Is There a VA Advantage? Evidence from Dually Eligible Veterans

David C. Chan
David Card
Lowell Taylor*

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Abstract

Societies face a difficult choice between private and public provision of health care, and there is a shortage of credible evidence to guide this choice. The structure of health care delivery to US veterans provides a distinctive research setting to study this issue. Specifically, veterans aged 65 and older are dually eligible for care in private hospitals (financed by Medicare) or in public-sector hospitals operated by the US Department of Veterans Affairs (VA) in the nation's largest integrated health care delivery system. We utilize the ambulance design of Doyle et al. (2015) to examine the effect of VA vs. non-VA emergency care on mortality in this high-risk population. We find a VA advantage: a 28-day mortality reduction of 46% (4.5 percentage points, with a 95% confidence interval of 1.1 to 8.0). Survival gains persist for at least a year after the initial ambulance ride, and they accrue despite *lower* spending in the VA. Evidence suggests that the VA advantage arises in part from some combination of continuity of care and health IT. These results have policy relevance—as the federal government is deciding whether to maintain the existing VA system or to expand financing of private care outside of the VA—and they shed light on sources of inefficiency in private-sector health care in the US.

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*Chan: Stanford University, Department of Veterans Affairs, and NBER, david.c.chan@stanford.edu; Card: UC Berkeley and NBER, card@berkeley.edu; Taylor: Carnegie Mellon University and NBER, lt20@andrew.cmu.edu. We thank Steve Asch, Marika Cabral, Joe Doyle, Amy Finkelstein, Josh Gottlieb, Kate Ho, Peter Hull, Bob Kaestner, Chad Kessler, Tim Layton, Michael McWilliams, Maria Polyakova, Jim Rebitzer, Adam Sacarny, David Silver, Anita Vashi, Todd Wagner, and Chris Walters for helpful comments and suggestions. We also thank Sophie Andrews, Sydney Costantini, Kaveh Danesh, Johnny Huynh, Kevin Kloiber, Uyseok Lee, Chris Lim, Matthew Merrigan, Kevin Todd, and Justine Weng for excellent research assistance. Finally, we thank Melissa Taylor for valuable insight with regard to our research design. Chan gratefully acknowledges generous support from VA HSR&D IIR 18-146, the Laura and John Arnold Foundation, and the Stanford Institute for Economic Policy Research.

1 Introduction

A key question in the design of health care systems across the world is whether care should be provided by government or by the private sector. In the US, the choice between public and private provision has become a top policy issue for the Department of Veterans Affairs (VA). Seeking to improve veteran access to health care, policymakers have debated whether the VA should expand the capacity of its own system—the Veterans Health Administration—or shift health care delivery to private providers.

An extensive descriptive literature (e.g., Reid 2010; Blank et al. 2017) has compared health care outcomes in public vs. private systems. More generally, economists long have debated the appropriate size and role of the public sector in the economy, highlighting theoretical arguments about competitive pressure, ownership structure, and differences in the objectives and constraints in the public vs. private sector (Alchian 1965; Stigler 1965; Hart et al. 1997). Nevertheless, rigorous empirical evaluations of the performance of public vs. private health care providers have been relatively rare, in no small part because public and private providers of health care usually serve different patient populations, either by statute or by patient selection.

In this paper, we focus on "dually eligible" veterans aged 65 and older who can receive health care at both VA facilities and private hospitals that accept Medicare. We use the ambulance design proposed by Doyle et al. (2015) to study the causal effect of receiving emergency care at the VA vs. a non-VA facility. Our approach compares veterans sharing key characteristics—zip code of residence, prior VA and non-VA utilization, and location of pick-up (e.g., their home residence vs. a nursing home)—who receive the same dispatched level of ambulance service (i.e., advanced vs. basic life support) from different ambulance companies. Our main analytic sample includes 401,319 911-dispatched ambulance rides from 2001 to 2014, for veterans with prior attachment to the VA and in a zip code served by at least two ambulance companies. As in Doyle et al. (2015), we show that the leave-out share of dually eligible veterans transported to the VA by the assigned ambulance company is a strong predictor of hospital assignment. Under the plausible assumption that ambulances are quasi-randomly assigned within zip codes and in cells of key characteristics, this design allows us to study the effect of VA vs. non-VA emergency care on health outcomes.

We find that in the high-mortality population of elderly veterans with emergencies, there is a VA advantage—a 46% reduction in 28-mortality relative to baseline (4.5 p.p., with a 95% confidence interval of 1.1 to 8.0 p.p). Importantly, we show that our instrumental variables (IV) estimates of the

VA effect are robust to the inclusion of a long list of characteristics of the index patient and of other patients transported by the same ambulance company. The latter set of ambulance co-rider controls can account for unobserved patterns of selection across ambulance companies (Altonji and Mansfield 2018). The IV estimates are larger in magnitude than the corresponding OLS estimates, which center around 0.024 p.p., with tight confidence intervals. A possible explanation for this difference is that VA "always takers" (patients who go to the VA even with an ambulance company with a low VA rate) in fact have worse health than VA "never takers," a pattern suggested by the medical literature and one that we examine in greater detail below (Agha et al. 2000).

A critical question for interpreting the survival benefits of VA care is whether these effects fade over longer horizons—as would happen if VA emergency care only temporarily displaces the mortality of fragile patients under "harvesting" (Schwartz 2000). To address this, we use an insight from Abadie (2002) to estimate the weekly potential death rates in the year after the initial ambulance ride among compliers of the quasi-experiment, i.e., patients whose destination hospital is determined by the ambulance company. With this tool, we disentangle the short-term vs. long-term effects of the VA in the setting of competing risks. Despite a high long-term mortality rate (close to one in three veterans will be dead within one year of the ambulance ride), we find that the mortality impact of presenting at the VA is concentrated in the first week, suggesting VA survival gains from care addressing temporary emergency conditions. We find no evidence of harvesting, suggesting that the survival gains are long-lasting. Relying on intuition from Kitagawa (2015), we also use this potential outcomes framework to develop a sharper test of IV validity than the tests typical in the applied literature.[1] Finally, we use this framework to document systematically *larger* long-run mortality hazards for VA always takers than for VA never takers, based on the fact that differences between OLS and IV estimates of the VA advantage consistently widen with longer time horizons.

The key potential threat to our research design is that veterans who are taken to the VA are healthier than veterans who are taken to non-VA hospitals. While this runs counter to the selection that we demonstrate above, this could arise if the choice of a specific ambulance company from among those that serve a given zip code is correlated with the risk of death. We present four additional pieces of evidence to address this concern. First, we show balance in characteristics of patients assigned to

[1] Specifically, we use the fact that, under IV validity, all indicators for potential outcomes must occur with positive probability among compliers (Balke and Pearl 1997; Kitagawa 2015). In the setting of survival, this implies that the incremental mortality risk must be positive for compliers in every week after the ambulance ride and in both VA and non-VA assignment. This prediction may fail if there are violations of monotonicity that may arise, for example, because ambulance companies with higher VA propensities are *less* likely to send veterans with certain potential mortality outcomes to the VA. Chan et al. (2019) show that this approach may detect violations in IV validity that remain hidden under standard "judges design" tests of monotonicity (e.g., Arnold et al. 2018).

companies with different propensities of taking patients to the VA. Second, we conduct an extensive analysis along the lines suggested by Altonji et al. (2005), evaluating the stability of our estimates as we add controls to the models, including controls that measure the characteristics of *other* patients transported by the company. Third, in our IV analysis of longer-run survival, we show that aside from the first week, in which there is a large effect on mortality, compliers who go to the VA have mortality hazard rates that are statistically indistinguishable from rates of those who go to non-VA hospitals—ruling out significant differences in underlying health between the two groups. Finally, in heterogeneity analyses, we show that the VA advantage is highly stable across VA and non-VA hospital characteristics that may be related to selection of high-risk patients.

In the final section of the paper we then turn to an evaluation of the mechanisms behind the VA advantage. We consider three broad classes of mechanisms. First, the VA might be better suited to treating conditions specific to veterans. Second, along the lines of Doyle et al. (2015), VA hospitals could achieve better outcomes by spending more. Third, better access to patient information and coordination of care may improve the productivity of VA-delivered care, particularly in high-uncertainty and high-stakes environments such as emergency care. This last mechanism is perhaps most consistent with a descriptive literature that highlights integration of care and health information technology (IT) as distinguishing features of the VA relative to the private sector since reforms in the mid-1990s (Mccarthy and Blumenthal 2006; Jha et al. 2009).

