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THE PRODUCTIVITY OF PROFESSIONS: EVIDENCE FROM THE EMERGENCY DEPARTMENT

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The Productivity of Professions: Evidence from the Emergency Department
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ABSTRACT

This paper studies the productivity of nurse practitioners (NPs) and physicians, two professions performing overlapping tasks but with stark differences in background, training, and pay. Using quasi-experimental variation in patient assignment to NPs versus physicians in 44 Veterans Health Administration emergency departments, we find that, on average, NPs use more resources but achieve worse patient outcomes relative to physicians. The costs of lower productivity surpass the pay differences between the professions. Yet even larger productivity variation exists within each profession, implying substantial productivity overlap between the two professions. Within professions, wages and assigned patient complexity vary only weakly with productivity.

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1 Introduction

Professions play a key role in determining the division of labor and the returns to skilled work (Abbott 2014). In selecting and training future members, professional groups may both restrict the supply of professionals and impact their quality. Professional groups also perform an important function as special interest groups for their members, lobbying policymakers and negotiating with payers. Large differences in pay between professional groups may therefore reflect differences in worker productivity or rents from restricted supply or privileged arrangements (Freidson 1974; Shapiro 1986).

Evidence comparing the productivity of distinct classes of professionals performing overlapping tasks remains scant. Professional groups, by nature, act to exclude outsiders from their jurisdictions (Abbott 2014). While the medical profession provides a well-documented case study of historical exclusion, it now provides an opportunity for study. Recent decades have witnessed an increased demand for health care, outstripping the supply of physicians, and the rise of NPs seeking to perform some of the same tasks that physicians do. The number of NPs has reached about one-third of the number of physicians in the US (Bureau of Labor Statistics 2021a; 2021b), and various states in the US have responded to the shortfall in physicians by granting NPs "scope of practice" to perform tasks traditionally performed by physicians (e.g., McMichael and Markowitz 2022). Yet on the basis of training, income, and social class, the comparison between the two professions of NPs and physicians is stark.

In this paper, we exploit a quasi-experiment in the Veterans Health Administration (VHA) to study the productivity differences between NPs and physicians. 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 on duty. We show that the number of on-duty physicians declines with the number of on-duty NPs, and the number of NPs on duty 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.

We find that, on average, NPs use more resources and achieve worse health outcomes than physicians along various measures. Our IV results show that NPs increase patient length of stay by 11 percent and raise the cost of ED care by 7 percent. 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 than physicians do.

We undertake several analyses to assess the validity of our IV quasi-experiment. We show that, conditional on the baseline controls, a broad range of patient characteristics that predict outcomes are well balanced across values of our instrument, the number of NPs on duty. Relatedly, our IV estimates are remarkably stable, regardless of the inclusion of a wide set of patient covariates; in contrast, OLS estimates under the full set of covariates are less than half the magnitude (but still opposite-signed relative to IV estimates) of those when only baseline covariates are included. We also assess the validity of the exclusion restriction, that the number of NPs on duty is not correlated with other factors that could drive care delivery or patient outcomes. Specifically, we show that our instrument is conditionally unrelated to the characteristics of on-duty physicians and NPs. We further examine potential spillovers between NPs and physicians and find no evidence suggesting such spillovers.[1] Finally, we show that our results are robust 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, under various analytical lenses, we uncover mechanisms behind and responses to the lower average productivity of NPs versus physicians. First, we find suggestive evidence that experience matters: The NP-physician gap in some outcomes narrows among providers who have seen more prior patients, both in general and for the diagnosis in question. This indicates that differences in training may play some role in the productivity differences between NPs and physicians. Second, we document differences in clinical decision-making. Compared to physicians, NPs are likelier to gather information from external sources: They are likelier to obtain radiology tests and formal consults. NPs also exhibit prescription patterns consistent with responses to lower skill (Chan, Gentzkow, and Yu 2022): Relative to physicians, NPs are *less* likely to prescribe opioids, which have higher health risks if incorrectly prescribed, but they are *more* likely to prescribe antibiotics, which have higher health risks if incorrectly not prescribed. Third, we examine heterogeneity in the NP-physician gap by 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. Finally, case assignment seems to respond to NPs' relative and

1. Specifically, we examine whether NP presence may affect physician performance. E.g., NPs asking physicians for assistance could slow down physicians. We thus examine whether physicians' treatment outcomes change with the presence of NPs; we find no such evidence. We also examine whether on-duty physician quality impacts NP performance; we find that the outcomes of patients treated by NPs are unrelated to the value-added or experience of the physicians on duty.

absolute disadvantage: On average, NPs are assigned healthier patients of those available for assignment; NPs are also assigned a (modestly) smaller share of patients when the ED is less busy.

We perform two counterfactual analyses to understand the magnitude of the average NP-physician productivity difference in financial terms relative to differences between physician and NP wages. First, we compare the allocation of one quarter of all VHA ED patients to NPs and the counterfactual scenario of staffing the EDs only with physicians. Relative to the counterfactual scenario of no NPs, allocating one quarter of ED patients to NPs increases non-wage spending by $160 million per year to the VHA. Accounting for lower NP wages that are half that of physicians' wages, it still implies a net cost of $92 million per year. Second, we consider the scenario where EDs reduce wait times by hiring additional NPs. Given the lower productivity of NPs relative to physicians, increasing the number of NPs on duty decreases wait times but increases resource utilization and adverse outcomes. Strikingly, we find that the wage costs of hiring additional NPs account for only one-fifth of the total cost of reducing wait times; the lower productivity of NPs accounts for four-fifths of this cost.

Finally, we examine variation in productivity across providers within the professional classes of NPs and physicians. 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 on-duty providers. Using a method developed by Efron (2016) and adapted by Kline, Rose, and Walters (2022), we deconvolve the estimates of provider-specific productivity into underlying productivity distributions for each of the two professional classes. We find strikingly wide variation in productivity within professions and substantial overlap between the productivity distributions of NPs and physicians. The probability that a randomly chosen NP is more productive than a randomly chosen physician can be as large as 38 percent. Within each professional class, the productivity distribution implies a greater medical spending of around $650,000 per year under a provider at the 25th percentile of productivity than under a provider at the 75th percentile—about three times the mean annual spending difference between NPs and physicians.

We extend our distributional analysis of productivity to examine the complexity of cases assigned to and the wages paid to individual providers. While productivity differences are much larger within each profession than the average difference between NPs and physicians, within each profession, a provider's productivity shows a limited relationship with her wages or the complexity of her assigned cases. This starkly contrasts with differences in case assignment and wages between NPs and physicians: EDs assign noticeably less complex cases to NPs—enough to reverse OLS estimates for key outcomes relative to the corresponding IV estimates—and pay NPs half the wages of physicians. In other words, a provider's productivity is far from a sufficient statistic explaining her pay or determining her case assignment; professional class is a much stronger predictor of both wages and assigned case complexity.

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 NP provision of care. A recent body of research has examined the impacts of liberalizing state "scope of practice laws" for NPs.[2] By design, these papers study the 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 the effect of assigning patients to NPs versus physicians.[3] Despite the average productivity difference between NPs and physicians, our findings do not imply inefficiency in using NPs within organizations. NPs could provide valuable additional labor supply, and heterogeneity in treatment effects by case complexity and NP experience suggests potential returns to optimal case assignment and further training of NPs.

Perhaps more striking, productivity variation within each of the NP and physician professions is much larger than the average productivity difference between the two professions. This finding puts the prior literature on practice variation (e.g., Epstein and Nicholson 2009; Gowrisankaran, Joiner, and Léger 2023) in context: In our setting, variation within professions leads to productivity differences several-fold larger than the overall productivity difference between two professions with starkly different selection and training processes. In contrast to the assignment of cases between NPs and physicians, in which less-complex cases are, on average, assigned to NPs, we find essentially no matching between case complexity and provider productivity within professions. This suggests substantial informational and organizational barriers to such "skill-task matching" within professions, though professional boundaries may provide a mechanism for this matching between professions (Acemoglu and Autor 2011).

Second, our research relates to the widespread practice of occupational licensing, affecting about a third of all jobs in the US (Kleiner and Krueger 2013). The existing literature suggests that occupational licensing

2. The findings of this literature are varied and somewhat mixed. Perry (2009), Kleiner et al. (2016) and Dillender et al. (2022) find that these laws affect 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. Yet 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.

3. An older medical literature has raised this question, but the generally small numbers of providers and other features of these earlier studies limit inference on systematic differences between the classes of providers. 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 in a handful of clinic locations. The studies tend to focus on primary care settings and 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."

Recently, in examining correlations across performance metrics among VHA primary care providers, Currie and Zhang (2021) find that non-physician primary care providers (NPs and physician assistants) are not correlated with worse outcomes than physicians. The paper's study period is mostly before the VHA implementation of NP full practice authority, and the VHA had not allowed physician assistants to practice independently as of 2022, thus placing non-physician providers mostly under physician oversight.

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).[4] Studies in this literature compare differences in quality within professions (along the margin of occupational licensing), while ours compares two competing professions differing 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 health care (e.g., Doyle, Ewer, and Wagner 2010; Currie and MacLeod 2017; 2020; Chen 2021; Chan, Gentzkow, and Yu 2022) and more broadly (e.g., Gennaioli et al. 2013; Chetty, Friedman, and Rockoff 2014). Our study contributes to this literature by comparing the productivity of two distinct professional classes with human capital differences that may stem from selection and training. In the rich ED setting, we also uncover key mechanisms connecting productivity to human capital along dimensions of experience, information-gathering, decision-making, and case complexity. Our results suggest that professional selection and training may give rise to important productivity implications, though these implications may be small relative to the variation within professions.

Fourth, a broad set of questions concerns the distribution of wealth in society across occupations and strata of educational attainment. In recent decades, societies have witnessed an increased concentration of wealth in occupations associated with high human capital (Smith et al. 2019). Training to reach the highest levels 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 larger than wage differences, at least in our resource-intensive and information-dependent setting within health care. Yet, the lack of relationship between worker-specific productivity and wages within professions suggests frictions in observing or contracting by actual productivity across similar workers (e.g., Acemoglu and Pischke 1998). 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 and responses. Section 6 presents analyses on policy-relevant counterfactual scenarios. Section 7 reports distributions of productivity, case assignment, and wages within professions. Section 8 concludes.

4. 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).

2 Background and Data

2.1 NPs and Physicians in the US

To understand the physician and NP professions in the US, it is instructive to consider their distinct origins in the American context. According to the landmark work by Starr (1982), "among the professions, medicine is both the paradigmatic and the exceptional case: paradigmatic in the sense that other professions emulate its example; exceptional in that none have been able to achieve its singular degree of economic power and cultural authority." Aided by scientific advances and demographic shifts, the US medical profession in the early twentieth century captured authority by standardizing education and licensing toward a scientific orientation, in the process excluding a large swath of practitioners (Brown 1979; Larson 1979).[5]

The "professionalization" of American nursing began in the 1870s, with the establishment of the first three nursing schools in the US. In contrast to the scientific orientation of the medical profession, the driving force behind the nursing profession was to install (female) staff in hospitals to improve hygiene and cleanliness (Ashley 1976). Hospitals generally employed nurses and subjected them to downward wage pressure (Staiger, Spetz, and Phibbs 2010; Maggs 2016). NPs emerged from the nursing tradition in the 1960s, in the setting of increasing specialization in medicine, worsening access to care in urban and rural areas, and new federal funding from Medicare and Medicaid to increase the training of providers (Fairman 2009; Hallett 2016). Over the next few decades, pressures to contain costs and increase throughput further expanded the boundaries of NP practice in a variety of settings (e.g., primary care, emergency care) (Fairman 2009; Kleinpell, Cook, and Padden 2018). The growth of the NP workforce has been a distinctly American phenomenon.[6]

Present-day NPs and physicians remain starkly different regarding training, income, and social class, despite performing overlapping tasks in many settings. Physicians undergo a highly selective process and long training periods to enter the profession. They comprise the 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 come from families in the bottom quintile (Kahn

5. Much of this transformation centered around the Flexner (1910) Report, which strongly advocated for a scientific orientation of medical education and the exclusion of alternative practices (Beck 2004). Following the report, more than half of medical schools closed or consolidated (Patel and Rushefsky 2004). All but two of the historically Black medical schools closed as a result (Sullivan and Suez Mittman 2010); medical schools, which had begun admitting women, reverted to male-only admittance.

6. In a survey of 39 countries, only 11 countries (the Netherlands and 10 English-speaking countries) granted NPs clinical autonomy on seven clinical dimensions and defined educational requirements to become an NP (Maier and Aiken 2016). Of the eight countries studied in greater detail, only the US, Canada, and the Netherlands included NPs in their health care workforce planning (Maier et al. 2018). As of 2016, the US had 175,000 NPs, representing 5.6 percent of nurses, while the next two highest countries, the Netherlands and Canada, had 2,700 and 3,600 NPs, representing 1.5 and 1.3 percent of nurses in these countries, respectively (Maier et al. 2016).

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.[7] Admission rates to nursing programs are around 10 times higher than admission rates to medical school, and nursing has been highlighted as a realistic path to the middle class for women of working-class backgrounds (Searcey, Porter, and Gebeloff 2015; Friedman, Laurison, and Macmillan 2017).[8]

2.2 VHA and ED Setting

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

Several features make the ED a setting well suited to studying provider productivity. First, each patient visit is generally assigned to a single ED provider. Such independence allows us to attribute patient outcomes to individual ED providers. Second, patient flow in the ED is highly unpredictable, while provider schedules are typically set well in advance. Variation in NP availability is thus unrelated to the types of patients arriving. Third, patients present at the ED with a wide spectrum of conditions, ranging in complexity and severity, which provides an opportunity to investigate productivity across a range of tasks. Finally, the ED is an important setting using the NP workforce. Across the nation in 2019, NPs saw 13 percent of ED visits in total and saw 6 percent of ED visits without physician involvement (Cairns and Kang 2022). In the VHA, the share of ED visits seen by NPs has increased to 11 percent in 2019—close to the share of visits seen by NPs in primary care at around 20 percent (Morgan et al. 2012).[10]

In Appendix Table A.1, we report the characteristics of NPs and physicians working at the VHA and non-VHA EDs. NPs in VHA EDs are representative of their non-VHA counterparts in female share (about 80 percent), while they appear to be more experienced (as indicated by age, 51 versus 43 years old). Among ED physicians, those practicing at the VHA are slightly more likely to be female than those outside of the VHA (34 versus 27 percent), but the two groups have a similar average age (48 versus 46 years old). Appendix

7. According to the Association of American Medical Colleges (2020), physicians must complete a four-year undergraduate degree, a four-year Doctor of Medicine (MD) degree, and three to seven years of residency training. According to the American Association of Nurse Practitioners (2020), NPs must complete a four-year Bachelor of Science in Nursing degree and may choose between one to two years in a Master of Science in Nursing degree or three to four years in a Doctor of Nursing Practice degree.

8. 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.

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).

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

Table A.1 also compares NPs practicing at the ED and the overall NP workforce, showing that ED NPs are representative of the NP workforce in age and gender.

2.3 Data

We use administrative health records from the VHA, the largest health care delivery system in the US, serving more than nine million veterans. For each ED visit, the data record the type of provider treating the patient (i.e., NP or physician), resource utilization, and patient health outcomes (e.g., length of stay, mortality). The data also contain detailed information on patient characteristics (e.g., demographics, comorbidities, and vital signs) and provider characteristics (e.g., birthdate, gender). The large sample size, as well as the availability of detailed information (e.g., time stamps for measuring ED length of stay, provider orders), enable a comprehensive analysis of NP- and physician-provided care.

Sample Construction. We restrict our analysis sample in the following ways, summarized in Appendix Table A.2. First, we restrict the sample to ED visits between January 2017 and January 2020, i.e., 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 focus on visits to VHA EDs using NPs to treat patients and in months after the ED adopted the full practice authority policy. Though the VHA granted full practice authority to NPs at all VHA facilities, local facilities varied in when they adopted the policy and whether they used NPs in the ED.[12] Fourth, to examine a single margin between NPs and physicians, we exclude EDs that used non-physician providers other than NPs (mainly physician assistants).[13] Finally, we drop a small number of cases with missing age or gender or aged under 20 or above 99 years old. The final sample contains 1.1 million cases, over 44 EDs.[14]

Outcome Variables. To measure medical resource use, we include two primary outcomes that are frequently

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 per shift in the evening/night. Our conversations with VHA ED leadership revealed that EDs are required to have at least one physician on duty at all times.

12. We define a VHA ED as having granted full practice authority to NPs and using NPs in a month if it has at least 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.

13. In addition, unlike NPs, physician assistants had not been granted full practice authority at the VHA as of 2022 nor in any state as of mid-2019. In Section 4.5, we expand the sample to cases in all EDs that use NPs despite the use of physician assistants but exclude cases in ED-day cells with physician assistants to focus on the margin between NPs and physicians. This analysis expands the sample from 1.1 million cases across 44 EDs seen by 1,348 physicians and 156 NPs in our main analysis to 2.2 million cases across 110 EDs seen by 3,499 physicians and 491 NPs. We find similar results in this expanded sample.

14. Appendix Table A.3 shows that EDs included in our sample are a representative set of all VHA EDs, as measured by ED, provider, and patient characteristics. The EDs in our sample include those in large metropolitan areas (e.g., New York City, Los Angeles, the Bay Area) as well as those in smaller areas (e.g., Detroit, Michigan; Madison, Wisconsin).

used in the ED setting: (i) the patient length of stay (i.e., the time between patient assignment to the provider and patient discharge) and (ii) the cost of care during the ED visit (excluding costs due to a resulting hospital admission, measured separately next).[15] 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 use linked inpatient data to construct indicators of 30-day preventable hospitalization as defined by Agency for Healthcare Research and Quality (2021).[16] We exclude from 30-day preventable hospitalization the inpatient admissions immediately following the ED visit, as they reflect the hospital admission decision described above.

In examining mechanisms behind the effect of NPs, we include the following sets of outcomes: (i) whether the provider orders consults (which are typically from specialists outside of the ED); (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; Neuman, Bateman, and Wunsch 2019).

Descriptive Statistics. Table 1 summarizes the characteristics of the cases included in our analysis. In addition to demographics, we measure patient comorbidities as Elixhauser indices (Elixhauser et al. 1998), which are 31 indicators for comorbidities (e.g., cancer, diabetes) based on patient medical histories in the prior 365 days. We also report the average length of stay, average ED spending (inflation-adjusted to 2020 dollars), and 30-day preventable hospitalization rate. Column 1 shows the characteristics of the overall sample. Columns 2 and 3 compare cases treated by NPs with those treated by physicians. Along several dimensions, cases treated by NPs are healthier than those treated by physicians: Cases treated by NPs are younger (60.7 versus 62.5 years old), have fewer Elixhauser comorbidities (3.2 versus 3.7), and have fewer outpatient visits and fewer inpatient stays in the prior 365 days (5.7 versus 6.4 outpatient visits and 0.4 versus 0.7 inpatient stays). Consistent with selection, 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 (0.8 versus 1.4 percentage points).

15. For length of stay, we use detailed time-stamped data on patient assignment and discharge. For the cost of ED care, we use cost accounting by the VHA that measures resource utilization in each ED visit.

16. Specifically, preventable hospitalizations are defined by the well-set algorithm developed by Agency for Healthcare Research and Quality (2021), which include hospital admissions due to an established set of conditions that are potentially preventable, e.g., hospitalizations due to diabetes with complications, heart failure, and hypertension.

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 availability of NPs on duty to instrument for whether an NP or a physician treats a case. In this section, we begin by describing our instrumental variables (IV) approach. We then show evidence that supports the validity of our identification strategy.

3.1 Specification

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

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

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

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 number of NPs on duty between 8 a.m. and 6 p.m., our analysis time window) at the ED on the day that case i visits.[17] 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 class of provider under a different number of NPs on duty.

The vector $\mathbf{T}_i$ encodes 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 the 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 potential systematic differences.[18]

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 VHA facilities in the prior

17. 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. 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. The main analysis includes the index case in calculating Z_i. 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 and to one that leaves out the index case in defining whether an NP is on duty.

18. While fixed effects for ED-by-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.

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 three-digit ICD-10 code of patient's primary diagnosis of the visit.[19] For each patient covariate with missing values, we add an indicator for missing values and replace the missing values with zero. Finally, ε_i and ν_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: relevance, conditional independence, exclusion, and monotonicity. In this section, we summarize empirical evidence supporting the validity of the identifying assumptions.

Relevance. 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, even conditioning on $\mathbf{T}_i$) and is close to linear.

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. A related 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 large variation in the number of NPs on duty within EDs across days (standard deviation of 0.49; Appendix Figure A.2 shows the distribution of the number of NPs on duty within each day of the week and month). Consultations with VHA ED providers and administrators point to sources of random variation in scheduling:[20] As EDs have long open hours (typically 24/7), EDs rotate providers

19. 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 three-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 three-digit diagnosis indicators or not. In Appendix A.1, we also show that NPs and physicians appear to be similar in their coding of three-digit diagnoses.

20. We spoke with eight ED physicians and NPs about scheduling; we also distributed a survey via the VHA national ED leadership

on duty. Providers' on-duty schedules are generally set months in advance, restricting the possibility of arranging on-duty providers based on specific conditions occurring in the ED on the day. Each ED generally has a staffing model that pre-specifies staffing level in shifts defined by time categories (matching $\mathbf{T}_i$) to meet patient care needs predicted by the time categories. Conditional on the time categories, there is generally no systematic variation in scheduling within EDs.[21] In the empirical evidence below, we show that patient characteristics, as well as on-duty physicians' and NPs' characteristics, are well balanced across the number of NPs on duty (conditional on $\mathbf{T}_i$), consistent with quasi-random variation in the instrument. Otherwise (e.g., if the variation is driven by patient or staffing shocks), we could observe systematic changes in patient and/or on-duty physicians' and NPs' characteristics with the instrument. Appendix Figure A.3 additionally decomposes the variation of NP staffing. We find that conditional on $\mathbf{T}_i$, patient characteristics, on-duty provider characteristics, and other factors that may vary across days (e.g., patient volume) have virtually no explanatory power for the number of NPs on duty.

Conditional Independence. For our instrument to be valid, the number of NPs on duty must be uncorrelated with potential patient outcomes, conditional on baseline controls $\mathbf{T}_i$. The institutional features described above support the conditional independence assumption. Further, 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 stable across the instrument, conditioning on $\mathbf{T}_i$. For completeness, Appendix Figure A.4 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-by-time-category indicators, see Appendix Figure A.5), 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 empirical evidence, we examine the stability of our IV estimates under different sets of controls for patient covariates. Specifically, we divide observable patient characteristics into eight groups and estimate separate regressions that control for each of the $2^8 = 256$ different combinations of patient covariates. We show in the 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 predict patient outcomes. In sum, conditional on $\mathbf{T}_i$, there appears to be little relationship

to VHA ED administrators about scheduling and received responses from VHA ED administrators in seven states.

