent coefficients on a set of

[5] Since provider effects are normalized to have a mean of 0 in each ED, the variance is interpretable as the within-ED variance of provider effects.

splines, with a regularization parameter to tamp down excursions from a flat prior.

Next, assuming that s_j is independent of c_j, we can derive an estimate of the distribution of provider effects $\hat{F}$, with density function $\hat{f}(\cdot)$ for each value θ:

$$\hat{f}(\theta) = \frac{1}{J}\sum_{j=1}^{J}\frac{1}{s_j}\hat{g}_c(\theta/s_j). \tag{A.6}$$

Following Kline, Rose and Walters (2022), we assess the independence of z_j and s_j by reporting regressions of z_j on s_j. To account for possible correlated estimation errors in z_j and s_j, we also present split-sample versions of these regressions that randomly split cases for each provider into two approximately equal-sized partitions and regress z-scores from one partition on standard errors from the other partition. The results are reported in Appendix Table A.17, which show no significant relationship between z_j and s_j, suggesting that independence between z-scores and standard errors is plausible.

We apply the empirical Bayes deconvolution estimator to NPs and physicians separately.[6] As in Kline, Rose and Walters (2022), we calibrate the regularization parameter in the maximum likelihood estimation so that the variance of the deconvolved distribution of provider effects matches the corresponding split-sample variance estimates reported in Appendix Table A.16. We demean both the physician and NP distribution to have a mean of zero, and then shift the distribution of NPs to the right by 0.067, where 0.067 is the 2SLS estimate of the NP effect on the log total spending associated with the ED visit obtained by Equations (1) and (2). Panel A of Figure 9 displays the deconvolved density of provider effects for NPs and physicians.

Using the deconvolved density of NP and physician effects, we estimate the probability that a randomly drawn NP is costlier than a randomly drawn physician by

$$\Pr\left(\theta_j < \theta_{j'} \middle| j \in \mathcal{J}_{MD}, j' \in \mathcal{J}_{NP}\right) = \int_0^1 \hat{F}_{MD}(\theta)d\hat{F}_{NP}(\theta), \tag{A.7}$$

where $\hat{F}_{MD}(x)$ and $\hat{F}_{NP}(x)$ are the deconvolved cumulative density functions of physician effects and NP effects, respectively, and $\mathcal{J}_{MD}$ and $\mathcal{J}_{NP}$ are the sets of providers who are physicians and NPs, respectively. We find the probability that a randomly drawn NP is costlier than a randomly drawn physician, in terms of the total spending associated with the ED visit defined above, is 62 percent; put differently, the probability that a randomly drawn NP is less costly than a randomly drawn physician is as high as 38 percent.

A.4.4 ROC Curve Representation

The probability in Equation (A.7) is equivalent to the c-statistic, or area under the curve (AUC), of a receiver operating characteristic (ROC) curve. The ROC curve displays the performance of a classification exercise in which one were to classify providers by a certain characteristic. In the case of provider effects, the c-statistic of 0.62 indicates relatively poor performance in classifying providers as NPs versus physicians depending on their (true) provider effects from their respective deconvolved distribution.

[6] To restrict the inclusion of noisy δ_j, our deconvolution excludes providers with less than 150 cases. We set the support of provider effects to $[\delta^5 - SD, \delta^{95} + SD]$, where δ^5, δ^{95}, and SD are, respectively, the 5th percentile, 95th percentile, and standard deviation of estimated NP and physician effects for the NP and physician deconvolution, respectively.

We construct ROC curves, based on respective provider characteristics of productivity and wages, where we consider physicians as the "positive" class and NPs as the "negative" class. For each characteristic of productivity and wages, a provider with a higher value of the characteristic is more likely to be a physician (i.e., in the positive class). We define productivity as the additive inverse of the provider effect on log total spending: $\mu_j = -\theta_j$. For a given characteristic x, we plot the ROC curve with $1 - \hat{F}^x_{MD}$ (i.e., the true positive rate) on the y-axis and $1 - \hat{F}^x_{NP}$ (i.e., the false positive rate) on the x-axis, where $\hat{F}^x_{MD}$ and $\hat{F}^x_{NP}$ are the empirical cumulative distribution functions of x among physicians and NPs, respectively. For productivity, we use the deconvolved distributions previously described in Appendix A.4.3, noting that $\hat{F}^\mu_{MD} = 1 - \hat{F}^\theta_{MD}$ and $\hat{F}^\mu_{NP} = 1 - \hat{F}^\theta_{NP}$. For wages, we use the empirical cumulative distribution function based on the annualized full-time-equivalent ("yearly") wage of each provider j, inflation-adjusted to 2020 dollars.

We show both ROC curves in Appendix Figure A.7. As mentioned above, the c-statistic based on productivity is 0.62. The c-statistic based on wages is 0.99.

A.4.5 Correlation Between Productivity and Wages

Separately for NPs and physicians, we estimate the correlation between provider wages and productivity, measured (as an additive inverse) by provider effects on log total spending associated with the ED visit (i.e., θ_j), with the following regression:

$$\text{wage}_j = \alpha\tilde{\theta}_j + \mathbf{L}_j\pi + \varepsilon_j. \tag{A.8}$$

The dependent variable wage_j is the yearly wage of provider j (inflation-adjusted to 2020 dollars).[7] $\mathbf{L}_j$ is a vector of ED indicators since provider effects are only identified relative to one another within EDs. Since provider effects $\hat{\theta}_j$ is estimated with noise, we use empirical Bayes posteriors of provider effects, $\tilde{\theta}_j$, which we calculate as

$$\tilde{\theta}_j = w_j\hat{\theta}_j + (1 - w_j)\hat{\theta}. \tag{A.9}$$

where $w_j = \frac{\hat{\psi}^2}{s_j^2+\hat{\psi}^2}$ is the weight based on $\hat{\psi}^2$ and s_j^2, which are, respectively, the variance of the prior distribution of θ_j, estimated separately for NPs and physician in Appendix A.4.2, and the variance of the sampling error for each $\hat{\theta}_j$ calculated as the square of the standard error of $\hat{\theta}_j$. $\hat{\theta}$ is set to 0 for physicians, and 0.067 for NPs (i.e., the average NP effect estimated by the 2SLS model in Equations (1) and (2), using patient log total spending as the outcome).

[7] For each provider, we observe detailed payment records for each pay period from year 2011 to 2020. We convert these data to annualized provider wages by, first, inflation-adjusting payments in any year to corresponding payments in 2020 dollars; next, computing a per-hour wage by dividing the sum of (inflation-adjusted) payments by the sum of hours in an 80-hour pay week, where the VHA considers 80 hours to be one full-time-equivalent pay period; and finally, multiplying this figure by 26 pay periods and 80 hours per pay period.

A.5 ED-Specific NP Effects

In this appendix, we estimate heterogeneity in the ED-specific NP effect. In separate 2SLS regressions for each ED ℓ, we estimate the NP effect using only cases at that ED:

$$\begin{aligned} y_i &= \delta_\ell \text{NP}_i + \mathbf{t}_i \eta_\ell + \mathbf{X}_i \beta_\ell + \varepsilon_i, \\ \text{NP}_i &= \lambda_\ell Z_i + \mathbf{t}_i \zeta_\ell + \mathbf{X}_i \gamma_\ell + v_i, \end{aligned}$$

where $\mathbf{t}_i$ is a vector of indicators for patient arrival year, month, day of the week, and hour of the day.

In Appendix Figure A.12, we plot the distribution of $\hat{\delta}_\ell$ for all EDs in our sample. We also plot the empirical Bayes posteriors for all EDs, calculated as

$$\tilde{\delta}_\ell = w_\ell \hat{\delta}_\ell + (1 - w_\ell)\hat{\delta}. \tag{A.10}$$

The shrinkage factor is given by $w_\ell = \frac{\hat{\pi}^2}{s_\ell^2 + \hat{\pi}^2}$, where $\hat{\pi}^2$ and s_ℓ^2 are, respectively, the variance of the prior distribution of $\hat{\delta}_\ell$ and the variance of the sampling error for each $\hat{\delta}_\ell$. We calculate s_ℓ^2 as the square of the standard error of $\hat{\delta}_\ell$. We calculate $\hat{\pi}^2$ as the difference between the case-weighted variance of $\hat{\delta}_\ell$ and the case-weighted mean of s_ℓ^2. Finally, $\hat{\delta}$ is the overall IV estimate of the NP effect in Equations (1) and (2), which is reported in Section 4.

The solid lines in Appendix Figure A.12 plot the empirical Bayes posteriors for EDs in our sample.[8] The distribution of posteriors is more compressed than that of the raw estimates of ED-specific effects or the prior distribution, reflecting shrinkage due to sampling error in the raw estimates. The results show a fair amount of heterogeneity. Nonetheless, the large majority of EDs exhibit positive effects of NPs on raising patient length of stay, cost of the ED visit, and 30-day preventable hospitalization rate.

[8] The figure reports results for all EDs for log length of stay and log cost (in total 44 such EDs). For 30-day preventable hospitalization, since it is relatively uncommon (occurs in less than 2 percent of the sample), the estimates are relatively imprecise when based on observations from a specific ED, we thus include only EDs with at least 25,000 cases in the analysis sample (in total 20 such EDs).