To evaluate the first explanation—that the VA system is uniquely suited to care for veterans—we note that, although we do not observe non-veterans being treated in the VA, we observe a detailed set of veteran and neighborhood characteristics that predict whether a veteran is more likely to be attached to the VA. We find a VA advantage for patients with very different patterns of prior utilization and comorbidities, although medically needier patients (e.g., those with substance abuse and mental health problems) and those with greater attachment to the VA tend to benefit more. We evaluate the second explanation by examining VA and Medicare spending, using information on actual spending by taxpayers and veterans. Spending following VA care is *lower* by $\$2,548$, or about 21%, at 28 days. This suggests that the VA is more productive, achieving better outcomes at lower cost.

The third explanation centers on the idea that coordination and continuity of care in an integrated delivery system may improve health outcomes—an explanation consistent with the larger impacts of the VA on medically needy patients and those with greater prior attachment. To provide further evidence on this, we draw on a secondary sample of veterans who have prior attachment to a specific *non-VA* hospital but no recent use of the VA. As noted, these veterans have little chance of receiving

emergency care at a VA hospital but may return to the hospital they visited most in the prior year, where their records may be more easily accessed, or to another facility. Ambulance-based assignment to a patient's most-visited prior hospital (i.e., their "modal" hospital) indeed reduces 28-day mortality, though only modestly (by about 0.6 p.p.). We infer that the VA survival benefit arises from more than just repeated use of the same facility; instead, it may reflect better care coordination and/or more effective information retrieval in the VA health care system.

To probe these channels, we exploit two policy reforms that aimed to improve care coordination and information technology among US hospitals. In 2009 the Health Information Technology for Economic and Clinical Health (HITECH) Act stimulated a large increase in health IT adoption among non-VA hospitals, and in 2011 Medicare began experimenting with alternative payments to "Accountable Care Organizations" (or ACOs) (Blumenthal 2010; Greaney 2011). Consistent with these changes, we find that the modal-hospital survival benefit increases from a negligible effect prior to 2010 to about 1.9 p.p.—approximately one-half of the VA survival benefit—after 2010. We also find tentative evidence linking the increase in the modal-hospital survival benefit to hospital-specific health IT adoption and, to a lesser degree, ACO participation.

Our findings contribute to three sets of related literature. First, the public vs. private provision of health care is a central question for the field of comparative health policy, which compares health care systems across the world to inform the design of health care systems (Blank et al. 2017). The literature in this field has been mainly descriptive.[2] Comparing the performance of health care systems across countries is intrinsically difficult for obvious reasons: Countries differ in their populations and in other determinants of health. To our knowledge, the only quasi-experimental exception in this literature is a recent working paper by Frakes et al. (2020). Studying military mothers who give birth in two different hospitals due to a move in between births, they find higher spending but lower rates of complications in private hospitals.

Second, an important literature has sought to measure the quality of care in the VA, which budgeted $84 billion for medical care in 2020 (Department of Veterans Affairs 2020).[3] Following a well-known reorganization and investment in health IT in the mid-1990s (Mccarthy and Blumenthal 2006), this literature has documented favorable VA quality, compared to care outside of the VA, in

[2] As an example of the amount of material devoted to such comparative studies, the European Observatory on Health Systems and Policies (`www.euro.who.int`) produces policy commentary and "health system reviews" on the health care systems of individual countries.

[3] Spending continues to grow. The 2019 enacted budget allocated $77 billion for VA medical care, and the 2021 proposed budget requests $94 billion for medical care. For the last ten years, spending on medical care has nearly doubled (Department of Veterans Affairs 2020).

terms of process measures and health outcomes (e.g., Jha et al. 2003). The question of performance in the VA health care system has become particularly relevant in recent years, as the Department considers ways to improve access to care for veterans and as Congress has sought to increase private health care delivery for veterans (113th Congress 2014; 115th Congress 2018). So far, however, this literature has mainly compared outcomes of veterans receiving care in the VA system to those of non-veterans outside of the VA.

A third and very large literature studies why health care in the US appears to be a low-productivity outlier among developed countries, spending more as a percentage of GDP than any country but with poor outcomes relative to this spending (Garber and Skinner 2008). Experts have drawn attention to fragmentation in the US health care system, which could increase spending and potentially worsen outcomes (Cebul et al. 2008; Cutler 2010; Agha et al. 2019). Policymakers have responded by incentivizing adoption of health IT and coordination of care, but whether such policies improve health outcomes remains an open empirical question.[4] Our results are consistent with a productivity advantage (better outcomes at lower cost) at the VA, the nation's largest integrated health care system. We also find suggestive evidence that government regulations to incentivize private hospitals to adopt health IT and integrate care may achieve better outcomes, at least in our setting for patients with continuity of care at these hospitals.

The remainder of this paper proceeds as follows. Section 2 describes the setting and data. Section 3 presents our main analysis of the VA survival benefit. Section 4 discusses our survival analysis over time. Section 5 presents evidence on mechanisms driving the VA survival benefit. Section 6 discusses policy implications and concludes.

2 Setting and Data

2.1 The Veterans Health Administration and US Health Care

The Veterans Health Administration (VHA) of the US Department of Veterans Affairs (VA) provides health care for 9 million veteran enrollees every year. The VHA is the nation's largest integrated health care delivery system, including 170 medical centers and more than 1,000 outpatient sites of

[4] A recent empirical literature documents modest reductions in spending and improvements in patient satisfaction among provider forming ACOs (McWilliams et al. 2014, 2016; Trombley et al. 2019). An older literature on health maintenance organizations (HMOs) documents impacts on spending and technology adoption in the 1990s (Baker 2001; Cutler et al. 2000), although policy analysts have noted that HMOs were primarily insurance products, focused more on limiting utilization than on improving quality (Luft 2010). Finally, a mixed literature on health IT adoption has shown health improvements in some cases (e.g., Miller and Tucker 2011) but null results in general (e.g., Agha 2014). To our knowledge, our paper is the first to assess the complementarity between health IT and continuity of care.

care, with a budget of $84 billion in 2020 for medical care (Department of Veterans Affairs 2020).

Two features of the VHA distinguish it from the rest of the US health care delivery system. First, the VHA is owned and administered by the government, while the rest of US health care system is largely run by private parties. Second, health care is integrated at the VHA. The VHA directly employs all of its physicians and health care workers. In contrast, physicians outside of the VA are mostly independent of the hospitals at which they work and can affiliate at will with multiple hospitals. Health care in the VHA is organized by region and coordinated across inpatient, emergency department, and outpatient locations, as well as across different services and specialties of care. In the mid-1990s, the VHA implemented one of the first and most widely used electronic health record (EHR) systems in the US. VHA spending on health information technology (IT) infrastructure, currently at $5.7 billion yearly, has grown dramatically; the VHA also spends around $800 million yearly on research and development in disease-specific areas (e.g., substance abuse, chronic disease), coordination of care, quality measurement and improvement, access to care, and veteran-specific concerns (e.g., suicide, homelessness) (Department of Veterans Affairs 2020).

Outside of the VA, the US health care system is marked by a high level of complexity involving multiple private and public (federal, state, and local) parties. The US spends more on health care per capita than any other country—50% greater than the second-highest country, Norway—but has lower life expectancy than most other high-income countries (Rice et al. 2013). Compared to other high-income countries, the private sector plays a greater role in the US health care system, and health care financing in the US is largely fee-for-service. Despite a large and well-trained workforce, as well as advanced institutions providing secondary and tertiary care, experts have noted poor coordination of care and strikingly low adoption of health IT (Cebul et al. 2008; Cutler 2010). Prior to the Affordable Care Act (ACA), only 1.5% of US hospitals maintained a comprehensive EHR system (Jha et al. 2009). In the wake of the ACA, federal policies have attempted to spur care coordination and health IT adoption in the private sector (Blumenthal 2010; Greaney 2011).

2.2 Comparing VA and Non-VA Care

Over the past decade, lawmakers have enacted major reforms that allow veterans to receive VA-funded care at private facilities (113th Congress 2014; 115th Congress 2018).[5] These reforms broaden the

[5]There have been additional well-funded efforts to shift care further into the private sector (Rein et al. 2018; Kefe 2018; Shulkin 2018; Gordon 2019). According to an official recommendation to the congressionally established Commission on Care, some have even proposed that "if veteran choice dictates it over time, the long term goal of the transformation is the total transition to community care" (Blom 2016).

role of the VA to that of an *insurer* of care for veterans (similar to the role of Medicare for the elderly), with concomitant functions such as authorizing care, processing claims, and *ex post* monitoring of claims for waste and fraud.