21. There may be revisions to the on-duty schedules, but most often for providers' personal reasons (e.g., family events) that are arguably unrelated to the characteristics of arriving patients; in addition, the revisions have to be done in advance of the shifts.

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 restriction. That is, the number of NPs on duty impacts patient outcomes only through the patient's probability of being treated by NPs, not through any other channels. After discussing our main results, we present empirical evidence supporting the validity of the exclusion restriction. We first show that characteristics of both on-duty physicians and on-duty NPs are well balanced across the number of NPs on duty conditional on $\mathbf{T}_i$, consistent with quasi-random variation in the instrument as well as our IV estimates unlikely being driven by different sets of physicians or NPs available across days. Second, we find no evidence of spillovers that NP presence influences physician performance. Third, we investigate a series of alternative explanations, finding no evidence indicating a 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 a 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.6, 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, number of Elixhauser comorbidities, and predicted 30-day mortality.[22] Appendix Figure A.6 shows that for all subsamples, the first-stage estimates are positive and statistically different from zero, consistent with the validity of the monotonicity assumption.[23]

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. Yet they achieve less favorable patient outcomes, as measured by 30-day preventable

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

23. An interesting pattern in Appendix Figure A.6 is that the first-stage estimates appear to be larger for healthier patients: younger patients, those with fewer Elixhauser comorbidities, and those with a lower predicted 30-day mortality. This pattern reflects that compliers are more heavily concentrated among healthy patients. In Section 4.3, we compute the characteristics of compliers and never-takers. We find consistent evidence that compliers are healthier than the overall sample, while never-takers are riskier.

hospitalizations. We also characterize compliers relative to the overall sample, present evidence supporting the exclusion restriction, and consider a series of additional robustness checks.

4.1 Length of Stay and Cost

As summary measures of 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.[24]

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 physicians, at least in terms of observable characteristics shown in Table 1. Exploiting plausibly quasi-random variation in the patient probability of being treated by NPs, the IV estimates (Columns 3 and 6) suggest that NPs 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 sample means, the NP effects equal an 18-minute increase in length of stay and a $66 increase in cost per ED visit.

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 three-digit primary diagnosis of the visit. We then estimate separate regressions that control for each of the $2^8 = 256$ 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

24. We form these predictions using linear regressions of actual outcomes on the full set of patient covariates $\mathbf{X}_i$ in Equations (1) and (2).

and decline sizably in magnitude with the addition of patient controls. For example, in the OLS results, 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 suggest selection bias due to unobservable patient characteristics. In contrast, the IV estimates are remarkably robust to controlling for any combination of patient covariates: Despite any 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.[25]

4.2 Hospital Admission and Patient Outcomes

Having examined resource use in the ED, we next assess NP effects on hospital admission and downstream patient outcomes, in Table 3. In our overall sample, the hospital admission rate does not differ significantly between NPs and physicians, but we show in Section 5 that NPs increase admissions for severe cases. We find no significant effect of NPs on 30-day mortality for most cases, but we find evidence in Section 5 for increases in mortality for a subset of highly severe cases.[26]

Finally, we find a significant NP effect on increasing patient 30-day preventable hospitalizations: Compared to physicians, NPs raise 30-day preventable hospitalization rate by 0.25 percentage points, which is equivalent to a 20 percent increase compared to the mean of the sample. Appendix Figure A.7 plots actual and predicted outcomes against the instrument for the three outcomes in this subsection; Appendix Figure A.8 shows that the IV estimates for these three outcomes are remarkably robust to the inclusion of $2^8 = 256$ different combinations of patient controls.

Taken together, empirical evidence suggests that NPs and physicians operate on different production

25. Another possible explanation for the divergence between the OLS and IV estimates is heterogeneity in treatment effects, since OLS reports the average effect 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 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 from the IV estimates, suggesting that the difference between the OLS and IV estimates cannot be accounted for by heterogeneity in NP effects, at least not by heterogeneous effects across observables.

26. The 30-day mortality of the overall sample is low at 1.25 percentage points, potentially rendering the IV estimate for mortality effects noisy and uninformative. When focusing on a type of patients with high mortality—those with a sepsis diagnosis (30-day mortality: 11.5 percentage points)—we find a marginally significant and clinically meaningful NP-driven increase in mortality (IV point estimate: 24.5 percentage points, p-value: 0.106). A related question is why cases with severe conditions such as sepsis are assigned to NPs. While the data show a reduced probability of being assigned to NPs among riskier patients, the probability remains positive. A possible explanation is that triage providers may misevaluate severity (Chan and Gruber 2020) and assign to NPs cases that should have been assigned to physicians. Another possible explanation is that when physicians are occupied with earlier cases, it could be more efficient to assign severe cases to available NPs than delaying care.

functions: NPs use more inputs (longer lengths of stay and higher costs) but achieve less favorable patient outcomes (higher 30-day preventable hospitalization rates). In other words, comparing NPs and physicians as two professional classes, NPs, on average, exhibit lower productivity than physicians. Yet note that this evidence may not indicate that we can cut back on care for NPs without compromising patient outcomes. A lower production function may still exhibit positive returns to inputs (Chandra and Staiger 2007; Silver 2021; Chan, Gentzkow, and Yu 2022). That is, higher intensity of care may be allocatively efficient for NPs.

4.3 Complier Characteristics

Our IV estimates represent the LATE, i.e., the average causal effect among complier cases 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.4 reports the results. Consistent with NPs treating less severe cases, compliers are healthier than the average case. Compared to the average case, compliers are younger, have fewer Elixhauser comorbidities, have fewer inpatient stays in the prior year, and exhibit lower predicted mortality. Appendix Table A.4 also examines characteristics of never-takers (of NPs), following an approach from Dahl, Kostøl, and Mogstad (2014) that we detail in Appendix A.2. In line with the notion that NPs treat healthier cases than physicians, never-takers are riskier than the average case, and both are riskier than compliers.

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 exclusion restriction. We first show that the characteristics of both on-duty physicians and on-duty NPs are well balanced across the number of NPs on duty. Second, we assess and find little support for the possibility of productivity spillovers that NP presence influences physician performance in our setting. Third, 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 on-duty 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 on-duty physicians' average characteristics (weighted by the number of cases treated by each physician) are independent of the number of NPs on duty, conditional on ED-by-year, ED-by-month, and ED-by-day-of-the-week indicators.[27]

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 patient 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 construction details). Figure 5 shows that each of these physician characteristics is well balanced across the instrument, conditional on the baseline controls.

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.[28] Following Figure 5, Figure 6 shows that an analogous set of NP characteristics are well balanced across days with different numbers of NPs, conditional on baseline controls.

The balance in on-duty physicians' and NPs' characteristics across the instrument also supports quasi-random variation in the instrument, as with the balance in patient characteristics. Otherwise (e.g., the variation is driven by patient or staffing shocks), we would observe systematic changes in these characteristics with NP staffing.

Assessing Productivity Spillovers. We then consider the possibility of spillovers between NPs and physicians. NP presence may influence physician performance: For example, if NPs ask physicians in the ED for assistance, they could slow down physicians. Alternatively, a change in peers from days without any NP to days with NPs may influence physician performance as, e.g., 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 may expect the outcomes of patients treated by NPs to depend on the quality of physicians on duty. Using value-added measures described in Appendix A.3 and experience measured by age, we find no such relationship in Appendix Table A.5. Second, with spillovers—either

27. The empirical specification takes the form $\overline{y}_{jd} = \tilde{\lambda} Z_{jd} + \tilde{\mathbf{T}}_{jd}\tilde{\eta} + \varepsilon_{jd}$, where $\overline{y}_{jd}$ 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-by-year, ED-by-month, and ED-by-day-of-the-week indicators. We cluster standard errors by ED. Since our main sample has only 44 EDs, a potential question is 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 using Wild cluster bootstrap as suggested by Cameron and Miller (2015); we find no meaningful change in the standard error estimates.

28. This concern applies to our baseline IV strategy since it uses variation in the number of NPs in addition to whether there is an NP on duty. In Section 4.5, we include alternative estimations using variation in the extensive margin of whether there is any NP on duty and restricting the sample to days with zero or only one NP on duty; results are virtually unchanged.

through assistance or peer pressure, or any other channels by which NP staffing may affect physician performance—outcomes for patients treated by physicians could change with the presence of NPs. Directly regressing outcomes of patients treated by physicians on the NP presence suffers from patient selection since physicians are allocated riskier cases on days with NPs (as healthier cases are assigned to NPs). To circumvent this issue, we look at cases arriving between 5 and 8 a.m., i.e., patients who arrive before the typical start of NP shifts so that they are unlikely to be assigned to NPs, but whose stay overlaps with NP shifts so that their physicians could be subject to spillover effects from NPs.[29] As shown in Appendix Table A.6, Panel A, we find no evidence of spillovers from NP presence on physicians' patients overall; in Panel B, we focus on days with high workloads, when NP spillovers may be more detectable, and again find no evidence of spillovers in this subsample.[30]

Robustness to Additional Factors. Finally, we show the robustness of our IV estimates to 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 (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.[31] 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.8 shows that our IV estimates are remarkably robust to controlling for these factors.

We also ask if the estimated NP effect is driven by patient-provider gender mismatch, since the vast majority of patients are male (91 percent), physicians are primarily male (74 percent), and NPs are mostly female (79 percent). Appendix Table A.9 explores this possibility by asking whether the effect of NPs varies by whether the patient's and provider's genders match. The results show little heterogeneity.

29. To restrict further the possibility of patient selection between NPs and physicians, we exclude patients arriving between 5 and 8 a.m. in ED-day cells with any patient assigned to NPs.

30. A potential question is whether the increased average health risk of patients assigned to physicians on days with NPs affects physicians' overall performance, violating an exclusion restriction. To assess this concern, Appendix Table A.7 controls for multiple measures of the average health risk (age, number of Elixhauser comorbidities, and predicted 30-day mortality) of patients assigned to physicians in the ED-day cell. The results show that our IV estimates are remarkably stable, suggesting that the different sets of patients assigned to physicians on days with NPs are unlikely to affect our IV estimates.

31. 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 insufficient to substitute for one physician given that NPs take longer to discharge patients but do not handle more patients simultaneously); consequently, the 2SLS estimates for the NP effect conditional on the total number of on-duty providers would be confounded by lower staffing.

4.5 Additional Robustness Checks

Appendix Tables A.10-A.14 report additional robustness checks. Appendix Table A.10 shows that our findings are stable with alternative standard error clustering approaches: clustering by ED-day or two-way clustering 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 remain stable with two-way clustering by ED-day and provider relative to the baseline model. Panels A and B of Appendix Table A.11 show the robustness of our estimates to, respectively, an alternative count of on-duty NPs that includes any NP with at least one case (instead of two cases) in the analysis time window of an ED-day cell and another count that includes any NP with any case besides the index case.[32] In Appendix Table A.11, 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.12 shows that our results remain similar when looking at the margin between days with no versus only one NP on duty. Appendix Table A.13 shows that our results for hospital admissions in the ED visit and 30-day preventable hospitalizations are robust to considering hospital stays outside of the VHA.[33] Appendix Table A.14 expands the sample to all 110 VHA EDs that use NPs—doubling the number of cases and tripling the number of providers in our sample—and finds the results are similar.[34]

5 Mechanisms and Responses

The evidence in the previous section suggests that NPs have lower productivity than physicians: They use more medical resources and produce less favorable patient outcomes. This section examines mechanisms and responses related to this productivity gap. First, we show that experience may play a role in the NP-physician gap in some (though not all) outcomes. Second, we show NP responses to lower skill in their clinical decision-making, in calling on external resources and setting prescription thresholds; these responses, in turn, manifest as lower productivity. Third, we show that the NP effect is larger for more complex and severe patients, suggesting a comparative disadvantage for NPs in treating these patients. Finally, we show patient assignment responses toward optimality given the NP comparative and absolute disadvantage: NPs receive

32. See Section 3.1 and footnote 17 for explanations for these two robustness checks.

33. Since a share of patients has health insurance coverage in addition to the VHA's (mainly Medicare), we report robustness checks that include hospital stays outside of the VHA for patients who enroll in both the VHA and traditional Medicare. Note that this is likely an upper-bound estimate of the possible bias for our sample since patients without non-VHA health insurance are much less likely to have hospital stays outside of the VHA. Regardless, the estimates are stable.

34. As described in Section 2.3, to focus on a single margin between NPs and physicians, our main analysis focuses on EDs that use only NPs and physicians; in Appendix Table A.14, we include all VHA EDs that use NPs regardless of using physician assistants (but exclude cases in ED-day cells with physician assistants).

healthier patients and take on a lower share of the caseload when the ED is less constrained to meet demand.

5.1 Provider Experience

First, we ask whether experience impacts the magnitude of the performance difference between NPs and physicians. The professions of NPs and physicians entail stark differences in the training that new members undergo and in the selectivity of choosing new members. Whether physicians are more productive than NPs because of their training or innate ability has clear policy relevance. While it is difficult to disentangle these two mechanisms, the extent to which the NP-physician performance gap varies with experience may shed suggestive light on this question. If NPs could be made more productive with more extensive training, we may see that the gap narrows with experience. On the other hand, if the gap derives from lower innate ability, we may see that the gap persists or even widens with experience.

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 three-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.

Our empirical model takes the following form:

$$y_i = \delta_1 \text{NP}_i \times \text{Experience}_i + \delta_2 \text{NP}_i + \delta_3 \text{Experience}_i + \mathbf{T}_i \eta + \mathbf{X}_i \beta + \varepsilon_i. \quad (3)$$

Experience_i denotes standardized experience within each provider type.[35] 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 some (but not all) outcomes, experience predicts a smaller NP-physician performance gap. Panel A of Table 4 examines the role of specific experience. For length of stay, Column 1 indicates that a one-standard-deviation increase in specific experience among NPs and physicians is associated with a 5.8 percent decline in the performance gap, reducing the gap at the mean experience levels by about half. The NP-physician gap in costs similarly decline with specific experience. Panel B shows that increasing general experience by one standard deviation in both NPs and physicians is associated with a 10 percent decline in the NP-physician gap in length of stay. However, the NP-physician gap in 30-day preventable hospitalizations remains stable despite the level of general or specific experience, suggesting that experience may not be able

35. 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.

to fully eliminate the NP-physician performance difference.

We examine alternative measures of experience to address measurement concerns. One concern is that, since we do not observe cases treated by providers since the start of their careers, our measures of experience are imperfect representations of providers' true experience. To mitigate this concern, we restrict the sample to cases visiting in or after 2018, so that our experience measures have at least a one-year look-back window. We also measure experience based on cases seen in the prior year (i.e., 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 experience gained in the relatively distant past. A second concern is that the number of cases a provider has seen may capture speed, which may affect productivity independent of experience (e.g., faster providers accumulate more cases and discharge patients earlier). 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 Tables A.15 to A.17 show that our estimates remain qualitatively similar under these alternative measurements.[37]

5.2 Clinical Decision-Making

Next, we examine clinical decision-making that may respond to the lower human capital, particularly lower diagnostic skill, of NPs. With lower diagnostic skill, providers may draw on more external resources, such as consults and diagnostic tests. They may also adjust treatment thresholds for decisions with asymmetric costs between type I and type II errors (Chan, Gentzkow, and Yu 2022). While these could represent optimal responses to skill differences, they increase the cost of providing care, manifesting in productivity differences.

Informational Resources. Columns 1-3 of Table 5 report the effect of NPs on the use of informational resources, using the 2SLS estimation 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, the two primary diagnostics in the ED setting. NPs increase CT scan and X-ray ordering by 1.2 and 2.0 percentage points, respectively, or 8.3 and 6.9 percent of the respective sample means.

These results suggest NPs are more likely to collect resource-intensive information from external sources

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 three-digit ICD-10 primary diagnosis as the current case the provider treated in the proceeding 365 days. We exclude from this estimation cases visiting in the first year of our analysis since we cannot fully observe their providers' experience in the proceeding 365 days.

37. We do not include age as a measure of general experience in this heterogeneity analysis because NPs often practice as registered nurses for varying years before becoming an NP, thus, unlike for physicians, age is a noisy measure of experience for NPs.

than physicians. This could directly increase lengths of stay and medical costs, since consults and diagnostics take time and resources. On the other hand, consults and diagnostics allow lower-skilled providers to improve decision-making by incorporating information from other experts.

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 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 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.[38] We estimate the NP-physician difference in prescriptions using the 2SLS estimation specified in Equations (1) and (2). Column 4 of Table 5 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, 6.3 percent of the sample mean.[39] The joint evidence from prescription thresholds is consistent with NPs responding to lower skill.[40]

5.3 Case Complexity and Severity

In this section, we exploit the wide variety of cases that arrive at the ED to examine heterogeneity in the effects of NPs by case complexity and severity. Following Imbens and Rubin (1997), we estimate complier

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

39. Since opioids apply to a wide range of conditions, we include all patients in examining opioid prescriptions. As antibiotics 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.

40. The different treatment thresholds between NPs and physicians in consults, diagnostic tests, and prescriptions could reflect NPs responding to lower skill, being less confident in their skill, or exhibiting different risk preferences without any difference in skill. While we cannot rule out the latter two explanations, the pattern that NPs achieve less favorable outcomes (as measured by 30-day preventable hospitalizations) despite using more resources supports lower skill of NPs.

potential outcomes under NPs and under physicians. Specifically, we estimate complier potential outcomes under NPs using the following IV regression:

$$y_i \cdot \mathrm{NP}_i = \sum_{g=1}^{G} \mathbf{1}(\mathrm{Group}_i = g) \left[\delta_g \mathrm{NP}_i + \lambda_g\right] + \mathbf{T}_i \eta + \mathbf{X}_i \beta + \varepsilon_i, \tag{4}$$

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 severity. As a natural extension of our main IV model, we instrument for the interactions between $\left\{\mathbf{1}(\mathrm{Group}_i = g)\right\}_{g=1}^{G}$ and NP_i by interacting $\left\{\mathbf{1}(\mathrm{Group}_i = g)\right\}_{g=1}^{G}$ with Z_i, where Z_i is the number of NPs on duty. We estimate complier potential outcomes under physicians using an IV regression similar to Equation (4) but with a dependent variable of $y_i \cdot (\mathrm{NP}_i - 1)$.

We consider two partitions of cases. First, we divide cases into quartiles by their number of Elixhauser comorbidities and refer to higher quartiles as more complex cases. Second, we divide cases by whether condition severity measured by 30-day mortality of the three-digit diagnosis is equal to or above the 95th percentile of the sample. The 30-day mortality for this top-severity group is 8.6 percentage points against 1.2 percentage points for the whole 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.[41]

As shown in Figure 7, the effect of NPs on lengths of stay and medical costs grows with case complexity and severity. For example, for cases in the lowest complexity quartile, NPs increase their length of stay by about 5 percent; 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 raise their hospital admission rate by about 30 percentage points, nearly a 100 percent increase from the potential admission rate under physicians. Appendix Table A.19 summarizes the heterogeneity in treatment effects.[42] The table further examines the NP effect among four severe conditions: stroke, acute myocardial infarction, sepsis, and heart failure. We consistently find larger NP effects among cases with these severe conditions than others.

41. Appendix Table A.18 summarizes the 10 most common three-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. While the data show a reduced probability of NP assignment among riskier patients, the probability remains positive (the share of patients assigned to NPs among patients whose condition severity is below and at least as high as the 95th percentile of the sample are, respectively, 0.24 and 0.11). Possible explanations for why severe cases are assigned to NPs, as described earlier, may be: (i) Triage providers may misevaluate patient severity (Chan and Gruber 2020) and assign severe cases to NPs; (ii) when physicians are occupied with earlier cases, it may be more efficient to assign severe cases to available NPs than delaying care.

42. Results in this table are estimated using Equation (4) with patient outcome y_i as the dependent variable. By construction, this dependent variable is the difference between the dependent variables used to estimate potential outcomes: $y_i = y_i \cdot \mathrm{NP}_i - y_i \cdot (\mathrm{NP}_i - 1))$.

We do not find increasing NP effects on 30-day preventable hospitalizations with case complexity or severity (Column 5 of Appendix Table A.19). If NPs are less skilled at treating more complex or more severe cases, they may obtain worse outcomes for these cases. 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.[43]

The NP-physician performance difference among patients in the bottom two complexity quartiles provides some suggestive support for the generalizability of our findings to less complex settings: While ED patients overall are riskier than primary care patients, ED patients in the bottom two quartiles of complexity have similar mortality risks as the average primary care patient (30-day mortality: 0.56 and 0.53 percentage points, respectively).[44] Yet we also emphasize that the ED setting features more uncertainty than primary care, as providers have less first-hand knowledge of patients' baseline health. Consequently, variation in provider skill could generate larger effects in the ED.

5.4 Patient Assignment

Finally, we examine patient assignment between NPs and physicians. On the whole, the evidence on case heterogeneity in Section 5.3 suggests a comparative disadvantage for NPs in treating complex and severe cases. We do not find a set of cases in which NPs outperform physicians, which suggests that NPs are also at an absolute disadvantage on average, at least in the ED setting. These stylized facts imply a qualitatively optimal assignment of patients to NP versus physician provider classes, in the sense of skill-task matching in organizations (Acemoglu and Autor 2011): Of patients available, NPs should receive the healthier ones, and NPs should receive fewer patients when physicians have bandwidth to see them.

Appendix Figure A.9 provides descriptive insight into patient assignment by exploiting variation in NP staffing (i.e., the instrument) and variation in patient arrivals, conditional on our baseline controls. Panels A-C show that NPs overall are assigned healthier cases, consistent with Table 1. The average complexity and severity of cases assigned to NPs, as measured by patient age, comorbidities, and predicted 30-day mortality, increase with NP staffing, despite the average complexity and severity of all cases remaining stable. The pattern suggests an assignment process in which the first cases assigned to NPs have the lowest health risks; when more NPs (and fewer physicians) are available, cases incrementally assigned to NPs are riskier

43. As shown in Panel A of Appendix Table A.19, NPs sizably increase lengths of stay and medical costs for cases in the highest complexity quartile, without leading to a significant change in 30-day preventable hospitalizations. In contrast, NPs reveal a smaller-magnitude effect on lengths of stay and medical costs for cases in lower complexity quartiles but significantly raise 30-day preventable hospitalizations.