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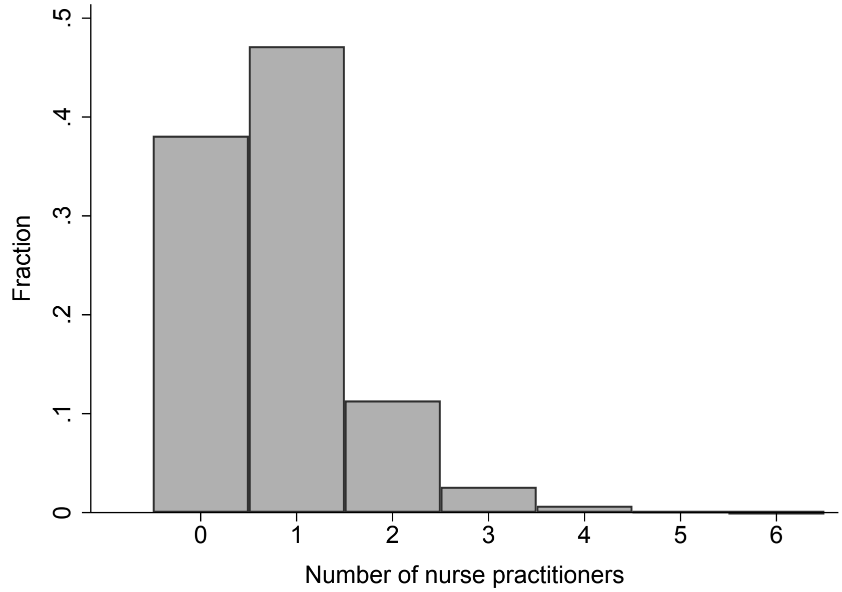
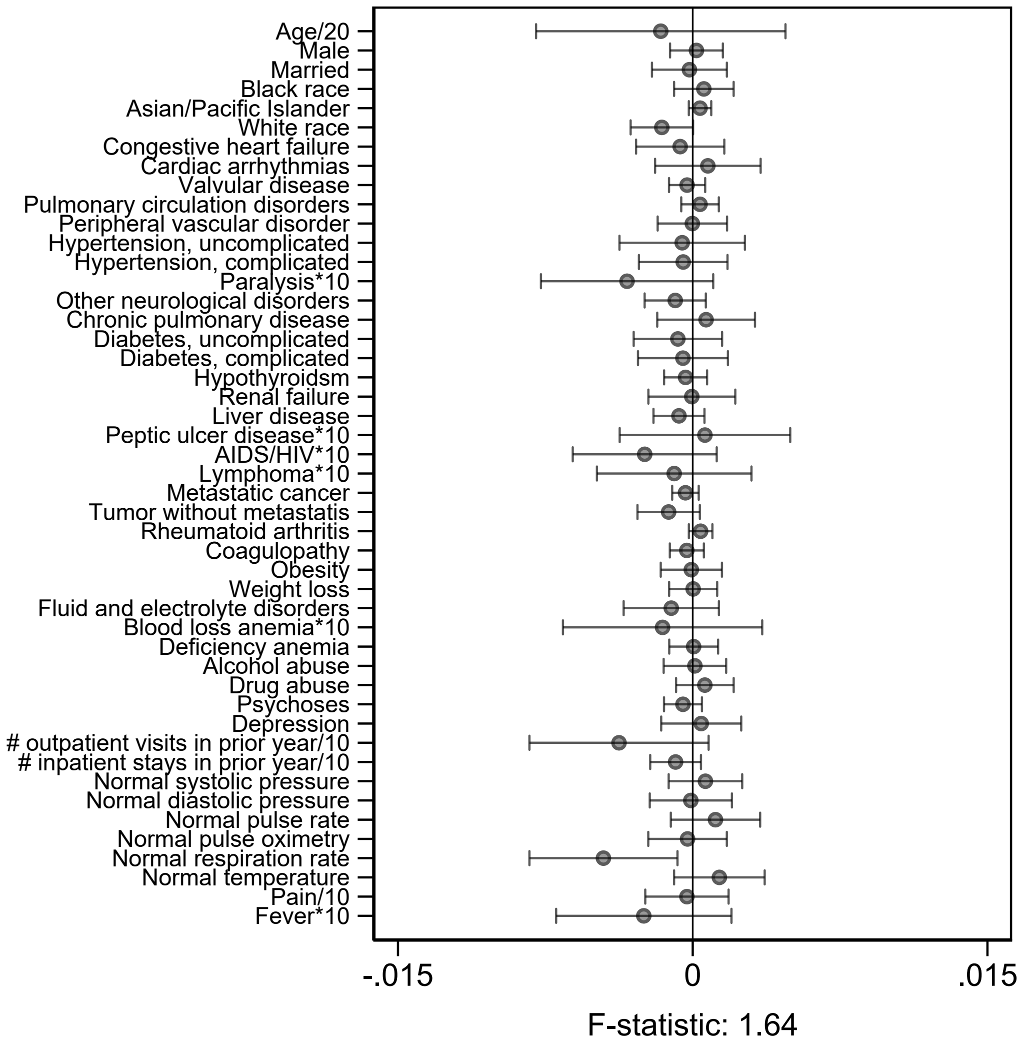
Figure A.1: Number of Nurse Practitioners on Duty



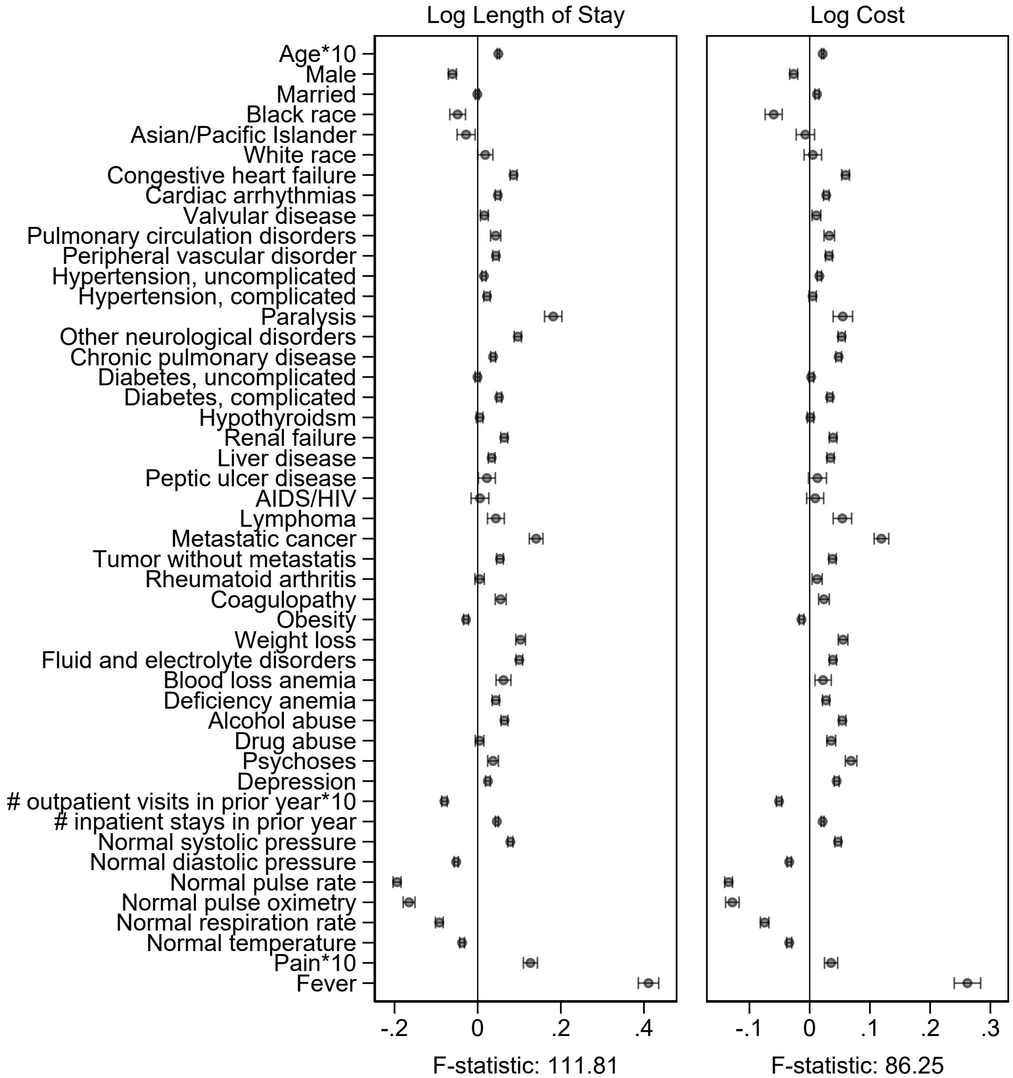
Notes: This figure shows the histogram of the number of NPs on duty in an ED-day cell. The unit of observation is at the ED-day level.

Figure A.2: Balance in Patient Characteristics



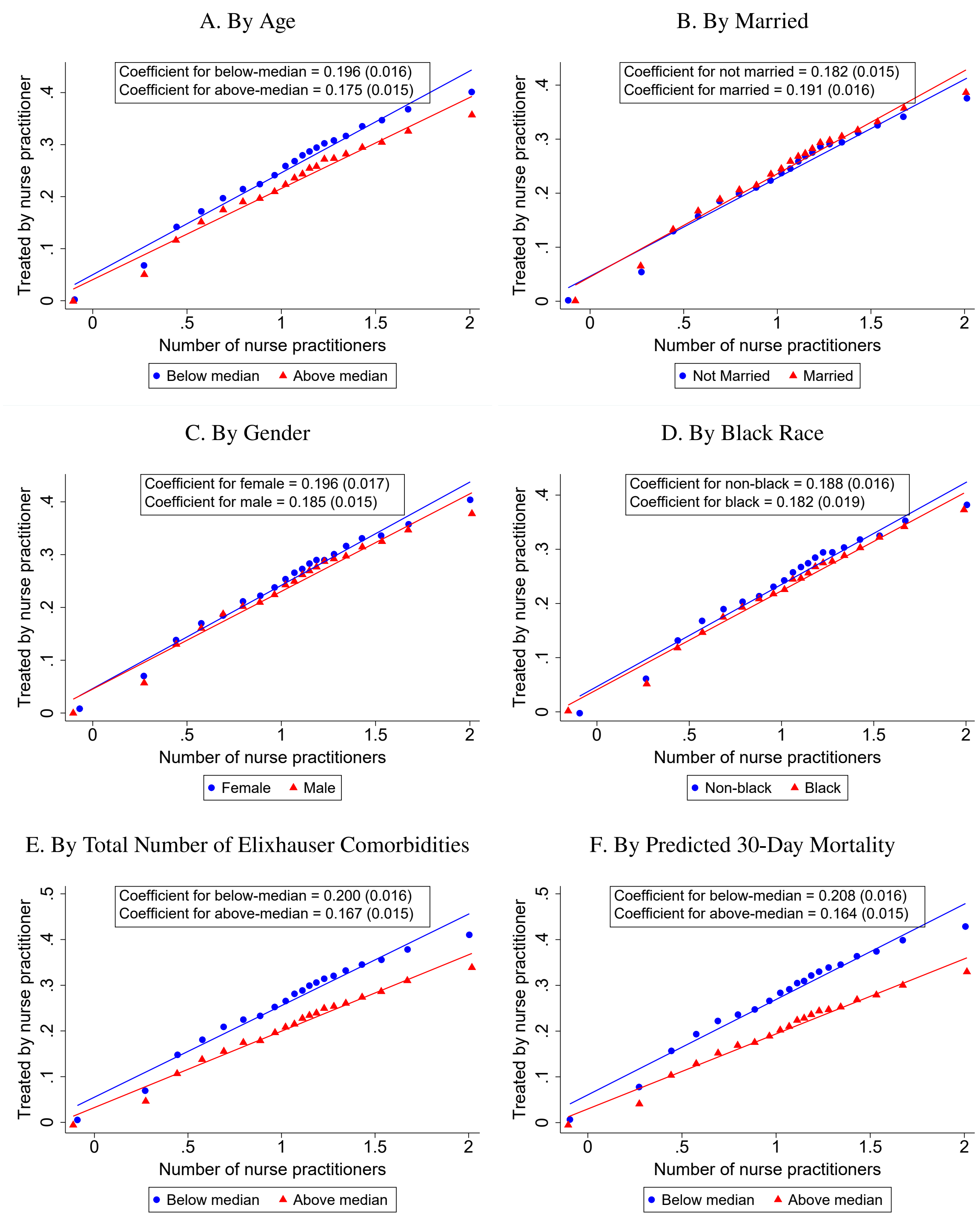
Notes: This figure shows estimated coefficients and 95% confidence intervals from regressions of each patient characteristic listed on the *y*-axis on the number of NPs on duty, controlling for the baseline control vector (i.e., indicators for ED-year, ED-month, ED-day-of-the-week, and ED-hour-of-the-day). For improved readability, a few coefficients (and their confidence intervals) are scaled up and down by, e.g., 10, as shown by "*10" and "/10" on the *y*-axis, respectively. At the bottom of the figure, we report the *F*-statistic from the joint F-test for all patient characteristics in a reverse regression with the number of NPs on duty as the dependent variable, conditioning on the baseline control vector. Standard errors are clustered by provider.