Related to these initiatives, the quality of VA vs. non-VA care has been a longstanding subject of interest to policymakers and researchers. The health services literature has documented that the VA provides care that is of the same or higher quality than that of the private sector, as measured by a wide variety of process measures and health outcomes.[6] However, these comparisons are potentially confounded by differences, due to eligibility and self-selection, between the populations that utilize care in the VA and in non-VA facilities. Indeed, the vast majority of existing research has compared the care of veterans in the VA with the care of non-veterans in non-VA facilities.[7]

We use two key ideas to extend the literature on comparisons between VA and non-VA care. First, we focus on dually eligible veterans who are aged 65 and older. These veterans may receive care in the VHA and at non-VA hospitals using Medicare (Hynes et al. 2007). Second, we build on the ambulance design strategy of Doyle et al. (2015) to sidestep concerns about the endogenous selection of where to obtain care. Specifically, we study veterans who arrive at a hospital via a 911-dispatched ambulance, comparing veterans from the *same zip code* who could have obtained services from different ambulance companies with different propensities to transport patients to a VA hospital. Importantly, Doyle et al. (2015) document that the company dispatched to serve a given patient may be chosen independently of the patient's characteristics, due to rotational assignment, direct competition between available providers, or software that may consider the placement of available ambulance units at the time of the 911 call (Chiang et al. 2006; Ragone 2012). Ambulance companies also exhibit different tendencies to transport patients to different hospitals, based on their ownership, headquarter location, and other characteristics (Skura 2001). We further describe our quasi-experimental design and assess its assumptions in Section 3.

2.3 Data

We use data from two main sources—Medicare claims and VHA administrative data—for the universe of enrolled veterans in the VHA from the years 2000 to 2014. We observe all Medicare claims

[6] See Shekelle et al. (2010), Trivedi et al. (2011), and O'Hanlon et al. (2017) for systematic reviews. The literature includes dozens of studies on hundreds of quality of care process measures, as well as several studies on health outcomes.

[7] Two studies are noteworthy for having better identification. Nuti et al. (2016) compare outcomes for veterans in VA hospitals with outcomes for non-veterans in non-VA hospitals but restrict comparisons between VA and non-VA hospitals in the same metropolitan statistical areas. In an older study, Wright et al. (1999) look at 47,598 dually eligible veterans with a myocardial infarction. These studies find no difference or slightly better mortality outcomes in VA hospitals. Of note, a related literature suggests that veterans generally have poorer health than non-veterans (e.g., Agha et al. 2000).

for any dually enrolled veteran. These claims data include the beneficiary's zip code and demographic information (age, race, and gender), as well as a record of medical services, each defined by an encounter date, Current Procedural Terminology (CPT) code(s), diagnostic (International Classification of Diseases, Ninth Revision, or ICD-9) codes, and provider identity. On the VHA side, we have a complete record of clinical encounters in the electronic health record system that we transform into a corresponding set of encounter dates, CPT codes, ICD-9 codes, and provider identities.[8]

We begin by selecting ambulance ride events for dually eligible veterans, as recorded in the Medicare claims.[9] We restrict attention to "lights and sirens" emergency ambulance rides that originate from 911 dispatch calls.[10] As in Doyle et al. (2015), we extract the date of the ambulance ride and the identity of the ambulance company, based on its tax identification number (TIN). We use the ambulance company identity to develop our instrumental variable for the propensity of the ambulance company to deliver patients to the VA or to non-VA hospitals. We also extract information on interventions provided by the ambulance (e.g., intravenous fluids, intubation), the level of care (advanced life support or basic life support), the pick-up location (e.g., private residence, nursing home, skilled nursing facility, accident), and the ambulance diagnosis (ICD-9) codes assigned by the ambulance personnel.

We then link these ambulance rides to emergency department (ED) visits at VA and non-VA hospitals. This constitutes our main treatment of interest. For each patient we collect information on medical conditions and outpatient, ED, and inpatient utilization over the prior year, as recorded in the Medicare claims and VHA records. We use the ICD-codes for past medical conditions to identify 31 Elixhauser indices (Elixhauser et al. 1998) of comorbidities, noting the source of each condition (i.e., from visits to the VA, to non-VA facilities, or both). These comorbidities range from common conditions such as hypertension to rarer conditions such as lymphoma.

Our main outcome measure is mortality. We obtain information on exact date of death from three sources: records of inpatient deaths from the VA and Medicare claims; records of death from the Veterans Benefits Administration (VBA), and records of death from the Social Security Administration (SSA). The latter two sources are particularly reliable, as they determine whether the veteran will re-

[8]The VHA system includes patient home address information. However, we use the zip code information from the Medicare claims records as our source of home location, since this information is updated frequently and has been widely used in previous studies, including Doyle et al. (2015).

[9]VHA policy is that patients with outside insurance should have ambulance services paid for by that insurance. In our dually eligible population, therefore, ambulance rides will be recorded in the Medicare claims.

[10]We select ambulance rides with Healthcare Common Procedure Coding System (HCPCS) codes A0322, A0328, A0330, A0362, A0368, A0370, A0427, A0429, A0433, or Q3019. We restrict to modifiers "RH", "SH", "NH", and "EH", corresponding to rides to a hospital from a residential location, a scene of an accident or acute event, a skilled nursing facility, and an extended care facility, respectively.

ceive payments from either the VBA or the SSA, and draw on reports from family, funeral directors, post offices, financial institutions, other federal agencies, and state vital records agencies.

To construct our main analytical sample of 401,319 ambulance rides, we make the following restrictions (see Appendix Table A.1). First, we remove patients who live in zip codes more than 20 miles away from either the nearest VA hospital or the nearest non-VA hospital. We also drop patients who traveled more than 50 miles from their zip code to the hospital. Second, we require that patients live in zip codes served by at least two ambulance companies with at least 20 rides, at least 5% of rides transported to a VA hospital, and at least 5% transported to a non-VA hospital. Finally, for our baseline analysis of VA vs. non-VA care, we drop veterans with no VA primary, ED, or inpatient care in the prior year, since fewer than 1% of these veterans are ever transported to the VA. In our secondary analysis of veterans who may be transported to modal or non-modal hospitals outside of the VA, in Section 5, we study an analogous sample of 1,414,217 ambulance rides of veterans who did not use VA care in the previous year and who live in zip codes with at least two non-VA hospitals within 20 miles. Appendix Table A.14 describes the selection process for this sample.

Table 1 describes the characteristics of the veterans and the episode associated with their ambulance ride at different steps in the creation of the main analytical sample. The average 28-day mortality rate is stable across steps and relatively high, between 9.7 and 11.5 p.p., reflecting the illness acuity of elderly veterans who arrive by 911-dispatched ambulance. Similarly, the weekend rate, which is the proportion of ambulance rides arriving on a weekend day, is remarkably stable and close to two-sevenths, which reflects the unplanned nature of these health events (Card et al. 2009). The main impact of our sample restrictions is to increase the share of rides going to a VA hospital. In some of the steps, such as the step imposing zip-code distance restrictions, the sample becomes more concentrated in urban areas with shorter distances to nearby VA and non-VA hospitals. Black veterans also comprise a larger share of the sample. Patient characteristics otherwise remain stable across sample restriction steps.

3 Benchmark Analysis

3.1 Quasi-Experiment

Following Doyle et al. (2015), our main empirical strategy relies on the assignment of ambulances to patients in emergencies and the role of ambulance companies in determining the hospital that a patient is taken to for emergency care. As detailed in Doyle et al. (2015), several companies typically

serve the same narrow geographic area. The assignment of a particular company may be quasi-experimentally determined such that the identity of the assigned ambulance company is plausibly unrelated to patient characteristics. Furthermore, ambulance companies exhibit "preferences" for delivering patients to certain hospitals, due to their ownership or the location of their operations.

We define conditioning sets within which ambulance assignment may be as good as random. First, we condition on the origin zip code $z(i)$ of ambulance ride i, so that we compare patients from the same zip code who are picked up by different ambulance companies. Second, we categorize the ambulance by whether it offers advanced life support (ALS) or basic life support (BLS) based on ambulance Healthcare Common Procedure Coding System (HCPCS) codes. We further categorize rides by the pickup site category (e.g., residential address, nursing home, scene of an accident), the day of the week, and month-year interactions (e.g., January 2010). Finally we condition on measures of the patient's primary care, ED, and inpatient utilization over the past year at VA and non-VA facilities.[11] For simplicity we refer to the joint set of controls for the type of ambulance, pickup site, date of pickup, and patient prior utilization as $\mathbf{X}_i^0$.

Unlike Doyle et al. (2015), we do not include patient demographics, prior medical conditions, or ambulance diagnoses in the set of baseline controls. Instead, we "hold out" these variables—many of which are highly predictive of mortality—and show that they are balanced across local ambulance companies with differing propensities to send patients to the VA, conditional on $\left(z(i), \mathbf{X}_i^0\right)$.

Our treatment of interest is delivery to a VA hospital, which we denote by the indicator $D_i \in \{0,1\}$ for ambulance ride i. Transfers are rare in our sample.[12] Ride i is provided by company $j(i) \in \mathcal{J}_{z(i)}$,where $\mathcal{J}_z$ is the set of companies that serve zip code z.[13] Associated with each ride and company is a potential treatment indicator $D_i(j)$; thus $D_i = D_i(j(i))$. Our main outcome is 28-day mortality of the patient, denoted by $Y_i \in \{0,1\}$. The associated potential outcomes, $Y_i(d)$, depend on whether the patient was transported to a VA hospital ($d = 1$) or not ($d = 0$), with $Y_i = Y_i(D_i)$.