44. We estimate 30-day mortality of primary care patients using the VHA administrative data on primary care visits, between January 2017 and January 2020, i.e., our study period.

compared to those initially assigned to NPs but are still relatively healthy among the remaining cases to be assigned.[45] Panel D of the figure assesses the probability that a patient is assigned to an NP as a function of ED busyness measured by the number of other patients arriving in the analysis time window (i.e., 8 a.m. to 6 p.m.) of the ED-day cell. We find a small but clear trend in which patients are more likely to be assigned to NPs when the ED is busier, consistent with the efficiency of having NPs on standby when physicians are less occupied.[46]

6 Counterfactual Scenarios

In this section, we consider two counterfactual policy scenarios that quantify the average NP-physician productivity difference in financial terms. First, we consider the overall cost implications of substituting physicians with NPs by assigning 25 percent of cases across VHA EDs to NPs, instead of physicians. Second, we consider augmenting the existing supply of physicians with NPs. Overall, these analyses show 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 cases in VHA EDs to NPs—approximately the share of cases treated by NPs in our sample, which consists of EDs that are early adopters of NPs under full practice authority. We assume that the treatment effects 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 for severe cases (Section 5.3), and the costs of 30-day preventable hospitalizations (Section 4.2). We find an extra cost of $160 million per year for the VHA.[47]

45. As a result, cases left for physicians become riskier with more NPs. A potential question is whether the increased risk of cases assigned to physicians on days with more NPs affects physicians' performance, violating an exclusion restriction. To assess this concern, Appendix Table A.7 controls for multiple measures of the average health risk (age, comorbidities, and predicted 30-day mortality) of cases assigned to physicians in the ED-day cell. The results show that our IV estimates are highly robust.

46. This relationship is qualitatively the same when further conditioning on the number of NPs and the number of physicians staffing the ED on that day. A related question is whether the increasing NP assignment with ED busyness may affect the estimated NP effects. In contrast to OLS, a correlation between NP assignment and busyness per se would not affect estimates from our IV approach. In Section 4.4, we also show that our IV estimates are highly robust to controlling for the number of patients arriving.

47. 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). 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 within 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 associated with using NPs may be larger than the estimate reported above. In this cost calculation and the productivity estimation in Section 7 below, we take the societal perspective and view extra medical spending as costs. In practice, hospitals in the US health care system may not fully internalize the societal cost of hiring lower-productivity workers, as they may

This figure is approximately twice the yearly NP wage costs that the VHA would encounter to assign 25 percent of its ED cases to NPs.[48]

The calculation ignores potential changes in provider wage costs, which may be large, as the average NP wage is only half the average physician wage (Bureau of Labor Statistics 2021a; 2021b). 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 may be no wage saving when substituting physicians with NPs. For a conservative estimate, we consider the scenario where one NP may substitute for one physician. Under this scenario, we again arrive at net costs, $92 million per year, for assigning 25 percent of VHA ED cases to NPs.

Using LATEs from our quasi-experiment, this analysis is well suited to assess counterfactual outcomes for compliers, likely to encompass the 25 percent of cases assigned to NPs. Alternatively, we consider a more conservative counterfactual scenario where EDs assign the least complex 25 percent of cases (by the number of Elixhauser comorbidities) to NPs. We note that such an allocation may not always be feasible: For example, in hours when all arriving patients are risky, EDs may need to assign some complex cases to NPs. Applying the estimates for the lowest complexity quartile patients in Appendix Table A.19, we find extra costs of $81 million per year to the VHA. Such extra costs may be reduced to zero through lower NP wages if an NP can substitute for 0.85 physicians.

6.2 Augmenting Provider Supply with NPs

Despite possible extra spending, our findings do not imply that NPs are inefficient to use. When physician capacity is limited, hiring additional NPs to improve throughput and reduce wait times could nevertheless be efficiency-improving. To examine this concept, we consider the trade-off between reducing wait times and increasing 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 analysis, we ask, holding fixed the number of cases arriving and the number of physicians on duty, how additional NPs may affect patient wait time and downstream outcomes:

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be reimbursed for extra utilization and may not bear the costs of downstream outcomes.

48. 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 654 NPs would be needed for treating 25 percent of VHA's ED cases annually. We then multiply the number of NPs needed with the mean NP wage reported by Bureau of Labor Statistics (2021a), yielding a total wage estimate of $74.9 million per year.

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

$\mathbf{1}(Z_i = n)$ is an indicator for $n \in \{0, \ldots, 5\}$ NPs being on duty at the ED on the day case i visits.[49] 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) 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.

Figure 8 presents the estimated trade-off based on Equation (5). Panel A shows the trade-off between wait time and length of stay. Each dot plots a pair of δ_n with the estimated δ_n for wait time on the y-axis and the estimated δ_n for 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 hospital stay, as in Section 6.1, and calculate the sum of the coefficients for the three cost-related outcomes: the cost of ED care, the cost of hospital admission (for high-severity patients), and the cost of 30-day preventable hospitalization.[50]

As implied 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) and increase total medical spending by 15 percent, or about $238 per case, not including the cost of additional NP wages (Panel B).[51] Including the wage costs of additional NPs brings this figure to about $300 per case.[52] That is, roughly four-fifths of the additional spending to reduce wait time by hiring NPs come from the lower productivity of NPs, while only one-fifth comes from additional NP wage costs.

7 Productivity Within Professions

Up to this point, we have focused on estimating the average productivity difference between the professional classes of NPs and physicians. We show that this difference is large, likely even larger than the difference in average wages between the two professions. In this section, motivated by growing evidence of produc-

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

50. As shown in Section 5.3, we only observe significant NP effects on raising hospital admissions among severe cases, i.e., cases with a three-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.

51. Figure 8 shows that a 34-minute decline in wait time increases length of stay by 18 minutes and total spending by $268 per case (comparing the first and last dot in each panel). We rescale the tradeoff to a 30-minute decline in wait time for ease of interpretation.

52. 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.

tivity variation across providers (within professions), we measure the distribution of productivity within each profession and ask how this intra-professional variation in productivity compares to the difference in productivity between professions, in the case of NPs versus physicians.[53] Using the detailed administrative data of the VHA, we then evaluate the extent to which productivity within each profession relates to cases assigned and wages paid to individual providers.

7.1 Distribution of Productivity

We first examine the distribution of productivity within each profession. We operationalize this examination by focusing on a measure of the total cost per case. Specifically, for each case, we aggregate the three components of resource utilization in which we find significant NP effects, the same components we previously considered in Section 6: ED costs, hospital admission, and 30-day preventable hospitalizations (we multiply the latter two components by the average cost of a hospital stay, $19,220). We then estimate provider effects on the log of this measure of the total cost of each ED visit, with higher effects indicating *lower* productivity. To account for the provider-specific selection of patients, 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.20 reports estimates of the variance of provider effects on log total cost of the ED visit defined above. Using a split-sample approach to account for finite-sample estimation error, we find a variance of 0.045 for physicians and 0.048 for NPs (see Appendix A.4.2 for the split-sample approach). These estimates suggest large variation in provider effects: A one-standard-deviation costlier physician and NP increase the total cost of the ED visit by 21 and 22 percent per case, respectively, which are about three times the average NP effect of 6.7 percent from the 2SLS model specified in Equations (1) and (2). Related to the wide variation in provider effects within physician and NP professional classes, Appendix Figure A.10 shows that the NP effect varies considerably across EDs (details in Appendix A.5). Accounting for sampling error, the standard deviation of the ED-specific effects for each outcome is similar in magnitude to the average NP effect reported in Section 4.

We then investigate the full distributions of provider effects on the log total cost per case, applying a

53. Doyle, Ewer, and Wagner (2010) show differences in resource utilization decisions among physician trainees, potentially driven by human capital. Abaluck et al. (2016) documents variation in physician testing thresholds in the context of pulmonary embolism, and Silver (2021) examines returns to time spent on patients by ED physicians and variation in the physicians' productivity. Currie and MacLeod (2017) and Currie and MacLeod (2020) show physician skill variation in the contexts of C-section and depression treatments. Chan, Gentzkow, and Yu (2022) demonstrate important variation in diagnostic skill among radiologists.

non-parametric empirical Bayes deconvolution approach adapted by Kline, Rose, and Walters (2022) from Efron (2016). This approach extracts a flexible empirical Bayes prior distribution of population provider effects, using the estimated provider effects and associated standard errors from the just-identified IV model described above. We apply this procedure 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). Panel A of Figure 9 displays the deconvolved density of provider effects. For interpretation, we convert provider effects on log total spending per case on the x-axis to annual spending (excluding provider wages) by incorporating the empirical distribution of log total spending per case and the average number of cases a provider sees per year. Appendix A.4.3 describes details of these estimations. Within each professional class, the productivity distribution implies greater non-wage spending of about \$650,000 and \$620,000 per year under a provider at the 25th percentile of productivity than under a provider at the 75th percentile for NPs and physicians, respectively—both of which are about three times the mean annual non-wage spending difference between NPs and physicians.

The area under the curve (AUC) corresponding to these two distributions implies that the probability that a randomly drawn NP is costlier than a randomly drawn physician is only 62 percent (details in Appendix A.4.4).[54] The large overlap in productivity suggests that professional class is a highly imperfect proxy for productivity. If provider-specific productivity could be observed and used in hiring, paying, or assigning cases, there could be scope for large efficiency gains beyond policies that depend on professional class alone.

7.2 Relationship with Wages and Case Assignment

Next, we evaluate the distribution of assigned patient risks and paid wages across providers within each professional class. In Panel B of Figure 9, we show the average patient risks, measured by predicted 30-day mortality, across providers in each professional class. While there is considerable overlap in the supports of the distributions for NPs and physicians, the difference in means between the two distributions is clear, as we found in Appendix Figure A.9. In Panel C of Figure 9, we show the distributions of annual wages across providers.[55] Despite substantial wage variation within each profession, there is virtually no overlap

54. This probability is robust to accounting for possible differences in treatment effects between the overall population and compliers. When assuming the average treatment effect is as large as the effect among patients in the highest-complexity quartile, which is twice the LATE estimate, the probability that a randomly drawn NP is costlier than a randomly drawn physician remains limited at 72 percent.

55. For each provider, we access detailed payment records of the full-time equivalents and wages 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 work hours across all pay periods, where each pay period covers two weeks and considers a number of 80 hours to be one full-time-equivalent, and (iii) multiplying this figure by 26 pay periods and 80 hours per pay period.

between the wage distributions for NPs and physicians. Under the AUC criterion, assigned patient risks are qualitatively more predictive of professional class than productivity is; yearly wages are extremely predictive of professional class (more details in Appendix A.4.4 and Appendix Figure A.11).

Finally, given the potential for large efficiency gains under policies using provider-specific productivity, we explore how our measure of provider productivity (i.e., provider effects on the log total spending per case) relates to assigned patient risks and wages paid to each provider. To account for sampling error, we compute the empirical Bayes posterior mean for each provider's effect on log total spending per case (details in Appendix A.4.5). In contrast to the differences between NPs and physicians, we find that provider productivity within each professional class has little bearing on the average predicted 30-day mortality of cases assigned to each provider (Panel A of Appendix Figure A.12).[56] Similarly, we find that, within each professional class, a provider's productivity shows little positive relationship with her wages (Panel B of Appendix Figure A.12).[57] Taken together, the evidence indicates that organizations make little use of provider productivity within each professional class when assigning cases or setting wages for providers.

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 selection and training of future members, and the economic returns to working as a professional. However, very little is known empirically about productivity differences between distinct professions performing overlapping tasks and how they compare to within-profession productivity variation, largely because professions exclude other groups from providing tasks 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. 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 incur

56. In an analysis that divides cases into low- versus high-risk by whether predicted 30-day mortality is below versus above the sample median and separately identifies provider effects on low- and high-risk patients, we find that the gap in spending effects between low- and high-productivity providers is greater for riskier patients. This implies that assigning riskier patients to higher-productivity providers could be efficient.

57. For NPs, though Panel B of Appendix Figure A.12 shows a relationship between wages and productivity, the relationship is small in magnitude and only marginally significant. The relationship is also opposite-signed: NPs with higher effects on spending, i.e., NPs who are lower-productivity, are paid higher wages.

greater resource costs to treat patients but achieve worse patient outcomes.

We also shed light on behavioral mechanisms and responses that connect productivity to human capital. We show that experience may play a role in the NP-physician performance gap. We demonstrate clinical decision-making in response to lower human capital: NPs are likelier to call on external information, potentially compensating for less information perceived on their own. NPs are also less likely to prescribe drugs with potentially high errors of commission (opioids), while they are more likely to prescribe drugs with potentially high errors of omission (antibiotics). Finally, the productivity gap between NPs and physicians is higher for complex and severe patients. We show descriptive evidence of patient allocation that responds to NP comparative and absolute disadvantage: On average, NPs receive healthier patients and take on a smaller share of the caseload when the ED is less constrained to meet demand.

Even under conservative assumptions, we find the resource costs implied by the lower productivity of NPs versus physicians likely outweigh salary savings from hiring NPs instead of physicians. However, we also find productivity variation within each profession several-fold larger than the difference between the two professions. The substantial overlap in the distributions of productivity for NPs and physicians suggests that professional class is, at best, a coarse indicator of productivity. While NPs and physicians receive qualitatively different patient risks and starkly different wages, within each profession, tasks and wages bear limited relationships with worker productivity. Thus, despite the average productivity difference between NPs and physicians, our findings need not imply that NPs are inefficient to use: Not only could NPs be valuable in augmenting an inelastic physician supply, the large productivity overlap between the two professions suggests that organizing work solely around professional lines may forego large gains in efficiency.

Considered together, our findings paint a nuanced picture of the role of professions in determining and revealing the productivity, tasks, and wages of workers. The intensive processes of professional selection and training may imply important productivity differences between professional classes that possibly justify sizable wage differences in industries such as health care. Nevertheless, professional institutions are likely more effective at separating wages and organizing tasks between professions than at standardizing productivity within professions. These relationships may derive from frictions in observing or acting on productivity in the labor marketplace (Acemoglu and Pischke 1998), where professions may provide an important function of certifying membership and organizing labor. 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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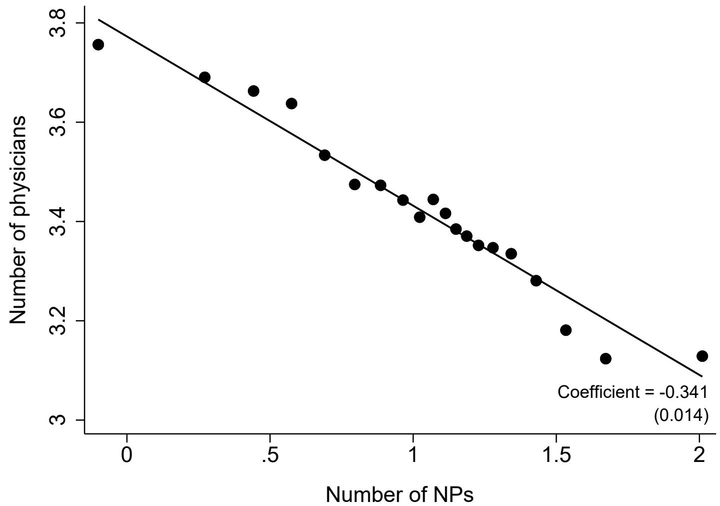
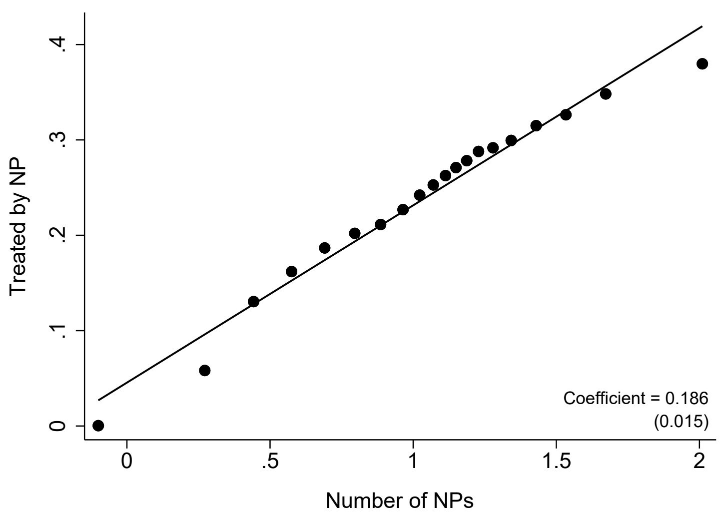
Figure 1: First Stage

A. Number of Physicians

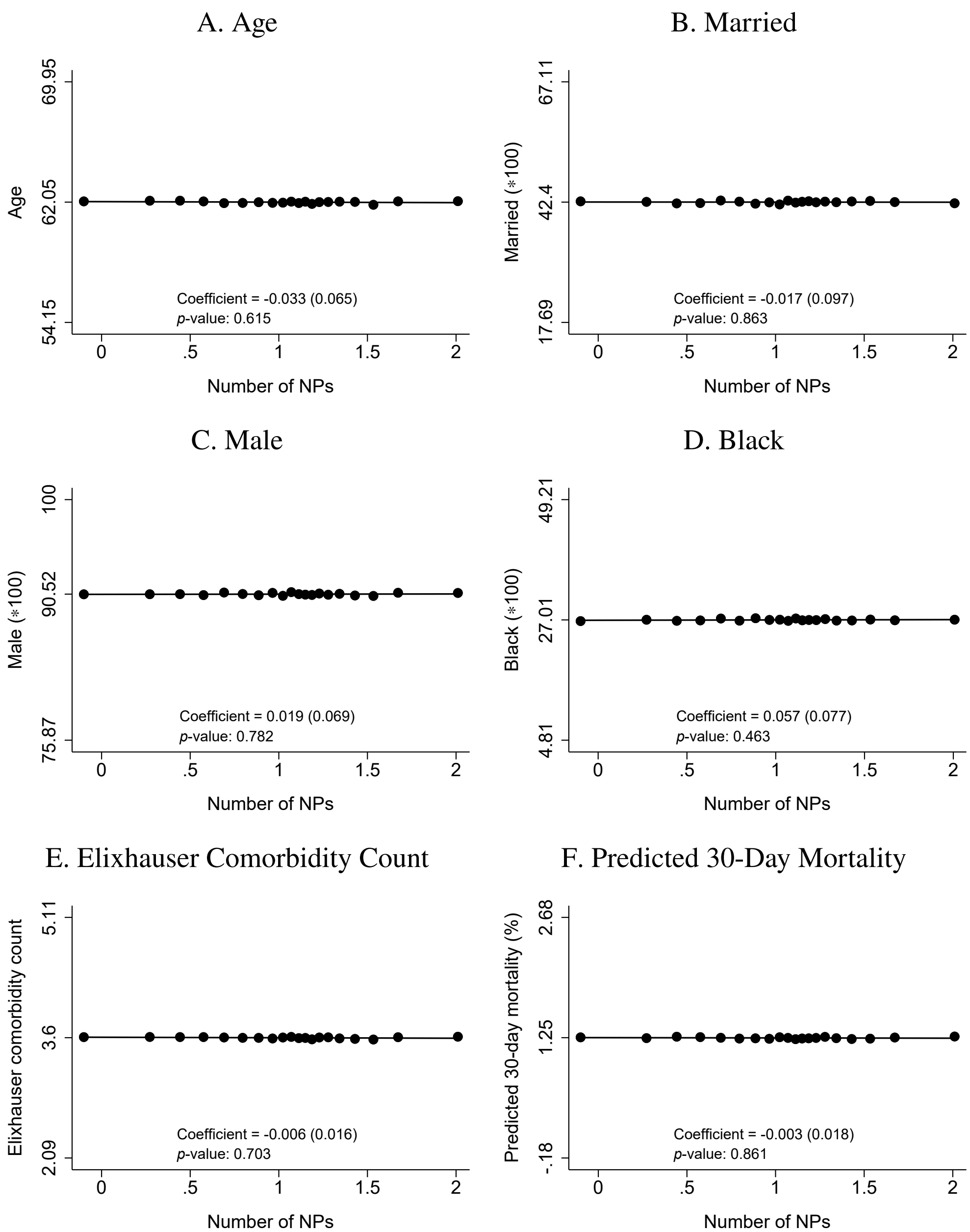


B. Treated by NP



Notes: This figure represents a graphical illustration of the first-stage estimation. Panel A shows a binned scatter plot of the number of physicians on duty versus the number of NPs on duty. Panel B shows a binned scatter plot of whether the case is treated by an NP versus the number of NPs on duty. To construct these binned scatter plots, we first residualize both the y-axis and x-axis variables with respect to the baseline control vector (i.e., indicators for ED-by-year, ED-by-month, ED-by-day-of-the-week, and ED-by-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 variables (conditional on the baseline control vector), with standard errors clustered by provider reported in parentheses.

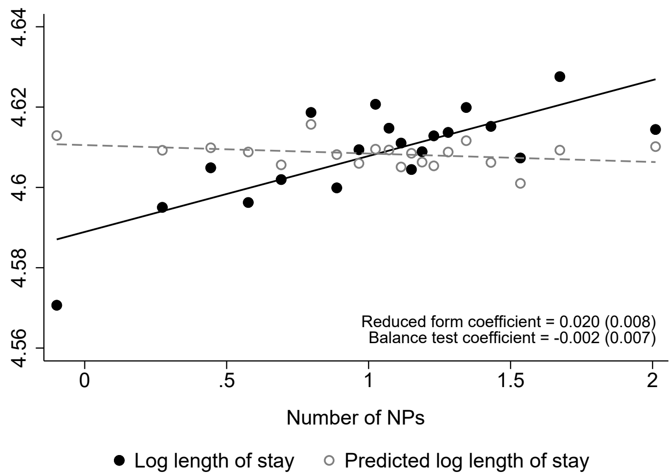
Figure 2: Balance in Patient Characteristics



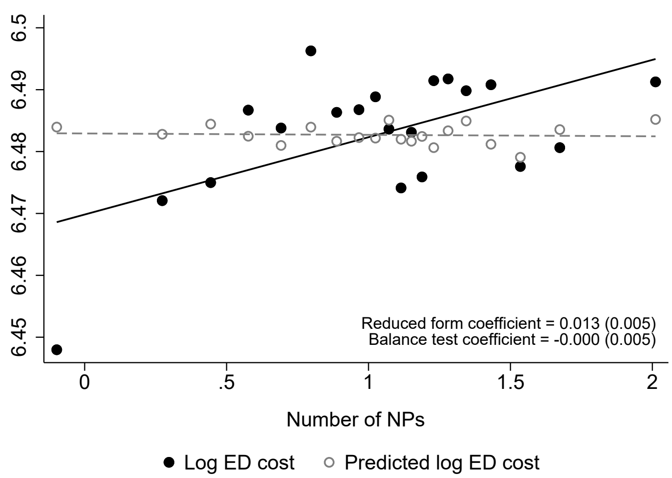
Notes: This figure shows balance in patient characteristics across the number of NPs on duty, conditional on the baseline controls (i.e., indicators for ED-by-year, ED-by-month, ED-by-day-of-the-week, and ED-by-hour-of-the-day). To construct these binned scatter plots, we first residualize both the y-axis and x-axis variables with respect to the baseline controls 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 deviation, respectively (except for Panel C which caps the top number at 100 since the mean plus a half standard deviation is beyond the maximum possible). The coefficients report the estimated slope of the best-fit line between the y-axis and x-axis variables (conditional on the baseline controls), 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 three-digit diagnosis indicators.