Figure A.3: Predicting Log Length of Stay and Log Cost



Notes: This figure shows estimated coefficients and 95% confidence intervals from regressions of patient log length of stay (Panel A) and log cost of the ED visit (Panel B) on patient characteristics, controlling for the baseline control vector (i.e., indicators for ED-year, ED-month, ED-day-of-the-week, and ED-hour-of-the-day). For improved readability, a few coefficients (and their confidence intervals) are scaled up by 10, as shown by "*10" on the *y*-axis. The bottom of each panel reports the *F*-statistic from the joint *F*-test of all patient characteristics, conditioning on the baseline control vector. Standard errors are clustered by provider.

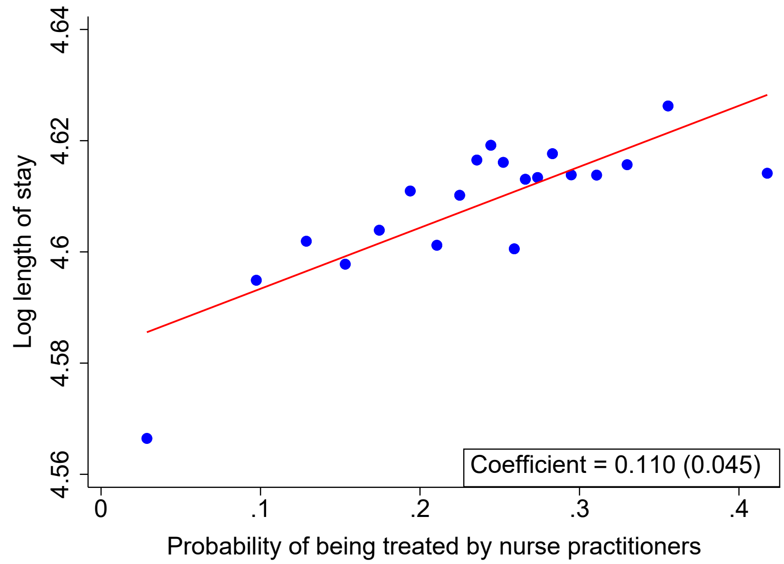
Figure A.4: Monotonicity Test



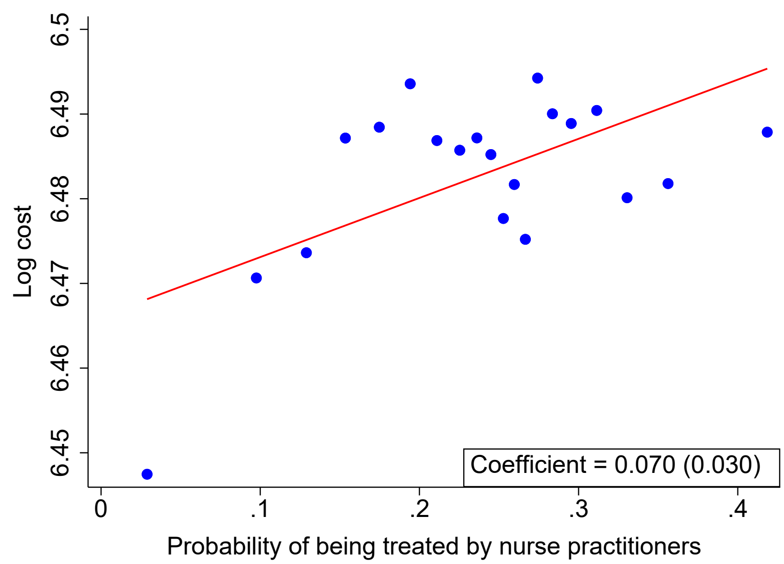
Notes: This figure shows the first-stage regression for cases of different characteristics. Panels A-F split the sample by, respectively, age (above versus below the median of the sample), marital status, gender, race (Black versus non-Black), total number of Elixhauser comorbidities (above versus below the median of the sample), and predicted 30-day mortality (above versus below the median of the sample). Predicted 30-day mortality is generated from a linear regression of actual 30-day mortality on patient characteristics $\mathbf{X}_i$ included in Equations (1) and (2), including patient demographics, comorbidities, prior health care use, vital signs, and 3-digit diagnosis indicators. To construct these binned scatterplots, we residualize both the *y*-axis and *x*-axis variable with respect to the baseline control vector (i.e., indicators for ED-year, ED-month, ED-day-of-the-week, and ED-hour-of-the-day) within each subsample and then add means back. The coefficients report the first-stage estimates for each subset of patients conditional on the baseline control vector, with standard errors clustered by provider reported in parentheses.

Figure A.5: Visual IV

A. Log Length of Stay

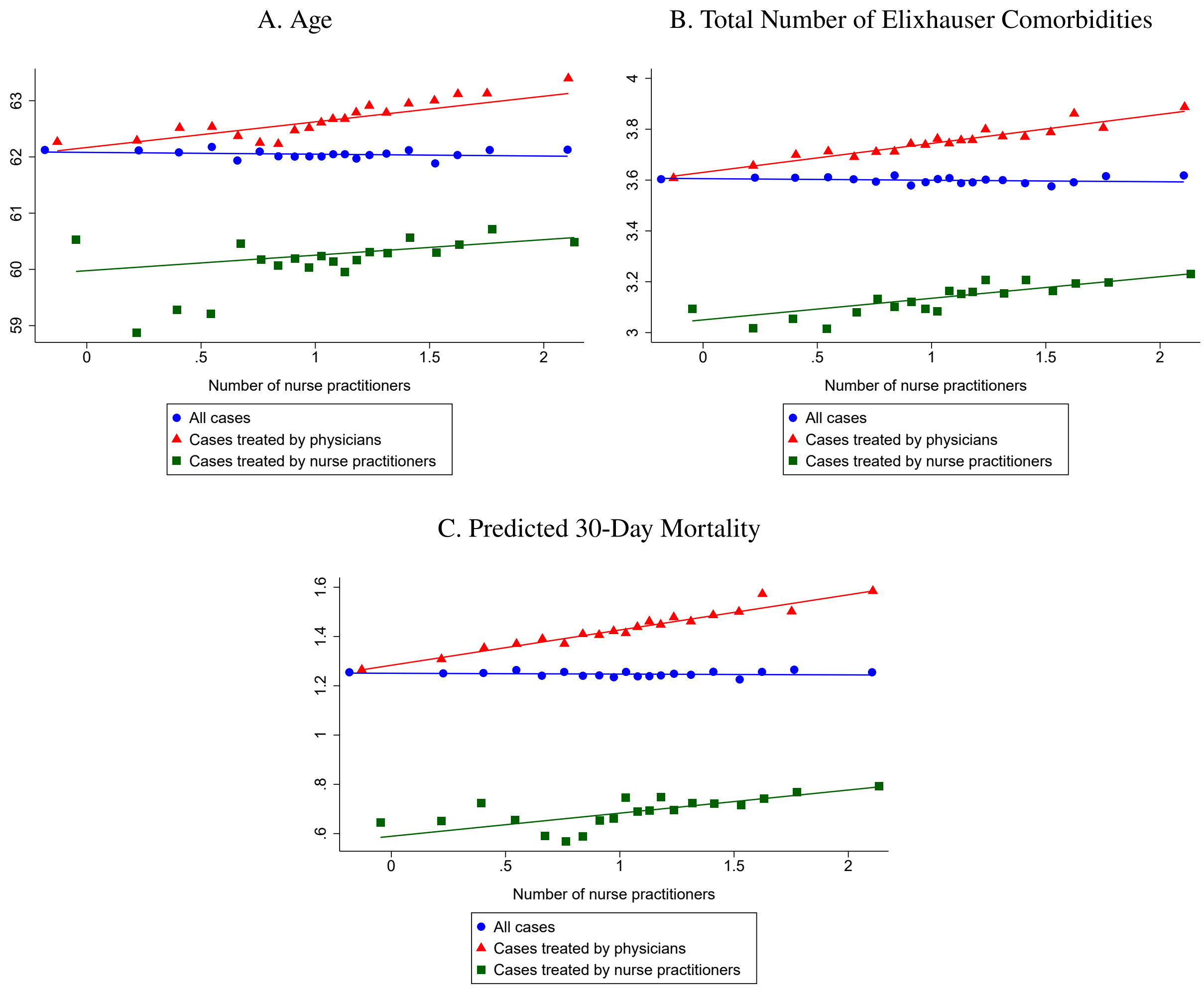


B. Log Cost



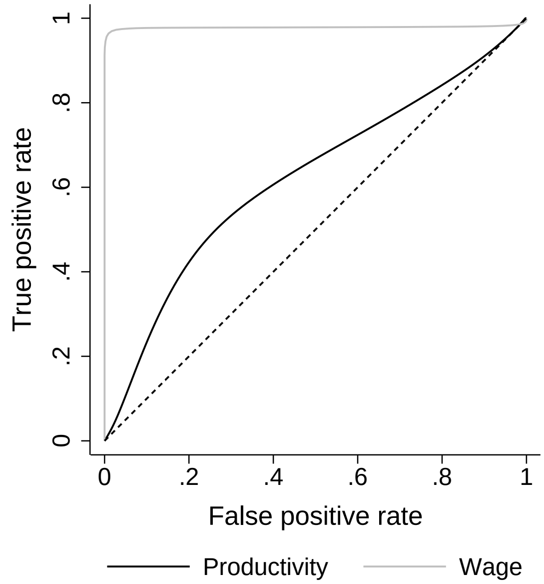
Notes: This figure shows the visual IV plot of the effect of NPs on patient log length of stay (Panel A) and log cost of the ED visit (Panel B). In each panel, we plot the mean outcome (log length of stay or log cost) on the y-axis versus patient probability of being treated by an NP on the x-axis. Patient probability of being treated by an NP is estimated using the first-stage regression in Equation (2). Patient outcomes on the y-axis are generated using the corresponding reduced-form regression with a dependent variable of log length of stay in Panel A and log cost in Panel B. The coefficients correspond to the IV estimates, with standard errors clustered by provider reported in parentheses.