Under the assumptions that different ambulance companies have systematically different tendencies to transport patients to the VA, and that the assignment of $j(i)$ is as good as random, conditional on $\left(z(i), \mathbf{X}_i^0\right)$, the identity of the ambulance company can be used to construct a a valid instrumental

[11] The latter set of prior utilization measures may capture ambulance service areas within large zip codes, which may in turn account for correlations between prior use of VA vs. non-VA care and the identity of ambulance companies.

[12] Of the 132,535 rides that go to the VA, 828 (0.6%) have a non-VA hospital ED visit on the subsequent day. Of the 268,784 rides to a non-VA hospital, 2,191 (0.8%) have an ED visit at a VA hospital the next day. Of 79,684 VA admissions, 418 (0.5%) were transferred to a non-VA hospital within 7 days of the ambulance ride. Of 157,682 non-VA admissions, 1,774 (1.1%) were transferred to a VA facility within 7 days.

[13] We define an "ambulance company" as the interaction between an ambulance company tax identification number (TIN) and the health referral region (HRR) of the ride. This accounts for a few large corporations with a single TIN that serve multiple regions.

variable for D_i. More formally, we consider the following conditions for IV validity (Imbens and Angrist 1994):

Condition 1 (IV Validity). *For a random sample of ambulance rides i provided by ambulance companies j, the following conditions hold:*

(i) Relevance: $E\left[D_i(j)\middle|z(i),\mathbf{X}_i^0\right]$ *is a nontrivial function of* $j \in \mathcal{J}_{z(i)}$.

(ii) Independence and Exclusion: *The vector of potential outcomes,* $(Y_i(0), Y_i(1), D_i(j))$*, is independent of the assigned ambulance company,* $j(i) \in \mathcal{J}_{z(i)}$*, conditional on* $\left(z(i),\mathbf{X}_i^0\right)$.

(iii) Monotonicity: *Conditional on* $\left(z(i),\mathbf{X}_i^0\right)$*, for any* j *and* j'*,* $D_i(j) \geq D_i(j')$ *for all i, or* $D_i(j) \leq D_i(j')$ *for all i.*

Because only veterans are typically treated in VA hospitals, the compliers in this quasi-experiment (i.e., the set of rides i such that $D_i(j) > D_i(j')$ for some $j, j' \in \mathcal{J}_{z(i)}$) will involve patients who can state to an ambulance that they are a veteran and who would be open to care at the VA. We estimate and report complier characteristics in Section 5.1.

As is standard in the judges-design literature (e.g., Kling 2006, Dahl et al. 2014), to deal with finite samples, we construct a leave-out (or jackknife) instrumental variable that reflects the propensity of the ambulance company $j(i)$ assigned to ride i to transport *other* patients to the VA. We compute this as the average fraction of other patient who were picked up by company $j(i)$ and went to the VA. Specifically, for ambulance ride i transporting patient $k(i)$ we define the leave-out probability Z_i of transport to the VA:

$$Z_i = \frac{1}{K_{j(i)} - 1} \sum_{i' \in \mathcal{I}_{j(i)}} \frac{\mathbf{1}\left(k(i') \neq k(i)\right) D_{i'}}{N_{k(i'),j(i)}}, \tag{1}$$

where K_j is the total number of patients transported by company j , $N_{k,j}$ is the total number of rides taken by patient k with company j, and $\mathcal{I}_j$ is the set of rides transported by ambulance company j. We estimate Z_i using the sample of dually eligible veteran ambulance rides (Column 1 of Table 1).

Under Condition 1, an IV estimate based on Z_i, conditioning on $\left(z(i),\mathbf{X}_i^0\right)$, recovers a local average treatment effect (LATE) of the VA on mortality among compliers. For comparison, we also consider the observational "treatment effect" of going to the VA on mortality of patients who arrive at hospital in a 911-dispatched ambulance, controlling for $\left(z(i),\mathbf{X}_i^0\right)$:

$$Y_i = \beta D_i + \mathbf{X}_i^0 \delta_0 + \zeta_{0,z(i)} + \varepsilon_{0,i} \tag{2}$$

where $\zeta_{0,z}$ represents an unrestricted fixed effect for rides originating in zip code z. Estimating Equation (2) by OLS yields $\hat{\beta}_{OLS}$, while instrumenting D_i with Z_i yields $\hat{\beta}_{IV}$. The gap between $\hat{\beta}_{OLS}$ and $\hat{\beta}_{IV}$ will depend on differences in the potential outcomes between never takers (i.e., patients who go to a non-VA facility regardless of the ambulance company) and always takers (i.e., patients who go to the VA regardless of the ambulance company), as well as on differences in treatment effects between compliers and non-compliers. We explore this gap more directly in Section 4.

3.2 First Stage, Balance, and Reduced Form

We begin our empirical analysis by demonstrating instrument relevance, Condition 1(i), with the following first-stage regression:

$$D_i = \pi_1 Z_i + \mathbf{X}_i^0 \delta_1 + \zeta_{1,z(i)} + \varepsilon_{1,i}. \tag{3}$$

The coefficient π_1 reflects the impact of ambulance company preferences on the probability that the ride goes to the VA, conditional on our baseline controls for ambulance type, pickup site, zip code, date categories, and veteran prior utilization. Figure 1, Panel A, shows a binned scatterplot of residualized D_i on the y-axis with respect to residualized Z_i on the x-axis and reports $\hat{\pi}_1 = 0.882$ (s.e. 0.034). The first-stage relationship between D_i and Z_i is very predictive and close to linear.

To assess independence, Condition 1(ii), we test whether Z_i is correlated with patient characteristics that are correlated with mortality. Specifically, we construct an estimate of predicted mortality $\hat{Y}_i$ using "hold-out" patient characteristics of patient demographics and 31 Elixhauser indices for prior medical conditions.[14] We then fit models for $\hat{Y}_i$ based on the same right-hand-side specification as in Equation (3). Panel B of Figure 1 shows (with hollow dots) that there is no relationship between $\hat{Y}_i$ and Z_i, controlling for $\left(z(i), \mathbf{X}_i^0\right)$. In contrast, the same panel shows (with solid dots) that the reduced-form relationship between actual mortality, Y_i, and Z_i is significantly negative, under the same controls. Specifically, for the reduced-form relationship,

$$Y_i = \pi_2 Z_i + \mathbf{X}_i^0 \delta_2 + \zeta_{2,z(i)} + \varepsilon_{2,i}, \tag{4}$$

[14] Patient demographics include age, gender, and race and ethnicity. Age is captured by two-year age bins from 65 years to 100 years. Race and ethnicity is captured with three dummies for white, Black, and Hispanic; the omitted category is Asian/other. We use the 31 Elixhauser indices as described in Elixhauser et al. (1998), interacting each index with the source of the record indicating the comorbidity. There are three possible sources: VA only, Medicare claims only, and VA and Medicare claims. This results in $3 \times 31 = 93$ dummies. These hold-out patient characteristics are described in Appendix Table A.3.

we find $\hat{\pi}_2 = -0.040$ (s.e. 0.016). This suggests that quasi-random assignment to an ambulance company more likely to transport to the VA results in an intention-to-treat reduction in mortality.

The exclusion condition in Condition 1(ii) asserts that ambulance companies do not affect outcomes other than through their effect on whether a patient arrives at a VA or non-VA hospital. Implicitly, however, our notation also assumes that each complier has a well-defined non-VA hospital that is stable across ambulance companies. In Appendix A.1.1, we evaluate the robustness of our results to potential violations of the exclusion condition. Specifically, we assess and find no evidence of any correlation between Z_i and ambulance treatments captured in summary charges or between Z_i and ambulance propensities to deliver patients to different non-VA hospitals.[15]

To assess the monotonicity assumption given by Condition 1(iii), we follow the standard practice in the judges-design literature to show that the first-stage relationship between D_i and Z_i remains positive for subgroups of patients defined by different observable characteristics (e.g., Arnold et al. 2018; Bhuller et al. 2020). We detail these analyses in Appendix A.1.2. In Section 4, we present a stronger test of monotonicity (and IV validity) based on *potential outcomes*. Following the reasoning in Kitagawa (2015), this test amounts to showing a positive density for the potential outcome of death in a given week among compliers.