Figure 3: Reduced-Form and Balance

A. Log Length of Stay



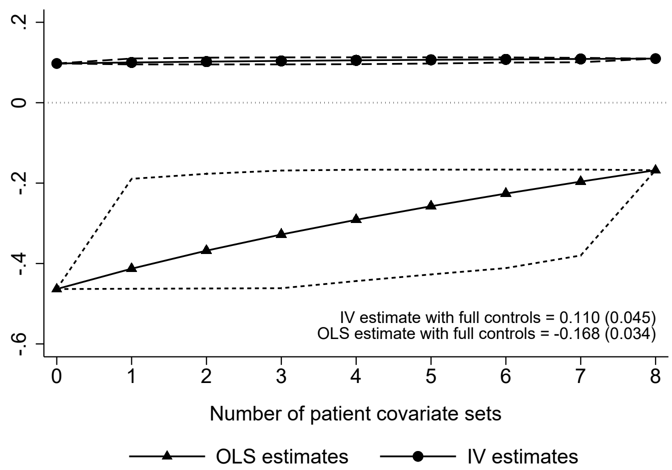
B. Log ED Cost



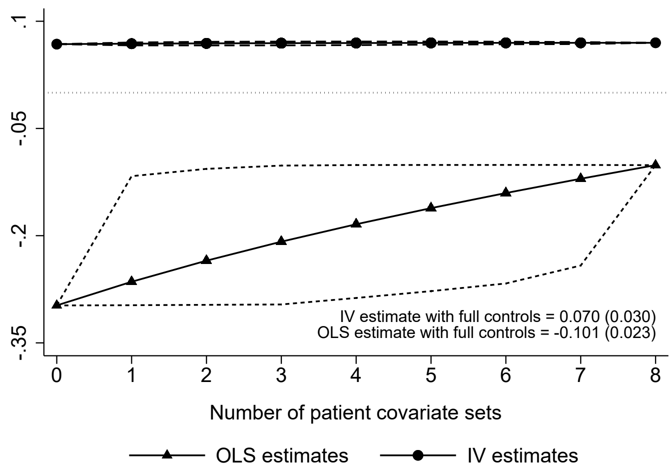
Notes: This figure shows binned scatter plots of patient actual and predicted outcomes on the *y*-axis versus the number of NPs on duty on the *x*-axis, controlling for the baseline control vector (i.e., indicators for ED-by-year, ED-by-month, ED-by-day-of-the-week, and ED-by-hour-of-the-day). Panel 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 generated based on patient characteristics $\mathbf{X}_i$ included in Equations (1) and (2), including patient demographics, comorbidities, prior health care use, vital signs, and three-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

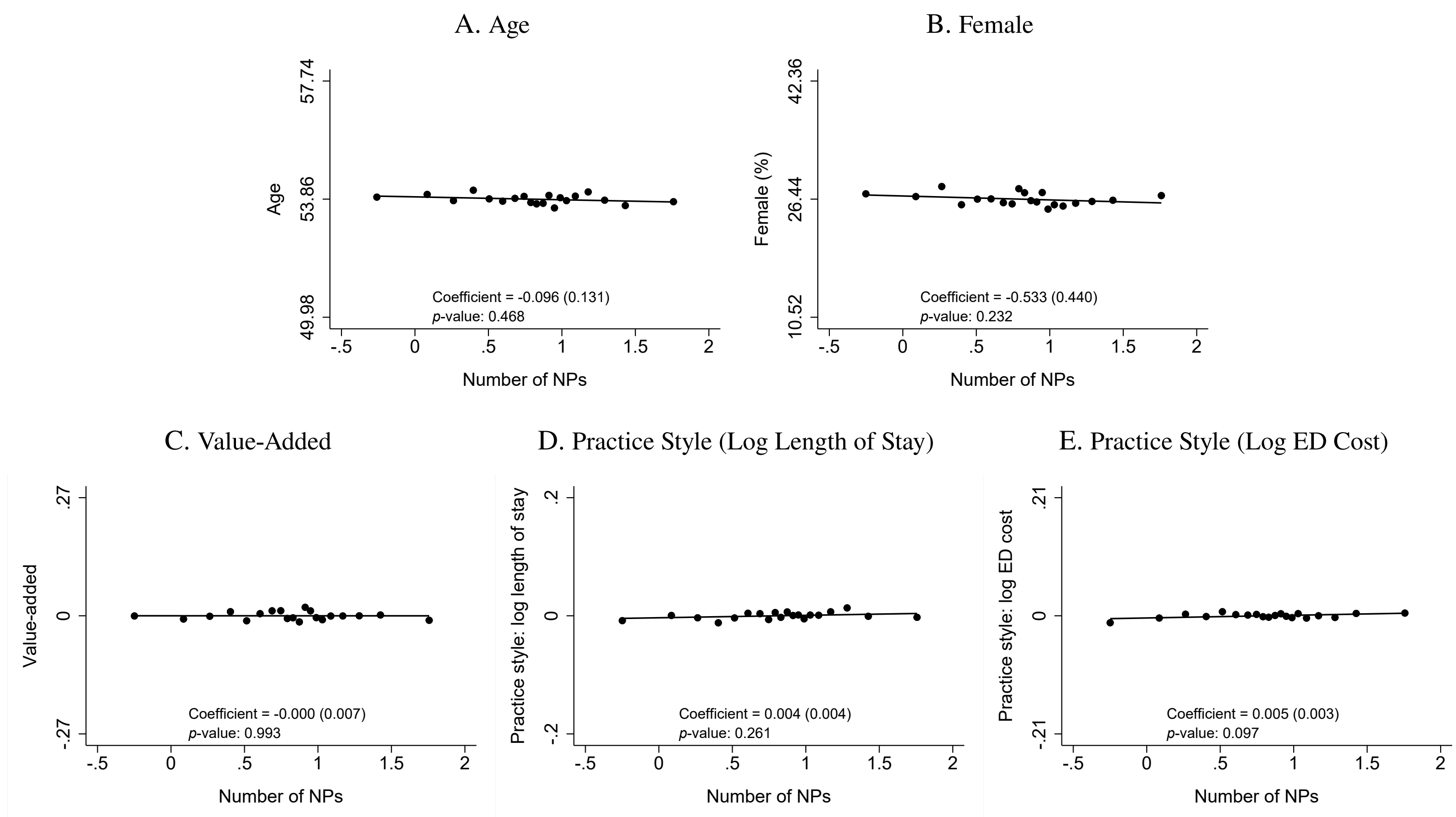


B. Log ED Cost



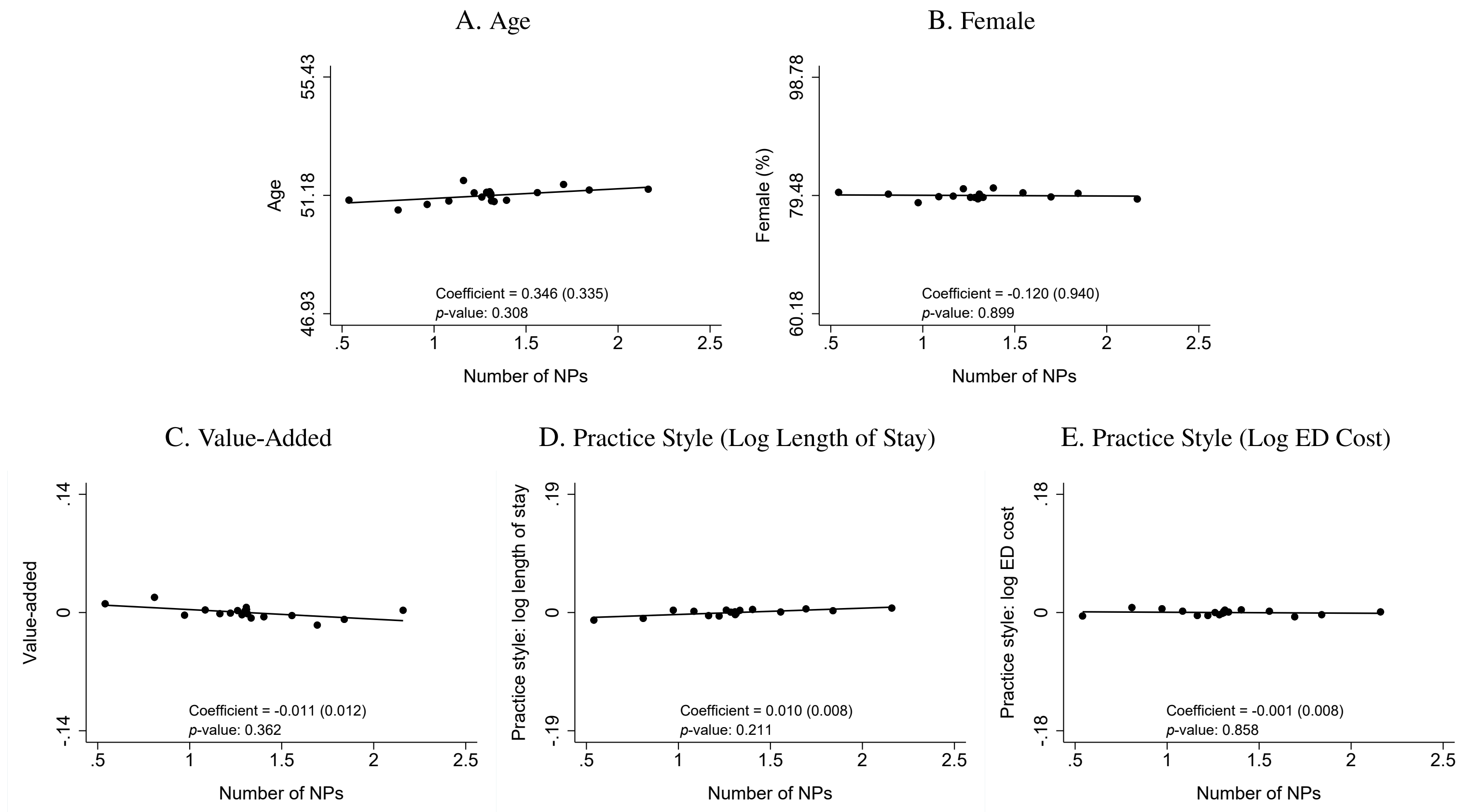
Notes: This figure shows the robustness of our OLS and IV estimates to the inclusion of different sets of patient controls. We divide patient observable characteristics into eight subsets: (i) five-year age-bin indicators; (ii) marital status; (iii) gender; (iv) race indicators; (v) indicators for 31 Elixhauser comorbidities; (vi) vital signs; (vii) prior health care use; and (viii) indicators for three-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. 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 possible combinations with n (out of eight) subsets of patient covariates. 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. The coefficients at the bottom of each panel show the IV and OLS estimates with the full set of patient controls, with standard errors reported in parentheses. 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



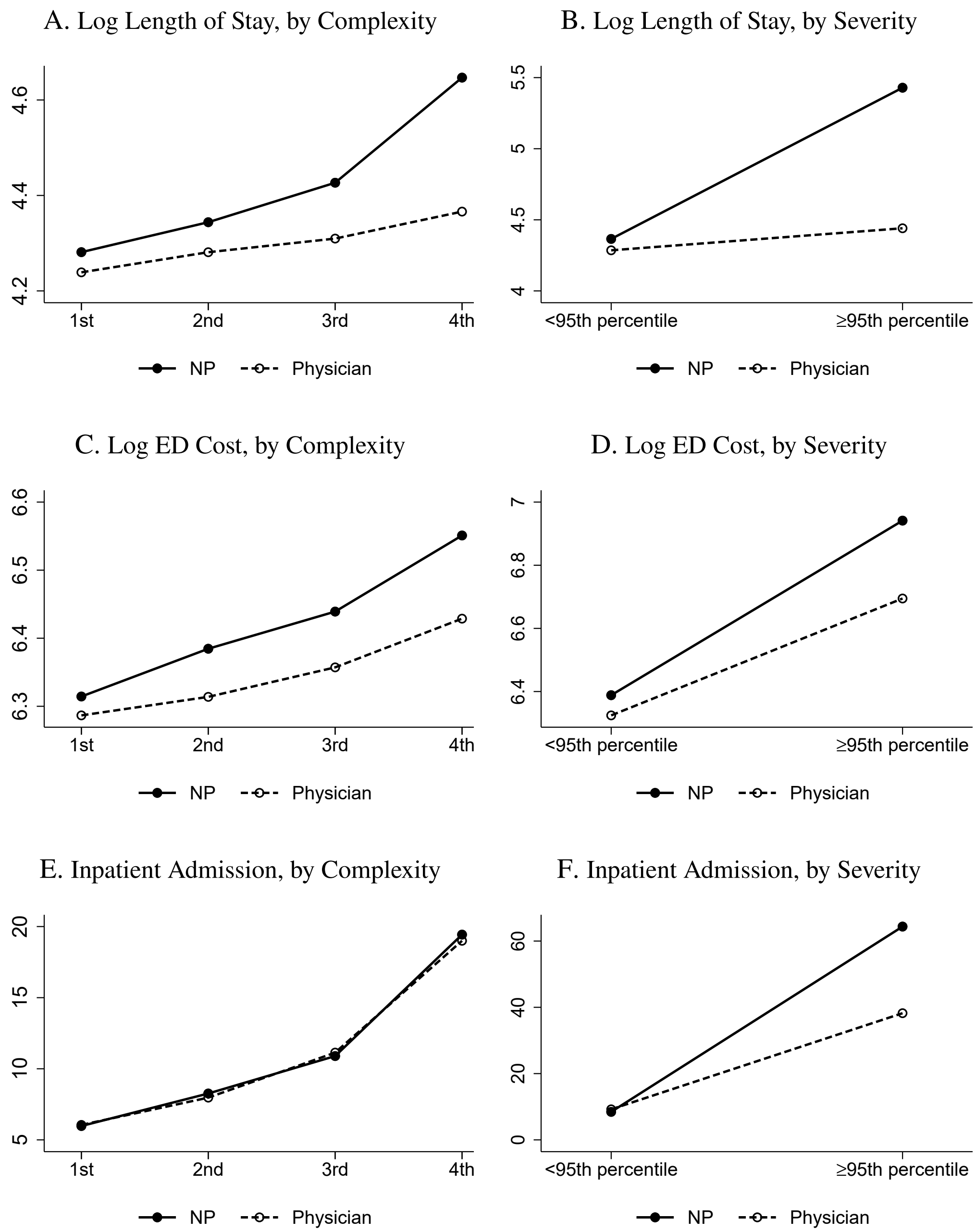
Notes: These panels are graphical representations of the balance-test regression at the ED-day level of physician average characteristics (weighted by the number of cases treated by each physician) on the number of NPs on duty, conditional on ED-by-year, ED-by-month, and ED-by-day-of-the-week indicators. Coefficients from the regressions are reported in each panel, along with standard errors (shown in parentheses) and *p*-values. To construct the binned scatter plots, 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-by-year, ED-by-month, and ED-by-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 deviation, respectively. The physician characteristics reported in Panels A-E are, respectively, age, gender, value-added, practice style in terms of patient log length of stay, and practice style in terms of 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 NP 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-by-year, ED-by-month, and ED-by-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 scatter plots, 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-by-year, ED-by-month, and ED-by-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 deviation, respectively. The NP characteristics reported in Panels A-E are, respectively, age, gender, value-added, practice style in terms of patient log length of stay, and practice style in terms of patient log cost of the ED visit. Construction details of value-added and practice style are described in Appendix A.3.

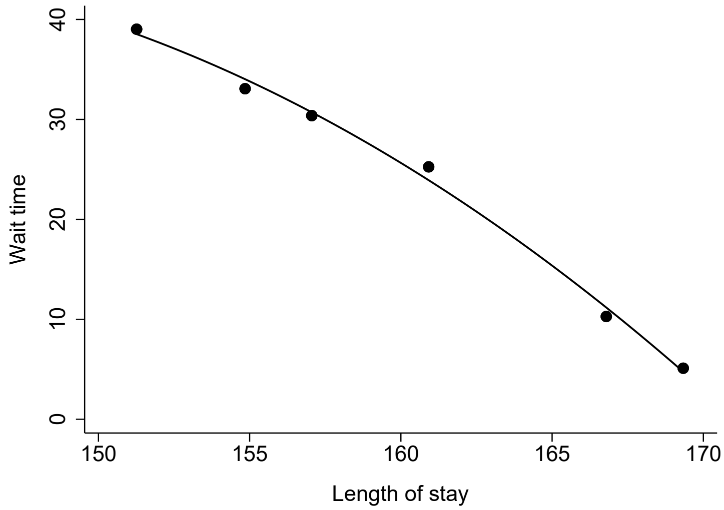
Figure 7: Heterogeneous Effects by Case Complexity and Severity



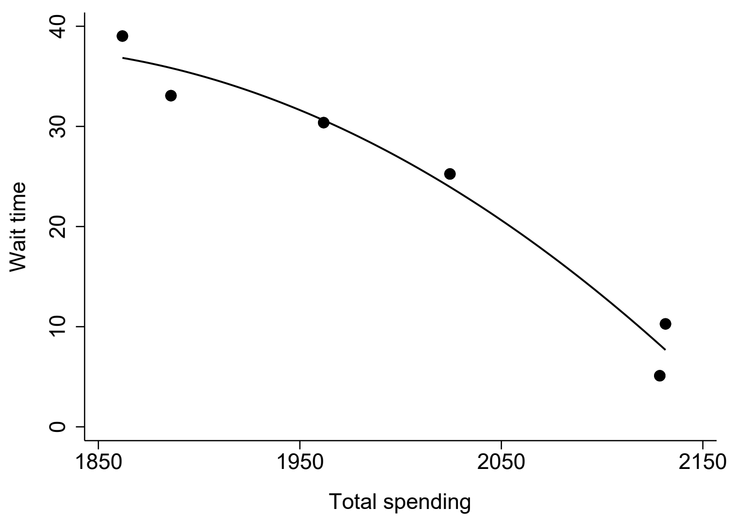
Notes: This figure shows heterogeneous effects of NPs by case complexity and severity. Panels A, C, and E divide cases into quartiles by their total number of Elixhauser comorbidities, with higher quartiles indicating more complex cases. Panels B, D, and F divide cases by whether condition severity measured by 30-day mortality of cases with the same three-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 (4). We estimate complier potential outcomes under physicians by an IV regression similar to Equation (4) but 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 ED cost, and inpatient admission in the ED visit, respectively.

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

A. Wait Time versus Length of Stay

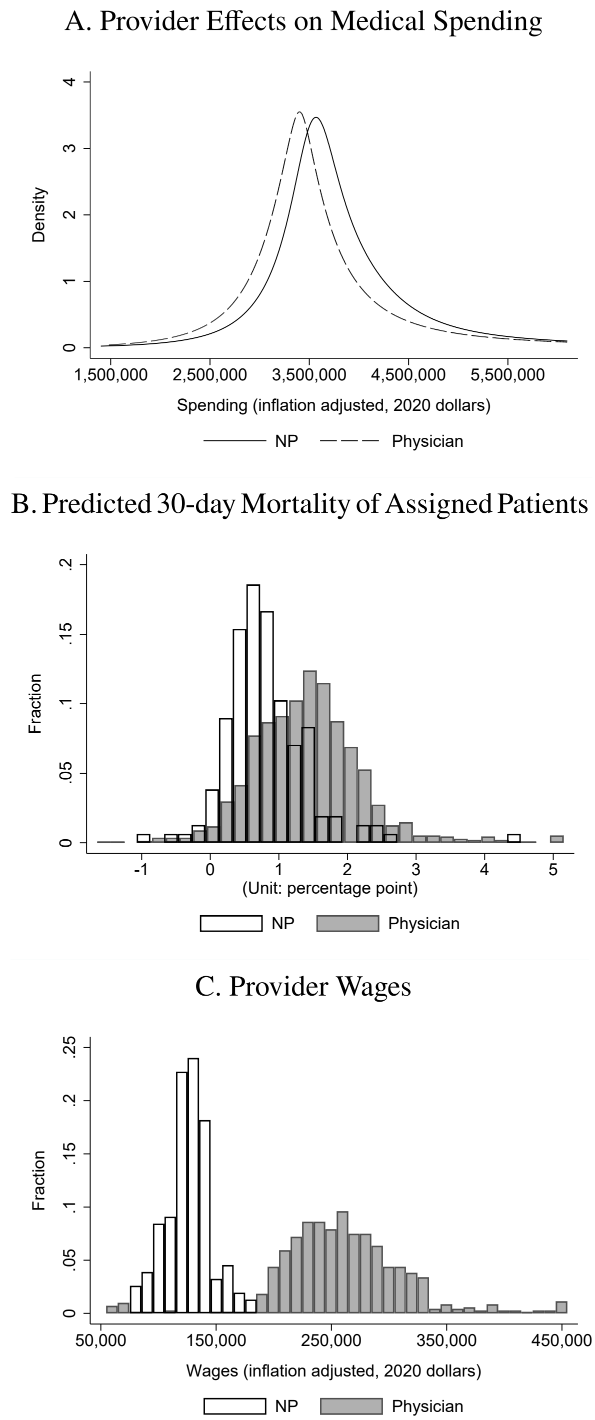


B. Wait Time versus Total Spending



Notes: This figure shows changes in patient average wait time and other outcomes with incremental numbers of NPs on duty, conditional on the number of physicians on duty, the number of cases arriving, and the controls included in the main specification (see details in Equation (5)). Panel A presents the trade-off between wait time and length of stay. Panel B presents the trade-off between wait time and total spending associated with the ED visit, which is the sum of the cost of care at the ED and the cost due to hospital admission in the ED visit and preventable hospitalizations in the 30 days after the ED visit. The solid lines show the quadratic fit estimated on the plotted points.

Figure 9: Distribution of Provider Productivity, Patient Assignments, and Wages



Notes: Panel A reports the deconvolved distributions of provider effects on annual medical spending. See Appendix A.4.3 for details of the estimation. The solid and dashed lines show the deconvolved distributions for NPs and physicians, respectively. Panel B plots histograms of average predicted 30-day mortality of patients assigned to each provider. Predicted 30-day mortality is winsorized at the value of 5 percentage points. Panel C plots histograms of provider wages observed in the VHA data (inflation adjusted to year 2020). Wages are winsorized at the value of $450,000. In Panels B and C, the white and gray bins show results for NPs and physicians, respectively.