Figure A.6: Cases Treated by Physicians versus Nurse Practitioners



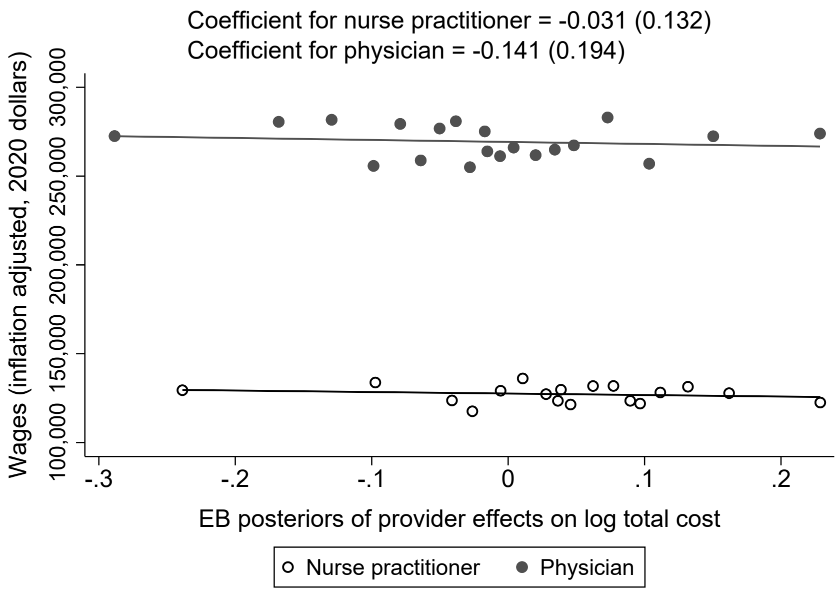
Notes: Panels A-C report patient characteristics (age, total number of Elixhauser comorbidities, and predicted 30-day mortality, respectively) on the *y*-axis against the number of NPs on duty on the *x*-axis. The unit of observation is at the case level. Both the *y*-axis and *x*-axis variables are residualized with respect to the baseline control vector (ED-year, ED-month, ED-day-of-the-week, and ED-hour-of-the-day indicators), with sample means added back to aid in interpretation. The circles, triangles, and squares show binned scatterplots for all cases, cases treated by physicians, and cases treated by NPs, respectively.

Figure A.7: Receiver Operating Characteristic Curve



Notes: This figure displays the receiver operating characteristic curves for productivity (in the black solid line) and wages (in the gray solid line). The dotted line plots the 45-degree line. Productivity is defined as the additive inverse of provider-specific effects on log total spending associated with the ED visit estimated in Appendix A.4.1. Physicians are defined as the "positive" class and NPs are defined as the "negative" class. See Appendix A.4.4 for more details.

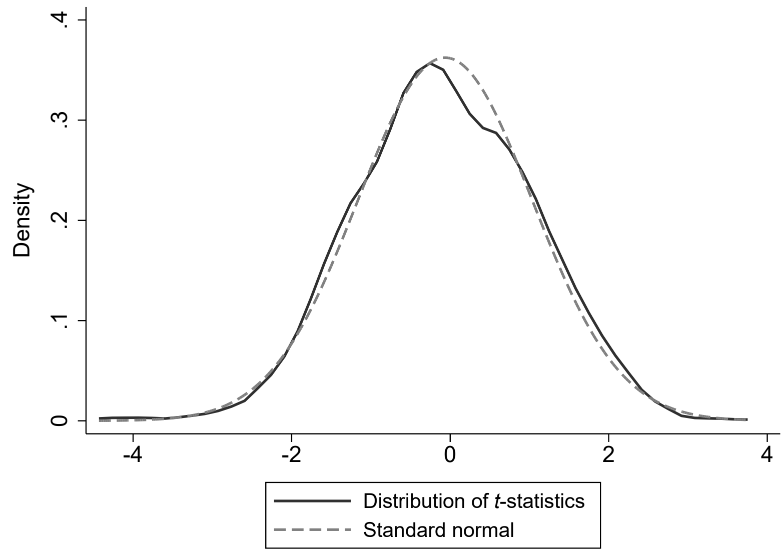
Figure A.8: Productivity versus Wage



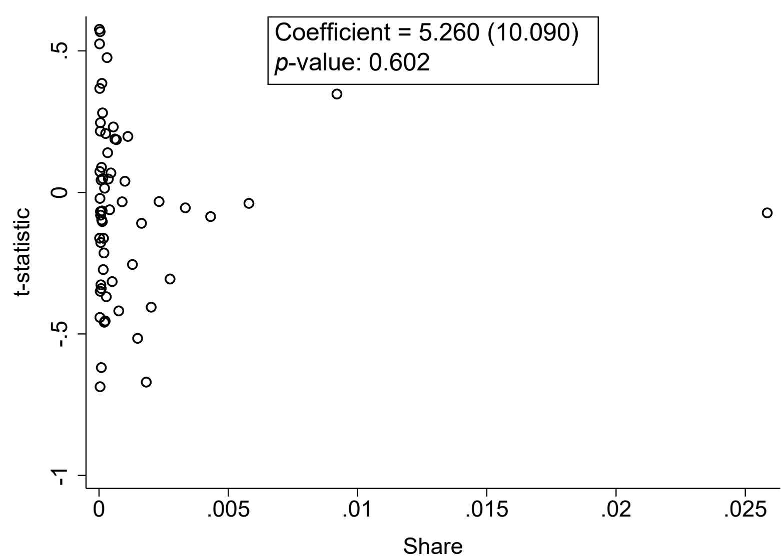
Notes: This figure shows binned scatterplots of providers' yearly wage on the *y*-axis versus EB posteriors of provider productivity estimated in Section A.4.1 on the *x*-axis. Both the *y*-axis and *x*-axis variables are residualized with respect to ED indicators, with means added back for ease of interpretation. Wages are inflation adjusted to year 2020. Coefficients from regressions of wages on EB posteriors of productivity estimates controlling for ED indicators are reported, with robust standard errors shown in parentheses. The hollow circles report results for NPs; the solid circles report results for physicians.