3.3 Mortality Effect

With this background, we now move to our main results on patient mortality. In Table 2, we show both OLS and IV estimation results for Equation (2). Panel A of the table shows $\hat{\beta}_{OLS}$ from Equation (2), while Panel B shows $\hat{\beta}_{IV} = \hat{\pi}_2/\hat{\pi}_1$ from the first-stage and reduced-form regressions in Equations (3) and (4). Column 1 shows our baseline specification, controlling for zip code and the variables in $\mathbf{X}_i^0$. The OLS estimate is $\hat{\beta}_{OLS} = -0.024$ (s.e. 0.001), while the IV estimate is $\hat{\beta}_{IV} = -0.045$ (s.e. 0.018).[16] Relative to the mean 28-day mortality of 9.7 p.p., both estimates imply a sizeable reduction in mortality for compliers who are taken to the VA.

The other columns in Table 2 show OLS and IV estimates as we include additional controls to the models: (i) patient demographics (age, race, gender), (ii) ambulance diagnostic (ICD-9) codes, (iii)

[15] Following Kolesar et al. (2015), these analyses correspond to the weaker assumption that there are no systematic correlations between our instrument and other ambulance-specific treatments that impact our outcome of interest. Specifically, under this weaker version of exclusion, we require that ambulance companies with higher values of $E\left[D_i(j)\middle|z(i), \mathbf{X}_i^0\right]$ do not also systematically apply treatments during the ambulance ride that affect mortality, or systematically deliver patients to higher- or lower-quality non-VA alternatives.

[16] Appendix Figure A.1 shows the IV estimate visually, by plotting the predicted first-stage probability of treatment from Equation (3) on the x-axis and predicted reduced-form effect on mortality from Equation (4) on the y-axis. The slope of this visual IV relationship corresponds to $\hat{\beta}_{IV} = -0.045$.

Elixhauser comorbidity indicators, and (iv) ambulance and co-rider controls, which are all described in Appendix Table A.3. The latter controls are meant to capture any unobservable patient selection at the ambulance company level by using characteristics of *other* rides and patients under the same ambulance company, following the reasoning in Altonji and Mansfield (2018). Specifically, these controls address the potential concern that sicker patients tend to be allocated to certain ambulance companies that may be more or less likely to take patients to the VA.

Reassuringly, both $\hat{\beta}_{OLS}$ and $\hat{\beta}_{IV}$ remain stable as we add additional controls. Figure 2 illustrates this stability as controls are added in a more granular fashion, and Appendix Figure A.2 shows stability of the IV estimates as we permute the order in which the extra controls are added. The stability of both the OLS and IV estimates suggests lack of selection on observable characteristics; under the reasoning of Altonji et al. (2005), this stability suggests limited scope for selection on unobservable characteristics that predict potential 28-day mortality. However, IV estimates are larger than OLS estimates, suggesting either that never takers are healthier than always takers (i.e., selection runs counter to treatment effects on mortality) or that the LATE is larger than the unconditional average treatment effect (ATE).[17] We investigate these possibilities in the next section and in Section A.4.

4 Survival Analysis

In this section we develop and apply a survival analysis framework to understand the dynamics of potential survival outcomes following the ambulance ride. We use this framework to draw several insights. First, we determine the time course of VA effects on mortality. Second, we use the empirical results of this framework to provide further validation of Condition 1, beyond the standard benchmark analysis in Section 3.2. Third, we investigate the implications of heterogeneity in mortality risks between compliers and non-compliers of our ambulance quasi-experiment.

4.1 Approach

Consider a set of potential survival outcomes $S_i(t;d) \in \{0,1\}$ under VA care ($d = 1$) and non-VA care ($d = 0$) for each week $t \in \{1,\ldots,52\}$ following the ambulance ride.[18] By definition, if $S_i(t;d) <$

[17] We note that a Hausman test for equality of the two estimates has a t-statistic of only 1.0, so based on this evidence alone, the gap between OLS and IV could be simply due to sampling error. In the next section, however, we show a dynamic pattern of IV and OLS estimates, over the year after the initial ambulance ride, that points more definitively to systematic differences. That is, using additional data over time, we infer with high confidence that the causal VA advantage is larger than the (precisely estimated) OLS effect.

[18] We adopt the convention that a death within the first 7 days is a death in week 1. Thus a death within 28 days is a death by the end of week 4.

$S_i(t-1;d)$, then the patient in ambulance ride i would die in the t^{th} week following the ambulance ride if exposed to treatment d. Of course, potential survival outcomes must weakly decrease over time, i.e., $S_i(t;d) \leq S_i(t-1;d)$ for all i, d, and t.

As with mortality outcomes, for each ambulance ride i, we can only observe the set of survival outcomes corresponding to $d = D_i$: $S_i(t) = D_i S_i(t;1) + (1-D_i) S_i(t;0)$. However, appealing to Abadie (2002), we can recover the expected survival outcomes for the set of compliers C whose hospital choice depends on which ambulance company picks them up. In particular, under Condition 1, we can estimate $s_{IV}(t;1) \equiv E[S_i(t;1) | i \in C]$ by two-stage least squares using the first-stage Equation (3) and a reduced-form equation similar to Equation (4) but with dependent variable $S_i(t) D_i$. Similarly, we can estimate $s_{IV}(t;0) \equiv E[S_i(t;0) | i \in C]$ using the same first stage model but replacing the reduced-form outcome variable in Equation (4) with $S_i(t)(D_i - 1)$. Note that by construction, the IV estimand of the VA treatment effect on 28-day mortality in Section 3, satisfies

$$\beta_{IV} = s_{IV}(4;1) - s_{IV}(4;0).$$

Given the potential survival outcomes, we can then estimate potential hazard rates for mortality, under VA and non-VA assignment:

$$\begin{aligned} h_{IV}(t;d) &\equiv E[1 - S_i(t+1;d) | S_i(t;d) = 1, i \in C] \\ &= \frac{s_{IV}(t;d) - s_{IV}(t+1;d)}{s_{IV}(t;d)}, \end{aligned} \tag{5}$$

for $d \in \{0,1\}$ and $t \in \{1,\ldots,52\}$, corresponding to weekly mortality hazard rates up to one year after the initial ambulance ride. Under Condition 1, differences between $\{h_{IV}(t;1)\}_t$ and $\{h_{IV}(t;0)\}_t$ can be interpreted as the causal effect of VA assignment, among compliers, on the set of mortality hazard rates.[19]

As in Section 3, we calculate risk-adjusted OLS survival functions and mortality hazard rates, conditional on D_i. We estimate $s_{OLS}(t;d) \equiv E[S_i(t;d) | D_i = d] = E[S_i(t) | D_i = d]$ by OLS, replacing the outcome variable in Equation (2) with $S_i(t) D_i$ for $s_{OLS}(t;1)$ and with $S_i(t)(D_i - 1)$ for $s_{OLS}(t;0)$. Our OLS estimand of the VA effect on 28-mortality, β_{OLS}, is similarly equal to $s_{OLS}(4;1) - s_{OLS}(4;0)$. Corresponding mortality hazard rates can also be calculated based on ob-

[19] We emphasize that any gap between $h_{IV}(t,1)$ and $h_{IV}(t,0)$ at some later time horizon (e.g., $t = 12$) could arise because treatments at the VA affected the population of compliers who survive to week $t-1$ and are therefore at risk of death in week t, or because of a treatment effect on the week t hazard, holding the population fixed.

served risk-adjusted survival:

$$\begin{aligned} h_{OLS}(t;d) &\equiv E\left[1 - S_i(t+1;d) \,|\, S_i(t;d), D_i = d\right] \\ &= \frac{s_{OLS}(t;d) - s_{OLS}(t+1;d)}{s_{OLS}(t;d)}. \end{aligned} \tag{6}$$

Compared to the potential survival functions and mortality hazards, the OLS analogues also incorporate outcomes for the always takers and never takers whose choice of hospital is unaffected by the specific ambulance company that picked them up. Specifically, $s_{OLS}(t;1)$ and $h_{OLS}(t;1)$ reflect survival outcomes for a combination of always takers and compliers, while $s_{OLS}(t;0)$ and $h_{OLS}(t;0)$ reflect survival outcomes for a combination of never takers and compliers.

4.2 Time Course of Mortality Effects

Since we examine potential survival outcomes one year after an ambulance ride, for the analysis in this section we restrict analysis to ambulance rides of patients with no prior ride within one year.[20] Figure 3 shows the estimated potential survival curves and potential hazard rates in weeks 0 to 52 for compliers assigned to the VA and those assigned to a non-VA hospital. The potential survival curves, shown in Panel A, reveal a high risk of mortality among compliers. Mortality at 28 days among compliers assigned to a non-VA hospital is greater than the sample mean of 9.7 p.p., and cumulative mortality at one year is approximately 30 p.p. However, despite the substantial mortality risk over the subsequent year, the gap in survival between VA- and non-VA-assigned compliers (i.e., the mortality treatment effect) is fully realized at 28 days and remains stable for the rest of the year.