Table 1: Characteristics of Baseline Sample

	All	Treated by NP	Treated by physician	*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 comorbidity count	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]	
ED cost ($, inflation-adjusted to 2020)	939	813	978	0.00
	[1,331]	[1,010]	[1,413]	
Inpatient admission (%)	16.62	7.87	19.34	0.00
	[37.23]	[26.92]	[39.50]	
30-day preventable hospitalization (%)	1.23	0.75	1.39	0.00
	[11.04]	[8.60]	[11.69]	
30-day mortality (%)	1.25	0.63	1.44	0.00
	[11.10]	[7.91]	[11.91]	
Observations	1,118,836	264,789	854,047	

Notes: Column 1 shows average characteristics of all cases in our 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 NPs on Length of Stay and ED Cost

	Log length of stay			Log ED cost		
	OLS	Reduced form	IV	OLS	Reduced form	IV
	(1)	(2)	(3)	(4)	(5)	(6)
NP assignment	-0.168		0.110	-0.101		0.070
	(0.034)		(0.045)	(0.023)		(0.030)
Number of NPs		0.020			0.013	
		(0.008)			(0.005)	
Full controls	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	4.608	4.608	4.608	6.483	6.483	6.483
S.D. dep. var.	1.161	1.161	1.161	0.878	0.878	0.878
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-by-year, ED-by-month, ED-by-day-of-the-week, and ED-by-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 VHA facilities in the prior 365 days), vital signs, and indicators for three-digit ICD-10 code of patient primary diagnosis of the visit. Standard errors clustered by provider are shown in parentheses.

Table 3: Effect of NPs on Additional Outcomes

	Dependent variable		
	Admission	30-day mortality	30-day prevent. hosp.
	(1)	(2)	(3)
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 controls	Yes	Yes	Yes
Mean dep. var.	16.625	1.247	1.234
S.D. dep. var.	37.230	11.099	11.041
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.

Table 4: Heterogeneous Effects by Provider Experience

	Dependent variable				
	Log length of stay	Log ED cost	Admission	30-day mortality	30-day prevent. hosp.
	(1)	(2)	(3)	(4)	(5)
Panel A: Provider specific experience					
NP assignment	0.101	0.072	0.092	-0.115	0.255
	(0.044)	(0.030)	(0.579)	(0.116)	(0.121)
NP assignment × experience	-0.058	-0.042	-0.504	-0.001	0.016
	(0.025)	(0.019)	(0.331)	(0.041)	(0.030)
Experience	-0.001	0.006	0.238	-0.001	-0.016
	(0.006)	(0.010)	(0.314)	(0.018)	(0.014)
Panel B: Provider general experience					
NP assignment	0.086	0.062	0.089	-0.100	0.255
	(0.043)	(0.029)	(0.608)	(0.116)	(0.121)
NP assignment × experience	-0.103	-0.035	0.340	0.088	0.043
	(0.056)	(0.033)	(1.048)	(0.093)	(0.068)
Experience	-0.036	-0.013	-0.719	-0.012	-0.048
	(0.015)	(0.011)	(0.221)	(0.023)	(0.025)
Full controls	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	4.608	6.483	16.625	1.247	1.234
S.D. dep. var.	1.161	0.878	37.230	11.099	11.041
Observations	1,110,798	1,108,961	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 three-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 (despite diagnoses) 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 1-5 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.

Table 5: Clinical Decision-Making

	Dependent variable				
	Consult	CT	X-ray	Opioid	Antibiotic
	(1)	(2)	(3)	(4)	(5)
NP assignment	0.026	0.012	0.020	-0.018	0.040
	(0.009)	(0.007)	(0.009)	(0.006)	(0.022)
Full controls	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.226	0.145	0.291	0.088	0.639
S.D. dep. var.	0.418	0.352	0.454	0.283	0.480
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 measures of clinical decision-making. 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.

Online Appendix

A.1 Diagnosis Coding: NPs versus Physicians

In this appendix, we explore whether NPs and physicians are significantly different in reporting three-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 three-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:

$$y_i = \delta \text{NP}_i + \mathbf{T}_i \eta + \varepsilon_i, \quad \text{(A.1)}$$

$$\text{NP}_i = \lambda Z_i + \mathbf{T}_i \zeta + \nu_i, \quad \text{(A.2)}$$

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 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.13 plots the distribution of t-statistics for the estimated δ coefficients from the 836 separate regressions that use each three-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 three-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.13 further plots t-statistics against the prevalence of the three-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 three-digit diagnosis coding between NPs and physicians could arise for the relatively straightforward cases who are compliers. Additional consults and diagnostics (Section 5.2) may also aid NPs to reach the same three-digit diagnosis as physicians. Perhaps also worth noting, VHA ED providers' 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 and Never-Takers

This appendix describes estimation of characteristics of compliers and never-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.4.

To estimate characteristics of never-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. There are no always-takers in our setting since patients cannot be assigned to NPs on days without any NPs on duty.

Columns 4 of Appendix Table A.4 report the average characteristics of never-takers. For each characteristic, we compute the mean of never-takers as well as the ratio between the mean and the overall sample mean. We estimate standard errors for the 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.4 shows that never-takers are the riskiest, followed by the overall sample, and finally, compliers. For example, the total number of Elixhauser comorbidities among never-takers, the overall sample, and compliers are, respectively, 4.0, 3.6, and 3.3; the average predicted 30-day mortality among these three types of cases are, respectively, 1.7, 1.2, and 0.9 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

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

where $\tilde{Y}_i$ is risk-adjusted 30-day mortality, or the difference between patient actual and predicted 30-day mortality. To deal with possible 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.

Still, since cases are not experimentally assigned among physicians, $A_{p,d}$ may reflect both a physician'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

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.

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., Chetty, Friedman, and Rockoff 2014). If patient sorting is important, the estimated physician value-added will change meaningfully with the addition of risk adjusters. Otherwise, our estimates should remain stable. Appendix Figure A.14 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 three-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 three-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.

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.14.

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.14 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 three-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, adapted by Kline, Rose, and Walters (2022) from Efron (2016), to recover the underlying population distribution of provider effects. We present the receiver operating characteristic (ROC) curves of provider productivity, case assignments, and wage payments as characteristics that may distinguish NPs and physicians. Finally, we assess the extent to which provider productivity relates to cases assigned and wages paid across individual providers.

A.4.1 Estimating Provider Effects

We generate a measure of total spending associated with the ED visit, as the sum of the three main components of costs for which we find significant NP effects: ED costs, hospital admission, and 30-day preventable hospitalizations (we multiply the latter two components by the average cost of a hospital stay, \$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:

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

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

$\mathbf{1}_{\{j(i)=j\}}$ 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.[3]

The F-statistics for the joint significance of on-duty provider indicators in the first-stage regressions, i.e., Equation (A.5), have shares of 0.99 and 0.68 above 10 and 100, respectively, suggesting that provider availability is strongly predictive of the treating provider.[4] Appendix Figure A.15 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.[5] 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.

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3. We also normalize provider effects to have a case-weighted mean of 0 within ED-provider types. We find the results are very similar: For the (split-sample) variance of provider effects reported below in Appendix A.4.2, the standard deviation of NP effects with normalization by ED and by ED-provider type are 0.22 and 0.19, respectively; the standard deviation of physician effects remains stable at 0.21. For the probability that a randomly selected NP incurs a lower spending than a randomly selected physician (Appendix A.4.3), the number changes slightly from 38 percent to 35 percent.

4. 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.

5. 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).

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 0.054 for NPs, and 0.064 for physicians (see Appendix Table A.20).[6] 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., 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.20 reports the case-weighted variance of provider effects from the split-sample approach. The variance for physicians is estimated to be 0.045, 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 raises medical spending 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

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

6. 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.

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 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.21, 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.[7] 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.20. We demean both the physician and NP distributions 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. For interpretation, we rescale provider effects on log total spending per case on the x-axis into annual spending, as follows: (i) estimating the empirical distribution of log total spending per case, where the log spending is residualized with respect to ED and provider fixed effects, with means added back; (ii) for each provider effect from the deconvolution, adding the provider effect to the spending distribution in (i); (iii) exponentiating the individual values of the distribution in (ii) and integrating over the distribution to calculate the mean total spending per case for each provider effect; and (iv) multiplying the mean in (iii) for each provider effect by the average number of cases a provider sees per year observed in the data.

Finally, using the deconvolved density of NP and physician effects on log total spending per ED case, we estimate the probability that a randomly drawn NP is costlier than a randomly drawn physician by

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

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

7. 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.

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. This statistic remains large when we adjust the deconvolved productivity distributions to account for possible differences in treatment effects between the overall population and compliers: When assuming the average treatment effect is as large as that among the highest complexity quartile patients which is twice of the LATE estimate (0.136 versus 0.067), the probability that NPs are less costly remains large at 28 percent.

A.4.4 ROC Curve Representation

The probability in Equation (A.7) is equivalent to the area under the curve (AUC) of a ROC curve. The ROC curve displays the performance of an exercise classifying providers by a certain characteristic. In the case of provider effects on log total spending, the AUC 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.

We construct ROC curves based on respective provider characteristics of productivity, the riskiness of assigned patients (measured by predicted 30-day mortality), and wages, where we consider physicians as the "positive" class and NPs as the "negative" class. For each characteristic of productivity, assigned patient predicted mortality, 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 per ED visit: $\mu_j = -\theta_j$. 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 NPs and physicians, 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 risks of assigned patients and provider wage payments, since we observe actual patient assignments and provider wages, we do not estimate them in a regression framework or apply deconvolution to obtain a population prior distribution. Rather, we simply apply the empirical cumulative distribution functions of average predicted 30-day mortality of patients assigned to each provider and each provider's annualized full-time-equivalent ("yearly") wage (inflation-adjusted to 2020 dollars).

We show the ROC curves in Appendix Figure A.11. As mentioned above, the AUC based on productivity is 0.62. The AUC based on assigned patient risks and wage payments are 0.75 and 0.99, respectively. Taken together, assigned patient risks are qualitatively more predictive of professional class than is productivity; yearly wages are extremely predictive of professional class.

A.4.5 Case Assignments and Wage Payments versus Productivity

Separately for NPs and physicians, we assess the extent to which case assignments and wages relate to individual provider productivity. As before, we measure productivity as provider effects on log total spending per ED visit (i.e., θ_j), with higher provider effects indicating lower productivity. The empirical specification takes the following form:

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

where y_j is the average risks (measured by predicted 30-day mortality) of cases assigned to provider j or yearly wage of provider j (inflation-adjusted to 2020 dollars). $\mathbf{L}_j$ 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 calculate empirical Bayes posteriors of provider effects as Kline, Rose, and Walters (2022):

$$\tilde{\theta}_j = s_j \times \frac{\int x\varphi(z_j - x)\hat{g}_c(x)dx}{\int \varphi(z_j - x)\hat{g}_c(x)dx}, \tag{A.9}$$

where φ denotes the standard normal density.

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.10, we plot the distribution of $\hat{\delta}_\ell$ for all EDs in our sample. We also plot the empirical Bayes posterior mean $\tilde{\delta}_\ell$ for each ED, 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 gray bins in Appendix Figure A.10 plot the empirical Bayes posterior mean $\tilde{\delta}_\ell$ for each ED in our sample.[8] The distribution of posteriors is more compressed than that of the raw estimates of ED-specific effects, reflecting shrinkage due to sampling error in the raw estimates. The results show a fair amount of heterogeneity. Nonetheless, most 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 in our main analysis sample 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 using 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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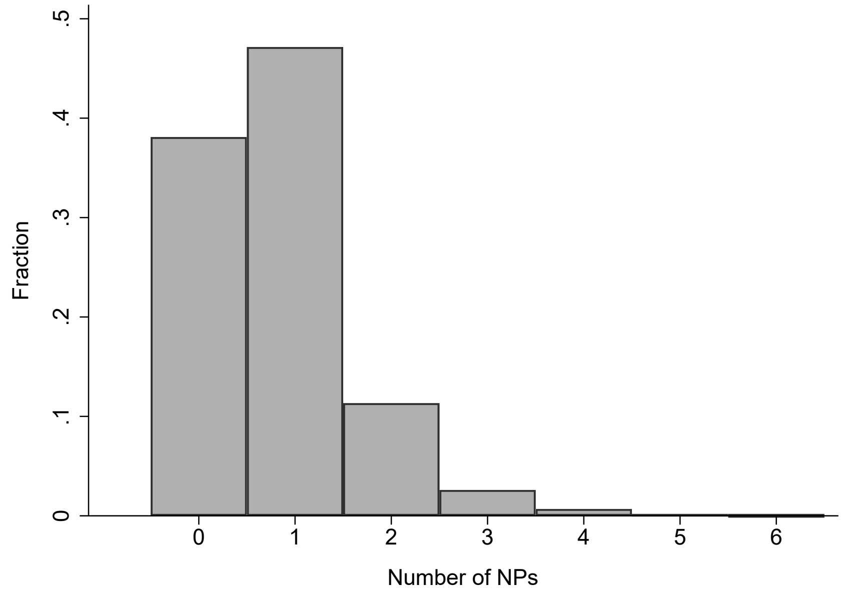
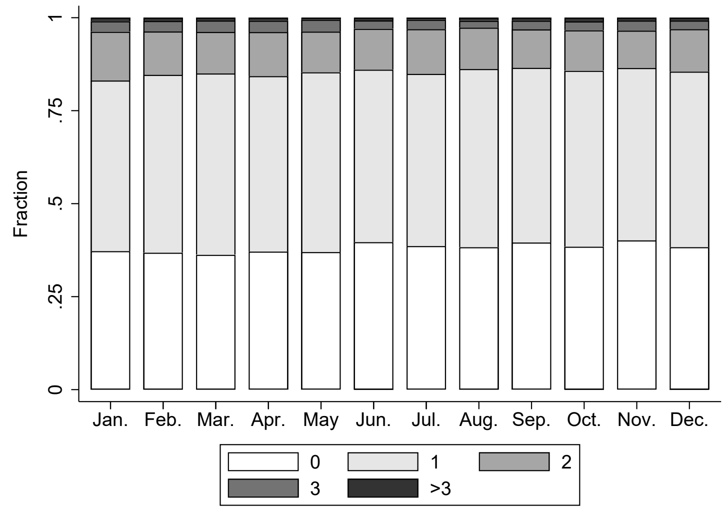
Figure A.1: Number of NPs on Duty



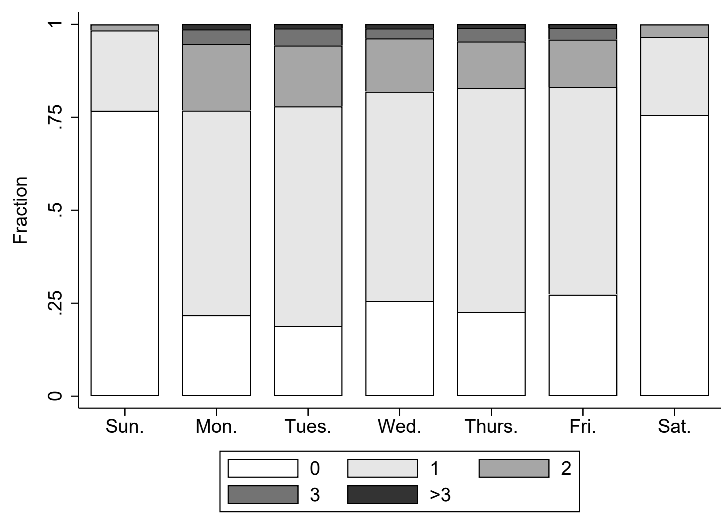
Notes: This figure shows the histogram of the number of NPs on duty in an ED-day cell. The unit of observation is at the ED-day level.

Figure A.2: Number of NPs on Duty by Month and Day of the Week

A. By Month

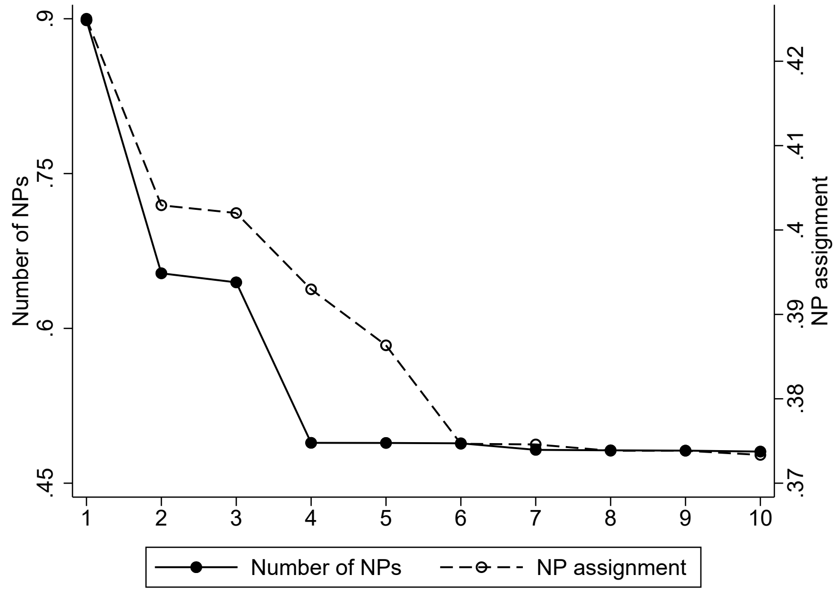


B. By Day of the Week



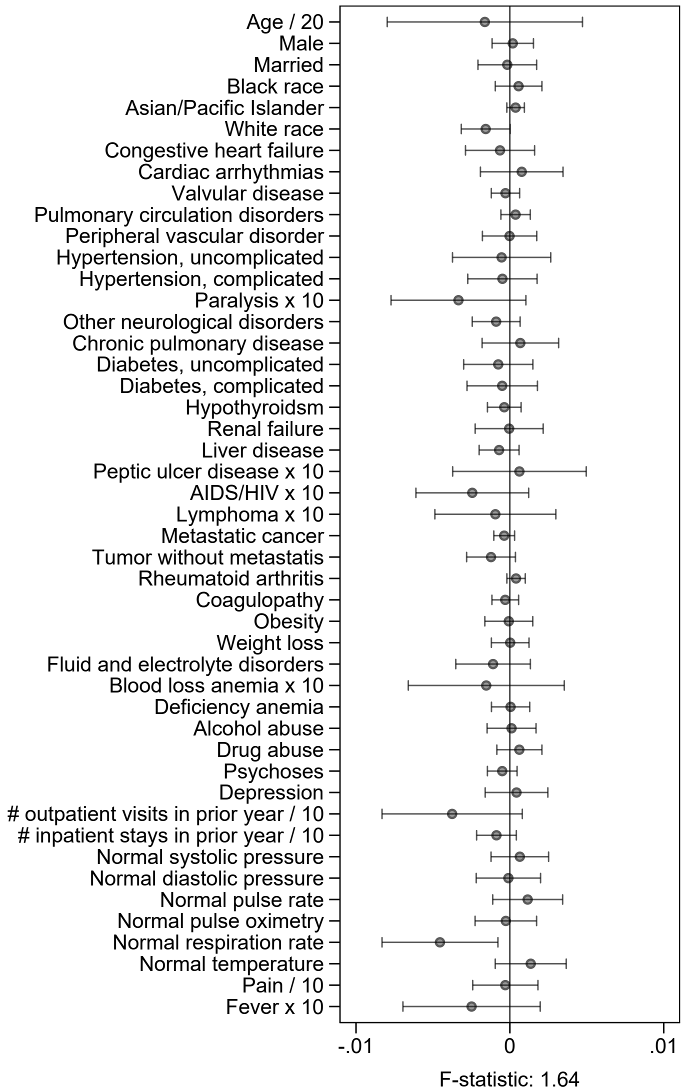
Notes: This figure plots the share of ED-day cells with different numbers of NPs on duty in each month (Panel A) and each day of the week (Panel B). Each color of the stacked bars represents a different number of NPs on duty, with the lightest color at the bottom representing ED-day cells with the lowest number of NPs on duty (i.e., zero) and the darkest color at the top representing ED-day cells with the highest number of NPs on duty (i.e., more than three). Since only a small share of ED-day cells have more than three NPs on duty, this figure groups ED-day cells with more than three NPs to the group “>3”.

Figure A.3: Standard Deviation of Number of NPs on Duty and NP Assignment



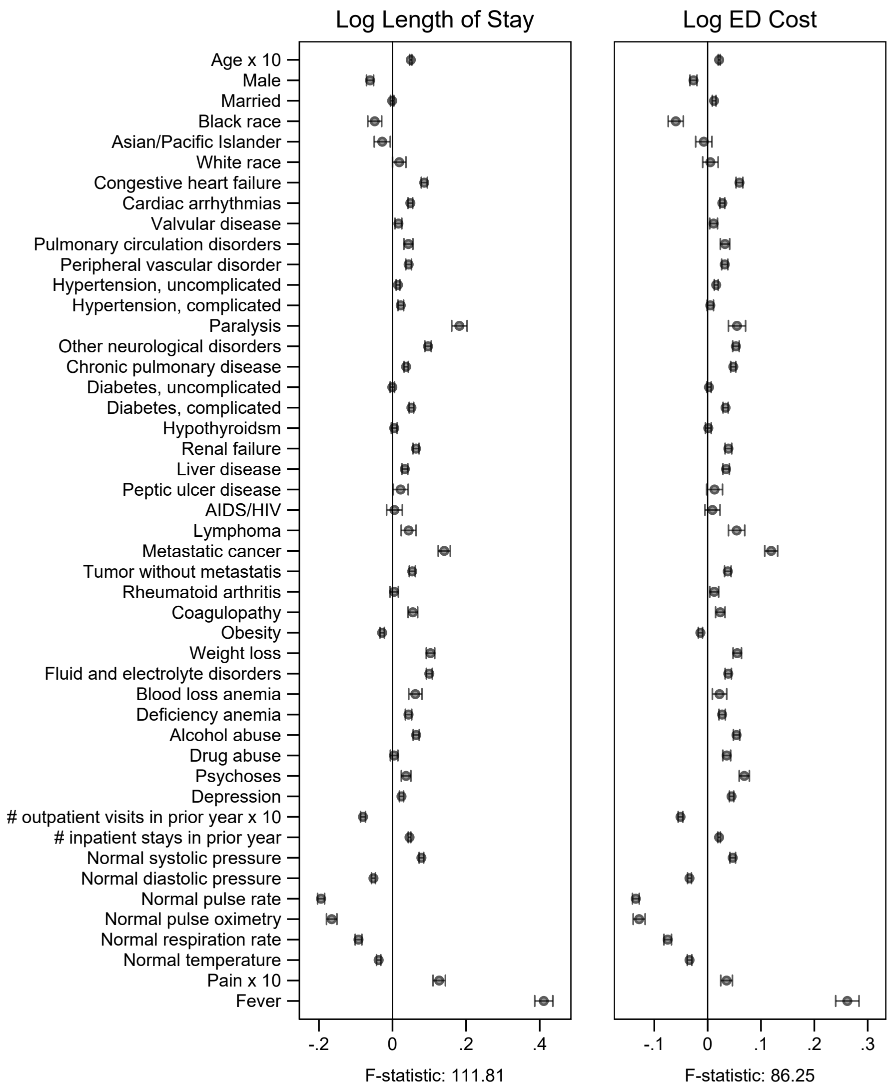
Notes: This figure shows standard deviations of the number of NPs on duty (solid dots) and whether the patient is assigned to NPs (hollow dots). The first dot shows the raw standard deviations without residualizing the number of on-duty NPs or NP assignment by any factors. The second through last dots show the standard deviations of the residualized number of on-duty NPs and NP assignment. The following factors are added to the residualization stepwise (cumulatively) from the second to the last dot: (i) ED-by-year indicators; (ii) ED-by-month indicators; (iii) ED-by-day-of-the-week indicators; (iv) ED-by-hour-of-the-day indicators; (v) patient covariates $\mathbf{X}_i$ in the main specification in Equations (1) and (2); (vi) patient volume in the ED-day cell; (vii) number of doctor equivalents on duty (i.e., the effective staffing level, which is calculated 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); (viii) average wait time of patients in the ED-day cell (leaving out the index patient); and (ix) average characteristics of on-duty physicians (age, gender, value-added, and practice style; see Appendix A.3 for the construction of value-added and practice style). Average characteristics of on-duty NPs are not included in the residualization because by definition these characteristics are missing for the 38 percent of ED-day cells without any NPs on duty.