Figure A.9: Diagnosis Coding: Nurse Practitioners versus Physicians

A. Distribution of *t*-Statistics

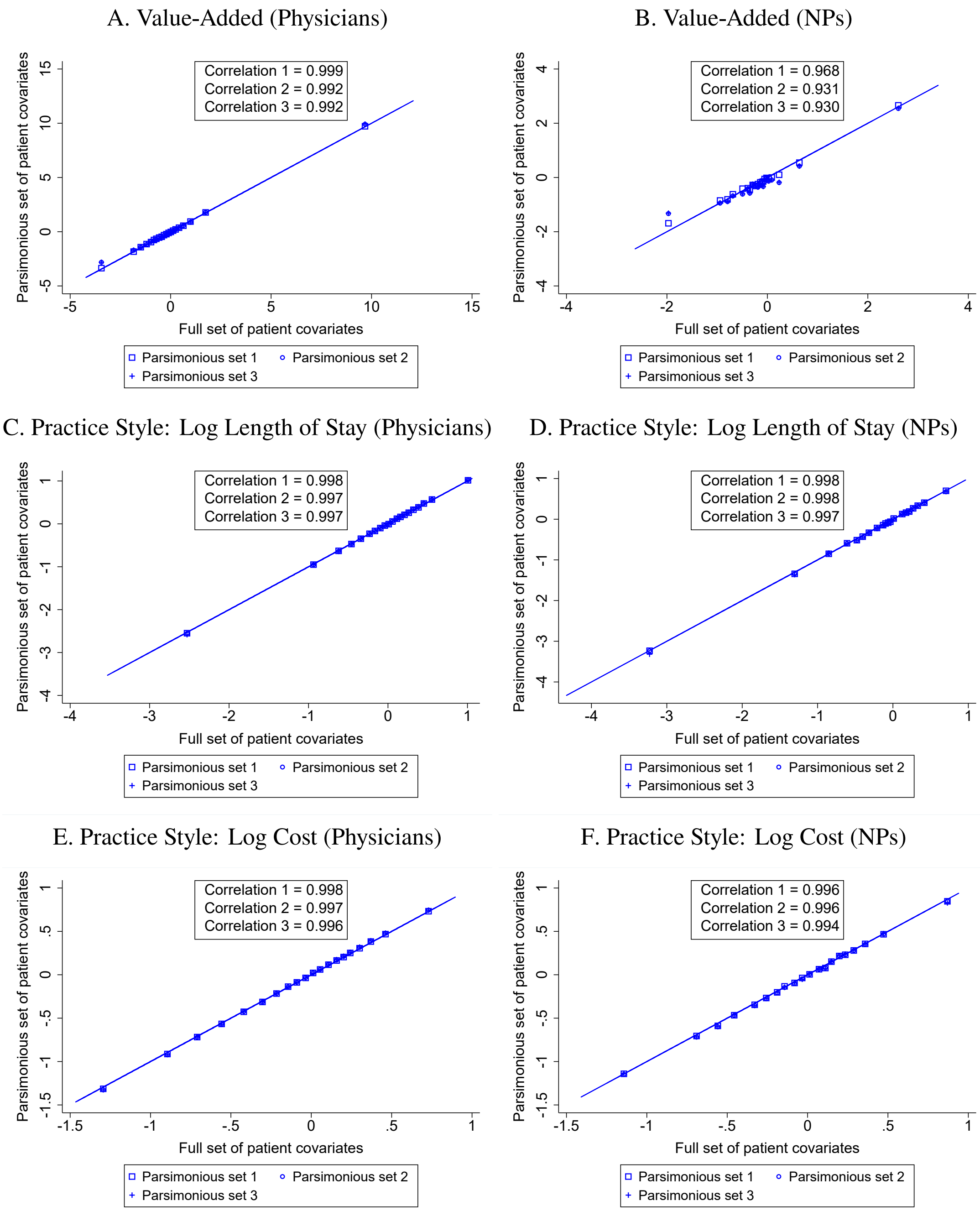


B. *t*-Statistics versus Diagnosis Prevalence



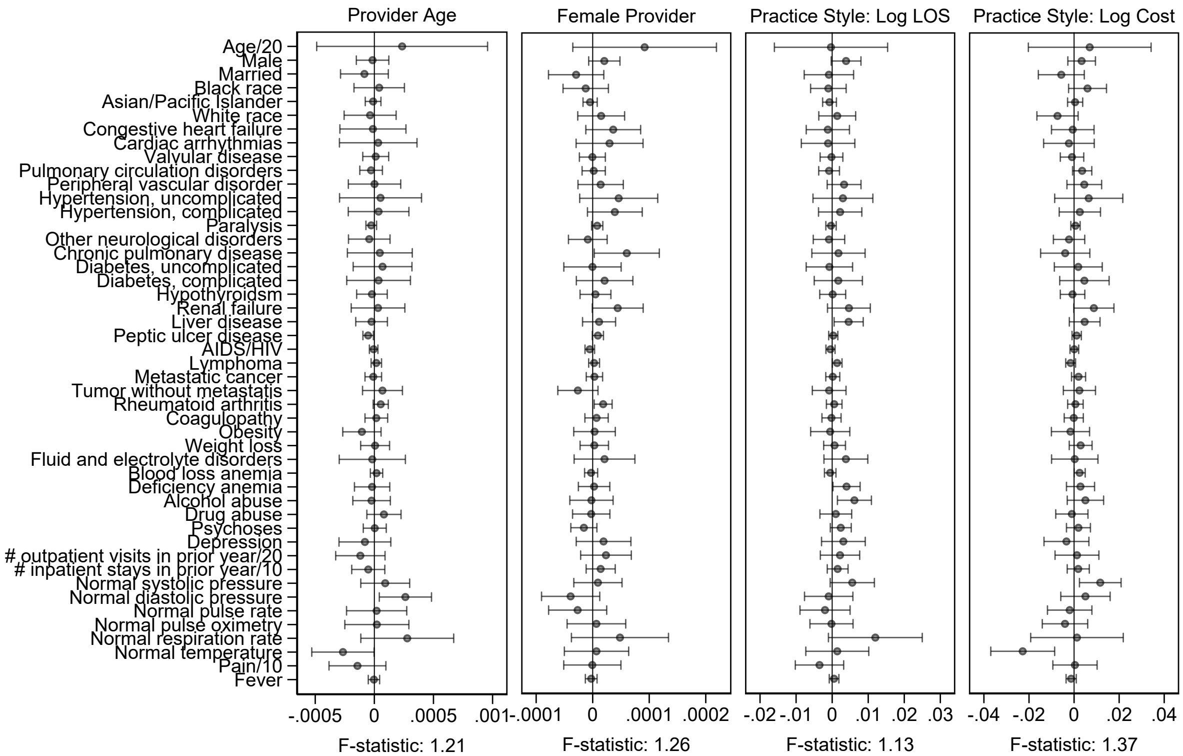
Notes: Panel A plots the distribution of the *t*-statistics on whether NPs and physicians are significantly different in diagnosis coding from 836 separate regressions that use each 3-digit diagnosis indicator as the outcome variable. The distribution is estimated using an Epanechnikov kernel with the optimal bandwidth and shown in the solid line. For comparison, the standard normal density is plotted in the dashed line. Panel B shows binned scatterplots of the *t*-statistics against the prevalence of the diagnosis (measured as the share of cases with the diagnosis among cases treated by physicians on days without any NP, to restrict influences of patient sorting between NPs and physicians). The coefficient from the regression of the *t*-statistics on prevalence is reported at the top of the panel, along with its standard error (shown in parentheses) and *p*-value.

Figure A.10: Stability of Provider Value-Added and Practice Style Estimates with Varying Patient Covariates



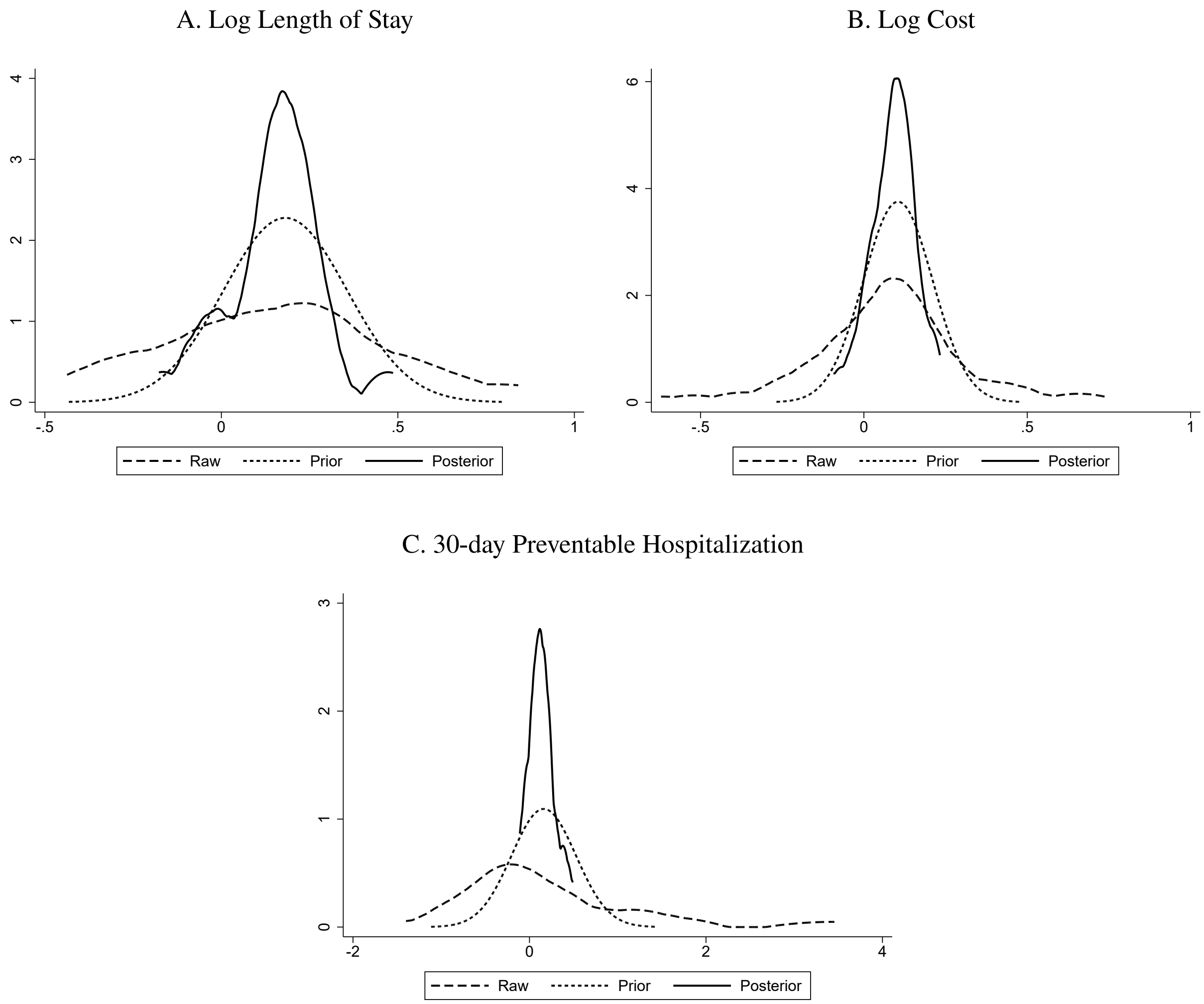
Notes: This figure shows the stability of provider value-added and practice style estimated using alternative patient covariates. See Section 4.4 for construction details of provider value-added and practice style. The full set of patient covariates includes demographics (five-year age-bin indicators, marital status, gender, and race indicators), indicators for 31 Elixhauser comorbidities, prior health care use (the number of outpatient visits and the number of inpatient stays in any VHA facilities in the prior 365 days), vital signs, and indicators for 3-digit ICD-10 code of patient primary diagnosis of the visit. Parsimonious set 1 includes demographics, 3-digit diagnosis indicators, and 31 Elixhauser comorbidities. Parsimonious set 2 includes demographics and 3-digit diagnosis indicators. Parsimonious set 3 includes five-year age-bin and 3-digit diagnosis indicators. Provider valued-added and practice style estimated using the full set of patient covariates are shown on the *x*-axis; alternative measures based on parsimonious sets 1-3 are shown on the *y*-axis in squares, circles, and "+", respectively. Correlations 1-3 report correlations of value-added and practice style estimated using the full set of patient covariates with alternative estimates based on parsimonious sets 1-3, respectively. The solid line shows the 45-degree line. Panels A, C and E report results for physicians. Panel B, D and F report results for NPs.

Figure A.11: Balance of Patient Characteristics across Available Provider Characteristics



Notes: This figure shows estimated coefficients and 95% confidence intervals from regressions of each patient characteristic listed on the *y*-axis on average characteristics of providers on duty in the ED-day cell of the patient's visit, controlling for the baseline control vector (i.e., indicators for ED-year, ED-month, ED-day-of-the-week, and ED-hour-of-the-day). All average on-duty provider characteristics are case-weighted, with the index case left out. The average on-duty provider characteristics in Panels A-D are, respectively, age, female, practice style in terms of patient log length of stay, and practice style in terms of patient log cost of care at the ED. For improved readability, a few coefficients (and their confidence intervals) are scaled down by 10 and 20, as shown by "/10" and "/20" on the *y*-axis, respectively. At the bottom of the figure, we report the *F*-statistic from the joint *F*-test for all patient characteristics in a reverse regression with the corresponding average on-duty provider characteristic as the dependent variable, conditioning on the baseline control vector. Standard errors are clustered by provider.

Figure A.12: ED-Specific Estimates of Nurse Practitioner Effect



Notes: This figure reports the distribution of ED-specific IV estimates of the NP effect. Panels A, B and C report results for the NP effect on log length of stay, log cost of the ED visit, and 30-day preventable hospitalization, respectively. The dashed lines show the distribution of ED-specific IV estimates without any adjustment to account for estimation noise. The short-dashed and solid lines show the prior and posterior distribution of the estimates with empirical Bayes adjustments (see details in Appendix A.5). Panels A and B display IV estimates for all 44 EDs in our sample. As 30-day preventable hospitalization is relatively uncommon (occurs in less than 2 percent of the sample), the estimates are relatively imprecise when based on observations from a specific ED, Panel C thus includes only EDs with at least 25,000 cases in the analysis sample (in total 20 such EDs).

Table A.1: Sample Selection

	Number of cases	Percent as full sample
All cases between January 2017 and January 2020	7,886,164	100.00
Include only cases visiting during daytime (8 a.m.-6 p.m.)	5,766,296	73.12
Include only cases from EDs in months that have granted NPs full practice authority and been using NPs	3,597,347	45.62
Drop cases in EDs that use providers other than physicians and NPs	1,119,396	14.19
Drop cases with missing age or gender, or aged above 99 or below 20	1,118,836	14.19

Notes: This table reports changes in sample size when applying each of the listed sample restrictions. Column 1 reports the number of observations remaining at each step. Column 2 reports the share as a percentage of the total number of observations shown in the first row.