In Panel B, we examine the implied hazard rates and show that the differences in mortality are concentrated in the first week following the ambulance ride. Thereafter, though the hazard rates for both VA- and non-VA-assigned compliers remain relatively high, they are indistinguishable from each other. This similarity suggests that the VA advantage results entirely from events within the first week following the ambulance ride.

The potential hazard profiles in Figure 3 suggest that mortality risks for the compliers in our

[20] This restriction attributes survival for a given patient in a given week to the "upstream" ambulance ride, rather than attributing the survival event to both upstream and downstream ambulance rides. This changes (decreases) the sample in Appendix Table A.1 to 254,782 rides and 188,299 patients. In Appendix Figure A.9, we show that this restriction (or any other restriction on prior rides) does not lead to qualitative differences in our estimated OLS or IV treatment effects on mortality over time. Qualitatively, regardless of the number of days within which we require no prior ride, the IV estimates are larger than 4 p.p. at 28 days and remain mostly stable within the year following the ambulance ride; the OLS estimates are between 2.0 and 2.5 p.p. at 28 days and essentially disappear by one year following the ambulance ride. We evaluate the implications of the long-term difference between IV and OLS treatment effects in Section 4.4.

analysis comprise two separate risks: (i) a relatively high short-term risk component that is affected by VA vs. non-VA assignment, and (ii) a relatively stable long-term risk component that is the same between compliers who go to the VA and those who go to a non-VA hospital. If the latter risk reflects underlying patient health and is independent of the risk that led to the ambulance call, then we would expect the long-run weekly mortality rate (after, e.g., three months) to be the same for veterans who were quasi-randomly assigned to VA and non-VA hospitals; we formalize this as a test in Section 4.3.

The potential hazard rates also allow us to assess whether excess mortality at non-VA hospitals involves "harvesting," or mortality displacement, in which deaths for patients at the VA are simply delayed (Schwartz 2000; Honore and Lleras-Muney 2006). Under this hypothesis, survival gains from VA care observed at 28 days are temporary and will fade in the long-term. Such mortality displacement would imply that the hazard of dying *increases* among VA-assigned compliers after a time. We find no evidence of this in the potential hazard rates in Panel B of Figure 3. In Appendix A.2, we formally test that $h_{IV}(t;1) \leq h_{IV}(t;0)$ for all t and cannot reject this null hypothesis of no harvesting.[21] This suggests that the VA *prevents* rather than *displaces* deaths, leading to a persistent survival benefit, as shown by the stable gap between potential survival curves in Panel A.

4.3 Extended IV Validity

We can also use the estimated potential survival outcomes to test the validity of our IV strategy based on ambulance assignment. Under Condition 1, the density of any characteristic, including characteristics defined by potential outcomes, must be positive among compliers of the quasi-experiment (Balke and Pearl 1997; Imbens and Rubin 1997):

$$\Pr(X_i = x, Y_i = y | i \in C) \geq 0, \tag{7}$$

for all possible characteristics $x \in \mathcal{X}$ and all possible potential outcomes $y \in \mathcal{Y}$. Kitagawa (2015) proposes a formal test of this implication, and Chan et al. (2019) show that applying this test to *potential outcomes* can provide a stronger test of the conditions for IV validity, particularly the monotonicity assumption in Condition 1(iii).[22]

[21] Our test builds on the suggestion of Wolak (1987) to form a test statistic based on a quadratic form that represents the deviations of the data from the predictions of a constrained model that imposes the inequality restrictions. We use a simple bootstrap procedure to derive critical values of the test.

[22] Specifically, testing Equation (7) with respect to potential outcomes $y \in \mathcal{Y}$ may be more likely to detect violations of Condition 1 than standard tests of monotonicity, focusing on patient characteristics, that we employ in Appendix A.1.2. The intuition behind this is that testing Equation (7) with respect to potential outcomes will reveal violations in Condition 1 that relate not only to observed patient characteristics but also to unobserved patient characteristics correlated with potential

In our setting, we partition survival potential outcomes into weeks of potential mortality for 52 weeks following the ambulance ride, for both VA- and non-VA-assigned compliers. Since survival can only decrease over time, the potential mortality hazard rates for any week must be positive (i.e., $h_{IV}(t;d) \geq 0$ for all $t \in \{0,\ldots,51\}$, $d \in \{0,1\}$). This prediction may be violated if patient risk of death in some week t is correlated with the assigned ambulance's propensity to take patients to the VA (a violation of independence), or if there exist "defiers" (i.e., patients that are *less likely* to go to the VA when assigned to ambulances that transport more often to the VA overall) with higher risk of death in some week t (a violation in monotonicity). In Appendix A.2, we formally test the joint inequality constraint that $h_{IV}(t;d) \geq 0$ for all $t \in \{0,\ldots,51\}$, $d \in \{0,1\}$, and cannot reject this null hypothesis, with a bootstrap-based p-value of 1.00.

If the short-term and longer-term mortality risks facing veterans are independent (as is typically assumed in a competing risks model) and treatment at the VA only affects the short-term risk component, then Condition 1 also implies that $h_{IV}(t;1) = h_{IV}(t;0)$ for $t \geq \bar{t}$, for some $\bar{t}$ after the acute ambulance episode. Specifically, if the short-term risk component disappears after some time $\bar{t}$, and if the assignment of compliers to VA and non-VA hospitals is as good as random, then the death rates of the two groups of compliers should be the same after $\bar{t}$. Visually, it appears that the potential hazard rates of the compliers are very similar in weeks $t \in \{1,\ldots,51\}$. Consistent with this impression, in Appendix A.2, we show that we cannot reject that $h_{IV}(t;1) = h_{IV}(t;0)$ for all weeks $t \geq 1$, with a bootstrap-based p-value of 0.31.

4.4 Heterogeneity in Mortality Risks

Finally, we take a closer look at death rates during the year after the ambulance ride to better understand the differences between our main OLS and IV estimates of the VA advantage. As shown in Panel A of Figure 4, we find that, remarkably, OLS survival curves cross about nine to ten months after the ambulance ride. This reflects a reversal in the sign of the OLS-estimated VA treatment effect: While patients arriving at the VA experience an immediate survival benefit that peaks at 14 days after the ambulance ride, the survival benefit eventually reverses, such that patients arriving at the VA are *more* likely to die within a year.

Consistent with this observed survival pattern, Panel B of Figure 4 reveals a cross-over in the observed hazard rates of death for patients who are taken to VA and non-VA hospitals. In the first

outcomes. Violations in quasi-random assignment or monotonicity may be more likely to occur along potential outcomes if agents act according to an objective function based on potential outcomes.

week after the ambulance ride, the death rate is lower for patients at the VA, though the gap between the VA and non-VA hazards is smaller than the corresponding potential-outcomes gap for compliers shown in Figure 3. Thereafter, the hazard rate for patients at the VA, $h_{OLS}(t;1)$, is consistently higher than the hazard rate for those who went to a non-VA hospital, $h_{OLS}(t;0)$. This gap suggests differences in baseline risk between always takers and never takers that are initially offset by the short-term VA advantage but reemerge, soon after the first week. These differences in baseline mortality hazards accumulate over time to generate large differences in long-term survival.

To identify differences in the baseline mortality risk between VA-assigned compliers and always takers, we compare $h_{IV}(t;1)$ and $h_{OLS}(t;1)$; to identify differences between non-VA-assigned compliers and never takers, we compare $h_{IV}(t;0)$ and $h_{OLS}(t;0)$. In Appendix A.2 we show that we cannot reject the null hypothesis that $h_{IV}(t;1) = h_{OLS}(t;1)$ for $t \geq 1$. However, we can strongly reject the null hypothesis that $h_{IV}(t;0) = h_{OLS}(t;0)$ for $t \geq 1$. The average value of $h_{IV}(t;0)$, for $t \geq 1$, is significantly larger than the corresponding average value of $h_{OLS}(t;0)$, for $t \geq 1$, which implies that never takers are healthier than compliers. This survival analysis shows, with substantially more precision than that afforded by the benchmark analysis in Section 3.3, that the VA advantage is in fact larger than the (precisely estimated) OLS effect would imply.

5 Mechanisms

This section probes further into the mechanisms behind the large VA mortality advantage. We first examine characteristics of compliers in our quasi-experiment. Second, we use a simple Olsen (1980) control function approach to estimate the average treatment effect (ATE) and compare it with the LATE estimated in Section 3. Third, we examine treatment effect heterogeneity by hospital and patient characteristics. Fourth, we ask whether the VA produces superior health outcomes by spending more; spending less would imply mechanisms that improve productivity. Fifth, we perform an analysis of health IT and integrated care among veterans using non-VA care, as potential mechanisms that may set the VA apart from non-VA care.