Figure A.4: Balance in Patient Characteristics



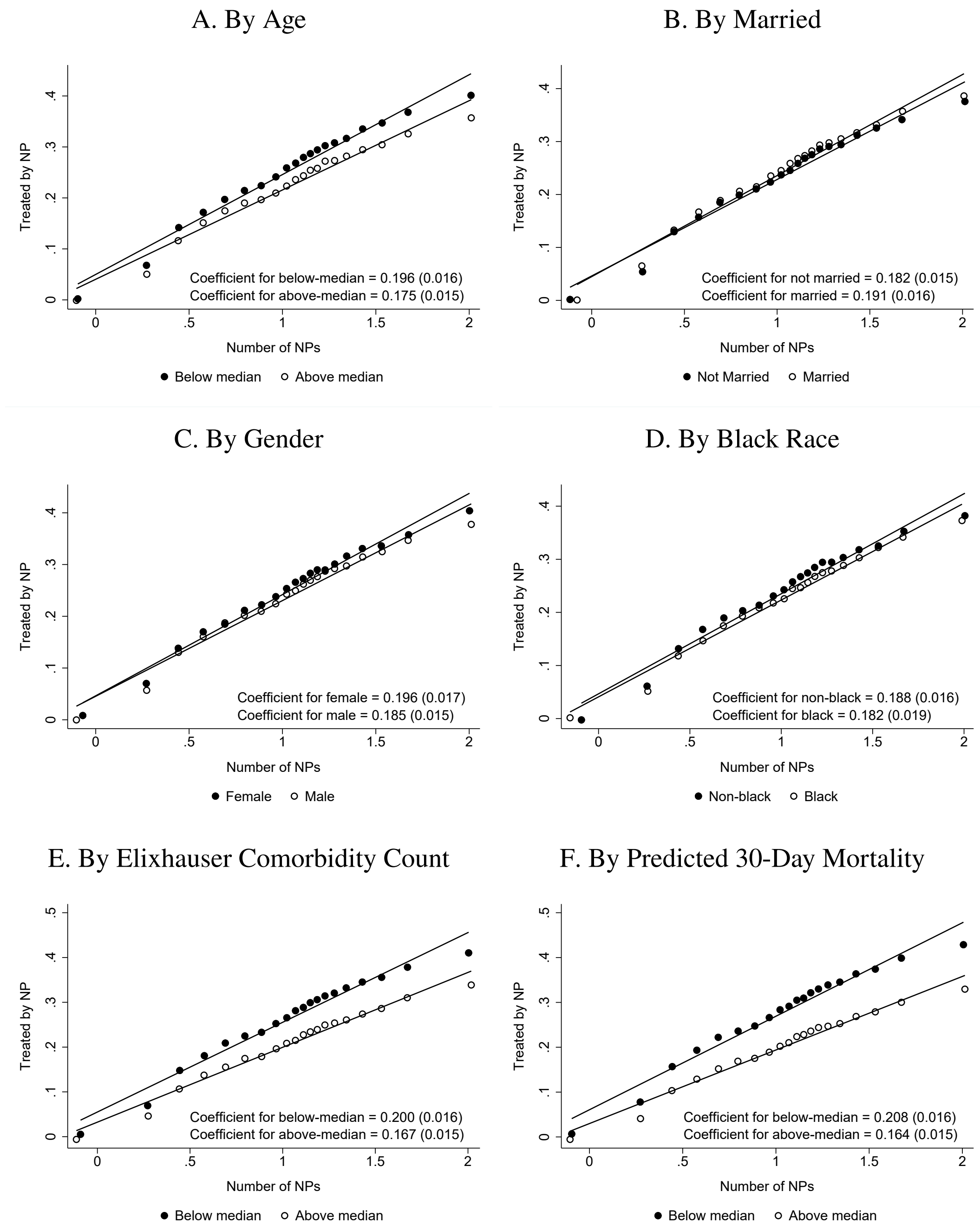
Notes: This figure shows estimated coefficients and 95% confidence intervals from regressions of each patient characteristic listed on the *y*-axis on the number of NPs on duty, controlling for the baseline control vector (i.e., indicators for ED-by-year, ED-by-month, ED-by-day-of-the-week, and ED-by-hour-of-the-day). 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.5: Predicting Log Length of Stay and Log ED Cost



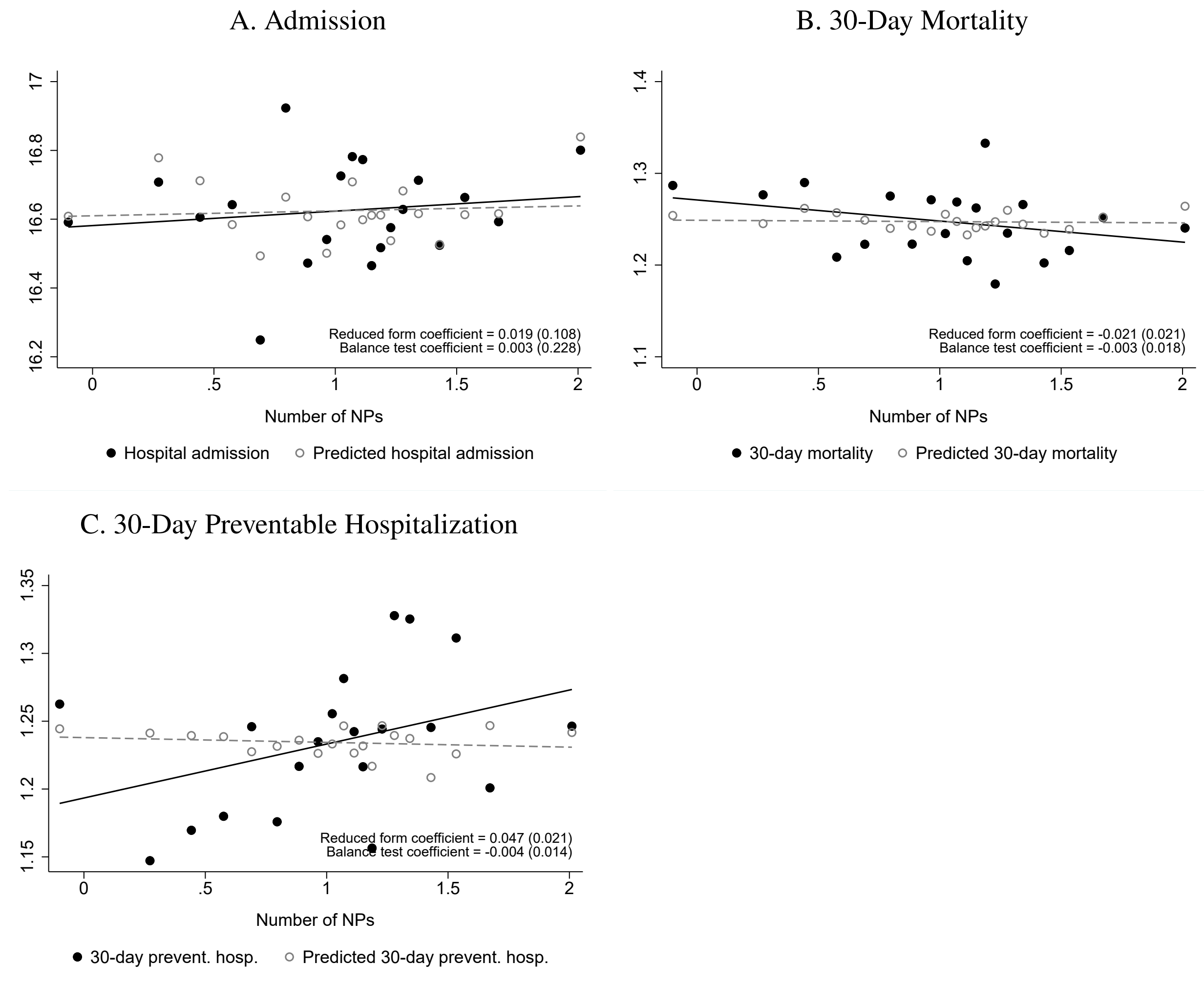
Notes: This figure shows estimated coefficients and 95% confidence intervals from regressions of patient log length of stay (Panel A) and log cost of the ED visit (Panel B) on patient characteristics, controlling for the baseline control vector (i.e., indicators for ED-by-year, ED-by-month, ED-by-day-of-the-week, and ED-by-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.6: Monotonicity Test



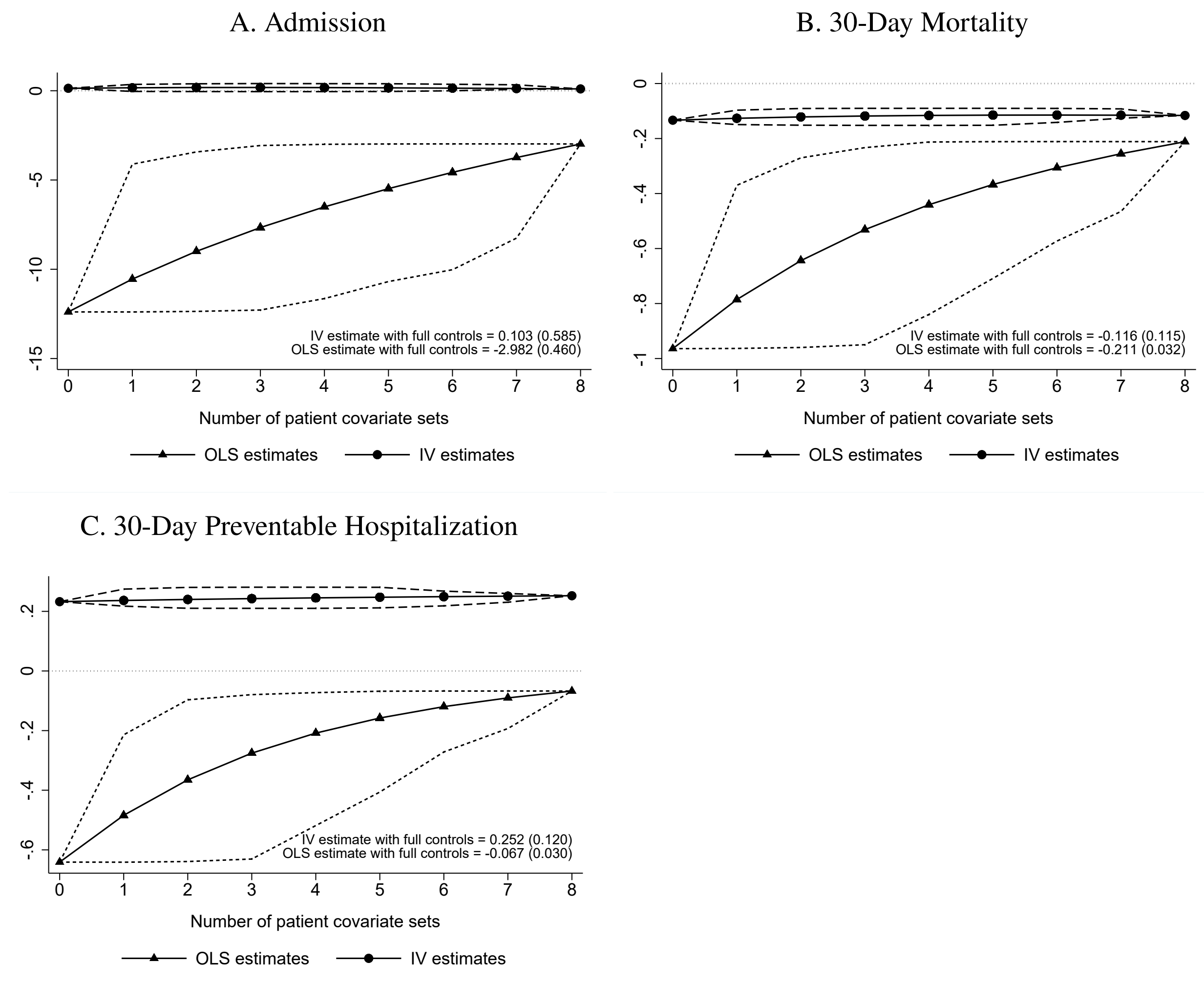
Notes: This figure shows the first-stage regression for cases of different characteristics. Panels A-F split the sample by, respectively, age (above versus below the median of the sample), marital status, gender, race (Black versus non-Black), total number of Elixhauser comorbidities (above versus below 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 three-digit diagnosis indicators. To construct these binned scatter plots, we residualize both the *y*-axis and *x*-axis variables with respect to the baseline control vector (i.e., indicators for ED-by-year, ED-by-month, ED-by-day-of-the-week, and ED-by-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.7: Reduced-Form and Balance



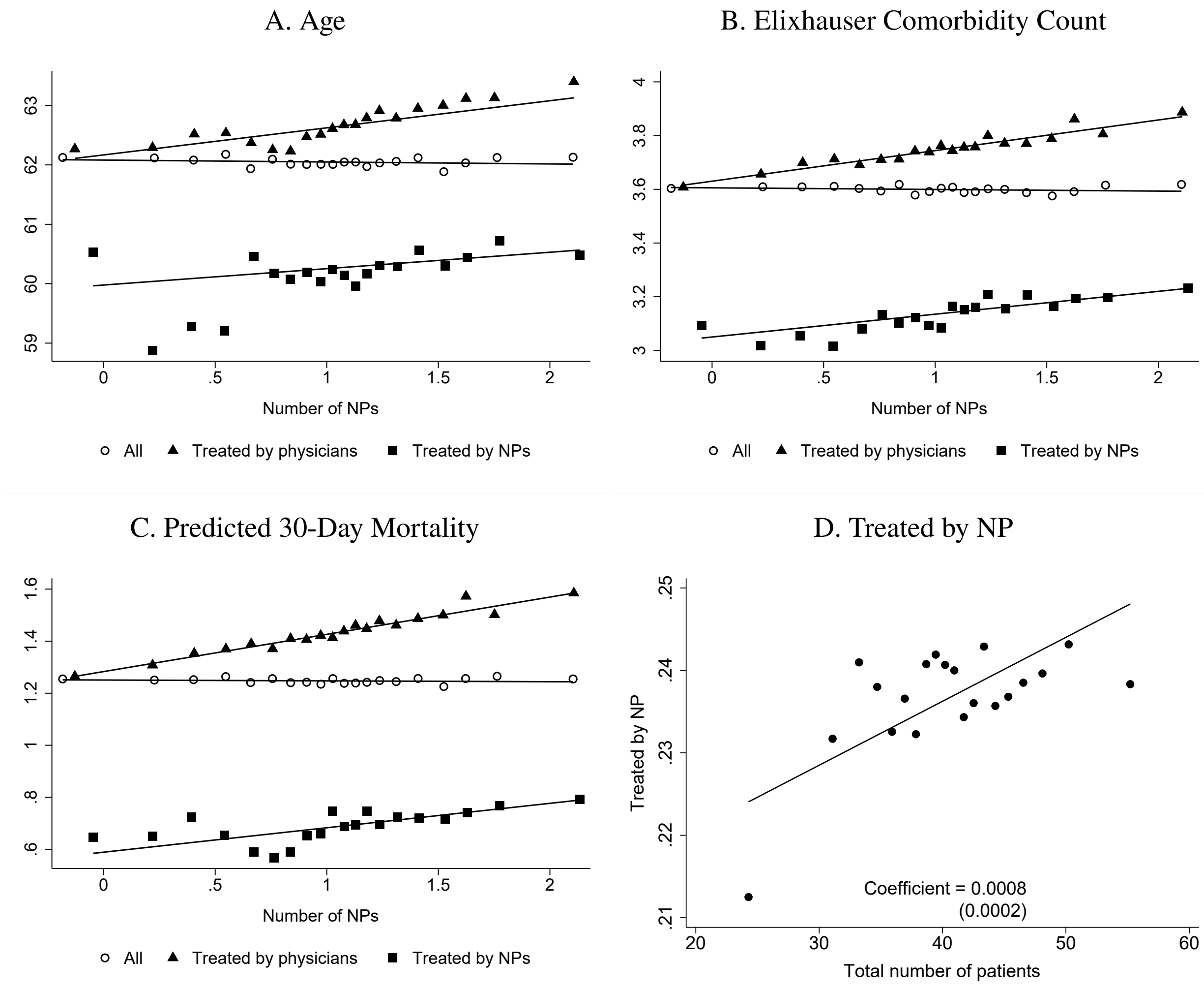
Notes: This figure shows binned scatter plots of patient actual and predicted outcomes on the *y*-axis versus the number of NPs on duty on the *x*-axis, controlling for the baseline control vector (i.e., indicators for ED-by-year, ED-by-month, ED-by-day-of-the-week, and ED-by-hour-of-the-day). Panels A, B, and C report results for hospital admission in the ED visit, 30-day mortality, and 30-day preventable hospitalization, respectively. The solid circles and lines represent patient actual outcomes. The hollow circles and dashed lines represent patient predicted outcomes generated based on patient characteristics $\mathbf{X}_i$ included in Equations (1) and (2), including patient demographics, comorbidities, prior health care use, vital signs, and three-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 A.8: Stability of OLS and IV Estimates



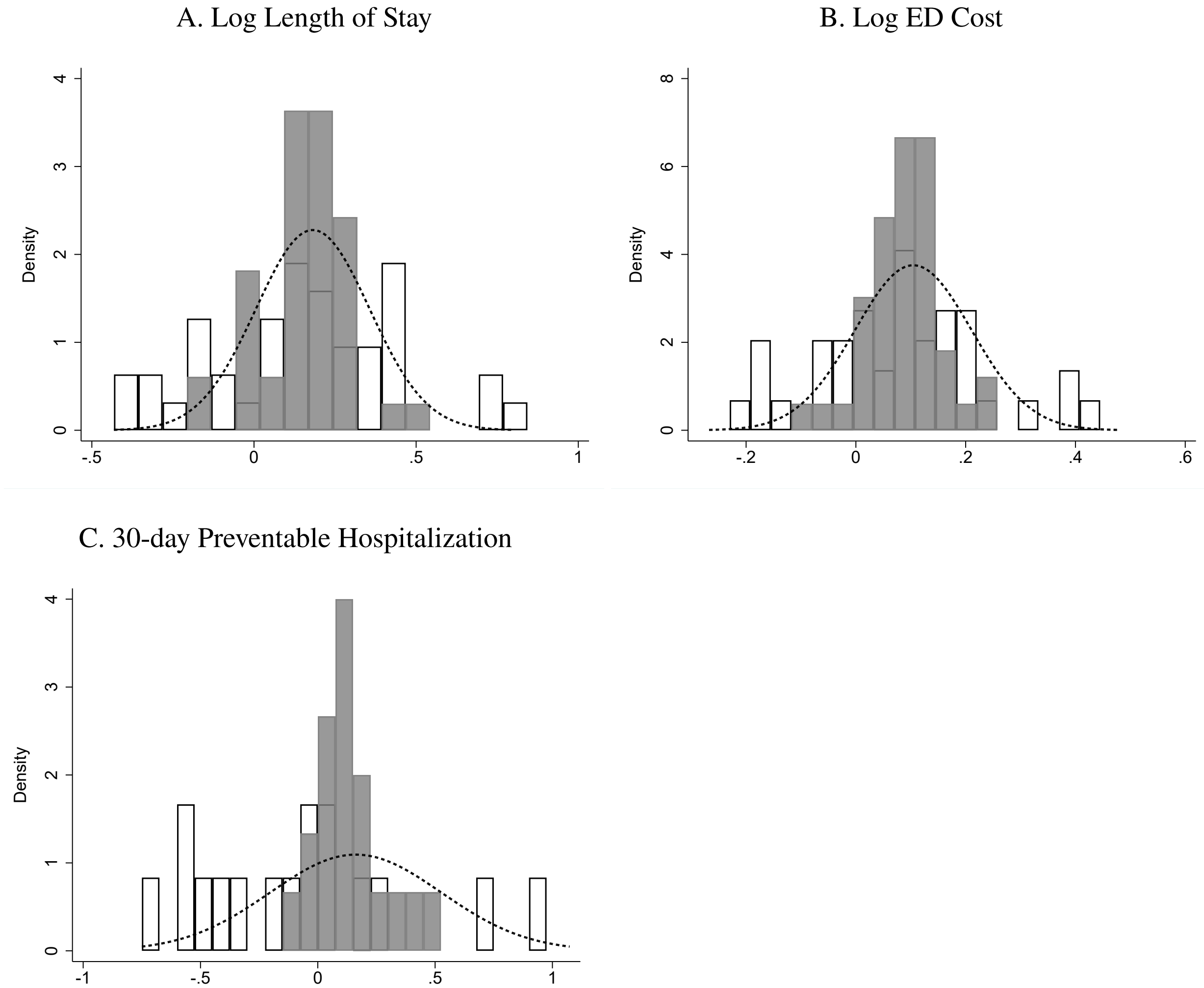
Notes: This figure shows the robustness of our OLS and IV estimates to the inclusion of different sets of patient controls. We divide patient observable characteristics into eight subsets: (i) five-year age-bin indicators; (ii) marital status; (iii) gender; (iv) race indicators; (v) indicators for 31 Elixhauser comorbidities; (vi) vital signs; (vii) prior health care use; and (viii) indicators for three-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. 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 possible combinations with n (out of eight) subsets of patient covariates. 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. The coefficients at the bottom of each panel show the IV and OLS estimates with the full set of patient controls, with standard errors reported in parentheses. Panels A, B, and C report results for hospital admission in the ED visit, 30-day mortality, and 30-day preventable hospitalization, respectively.