Table A.2: Characteristics of Compliers, Never Takers, and Always Takers

	All	Compliers		Never takers		Always takers	
	Mean	Mean	Ratio	Mean	Ratio	Mean	Ratio
Age	62.05	61.11	0.98	63.09	1.02	60.23	0.97
	(0.15)	(0.31)	[0.98, 0.99]	(0.21)	[1.01, 1.02]	(0.39)	[0.96, 0.98]
Married	0.424	0.424	1.00	0.405	0.96	0.395	0.93
	(0.004)	(0.008)	[0.97, 1.04]	(0.005)	[0.93, 0.98]	(0.020)	[0.84, 1.02]
Male	0.905	0.905	1.00	0.916	1.01	0.904	1.00
	(0.002)	(0.003)	[0.99, 1.01]	(0.002)	[1.01, 1.02]	(0.003)	[0.99, 1.01]
Black	0.270	0.262	0.97	0.241	0.89	0.318	1.18
	(0.011)	(0.019)	[0.83, 1.11]	(0.010)	[0.82, 0.97]	(0.050)	[0.81, 1.54]
White	0.708	0.716	1.01	0.732	1.03	0.655	0.93
	(0.011)	(0.019)	[0.96, 1.06]	(0.010)	[1.01, 1.06]	(0.047)	[0.79, 1.06]
Asian/Pacific Islander	0.021	0.020	0.95	0.026	1.26	0.024	1.18
	(0.001)	(0.002)	[0.74, 1.15]	(0.002)	[1.06, 1.46]	(0.005)	[0.71, 1.65]
# outpatient visits in prior year	6.242	5.824	0.93	6.744	1.08	5.685	0.91
	(0.080)	(0.129)	[0.89, 0.97]	(0.118)	[1.04, 1.12]	(0.263)	[0.83, 0.99]
# inpatient stays in prior year	0.612	0.490	0.80	0.731	1.19	0.418	0.68
	(0.014)	(0.029)	[0.71, 0.89]	(0.020)	[1.13, 1.26]	(0.028)	[0.59, 0.77]
# Elixhauser comorbidities	3.599	3.324	0.92	3.865	1.07	3.179	0.88
	(0.030)	(0.066)	[0.89, 0.96]	(0.048)	[1.05, 1.10]	(0.073)	[0.84, 0.92]
Predicted 30-day mortality (%)	1.247	0.902	0.72	1.630	1.31	0.645	0.52
	(0.032)	(0.067)	[0.62, 0.83]	(0.049)	[1.23, 1.38]	(0.075)	[0.40, 0.63]

Notes: This table reports average characteristics for the overall sample, compliers, never-takers, and always-takers. Complier characteristics are estimated by 2SLS regressions replacing the outcome variable y_i with $x_i \times \text{NP}_i$, i.e., the interaction between patient characteristic and the indicator for being treated by an NP, controlling for the baseline control vector (i.e., indicators for ED-year, ED-month, ED-day-of-the-week, and ED-hour-of-the-day). Standard errors clustered by provider are reported in parentheses. Never-takers are defined as cases treated by physicians in ED-day cells with the residual share of cases treated by NPs at least as high as the 90th percentile of ED-days with at least one case treated by NPs. Always-takers are defined as cases treated by NPs in ED-day cells with the residual share of cases treated by NPs equal to or below the 10th percentile of ED-days with at least one case treated by NPs. Residual shares are constructed by first collapsing the data to ED-days and then residualizing the share of cases treated by NPs by indicators for ED-year, ED-month and ED-day-of-the-week. Standard errors for the overall sample, never-takers, and always-takers are estimated by bootstrap, using 500 replications and blocking observations by provider. For each characteristic, the table reports the mean as well as the ratio between this mean and the overall sample mean. 95% confidence intervals of each ratio are shown in brackets. Predicted 30-day mortality is generated from a linear regression of actual 30-day mortality on patient characteristics $\mathbf{X}_i$ included in Equations (1) and (2), including patient demographics, comorbidities, prior health care use, vital signs, and 3-digit diagnosis indicators.

Table A.3: Physician Value-Added and Outcomes for Patients Treated by Nurse Practitioners

	(1) Number Elixhauser comorbidities	(2) Predicted 30-day mortality	(3) Log LOS	(4) Log cost	(5) Inpatient admission	(6) 30-day mortality	(7) 30-day preventable hospitalization
Physician value-added	-0.006	-0.021	-0.010	-0.005	-0.245	-0.005	-0.023
	(0.013)	(0.014)	(0.008)	(0.005)	(0.199)	(0.075)	(0.044)
Full control			Yes	Yes	Yes	Yes	Yes
Mean dep. var.	3.13	0.73	4.30	6.30	7.73	0.63	0.72
S.D. dep. var.	2.71	2.11	1.08	0.87	26.70	7.93	8.45
Observations	147,936	147,936	146,947	146,934	147,936	147,936	147,936

Notes: This table shows the balance in outcomes for cases treated by NPs across the average value-added of physicians on duty. See Section 4.4 for construction details of physician value-added. The outcomes in Columns 1-7 are, respectively, total number of Elixhauser comorbidities, predicted 30-day mortality, log length of stay, log cost of the ED visit, hospital admission in the ED visit, 30-day mortality, and 30-day preventable hospitalization. The sample is restricted to patients treated by NPs on days with one NP on duty and at least one physician on duty. Since Columns 1-2 examine the balance in patient characteristics, the set of controls includes only the baseline control vector (i.e., ED-year, ED-month, ED-day-of-the-week, and ED-hour-of-the-day indicators). The set of full controls in Columns 3-7 is detailed in the notes to Table 2. Standard errors clustered by provider are reported in parentheses. ***: $p \leq 0.01$; **: $p \leq 0.05$; *: $p \leq 0.1$.

Table A.4: Balance in Never-Taker Outcomes

	(1) Number Elixhauser comorbidities	(2) Predicted 30-day mortality	(3) Log LOS	(4) Log cost	(5) Inpatient admission	(6) 30-day mortality	(7) 30-day preventable hospitalization
Number of nurse practitioners	-0.037	-0.016	-0.033	0.011	0.522	-0.070	0.007
	(0.090)	(0.085)	(0.024)	(0.023)	(1.029)	(0.364)	(0.313)
Full control			Yes	Yes	Yes	Yes	Yes
Mean dep. var.	3.97	1.69	4.87	6.66	21.89	1.76	1.66
S.D. dep. var.	3.16	3.35	1.05	0.84	41.35	13.15	12.76
Observations	23,963	23,963	23,796	23,731	23,963	23,963	23,963

Notes: This table shows balance in never-taker outcomes across the number of NPs on duty. See notes to Appendix Table A.2 for the definition of never-takers. The outcomes in Columns 1-7 are, respectively, total number of Elixhauser comorbidities, predicted 30-day mortality, log length of stay, log cost of the ED visit, hospital admission in the ED visit, 30-day mortality, and 30-day preventable hospitalization. Since Columns 1-2 examine balance in patient characteristics, the set of controls includes only the baseline control vector (i.e., ED-year, ED-month, ED-day-of-the-week, and ED-hour-of-the-day indicators). The set of full controls in Columns 3-7 is detailed in the notes to Table 2. Standard errors clustered by provider are reported in parentheses. ***: p≤0.01; **: p≤0.05; *: p≤0.1.

Table A.5: Balance in Never-Taker Outcomes: Heterogeneity

	(1) Number Elixhauser comorbidities	(2) Predicted 30-day mortality	(3) Log LOS	(4) Log cost	(5) Inpatient admission	(6) 30-day mortality	(7) 30-day preventable hospitalization
Panel A. By whether total number of cases in top tercile							
Bottom two terciles	-0.036	0.008	-0.036	-0.006	-0.079	-0.062	0.017
	(0.098)	(0.086)	(0.029)	(0.026)	(1.126)	(0.383)	(0.352)
Top tercile	-0.058	-0.095	-0.028	0.034	1.437	-0.182	0.114
	(0.122)	(0.119)	(0.029)	(0.026)	(1.444)	(0.490)	(0.453)
Panel B. By whether total patient mortality risk in top tercile							
Bottom two terciles	0.031	-0.014	-0.029	0.003	0.928	0.337	0.108
	(0.090)	(0.078)	(0.026)	(0.023)	(1.118)	(0.387)	(0.343)
Top tercile	-0.170	-0.096	-0.041	0.024	-0.126	-0.760	-0.127
	(0.118)	(0.120)	(0.029)	(0.028)	(1.331)	(0.507)	(0.402)
Full control			Yes	Yes	Yes	Yes	Yes
Mean dep. var.	3.97	1.69	4.87	6.66	21.89	1.76	1.66
S.D. dep. var.	3.16	3.35	1.05	0.84	41.35	13.15	12.76
Observations	23,963	23,963	23,796	23,731	23,963	23,963	23,963

Notes: This table shows balance in never-taker outcomes across the number of NPs on duty. Panel A shows heterogeneous effects by whether the total number of cases in the ED-day cell is in the top tercile of all ED-days. Panel B shows heterogeneous effects by whether the sum of predicted 30-day mortality of all cases in the ED-day cell is in the top tercile of all ED-days. The empirical specification takes the form $y_i = \sum_{g=1}^{G} \mathbf{1}(\text{Group}_i = g) \left[\gamma_g Z_i + \lambda_g\right] + \mathbf{T}_i \eta + \mathbf{X}_i \beta + \varepsilon_i$, where $\mathbf{1}(\text{Group}_i = g)$ is an indicator for whether the ED-day cell of the never-taker's visit has a number of cases and sum of predicted 30-day mortality in the top or bottom two tercile(s) of all ED-days for Panels A and B, respectively. See notes to Appendix Table A.2 for the definition of never-takers. The outcomes in Columns 1-7 are, respectively, total number of Elixhauser comorbidities, predicted 30-day mortality, log length of stay, log cost of the ED visit, hospital admission in the ED visit, 30-day mortality, and 30-day preventable hospitalization. Since Columns 1-2 examine balance in patient characteristics, the set of controls includes only the baseline control vector (i.e., ED-year, ED-month, ED-day-of-the-week, and ED-hour-of-the-day indicators). The set of full controls in Columns 3-7 is detailed in the notes to Table 2. Standard errors clustered by provider are reported in parentheses. ***: p≤0.01; **: p≤0.05; *: p≤0.1.