5.1 Complier Characteristics

We perform a standard complier analysis examining characteristics of compliers relative to the overall sample.[23] Table 4 shows results for various characteristics. Compliers are more likely to be Black,

[23] Specifically, we employ the same approach from Abadie (2002) that we introduced in Section 4.1. Under IV validity in Condition 1, we can estimate $E[X_i | i \in C]$ for some characteristic X_i by two-stage least squares, involving the first-stage

to have lower income, to have a prior VA ED visit, and to suffer from mental illness and substance abuse. Compliers have slightly fewer recorded Elixhauser comorbidities and are less likely to receive Advanced Life Support (ALS). In Appendix Table A.7, we show similar patterns comparing always takers and never takers, following an approach from Dahl et al. (2014) that we describe in Appendix A.3. Consistent with our analysis in Section 4.4, we find that VA always takers have higher predicted mortality, based on observable characteristics, than either compliers or never takers.[24]

Researchers and policymakers have noted greater incidence of mental health and substance abuse issues among veterans (Adamson et al. 2008). Recognizing this need, Congress allocated $152 million for increasing mental health care programming in 1999; in the following two decades, VHA stations expanded mental health services and hired thousands of mental health providers (106th Congress 1999; U.S. Government Accountability Office 2015). This capacity to treat mental health disorders contrasts with the non-VA health care sector, where mental health services have long been underfunded and underprovided (Huskamp and Iglehart 2016).

5.2 Selection Model

We consider a structural model of selection, both to assess how VA treatment effects vary with a veteran's propensity to go to the VA and to infer the ATE. Following the "marginal treatment effects" (MTE) literature (see, e.g., Heckman and Vytlacil (2007) for a review), we exploit our multivalued ambulance instrument in order to characterize the relationship between treatment effects and veterans who are induced to go to the VA.

Specifically, we allow for flexibility in the returns to VA care among compliers who are induced into VA care, ranging from ambulances with low propensities to deliver patients to the VA to those with high propensities. Using a control function model, we also extrapolate this relationship in treatment effects to always takers and never takers, thereby imputing the ATE from the semiparametric structure of the model. We provide further details of our approach in Appendix A.4.

We find evidence of moderate "selection on gains," in which veterans with larger mortality reductions from going to the VA are more likely to go to the VA. In Appendix Figure A.7, we show the MTE function ranging from veterans who are most likely to use the VA to those who are least likely to use the VA. Veterans induced to go to the VA by lower-propensity ambulances have higher returns

Equation (3) and a reduced-form equation replacing the outcome variable in Equation (4) with $X_i D_i$.

[24]For Table 4 and Appendix Table A.7, we predict mortality using the same regression of mortality on baseline and hold-out characteristics described in Section 3.2 applied only on rides going to non-VA hospitals, in order to separate the VA advantage from coefficients used in the prediction. The intuition for this is described in Chetty et al. (2014, p. 2598). Adopting their approach of estimating coefficients on predictors while controlling for D_i yields nearly identical results.

to VA care than veterans who are induced by high-propensity ambulances. In Appendix Table A.8, we find a substantial ATE, only marginally smaller than the LATE, across a variety of specifications.

5.3 Heterogeneity by Hospitals and Patients

We next assess heterogeneity in the VA mortality effect by hospitals in a patient's choice set and by patient characteristics. We consider a wide range of characteristics, in three categories: (i) characteristics of non-VA hospitals serving a given zip code, weighting the hospitals by volume of rides from the zip code; (ii) characteristics of the VA hospital serving a given zip code; and (iii) patient characteristics.

For each of these characteristics x, we construct a binary indicator variable, $I_{x,i} \in \{0,1\}$. For example, for the non-VA hospital characteristic of number of staffed beds, we create a binary indicator variable for whether the volume-weighted average number of staffed beds across non-VA hospitals in a zip code is above or below the median. We include a demeaned $\tilde{I}_{x,i} \equiv I_{x,i} - \hat{E}_i\left[I_{x,i}\right]$ in the following linear control function regression:

$$Y_i = \beta_x D_i + \rho_x D_i \tilde{I}_{x,i} + \pi_x \tilde{I}_{x,i} + \gamma_x \hat{\varepsilon}_{1,i} + \mathbf{X}_i^0 \delta_x + \zeta_{x,z(i)} + \epsilon_{x,i}, \tag{8}$$

where $\hat{\varepsilon}_{1,i}$ is the first-stage error from Equation (3). Controlling for the endogeneity of selection, this approach yields estimates of binary heterogeneous treatment effects along several dimensions.[25] Since $\tilde{I}_{x,i}$ has a mean of 0, we can interpret β_x as the LATE, controlling for $\tilde{I}_{x,i}$; ρ_x is the difference in the VA effect on mortality between $I_{x,i} = 1$ and $I_{x,i} = 0$. We calculate standard errors by bootstrap, drawing blocks of data by zip code.

Table 5 shows differences in hospital characteristics between VA and non-VA hospitals. For example, VA hospitals have fewer ED visits and admissions per bed and are more likely to be teaching hospitals.[26] However, we find at most modest treatment heterogeneity with respect to any of these hospital characteristics (Appendix Tables A.9 to A.11). Heterogeneity along any of the VA or non-VA hospital characteristics across zip codes is less than 20% of the main VA advantage, suggesting that the VA advantage pertains across the spectrum of VA and non-VA alternatives. Non-VA hospital characteristics in a zip code that are associated with a modestly smaller VA advantage include trauma center status and number of nurses per 1,000 patient-days. As for heterogeneity by VA hospital

[25] For a discussion of this general approach, see Wooldridge (2015), Section III.

[26] VA hospitals appear to have more long-term care admissions, which explains a higher average length of stay (i.e., fewer admissions for slightly larger average daily census). As shown in Table 3, the difference in length of stay is not borne out in our sample; the IV estimate of the effect on length of stay suggests that the VA reduces length of stay.

characteristics, the VA advantage is perhaps slightly greater for larger VA hospitals. In Appendix A.6, we describe complementary results from an empirical Bayes approach to heterogeneity in station-specific OLS estimates of the VA advantage; in that approach, we fail to demonstrate meaningful heterogeneity in the VA advantage across VA stations.

Across patients (Appendix Table A.12), the VA advantage is likely as large for minority veterans (Black and Hispanic) as for non-minority veterans. The VA survival benefit appears greater for veterans who suffer from mental illness or substance abuse, who have more prior visits at the VA, or who have higher predicted mortality. However, the differences in the VA survival benefit across all of these dimensions are still smaller than the overall scale of the survival benefit. Importantly, the VA survival benefit is not limited to select medical conditions that stereotypical users of the VA might have; even patients who are less than likely to use VA care experience a similar VA survival benefit.

5.4 Effect on Spending and Utilization

In light of the important literature on the returns to spending in health care (e.g., Garber and Skinner 2008), we examine the causal effect of VA vs. non-VA care on spending. The motivation behind this analysis is similar to that in Doyle et al. (2015), who sought to understand whether higher-spending hospitals achieve better health outcomes. To perform this analysis, we rely on both internal VA cost data and Medicare payment data from claims. Internal VA cost accounting apportions costs by VA utilization data and scales the cost of each encounter so that total spending matches actual budgeted spending within each VHA station.[27] On the Medicare side, we include payments made both by the veteran (i.e., coinsurance and deductible) and by the government. Therefore, we measure the cost of both VA and non-VA health care in terms of dollars spent by the government and the veteran.

Using the same instrumental variables approach as in our benchmark analysis, we study the effect of VA vs. non-VA care on daily VA and non-VA spending over time since the ambulance ride. Specifically, we combine VA and Medicare spending in various weekly intervals since the ambulance ride. Table 3 further shows that the VA reduces 28-day combined spending by $\$2,548$, or by 21% of the mean 28-day spending. The reduction in spending reflects a lower probability of inpatient admission and fewer hospital days associated with VA care, although VA care results in slightly more outpatient visits in the following 28 days.[28] We find similar results when we use an alternative

[27] The apportioning is based on inputs such as Relative Value Units (RVUs) associated with CPT codes, Diagnosis-Related Group (DRG) weights, patient characteristics, and admission lengths of stay. This methodology is detailed in Wagner et al. (2003) and in Phibbs et al. (2019).

[28] Although the average Medicare outpatient visit costs less than the average VA outpatient visit, the average Medicare

measure of spending that fixes prices for utilization in VA and non-VA settings.[29]

In Figure 5, we show potential cumulative spending curves during the first year and implied weekly spending rates conditional on survival, for compliers transported to a VA hospital and those transported to a non-VA hospital. Differences in cumulative spending remain throughout the end of the year. Unlike survival outcomes, differences in spending accrue until at least three months after the ambulance ride.