Figure A.9: Patient Assignment



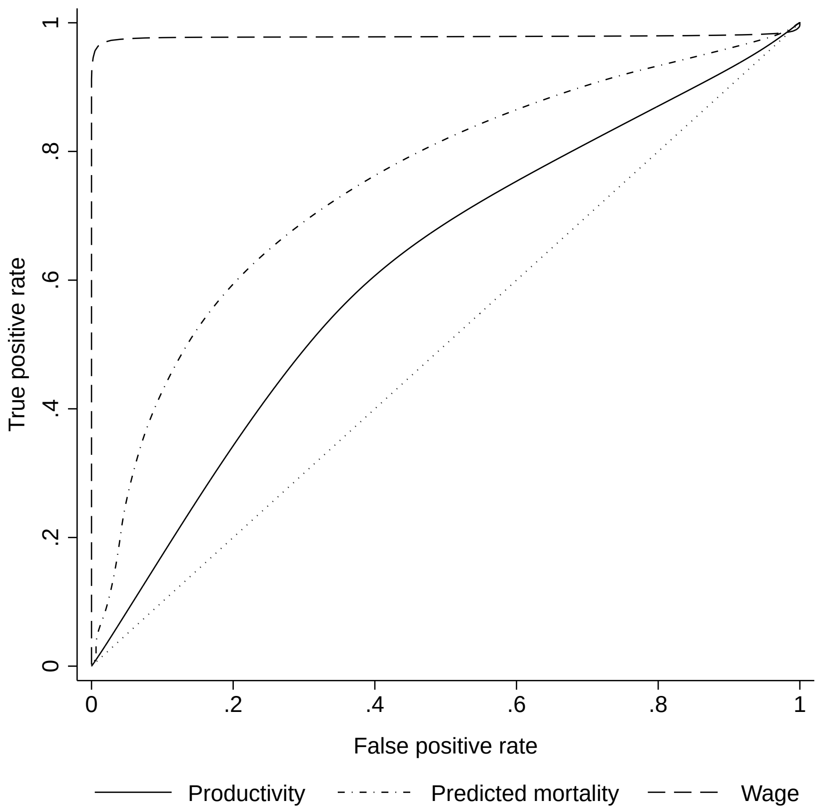
Notes: Panels A-C report patient characteristics (age, total number of Elixhauser comorbidities, and predicted 30-day mortality, respectively) on the *y*-axis against the number of NPs on duty on the *x*-axis. The unit of observation is at the case level. Both the *y*-axis and *x*-axis variables are residualized with respect to the baseline control vector (ED-by-year, ED-by-month, ED-by-day-of-the-week, and ED-by-hour-of-the-day indicators), with sample means added back to aid in interpretation. The circles, triangles, and squares show binned scatter plots for all cases, cases treated by physicians, and cases treated by NPs, respectively. Panel D plots whether the case is treated by NPs against the number of cases arriving in the analysis time window (i.e., 8 a.m. to 6 p.m.) of the ED-day cell of the case's visit. Both the *y*-axis and *x*-axis variables are residualized with respect to the baseline control vector, with means added back.

Figure A.10: ED-Specific Estimates of NP 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 white bins show the histogram of ED-specific IV estimates without any adjustment to account for estimation noise. The gray bins show the histogram of ED-specific IV estimates with empirical Bayes adjustments (see details in Appendix A.5). The dashed lines show the standard normal density with a variance of the prior distribution of ED-specific IV estimates for each outcome.

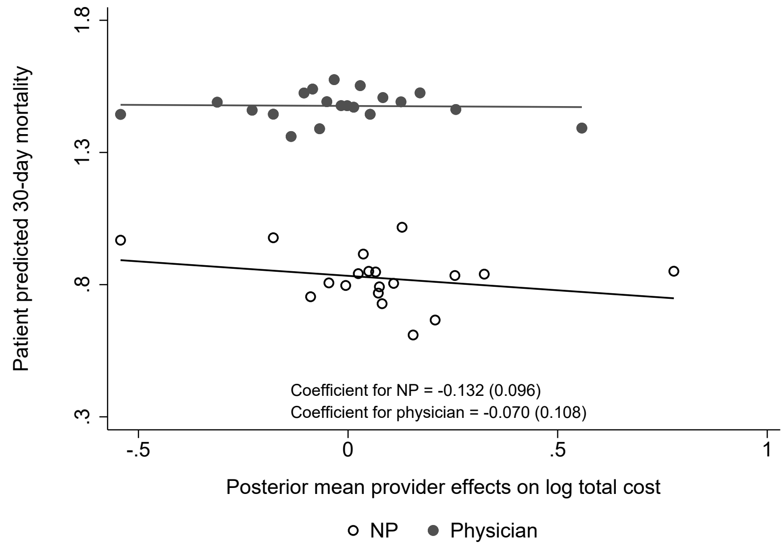
Figure A.11: Receiver Operating Characteristic (ROC) Curves



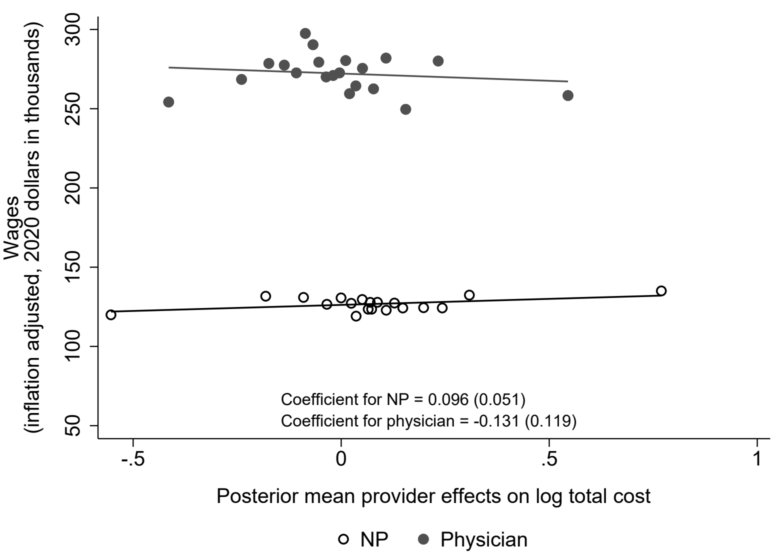
Notes: This figure displays the ROC curves for provider productivity (in the solid line), predicted 30-day mortality of cases assigned to the provider (in the dot-dashed line), and provider wages (in the dashed line). The dotted line plots the 45-degree line. Physicians are defined as the "positive" class and NPs are defined as the "negative" class. See Appendix A.4.4 for details.

Figure A.12: Case Assignment and Wage Payment versus Productivity

A. Predicted 30-Day Mortality of Assigned Cases



B. Wages

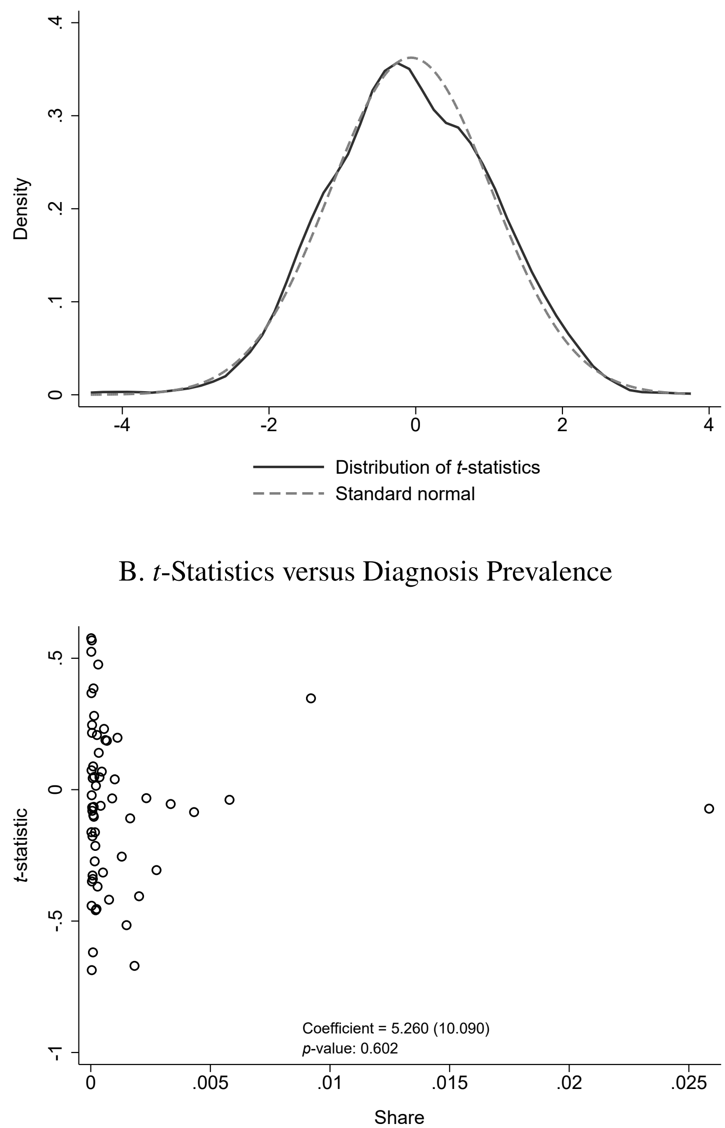


Notes: Panel A shows binned scatter plots of average risks (measured by predicted 30-day mortality) of cases assigned to a provider on the *y*-axis versus posterior mean provider effects on log total cost per ED visit on the *x*-axis. Panel B shows binned scatter plots of provider yearly wage on the *y*-axis versus posterior mean provider effects on log total cost per ED visit 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 posterior mean provider effects controlling for ED indicators are reported, with standard errors clustered by ED shown in parentheses. The hollow circles report results for NPs; the solid circles report results for physicians. See more details in Appendix A.4.5.

Figure A.13: Diagnosis Coding: NPs versus Physicians

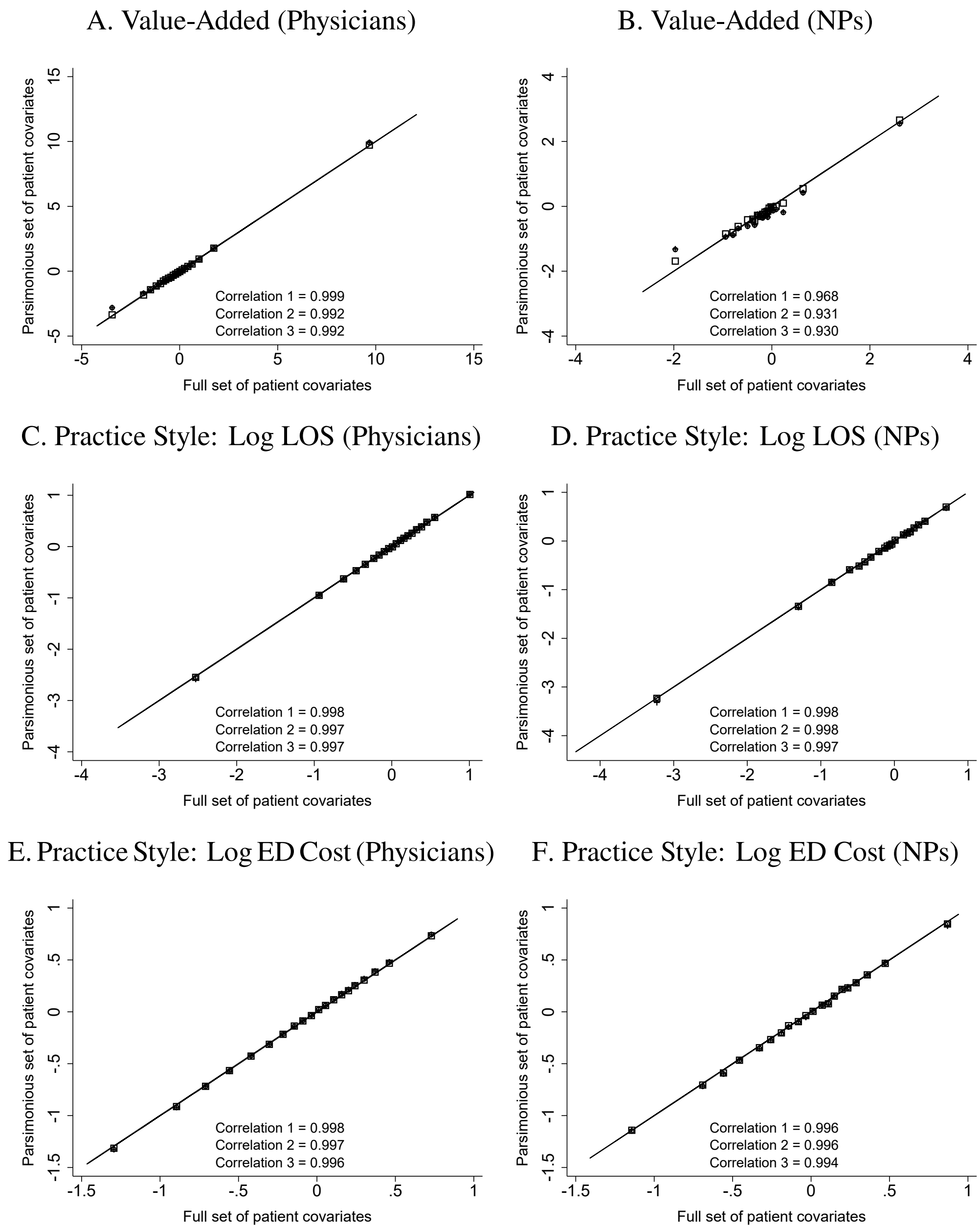
A. Distribution of *t*-Statistics

B. *t*-Statistics versus Diagnosis Prevalence



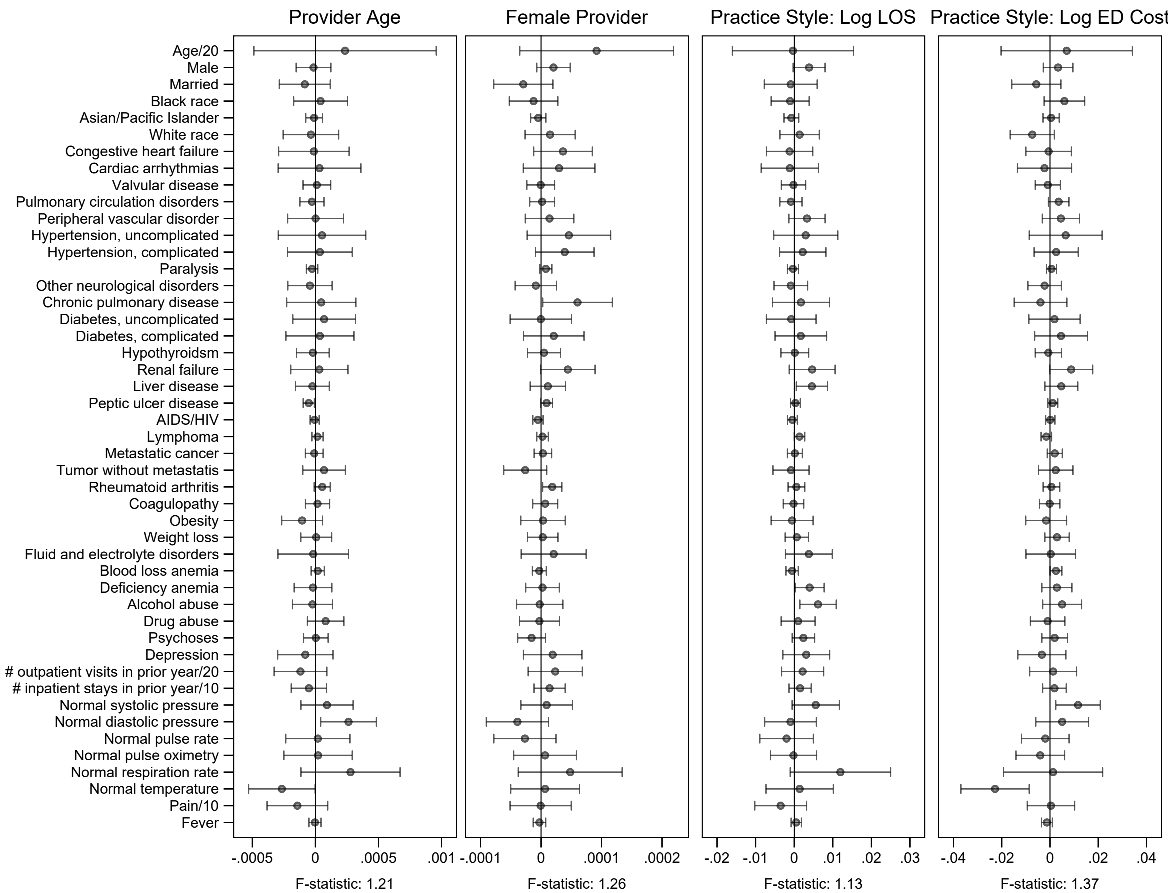
Notes: Panel A plots the distribution of the *t*-statistics on whether NPs and physicians are significantly different in diagnosis coding from 836 separate regressions that use each three-digit diagnosis indicator as the outcome variable. The distribution is estimated using an Epanechnikov kernel with 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 scatter plots 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 the influence of patient sorting between NPs and physicians). The coefficient from the regression of the *t*-statistics on prevalence is reported in the panel, along with its standard error (shown in parentheses) and *p*-value.

Figure A.14: Stability of Provider Value-Added and Practice Style with Varying Patient Covariates



Notes: This figure shows the stability of provider value-added and practice style estimated using alternative patient covariates. See Appendix A.3 for details. The x-axis in each panel reports provider value-added/practice style constructed using the full set of patient covariates, including 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 VHA facilities in the prior 365 days), vital signs, and indicators for three-digit ICD-10 code of the primary diagnosis of the visit. The y-axis reports provider value-added/practice style constructed using alternative sets of patient covariates: Parsimonious set 1 that includes demographics, three-digit diagnosis indicators, and 31 Elixhauser comorbidities; parsimonious set 2 that includes demographics and three-digit diagnosis indicators; parsimonious set 3 that includes five-year age-bin and three-digit diagnosis indicators. Valued-added and practice style constructed using parsimonious sets 1-3 are shown in squares, circles, and "+", respectively. Correlations 1-3 report correlations of value-added/practice style estimated using the full set of patient covariates with those using parsimonious sets 1-3, respectively. The solid lines show the 45-degree line. Panels A, C and E report results for physicians. Panel B, D and F report results for NPs.

Figure A.15: Balance of Patient Characteristics across On-Duty 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 baseline controls (i.e., indicators for ED-by-year, ED-by-month, ED-by-day-of-the-week, and ED-by-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 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 each panel, we report the *F*-statistic from the joint *F*-test for all patient characteristics in a reverse regression with the average on-duty provider characteristic as the dependent variable, conditioning on the baseline control vector. Standard errors are clustered by provider.

Table A.1: Characteristics of NPs and Physicians at VHA and Non-VHA

	VHA (ED)	Non-VHA (ED)	Non-VHA (all)
Panel A. NPs			
Female (%)	81.4	79.1	90.0
Age	51.3	42.9	44.8
Panel B. Physicians			
Female (%)	34.0	27.3	31.0
Age	48.1	45.8	50.4

Notes: Panel A reports summary statistics for NPs; Panel B reports summary statistics for physicians. Column 1 reports summary statistics for NPs/physicians working at the ED in our analysis sample. Column 2 reports summary statistics for NPs/physicians working at the ED observed in the 20 percent Medicare data (with age and gender information obtained from the Medicare Data on Provider Practice and Specialty (MD-PPAS)). To provide a description of providers outside of the ED, Column 3 reports characteristics of all NPs/physicians (regardless of working at the ED) observed in the 20 percent Medicare data. VHA ED NPs' and physicians' mean age and female share reported in Column 1 are slightly different from those reported in Figures 5 and 6 because the latter weight means by the number of ED-days a provider works and patient volume.

Table A.2: Selection of Baseline Sample

Sample step	Description	Cases	Providers: NPs	Providers: Physicians	EDs
1. Build sample of ED cases from January 1, 2017, to January 31, 2020.	We restrict the sample to cases after the VHA directive granting NPs full practice authority in December 2016 and before COVID pandemic in the US.	7,886,164	547	5,749	146
2. Include only cases visiting during daytime.	Empirically NPs do not work outside of the hours of 8 a.m. to 6 p.m. We drop cases visiting outside of the hours of 8 a.m. to 6 p.m. to focus on cases that could be assigned to an NP.	5,766,296	539	5,665	145
3. Restrict EDs to those with NPs, in months with full practice authority.	We restrict the sample to EDs where NPs work. We restrict to months in which these EDs have granted NPs full practice authority.	3,597,347	521	3,781	111
4. Restrict EDs to those in which NPs and physicians are the only providers.	To focus attention on the margin between NPs and physicians and hold the population of cases seen by an NP or physician fixed, we drop EDs that use other provider types, mainly physician assistants.	1,119,396	156	1,348	44
5. Drop cases with missing demographics or extreme ages.	We drop cases with missing age or gender, or age above 99 or below 20.	1,118,836	156	1,348	44

Notes: This table reports changes in sample size when applying each of the listed sample restrictions. Columns 3-6 report, respectively, the number of cases, NPs, physicians, and EDs remaining at each step.

Table A.3: EDs Included in Main Analysis versus All VHA EDs

	Included	All
Panel A: ED characteristics		
Daily census	30.69	36.06
	(17.58)	(21.34)
Urban location	0.86	0.87
	(0.35)	(0.34)
Panel B: Provider Characteristics		
Physician mean age	52.218	50.962
	(11.288)	(10.979)
Share physicians female	0.286	0.326
	(0.452)	(0.469)
NP mean age	51.245	50.915
	(8.940)	(9.352)
Share NPs female	0.803	0.752
	(0.398)	(0.432)
Panel C: Patient characteristics		
Age	61.987	61.770
	(15.857)	(15.828)
Married	0.416	0.432
	(0.493)	(0.495)
Male	0.905	0.901
	(0.294)	(0.299)
Black	0.275	0.272
	(0.447)	(0.445)
White	0.704	0.709
	(0.456)	(0.454)
Asian/Pacific Islander	0.020	0.018
	(0.140)	(0.133)
Outpatient visits in prior year	6.409	6.552
	(7.697)	(7.717)
Inpatient stays in prior year	0.643	0.689
	(1.612)	(1.684)
Elixhauser comorbidity count	3.626	3.705
	(3.035)	(3.100)
Predicted 30-day mortality (%)	1.200	1.229
	(2.753)	(2.782)

Notes: Columns 1 and 2 report characteristics of EDs that are included in our main analysis sample and of all VHA EDs, respectively. Panel A reports characteristics of the EDs, including daily census in the analysis time window (i.e., 8 a.m. to 6 p.m.) and urban location. Panel B reports characteristics of the physicians and NPs practicing in the EDs. Panel C reports characteristics of patients seen in the EDs. The standard deviation of each characteristic is reported in brackets. Patient and provider characteristics in the included EDs, i.e., Column 1, are slightly different from those reported in Table 1 and Figures 5 and 6 because Column 1 includes observations in the ED in all months between January 2017 and January 2020 regardless of whether the ED had adopted full practice authority for NPs in the month.