Table A.6: Robustness to Additional Controls

	(1) Log LOS	(2) Log cost	(3) Inpatient admission	(4) 30-day mortality	(5) 30-day preventable hospitalization
Panel A. Baseline					
Nurse practitioner	0.110**	0.070**	0.103	-0.116	0.252**
	(0.045)	(0.030)	(0.585)	(0.115)	(0.120)
Panel B. Control for workload					
Nurse practitioner	0.120***	0.082***	0.340	-0.118	0.256**
	(0.046)	(0.030)	(0.604)	(0.118)	(0.124)
Panel C. Control for total number of doctor equivalents: substitution rate 0.341					
Nurse practitioner	0.110**	0.070**	0.103	-0.116	0.252**
	(0.044)	(0.029)	(0.584)	(0.115)	(0.120)
Panel D. Control for total number of doctor equivalents: substitution rate 0.5					
Nurse practitioner	0.085**	0.061**	-0.019	-0.104	0.245**
	(0.043)	(0.029)	(0.574)	(0.113)	(0.117)
Panel E. Control for wait time					
Nurse practitioner	0.108**	0.069**	0.248	-0.095	0.268**
	(0.045)	(0.030)	(0.590)	(0.116)	(0.121)
Full control	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	4.61	6.48	16.62	1.25	1.23
S.D. dep. var.	1.16	0.88	37.23	11.10	11.04
Observations	1,110,798	1,108,961	1,118,836	1,118,836	1,118,836

Notes: Panel A repeats our main estimates reported in Tables 2 and 3. Panel B adds a control for the total number of cases being treated at the ED on the day the patient visits. Panels C and D add a control for the total number of doctor equivalents on duty at the ED on the day the patient visits. Panel C assumes a substitution rate of 0.341 between NPs and physicians; Panel D assumes a substitution rate of 0.5. Panel E adds a control for patient wait time. As wait time is potentially endogenous (healthier cases could be assigned a lower priority and hence wait longer), we instrument for wait time using the average wait time of cases visiting on the same day at the same ED as the index case. The outcomes in Columns 1-5 are, respectively, log length of stay, log cost of the ED visit, hospital admission in the ED visit, 30-day mortality, and 30-day preventable hospitalization. All estimations include the full set of controls described in the notes to Table 2. Standard errors clustered by provider are reported in parentheses. ***: p≤0.01; **: p≤0.05; *: p≤0.1.

Table A.7: Patient-Provider Gender Match

	(1) Number Elixhauser comorbidities	(2) Predicted 30-day mortality	(3) Log LOS	(4) Log Cost	(5) Inpatient admission	(6) 30-day mortality	(7) 30-day preventable hospitalization
Female NP*male patient	0.007	-0.065	0.027	-0.012	0.355	0.062	-0.058
	(0.103)	(0.100)	(0.017)	(0.015)	(0.593)	(0.085)	(0.093)
Female NP	0.029	0.062	0.006	0.191**	1.327	0.027	-0.015
	(0.153)	(0.153)	(0.133)	(0.081)	(1.886)	(0.091)	(0.091)
Full control			Yes	Yes	Yes	Yes	Yes
Mean dep. var.	3.19	0.74	4.30	6.34	7.87	0.63	0.74
S.D. dep. var.	2.77	2.15	1.14	0.86	26.92	7.91	8.60
Observations	264,772	264,772	262,959	263,045	264,772	264,772	264,772

Notes: This table examines whether NPs treat patients of the opposite gender differently compared to the same gender. We restrict the sample to patients treated by NPs, and regress each outcome on the interaction between the indicators for female NPs and male patients, female NP indicator, and male patient indicator. Columns 1-2 examine the balance in patient characteristics and adds controls for the baseline control vector (i.e., ED-year, ED-month, ED-day-of-the-week, and ED-hour-of-the-day indicators). Columns 3-7 adds the full set of controls described in the notes to Table 2. The outcomes in Columns 1-7 are, respectively, total number of Elixhauser comorbidities, predicted 30-day mortality, log length of stay, log cost of the ED visit, hospital admission in the ED visit, 30-day mortality, and 30-day preventable hospitalization. Standard errors clustered by provider are reported in parentheses. ***: $p \leq 0.01$; **: $p \leq 0.05$; *: $p \leq 0.1$.

Table A.8: Alternative Standard Error Clustering

	(1) Log LOS	(2) Log cost	(3) Inpatient admission	(4) 30-day mortality	(5) 30-day preventable hospitalization
Panel A. Clustering by provider					
Nurse practitioner	0.110**	0.070**	0.103	-0.116	0.252**
	(0.045)	(0.030)	(0.585)	(0.115)	(0.120)
Panel B. Clustering by ED-day					
Nurse practitioner	0.110***	0.070***	0.103	-0.116	0.252**
	(0.015)	(0.010)	(0.348)	(0.113)	(0.112)
Panel C. Two-way clustering by ED-day and provider					
Nurse practitioner	0.110**	0.070**	0.103	-0.116	0.252**
	(0.045)	(0.030)	(0.581)	(0.113)	(0.119)
Full control	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	4.61	6.48	16.62	1.25	1.23
S.D. dep. var.	1.16	0.88	37.23	11.10	11.04
Observations	1,110,798	1,108,961	1,118,836	1,118,836	1,118,836

Notes: This table reports the robustness of our estimates to alternative standard error clustering approaches. Panel A repeats our baseline estimates that cluster standard errors by provider. Panel B clusters standard errors by ED-day. Panel C clusters standard errors using two-way clustering by ED-day and provider. The outcomes in Columns 1-5 are, respectively, log length of stay, log cost of the ED visit, hospital admission in the ED visit, 30-day mortality, and 30-day preventable hospitalization. All estimations include the full set of controls described in the notes to Table 2. ***: p≤0.01; **: p≤0.05; *: p≤0.1.

Table A.9: Alternative Instruments

	(1) Log LOS	(2) Log cost	(3) Inpatient admission	(4) 30-day mortality	(5) 30-day preventable hospitalization
Panel A. Include nurse practitioners with only one case					
Nurse practitioner	0.102**	0.076**	-0.011	-0.127	0.285**
	(0.045)	(0.030)	(0.594)	(0.116)	(0.125)
Panel B. Leave out the index case					
Nurse practitioner	0.123***	0.074**	0.198	-0.110	0.260**
	(0.046)	(0.031)	(0.605)	(0.121)	(0.126)
Panel C. Leave-out share of cases treated by nurse practitioners					
Nurse practitioner	0.117**	0.069**	0.926	-0.033	0.208*
	(0.052)	(0.032)	(0.628)	(0.118)	(0.121)
Panel D. Indicator for whether any NP on duty					
Nurse practitioner	0.108**	0.080***	0.185	-0.048	0.211
	(0.049)	(0.030)	(0.643)	(0.121)	(0.130)
Full control	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	4.61	6.48	16.62	1.25	1.23
S.D. dep. var.	1.16	0.88	37.23	11.10	11.04
Observations	1,110,798	1,108,961	1,118,836	1,118,836	1,118,836

Notes: Panel A reports results using an alternative measure of the number of NPs on duty as the instrument, which includes NPs with only one case in the analysis time window of an ED-day cell. Panel B reports results leaving out the index case in measuring the number of NPs on duty. Panels C uses the share of cases treated by NPs in the ED-day cell (leaving out the index case in calculating the share) as the instrument. Panel D uses an indicator for any NP on duty as the instrument. The outcomes in Columns 1-5 are, respectively, log length of stay, log cost of the ED visit, hospital admission in the ED visit, 30-day mortality, and 30-day preventable hospitalization. All estimations include the full set of controls described in the notes to Table 2. ***: p≤0.01; **: p≤0.05; *: p≤0.1.

Table A.10: Sample Restricted to ED-Days with Zero or One NP

	(1)	(2)	(3)	(4)	(5)
	Log LOS	Log cost	Inpatient admission	30-day mortality	30-day preventable hospitalization
Nurse practitioner	0.109**	0.084***	0.146	-0.042	0.219
	(0.052)	(0.032)	(0.686)	(0.128)	(0.140)
Full control	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	4.59	6.45	16.30	1.24	1.24
S.D. dep. var.	1.15	0.89	36.94	11.07	11.04
Observations	862,416	860,798	868,930	868,930	868,930

Notes: This table shows results when using only patients in ED-day cells with 0 or 1 NP on duty. The outcomes in Columns 1-5 are, respectively, log length of stay, log cost of the ED visit, hospital admission in the ED visit, 30-day mortality, and 30-day preventable hospitalization. All estimations include the full set of controls described in the notes to Table 2. ***: p≤0.01; **: p≤0.05; *: p≤0.1.

Table A.11: Ten Most Common 3-digit Diagnoses with 30-Day Mortality $\geq$ 95th Percentile

ICD code	Description	30-day mortality (%)	Number of cases	Share
I50	Heart failure	5.6	12,637	0.22
N17	Acute kidney failure	6.5	4,278	0.07
R41	Other symptoms and signs involving cognitive functions and awareness	7.6	3,872	0.07
D64	Other anemias	5.0	3,634	0.06
I21	Acute myocardial infarction	7.5	3,162	0.06
A41	Other sepsis	11.5	2,754	0.05
J15	Bacterial pneumonia, not elsewhere classified	5.1	2,715	0.05
J96	Respiratory failure, not elsewhere classified	13.0	2,548	0.04
F03	Unspecified dementia	5.4	1,427	0.02
R62	Lack of expected normal physiological development in childhood and adults	16.6	1,033	0.02

Notes: This table summarizes the 10 most common 3-digit diagnosis codes in the group of diagnoses with a 30-day mortality rate equal to or above the 95th percentile of the sample. The olumns report, from the leftmost to the rightmost, the 3-digit ICD-10 code, description of the code, 30-day mortality rate of cases with the diagnosis code, number of cases in the analysis sample with the diagnosis code, and share of cases with the diagnosis code among all cases with a 3-digit diagnosis whose 30-day mortality equals or is above the 95th percentile of the sample.