Table A.4: Complier and Never-Taker Characteristics

	All	Compliers		Never-takers	
	Mean	Mean	Ratio	Mean	Ratio
Age	62.05	61.11	0.98	63.69	1.03
	(0.15)	(0.31)	[0.98 - 0.99]	(0.17)	[1.02 - 1.03]
Married	0.424	0.424	1.00	0.436	1.03
	(0.004)	(0.008)	[0.97 - 1.04]	(0.007)	[1.00 - 1.06]
Male	0.905	0.905	1.00	0.917	1.01
	(0.002)	(0.003)	[0.99 - 1.01]	(0.002)	[1.01 - 1.02]
Black	0.270	0.262	0.97	0.228	0.84
	(0.011)	(0.019)	[0.83 - 1.11]	(0.015)	[0.73 - 0.95]
White	0.708	0.716	1.01	0.756	1.07
	(0.011)	(0.019)	[0.96 - 1.06]	(0.015)	[1.03 - 1.11]
Asian/Pacific Islander	0.021	0.020	0.95	0.013	0.65
	(0.001)	(0.002)	[0.74 - 1.15]	(0.001)	[0.55 - 0.76]
Outpatient visits in prior year	6.242	5.824	0.93	6.537	1.05
	(0.080)	(0.129)	[0.89 - 0.97]	(0.110)	[1.01 - 1.08]
Inpatient stays in prior year	0.612	0.490	0.80	0.695	1.14
	(0.014)	(0.029)	[0.71 - 0.89]	(0.026)	[1.05 - 1.22]
Elixhauser comorbidity count	3.599	3.324	0.92	3.965	1.10
	(0.030)	(0.066)	[0.89 - 0.96]	(0.041)	[1.08 - 1.12]
Predicted 30-day mortality (%)	1.247	0.902	0.72	1.697	1.36
	(0.032)	(0.067)	[0.62 - 0.83]	(0.049)	[1.28 - 1.44]

Notes: This table reports average characteristics for the overall sample, compliers, and never-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-by-year, ED-by-month, ED-by-day-of-the-week, and ED-by-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. 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-by-year, ED-by-month and ED-by-day-of-the-week. Standard errors for the overall sample and never-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 three-digit diagnosis indicators.

Table A.5: On-Duty Physician Value-Added and Experience and Outcomes of Patients Treated by NPs

	Dependent variable						
	Elixhauser comorbidity count	Predicted 30-day mortality	Log length of stay	Log ED cost	Admission	30-day mortality	30-day prevent. hosp.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Physician value-added							
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)
Panel B: Physician experience							
Physician experience	-0.050	-0.100	0.000	-0.030	1.050	0.327	-0.103
	(0.087)	(0.072)	(0.051)	(0.044)	(0.801)	(0.224)	(0.244)
Controls	Baseline	Baseline	Full	Full	Full	Full	Full
Mean dep. var.	3.128	0.728	4.302	6.298	7.726	0.633	0.719
S.D. dep. var.	2.711	2.115	1.083	0.870	26.700	7.929	8.446
Observations	147,936	147,936	146,948	146,935	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 (Panel A) and across the average experience (measured by age) of physicians on duty (Panel B). Coefficients in Panel B is scaled up by 100 for readability. See Appendix A.3 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-by-year, ED-by-month, ED-by-day-of-the-week, and ED-by-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.

Table A.6: NP Presence and Outcomes of Patients Treated by Physicians

	Dependent variable						
	Elixhauser comorbidity count	Predicted 30-day mortality	Log length of stay	Log ED cost	Admission	30-day mortality	30-day prevent. hosp.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Baseline results							
NPs on duty	0.012	-0.013	-0.002	0.001	0.004	-0.022	-0.116
	(0.032)	(0.029)	(0.012)	(0.007)	(0.003)	(0.098)	(0.117)
Panel B: By tercile of case count in ED-day cell							
NPs on duty							
× Bottom two terciles	0.027	0.017	0.027	0.009	0.004	0.023	-0.084
	(0.043)	(0.038)	(0.016)	(0.010)	(0.004)	(0.142)	(0.162)
× Top tercile	0.002	-0.035	-0.017	-0.004	0.003	-0.053	-0.134
	(0.039)	(0.034)	(0.014)	(0.009)	(0.004)	(0.114)	(0.146)
Controls	Baseline	Baseline	Full	Full	Full	Full	Full
Mean dep. var.	3.535	1.070	4.545	6.486	0.154	1.051	1.324
S.D. dep. var.	2.996	2.758	1.276	0.836	0.361	10.200	11.432
Observations	68,863	68,863	68,214	68,208	68,863	68,863	68,863

Notes: This table shows balance in outcomes of patients treated by physicians against the presence of NPs. The sample is restricted to patients arriving between 5 and 8 a.m. in ED-day cells with all patients arriving between 5 and 8 a.m. being assigned to physicians. Panel A shows baseline results. The empirical specification takes the form $y_i = \gamma \mathbf{1}(Z_i > 0) + \mathbf{T}_i\eta + \mathbf{X}_i\beta + \varepsilon_i$, where $\mathbf{1}(Z_i > 0)$ is an indicator for whether there are NPs on duty during 8 a.m.-12 p.m of the ED-day cell of the patient's visit. Panel B shows heterogeneous effects by whether the total number of cases in the ED-day cell is in the top tercile of all ED-days. The empirical specification takes the form $y_i = \sum_{g=1}^{G} \mathbf{1}(\text{Group}_i = g)\left[\gamma_g \mathbf{1}(Z_i > 0) + \lambda_g\right] + \mathbf{T}_i\eta + \mathbf{X}_i\beta + \varepsilon_i$, where $\mathbf{1}(\text{Group}_i = g)$ is an indicator for whether the ED-day cell has a number of cases arriving between 5 and 8 a.m. in the top or bottom two tercile(s) of all ED-days. 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-by-year, ED-by-month, ED-by-day-of-the-week, and ED-by-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.

Table A.7: Robustness to Controlling for Average Risks of Patients Assigned to Physicians

	Dependent variable				
	Log length of stay	Log ED cost	Admission	30-day mortality	30-day prevent. hosp.
	(1)	(2)	(3)	(4)	(5)
Panel A: Baseline					
NP assignment	0.110	0.070	0.103	-0.116	0.252
	(0.045)	(0.030)	(0.585)	(0.115)	(0.120)
Panel B: Control for age					
NP assignment	0.107	0.070	0.101	-0.125	0.254
	(0.045)	(0.030)	(0.590)	(0.116)	(0.122)
Panel C: Control for Elixhauser comorbidity count					
NP assignment	0.106	0.069	0.117	-0.136	0.238
	(0.045)	(0.030)	(0.592)	(0.116)	(0.122)
Panel D: Control for predicted 30-day mortality					
NP assignment	0.100	0.068	0.191	-0.114	0.258
	(0.044)	(0.030)	(0.596)	(0.117)	(0.124)
Full controls	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	4.608	6.483	16.625	1.247	1.234
S.D. dep. var.	1.161	0.878	37.230	11.099	11.041
Observations	1,110,798	1,108,961	1,118,836	1,118,836	1,118,836

Notes: This table shows robustness of the estimates to controlling for the average health risks of cases (age, total number of Elixhauser comorbidities, and predicted 30-day mortality in Panels B, C, and D, respectively) assigned to physicians in the ED-day cell (leaving out the index case). For comparison, Panel A repeats our baseline estimates reported in Tables 2 and 3. 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.

Table A.8: Robustness to Additional Controls

	Dependent variable				
	Log length of stay (1)	Log ED cost (2)	Admission (3)	30-day mortality (4)	30-day prevent. hosp. (5)
Panel A: Baseline					
NP assignment	0.110	0.070	0.103	-0.116	0.252
	(0.045)	(0.030)	(0.585)	(0.115)	(0.120)
Panel B: Control for patient volume (linear controls)					
NP assignment	0.095	0.081	0.597	-0.082	0.237
	(0.045)	(0.030)	(0.606)	(0.116)	(0.118)
Panel C: Control for patient volume (linear splines)					
NP assignment	0.096	0.081	0.602	-0.080	0.239
	(0.045)	(0.030)	(0.606)	(0.116)	(0.118)
Panel D: Control for patient volume (restricted cubic splines)					
NP assignment	0.096	0.081	0.602	-0.080	0.238
	(0.045)	(0.030)	(0.606)	(0.116)	(0.118)
Panel E: Control for doctor equivalents (1 NP = 0.341 physicians)					
NP assignment	0.110	0.070	0.103	-0.116	0.252
	(0.044)	(0.029)	(0.584)	(0.115)	(0.120)
Panel F: Control for doctor equivalents (1 NP = 0.5 physicians)					
NP assignment	0.085	0.061	-0.019	-0.104	0.245
	(0.043)	(0.029)	(0.574)	(0.113)	(0.117)
Panel G: Control for wait time					
NP assignment	0.109	0.069	0.317	-0.074	0.250
	(0.045)	(0.030)	(0.594)	(0.124)	(0.121)
Full controls	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	4.608	6.483	16.625	1.247	1.234
S.D. dep. var.	1.161	0.878	37.230	11.099	11.041
Observations	1,110,798	1,108,961	1,118,836	1,118,836	1,118,836

Notes: Panel A repeats the baseline estimates reported in Tables 2 and 3. Panels B, C, and D add controls for patient volume in the analysis time window (i.e., 8 a.m. to 6 p.m.) of the ED-day cell of the patient's visit, with patient volume controlled linearly, as linear splines, and as restricted cubic splines, respectively. Panels E and F add a control for the total number of doctor equivalents on duty at the ED on the day the patient visits. Panels E and F assume a substitution rate of 0.341 and 0.5 between NPs and physicians, respectively. Panel G 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 add an instrument for wait time: 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.

Table A.9: Patient-Provider Gender Match

	Dependent variable						
	Elixhauser comorbidity count	Predicted 30-day mortality	Log length of stay	Log ED cost	Admission	30-day mortality	30-day prevent. hosp.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
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)
Male patient	0.743	0.745	-0.051	-0.016	0.262	0.042	0.103
	(0.092)	(0.092)	(0.014)	(0.013)	(0.508)	(0.080)	(0.078)
Controls	Baseline	Baseline	Full	Full	Full	Full	Full
Mean dep. var.	3.190	0.743	4.304	6.341	7.866	0.630	0.745
S.D. dep. var.	2.772	2.145	1.137	0.856	26.921	7.910	8.598
Observations	264,772	264,772	262,960	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 indicator for female NPs and the indicator for male patients, the indicator for female NPs, and the indicator for male patients. Columns 1-2 examine the balance in patient characteristics and add controls for the baseline control vector (i.e., ED-by-year, ED-by-month, ED-by-day-of-the-week, and ED-by-hour-of-the-day indicators). Columns 3-7 add 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.

Table A.10: Alternative Standard Error Clustering

	Dependent variable				
	Log length of stay	Log ED cost	Admission	30-day mortality	30-day prevent. hosp.
	(1)	(2)	(3)	(4)	(5)
Panel A: Clustering by provider					
NP assignment	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					
NP assignment	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					
NP assignment	0.110	0.070	0.103	-0.116	0.252
	(0.045)	(0.030)	(0.581)	(0.113)	(0.119)
Full controls	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	4.608	6.483	16.625	1.247	1.234
S.D. dep. var.	1.161	0.878	37.230	11.099	11.041
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.

Table A.11: Alternative Instruments

	Dependent variable				
	Log length of stay	Log ED cost	Admission	30-day mortality	30-day prevent. hosp.
	(1)	(2)	(3)	(4)	(5)
Panel A: Include NPs with only one case					
NP assignment	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					
NP assignment	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 NPs					
NP assignment	0.117	0.069	0.926	-0.033	0.208
	(0.052)	(0.032)	(0.628)	(0.118)	(0.121)
Panel D: Indicator for any NP on duty					
NP assignment	0.108	0.080	0.185	-0.048	0.211
	(0.049)	(0.030)	(0.643)	(0.121)	(0.130)
Full controls	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	4.608	6.483	16.625	1.247	1.234
S.D. dep. var.	1.161	0.878	37.230	11.099	11.041
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. Standard errors clustered by provider are shown in parentheses.

Table A.12: Sample Restricted to ED-Days with Zero or One NP

	Dependent variable				
	Log length of stay	Log ED cost	Admission	30-day mortality	30-day prevent. hosp.
	(1)	(2)	(3)	(4)	(5)
NP assignment	0.109	0.084	0.146	-0.042	0.219
	(0.052)	(0.032)	(0.686)	(0.128)	(0.140)
Full controls	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	4.594	6.445	16.301	1.241	1.235
S.D. dep. var.	1.147	0.887	36.937	11.069	11.045
Observations	862,416	860,798	868,930	868,930	868,930

Notes: This table shows results using only patients in ED-day cells with zero or one 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. Standard errors clustered by provider are shown in parentheses.

Table A.13: Hospital Admissions and Preventable Hospitalizations Outside VHA

	Dependent variable			
	Admission		30-day prevent. hosp.	
	(1)	(2)	(3)	(4)
	VHA only	VHA+Medicare	VHA only	VHA+Medicare
NP assignment	0.806	0.806	0.485	0.371
	(0.798)	(0.794)	(0.205)	(0.222)
Full controls	Yes	Yes	Yes	Yes
Mean dep. var.	20.054	20.267	1.704	2.166
S.D. dep. var.	40.040	40.199	12.941	14.556
Observations	545,791	545,791	543,253	543,253

Notes: This table shows the robustness of our results to including hospital admissions and 30-day preventable hospitalizations outside of the VHA by examining patients who enroll in both the VHA and traditional Medicare. The VHA provides linked Medicare claims for beneficiaries who are traditional Medicare enrollees. Columns 1 and 2 show the robustness of results for hospital admissions during the ED visit. Column 1 measures only hospital admissions in the VHA; Column 2 adds hospital admissions in the Medicare claims. To obtain full observation of hospital admissions in non-VHA hospitals, Columns 1 and 2 restrict the sample to patients who enroll in traditional Medicare in the month of the ED visit. Columns 3 and 4 show the robustness of results for 30-day preventable hospitalizations. Column 3 measures 30-day preventable hospitalizations in the VHA; Column 4 adds 30-day preventable hospitalizations in the Medicare claims. To obtain full observation of 30-day preventable hospitalizations in non-VHA hospitals, Columns 3 and 4 restrict the sample to patients who enroll in traditional Medicare in both the month of the ED visit and the month that follows. All estimations include the full set of controls described in the notes to Table 2. Standard errors clustered by provider are shown in parentheses.

Table A.14: Including All EDs Using NPs

	Dependent variable				
	Log length of stay	Log ED cost	Admission	30-day mortality	30-day prevent. hosp.
	(1)	(2)	(3)	(4)	(5)
NP assignment	0.107	0.047	-0.003	-0.087	0.141
	(0.030)	(0.018)	(0.004)	(0.076)	(0.083)
Full controls	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	4.600	6.522	0.171	1.216	1.268
S.D. dep. var.	1.151	0.869	0.377	10.958	11.190
Observations	2,167,104	2,166,520	2,184,032	2,184,032	2,184,032

Notes: This table shows the results that include cases in all VHA EDs that use NPs regardless of whether the ED uses physician assistants (but exclude cases in ED-day cells with physician assistants to focus on the margin between NPs and physicians). This table expands the analysis sample from 1.1 million cases across 44 EDs in our main analysis to 2.2 million cases across 110 EDs. 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.

Table A.15: Heterogeneous Effects by Provider Experience (Cases in 2018-)

	Dependent variable				
	Log length of stay	Log ED cost	Admission	30-day mortality	30-day prevent. hosp.
	(1)	(2)	(3)	(4)	(5)
Panel A: Provider specific experience					
NP assignment	0.081	0.077	-0.268	-0.222	0.335
	(0.046)	(0.032)	(0.646)	(0.142)	(0.149)
NP assignment × experience	-0.060	-0.050	-0.677	0.011	-0.012
	(0.025)	(0.021)	(0.308)	(0.044)	(0.033)
Experience	-0.006	0.004	0.204	-0.008	-0.018
	(0.005)	(0.009)	(0.303)	(0.017)	(0.012)
Panel B: Provider general experience					
NP assignment	0.104	0.089	-0.356	-0.238	0.347
	(0.048)	(0.034)	(0.700)	(0.146)	(0.152)
NP assignment × experience	-0.130	-0.069	0.249	0.108	-0.029
	(0.067)	(0.040)	(1.337)	(0.114)	(0.077)
Experience	-0.034	-0.009	-0.721	-0.017	-0.039
	(0.015)	(0.011)	(0.230)	(0.024)	(0.025)
Full controls	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	4.637	6.529	16.304	1.251	1.226
S.D. dep. var.	1.133	0.887	36.940	11.114	11.005
Observations	742,968	741,027	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 heterogeneity by provider specific experience in the case's condition, measured as the number of cases with the same three-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 heterogeneity by provider general experience, measured as the number of cases (despite diagnoses) 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 1-5 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.

Table A.16: Heterogeneous Effects by Provider Experience (Prior-Year Experience)

	Dependent variable				
	Log length of stay	Log ED cost	Admission	30-day mortality	30-day prevent. hosp.
	(1)	(2)	(3)	(4)	(5)
Panel A: Provider specific experience					
NP assignment	0.078	0.074	-0.295	-0.221	0.333
	(0.046)	(0.031)	(0.641)	(0.141)	(0.148)
NP assignment × experience	-0.053	-0.055	-0.646	0.016	-0.017
	(0.027)	(0.023)	(0.307)	(0.049)	(0.035)
Experience	-0.009	0.009	0.260	-0.012	-0.012
	(0.006)	(0.011)	(0.327)	(0.018)	(0.014)
Panel B: Provider general experience					
NP assignment	0.089	0.081	-0.331	-0.222	0.350
	(0.046)	(0.033)	(0.662)	(0.143)	(0.150)
NP assignment × experience	-0.120	-0.063	-0.142	0.040	-0.117
	(0.088)	(0.044)	(1.162)	(0.091)	(0.085)
Experience	-0.040	-0.002	-0.732	-0.008	-0.008
	(0.017)	(0.012)	(0.235)	(0.026)	(0.026)
Full controls	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	4.637	6.529	16.304	1.251	1.226
S.D. dep. var.	1.133	0.887	36.940	11.114	11.005
Observations	742,968	741,027	747,510	747,510	747,510

Notes: This table reports heterogeneous effects of NPs by provider experience in the prior year. Panel A shows heterogeneity by provider specific experience in the case's condition, measured as the number of cases with the same three-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 heterogeneity by provider general experience, measured as the number of cases (despite diagnoses) 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 1-5 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.

Table A.17: Heterogeneous Effects by Provider Experience (Measured in Days)

	Dependent variable				
	Log length of stay	Log ED cost	Admission	30-day mortality	30-day prevent. hosp.
	(1)	(2)	(3)	(4)	(5)
NP assignment	0.105	0.065	0.225	-0.100	0.263
	(0.045)	(0.029)	(0.623)	(0.117)	(0.123)
NP assignment × experience	-0.031	-0.036	1.098	0.126	0.101
	(0.068)	(0.037)	(1.157)	(0.111)	(0.080)
Experience	-0.015	-0.002	-0.403	-0.022	-0.053
	(0.014)	(0.009)	(0.227)	(0.024)	(0.026)
Full controls	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	4.608	6.483	16.625	1.247	1.234
S.D. dep. var.	1.161	0.878	37.230	11.099	11.041
Observations	1,110,798	1,108,961	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 1-5 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.

Table A.18: Ten Most Common High-Mortality Diagnoses

ICD code	Description	30-day mortality (%)	Cases	Share
I50	Heart failure	5.56	12,637	0.221
N17	Acute kidney failure	6.47	4,278	0.075
R41	Other symptoms and signs involving cognitive functions and awareness	7.59	3,872	0.068
D64	Other anemias	5.01	3,634	0.064
I21	Acute myocardial infarction	7.50	3,162	0.055
A41	Other sepsis	11.51	2,754	0.048
J15	Bacterial pneumonia, not elsewhere classified	5.12	2,715	0.047
J96	Respiratory failure, not elsewhere classified	12.99	2,548	0.045
F03	Unspecified dementia	5.40	1,427	0.025
R62	Lack of expected normal physiological development in childhood and adults	16.55	1,033	0.018

Notes: This table summarizes the 10 most common three-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 columns report, from the leftmost to the rightmost, the three-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 three-digit diagnosis whose 30-day mortality is equal to or above the 95th percentile of the sample.

Table A.19: Heterogeneous Effects by Patient Characteristics

	Dependent variable				
	Log length of stay	Log ED cost	Admission	30-day mortality	30-day prevent. hosp.
	(1)	(2)	(3)	(4)	(5)
Panel A: Elixhauser comorbidity count					
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: Diagnosis predicted 30-day mortality					
$<$ 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: Diagnosis category					
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	0.097	0.067	-0.343	-0.147	0.332
	(0.045)	(0.029)	(0.578)	(0.112)	(0.122)
Full controls	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	4.608	6.483	16.625	1.247	1.234
S.D. dep. var.	1.161	0.878	37.230	11.099	11.041
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 characteristics described in Section 5.3. Panel A divides cases into quartiles by their total number of Elixhauser comorbidities, with higher quartiles indicating more complex cases. Panel B divides cases by whether condition severity measured by 30-day mortality of cases with the same three-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.

Table A.20: Variance of Provider Effects on Medical Spending

	NPs	Physicians
Basic estimates	0.0537	0.0643
Split-sample estimates	0.0476	0.0445

Notes: This table reports variance of provider effects on log 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 for which we find significant NP effects: the cost of care at the ED, hospital admission, and 30-day preventable hospitalizations (we multiply the latter two components by the average cost of a hospital stay, \$19,220). Row 1 reports variance of provider effects $\hat{\theta}_j$ estimated using Equations (A.4) and (A.5). To account for biases due to estimation noise in $\hat{\theta}_j$, Row 2 reports variance using a split-sample approach (details in Appendix A.4.2). Column 1 reports variance for NPs. Column 2 reports variance for physicians.

Table A.21: Relationship Between z-scores and Standard Errors

	Dependent variable: provider z-score			
	(1)	(2)	(3)	(4)
Provider std. error	0.166	0.203	-0.456	0.085
	(0.434)	(0.348)	(0.385)	(0.471)
Estimation sample	Full	Full	Split	Split
Provider group	NPs	Physicians	NPs	Physicians
Mean dep. var.	0.018	-0.137	0.071	-0.056
S.D. dep. var.	1.332	1.681	1.227	1.537
Mean std. error	0.307	0.226	0.283	0.202
S.D. std. error	0.515	0.206	0.365	0.124
Providers	75	644	64	474
Observations	75	644	128	948

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. The number of unique providers in Columns 1 and 2 are smaller than those reported in Section 2.3 because our deconvolution includes only providers with at least 150 cases (to restrict the inclusion of nosiy provider effect estimates). Columns 3 and 4 have smaller numbers of providers than those in Columns 1 and 2, respectively, because Columns 3 and 4 further drop providers with less than 150 cases in each split sample.