Table A.12: Heterogeneous Effects by Patient Health Risks

	(1) Log LOS	(2) Log cost	(3) Inpatient admission	(4) 30-day mortality	(5) 30-day preventable hospitalization
Panel A. By number of Elixhauser comorbidities					
1st quartile	0.042	0.028	-0.077	-0.147	0.555***
	(0.045)	(0.032)	(0.636)	(0.120)	(0.119)
2nd quartile	0.063	0.071**	0.291	-0.041	0.438***
	(0.044)	(0.030)	(0.642)	(0.126)	(0.132)
3rd quartile	0.117**	0.082***	-0.245	-0.099	0.250
	(0.048)	(0.031)	(0.761)	(0.178)	(0.186)
4th quartile	0.281***	0.122***	0.435	-0.203	-0.513
	(0.066)	(0.041)	(1.476)	(0.340)	(0.347)
Panel B. By severity of condition					
$<$95th percentile	0.080*	0.064**	-0.768	-0.077	0.361***
	(0.044)	(0.029)	(0.573)	(0.110)	(0.118)
$\geq$95th percentile	0.988***	0.247**	26.140***	-1.253	-2.989**
	(0.239)	(0.115)	(7.829)	(2.127)	(1.492)
Panel C. By condition					
Stroke	1.863***	0.651**	72.609**	3.373	-0.062
	(0.677)	(0.311)	(31.758)	(6.038)	(2.379)
AMI	0.806	1.780***	123.007**	-11.517	-3.219
	(0.562)	(0.655)	(62.695)	(9.684)	(7.593)
Sepsis	1.480**	0.095	44.880*	24.533	11.117
	(0.609)	(0.329)	(24.114)	(15.169)	(7.961)
Heart failure	1.125***	0.088	20.263**	1.262	-11.469**
	(0.292)	(0.177)	(8.921)	(3.011)	(5.265)
Other conditions	0.097**	0.067**	-0.343	-0.147	0.332***
	(0.045)	(0.029)	(0.578)	(0.112)	(0.122)
Full control	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	4.61	6.48	16.62	1.25	1.23
S.D. dep. var.	1.16	0.88	37.23	11.10	11.04
Observations	1,110,798	1,108,961	1,118,836	1,118,836	1,118,836

Notes: This table shows heterogeneous effects of NPs by patient health risks described in Section 5.1. Panel A divides cases into quartiles by their total number of Elixhauser comorbidities, with the highest quartile indicating the riskiest cases. Panel B further divides cases by whether condition severity measured by 30-day mortality of cases with the same 3-digit ICD-10 primary diagnosis is equal to or above the 95th percentile of the sample. Panel C divides cases by their condition. The outcomes in Columns 1-5 are, respectively, log length of stay, log cost of the ED visit, hospital admission in the ED visit, 30-day mortality, and 30-day preventable hospitalization. All estimations include the full set of controls described in the notes to Table 2. Standard errors clustered by provider are shown in parentheses. ***: $p \leq 0.01$; **: $p \leq 0.05$; *: $p \leq 0.1$.

Table A.13: Heterogeneous Effects by Provider Experience: Cases in 2018 or after

	(1) Log LOS	(2) Log cost	(3) Inpatient admission	(4) 30-day mortality	(5) 30-day preventable hospitalization	(6) Consult	(7) CT	(8) X-ray
Panel A. By specific experience in the case's condition								
Nurse practitioner	0.081*	0.077**	-0.268	-0.222	0.335**	0.026**	0.013*	0.021**
	(0.046)	(0.032)	(0.646)	(0.142)	(0.149)	(0.011)	(0.007)	(0.011)
Nurse practitioner*specific experience	-0.060**	-0.050**	-0.677**	0.011	-0.012	-0.017**	-0.012***	0.006
	(0.025)	(0.021)	(0.308)	(0.044)	(0.033)	(0.007)	(0.004)	(0.009)
Panel B. By general experience in all conditions								
Nurse practitioner	0.104**	0.089***	-0.356	-0.238	0.347**	0.030***	0.015*	0.021**
	(0.048)	(0.034)	(0.700)	(0.146)	(0.152)	(0.011)	(0.008)	(0.011)
Nurse practitioner*general experience	-0.130*	-0.069*	0.249	0.108	-0.029	-0.026**	-0.013	-0.005
	(0.067)	(0.040)	(1.337)	(0.114)	(0.077)	(0.013)	(0.013)	(0.008)
Full control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	4.64	6.53	16.30	1.25	1.23	0.23	0.15	0.37
S.D. dep. var.	1.13	0.89	36.94	11.11	11.01	0.42	0.36	0.48
Observations	742,968	741,027	747,510	747,510	747,510	747,510	747,510	747,510

Notes: This table reports heterogeneous effects of NPs by provider experience using cases visiting in 2018 or after. Panel A shows heterogeneous effects of NPs by provider specific experience in the case's condition, measured as the number of cases with the same 3-digit primary diagnosis as the current case the provider has treated since the start of the study period to the day before the current case's visit. Panel B shows heterogeneous effects of NPs by provider general experience, measured as the number of cases the provider has treated since the start of the study period to the day before the current case's visit. For ease of interpretation, both specific and general experience are standardized to have a mean of zero and a standard deviation of one for NPs and physicians separately. The outcome variables in Columns 1-8 are, respectively, log length of stay, log cost of the ED visit, hospital admission in the ED visit, 30-day mortality, 30-day preventable hospitalization, whether the patient receives consults in the ED visit, whether the patient receives CT scans in the ED visit, and whether the patient receives X-rays in the ED visit. All estimations include the full set of controls described in the notes to Table 2. Standard errors clustered by provider are shown in parentheses. ***: $p \leq 0.01$; **: $p \leq 0.05$; *: $p \leq 0.1$.

Table A.14: Heterogeneous Effects by Provider Experience: Experience in the Prior Year

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log LOS	Log cost	Inpatient admission	30-day mortality	30-day preventable hospitalization	Consult	CT	X-ray
Panel A. By specific experience in the case's condition								
Nurse practitioner	0.078*	0.074**	-0.295	-0.221	0.333**	0.025**	0.012	0.021**
	(0.046)	(0.031)	(0.641)	(0.141)	(0.148)	(0.011)	(0.007)	(0.011)
Nurse practitioner*specific experience	-0.053**	-0.055**	-0.646**	0.016	-0.017	-0.018**	-0.012***	0.009
	(0.027)	(0.023)	(0.307)	(0.049)	(0.035)	(0.007)	(0.003)	(0.010)
Panel B. By general experience in all conditions								
Nurse practitioner	0.089*	0.081**	-0.331	-0.222	0.350**	0.027**	0.013*	0.021**
	(0.046)	(0.033)	(0.662)	(0.143)	(0.150)	(0.011)	(0.008)	(0.011)
Nurse practitioner*general experience	-0.120	-0.063	-0.142	0.040	-0.117	-0.025*	-0.014	-0.008
	(0.088)	(0.044)	(1.162)	(0.091)	(0.085)	(0.014)	(0.011)	(0.008)
Full control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	4.64	6.53	16.30	1.25	1.23	0.23	0.15	0.37
S.D. dep. var.	1.13	0.89	36.94	11.11	11.01	0.42	0.36	0.48
Observations	742,968	741,027	747,510	747,510	747,510	747,510	747,510	747,510

Notes: This table reports heterogeneous effects of NPs by provider experience in the prior year. Panel A shows heterogeneous effects of NPs by provider specific experience in the case's condition, measured as the number of cases with the same 3-digit primary diagnosis as the current case the provider has treated in the 365 days prior to the day of the current case's visit. Panel B shows heterogeneous effects of NPs by provider general experience, measured as the number of cases the provider has treated in the 365 days prior to the day of the current case's visit. The sample is restricted to cases visiting in 2018 or after, to allow for at least a one-year look-back window for measuring experience in the prior 365 days. For ease of interpretation, both specific and general experience are standardized to have a mean of zero and a standard deviation of one for NPs and physicians separately. The outcome variables in Columns 1-8 are, respectively, log length of stay, log cost of the ED visit, hospital admission in the ED visit, 30-day mortality, 30-day preventable hospitalization, whether the patient receives consults in the ED visit, whether the patient receives CT scans in the ED visit, and whether the patient receives X-rays in the ED visit. All estimations include the full set of controls described in the notes to Table 2. Standard errors clustered by provider are shown in parentheses. ***: $p \leq 0.01$; **: $p \leq 0.05$; *: $p \leq 0.1$.

Table A.15: Heterogeneous Effects by Provider General Experience Measured by Number of Days

	(1) Log LOS	(2) Log cost	(3) Inpatient admission	(4) 30-day mortality	(5) 30-day preventable hospitalization	(6) Consult	(7) CT	(8) X-ray
Nurse practitioner	0.105**	0.065**	0.225	-0.100	0.263**	0.024***	0.013*	0.019**
	(0.045)	(0.029)	(0.623)	(0.117)	(0.123)	(0.009)	(0.007)	(0.009)
Nurse practitioner*general experience	-0.031	-0.036	1.098	0.126	0.101	-0.015	0.006	-0.005
	(0.068)	(0.037)	(1.157)	(0.111)	(0.080)	(0.011)	(0.014)	(0.013)
Full control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	4.61	6.48	16.62	1.25	1.23	0.23	0.15	0.29
S.D. dep. var.	1.16	0.88	37.23	11.10	11.04	0.42	0.35	0.45
Observations	1,110,798	1,108,961	1,118,836	1,118,836	1,118,836	1,118,836	1,118,836	1,118,836

Notes: This table reports heterogeneous effects of NPs by provider general experience measured by the number of days the provider has worked since the start of the study period to the day before the current case's visit. For ease of interpretation, the experience measure is standardized to have a mean of zero and a standard deviation of one for NPs and physicians separately. The outcome variables in Columns 1-8 are, respectively, log length of stay, log cost of the ED visit, hospital admission in the ED visit, 30-day mortality, 30-day preventable hospitalization, whether the patient receives consults in the ED visit, whether the patient receives CT scans in the ED visit, and whether the patient receives X-rays in the ED visit. All estimations include the full set of controls described in the notes to Table 2. Standard errors clustered by provider are shown in parentheses. ***: $p \leq 0.01$; **: $p \leq 0.05$; *: $p \leq 0.1$.

Table A.16: Variance of Provider Effects on Medical Spending

	Nurse Practitioners	Physicians
Basic estimates	0.0537	0.0643
Split-sample estimates	0.0476	0.0445

Notes: This table reports variance of provider effects on total spending associated with the ED visit. Total spending associated with the ED visit is computed as the sum of the three main components of costs that we find significant NP effects: the cost of care at the ED, whether admitted to the hospital during the ED visit multiplied by the average cost per VHA hospitalization $19,220, and preventable hospitalizations in the 30 days after the ED discharge multiplied by $19,220. Row 1 reports variance of provider effects $\hat{\delta}_j$ estimated using Equations (A.4) and (A.5). To account for biases due to estimation noise in $\hat{\delta}_j$, Row 2 reports variance using a split-sample approach (details are described in Appendix A.4.2). Column 1 reports variance for NPs. Column 2 reports variance for physicians.

Table A.17: Relationship between z-scores and Standard Errors

	Full sample		Split sample	
	(1) Nurse practitioners	(2) Physicians	(3) Nurse practitioners	(4) Physicians
Standard error	0.166	0.203	-0.487	0.108
	(0.434)	(0.348)	(0.369)	(0.471)

Notes: This table reports coefficients from regressions of provider-specific z-scores on associated standard errors. Columns 1 and 2 report results using z-scores and standard errors estimated in the full sample. Columns 3 and 4 randomly split cases for each provider into two approximately equal-sized partitions and regress z-scores from one partition on standard errors from the other partition, stacking the two partitions in the regressions. Columns 1 and 3 report results for NPs. Columns 2 and 4 report results for physicians. Standard errors clustered by ED are reported in parentheses.