The result that the VA saves lives while reducing spending is significant for two reasons. First, the result speaks directly to the policy question of whether the VA should privatize its care in a Medicare-type arrangement. The potential role of the VA as an insurer of private care has featured heavily in recent policy proposals and laws (113th Congress 2014; 115th Congress 2018). We show that, at least for the patients in our design, this privatization arrangement would be dominated by the status quo, as it would lead to both higher spending and worse health outcomes. Second, this joint finding suggests that the general mechanism behind the VA survival benefit is not higher spending but higher productivity. Our evidence points to productive inefficiency, rather than "flat of the curve" spending that underlies the relatively low-returns to US health care. This implication complements a growing literature on productivity differences across personnel (Chan et al. 2019; Silver 2020) and hospitals (Chandra and Staiger 2007, 2020) by showing an important productivity difference between health care *systems*.

5.5 Health IT and Integrated Care

Our final analysis investigates the role of health IT and integrated care in explaining the VA survival advantage. A substantial literature has reported the qualitative importance of these mechanisms in the VA's "transformation" into a high-quality health care organization in the mid-1990s (e.g., Jha et al. 2003).[30] The lack of information flow and the high degree of fragmentation across providers in the US private health care sector have long been highlighted as potential roots of inefficiency (Cebul et al.

inpatient day costs more than the average VA inpatient day. In our main analytic sample described in Appendix Table A.1, the average VA outpatient visit costs $181.35, and the average VA inpatient day costs $1,577.38. In the same sample, the average Medicare outpatient visit costs $108.30 (lower than the average VA cost), but the average Medicare inpatient day costs $1,811.99 (higher than the average VA cost).

[29] We follow the methodology laid out in Finkelstein et al. (2016), where we impute spending based on Relative Value Units (RVUs) for procedures with CPT codes and Diagnosis-Related Group (DRG) weights for inpatient stays. We scale prices by a constant so that imputed total Medicare spending equals actual total Medicare spending. According to this spending measure, the VA reduces 28-day combined spending by $\$4,036$.

[30] For recent qualitative research that illuminates this mechanism between VA and non-VA care, see, e.g., Nevedal et al. (2019) and Rinne et al. (2019). Outside of the VA, qualitative research also suggests important complementarities between IT and organization for producing quality care (e.g., Haque et al. 2019).

2008; Jha et al. 2009; Cutler 2010). These information-based mechanisms would be consistent with greater health care productivity of the VA, particularly for regular users and patients whose conditions are more responsive to informed management (e.g., substance abuse). Effective coordination of care also seems necessary to explain the large body of evidence that documents superior VA performance on a wide range process measures characterizing timeliness and appropriateness of care (e.g., O'Hanlon et al. 2017).

Ideally, we would study these mechanisms by observing the VA's implementation of health IT and its reorganization into more integrated care in the mid-1990s. However, these reforms predate our data.[31] Similarly, it is not possible to examine the VA's effect on mortality among veterans who have no prior utilization at the VA, since it is exceedingly rare for these veterans to be transported to the VA by ambulance, as shown in Appendix Figure A.9. Nevertheless, this figure also shows that veterans may utilize more than one non-VA hospital system and that veterans may or may not be transported to their *modal* non-VA hospital, defined as the hospital system at which they had the largest number of utilization days in the prior year.

Our analytic strategy thus centers on a sample of veterans with no prior VA utilization but with some prior non-VA utilization.[32] While these veterans will almost certainly be transported to a non-VA hospital, we assess mortality outcomes as a function of whether they are quasi-randomly assigned—via a similar ambulance instrument as the one we use in our benchmark analysis—to their modal non-VA hospital. This modal-hospital effect on mortality arguably captures at least some of the potential effect of continuity of care in the private sector. In order to more explicitly investigate the role of health IT and integrated care, we further exploit two changes induced by incentives in federal laws and payment policies during our study period. First, the HITECH Act of 2009 dramatically increased the share of hospitals using health IT (Blumenthal 2010).[33] Second, in 2011, Medicare began to incentivize care integration via alternative payment arrangements to "Accountable

[31] Indeed, the VHA's adoption of a common health IT platform (VistA) in the mid-1990s paved the way for research on health services within the VHA system, including this study.

[32] We detail the sample selection process for this analysis in Appendix Table A.13 and present patient and ride characteristics in this sample Appendix Table A.14. Since no veteran in this sample has prior VA utilization, the sample is disjoint from our benchmark sample (Appendix Table A.1). We only include zip codes with at least two non-VA hospitals within 20 miles, but we make no requirement on proximity to a VA hospital. The probability of transport to a VA hospital in this sample is 0% (as opposed to 33% in the benchmark sample), but rates of weekend transport and 28-day mortality are remarkably similar.

[33] Jha et al. (2009) report in 2009 that 1.5% of US non-federal hospitals had an electronic health record (EHR) system in all clinical units and an additional 7.6% had an EHR system in at least one clinical unit. By 2014, 97% of such hospitals had possessed an EHR technology meeting requirements of the Department of Health and Human Services, and 76% of hospitals had implemented the EHR system in at least one clinical unit (Charles et al. 2015). However, interoperability (i.e., the ability to share electronic medical records) across private hospitals has remained low (Holmgren et al. 2017). To this day, multiple EHR platforms exist in the private sector, and they do not communicate with each other.

Care Organizations" (ACOs) (Greaney 2011).

As an analog to our benchmark VA instrument in Equation (1), we construct an instrument that reflects a given ambulance company's leave-out propensity to deliver patients to the index patient's modal non-VA hospital. Let $h(i)$ denote the hospital that ambulance ride i is transported to, and let $h^m(i)$ represent the modal non-VA hospital used by patient $k(i)$ in ride i. Our treatment of interest is $D_i^m \equiv \mathbf{1}(h(i) = h^m(i))$, which indicates whether ambulance ride i transports its patient $k(i)$ to his modal hospital. Our instrumental variable for this treatment is

$$Z_i^m = \frac{1}{K_{j(i),z(i)} - 1} \sum_{i' \in \mathcal{I}_{j(i),z(i)}} \frac{\mathbf{1}(k(i') \neq k(i)) D_{i'}^m}{N_{k(i'),z(i'),j(i')}}. \tag{9}$$

where $K_{j,z}$ is the number of patients transported by company j from zip code z, $N_{k,z,j}$ is the number of rides taken by patient k originating in zip code z with company j, and $\mathcal{I}_{j,z}$ is the set of rides transported by ambulance company j from zip code z. This is the leave-out probability that ambulance company $j(i)$ transports other patients from the same zip code to the modal hospital $h^m(i)$ of patient $k(i)$.[34] We use the following first-stage and reduced-form equations, similar to Equations (4) and (3):

$$D_i^m = \pi_1^m Z_i^m + \gamma_1^m \overline{Z}_i^m + \mathbf{X}_i^0 \delta_1^m + \zeta_{1,z(i)}^m + \varepsilon_{1,i}^m; \tag{10}$$

$$Y_i = \pi_2^m Z_i^m + \gamma_2^m \overline{Z}_i^m + \mathbf{X}_i^0 \delta_2^m + \zeta_{2,z(i)}^m + \varepsilon_{2,i}^m. \tag{11}$$

We include in these equations an additional control variable:

$$\overline{Z}_i^m = \frac{1}{K_{z(i)} - 1} \sum_{i' \in \mathcal{I}_{z(i)}} \frac{\mathbf{1}(k(i') \neq k(i)) D_{i'}^m}{N_{k(i'),z(i')}},$$

where K_z is the number of patients from zip code z, $N_{k,z}$ is the number of rides taken by patient k originating in zip code z, and $\mathcal{I}_z$ is the set of rides originating in zip code z. This is the leave-out probability that patients from the same zip code $z(i)$ are transported to hospital $h^m(i)$, *unconditional* on the ambulance company. The modal-hospital effect may also capture hospital quality or hospital-patient match effects. We further assess the modal hospital effect both (i) while including hospital fixed effects in Equations (10) and (11) and (ii) while splitting rides i into samples based on whether the ride was before or after the hospital $h(i)$ adopted health IT or joined an ACO.

[34] As with the benchmark instrument, we construct this instrument from data in the overall sample of ambulance rides with dually eligible veterans (Column 1, Table 1 and A.14). For patients with multiple hospitals that tie for highest utilization in the prior year, we designate the set of these highest-use hospitals as the "modal hospital."

In the sample of veterans with only non-VA prior utilization (Panel B of Appendix Table A.13), we demonstrate in Appendix Figure A.10 a well-behaved first-stage relationship between D_i^m and Z_i^m and balance between predicted mortality, $\hat{Y}_i$, and Z_i^m, conditional on $\left(\overline{Z}_i^m, \mathbf{X}_i^0, z(i)\right)$.[35] The IV estimate of the modal-hospital effect on mortality is −0.006 (s.e. 0.004), which is less than 20% of the VA effect on mortality. The visual IV graph in Appendix Figure A.11 shows that the overall relationship between the reduced form and first stage is not particularly striking.[36] However, computing the same IV estimate separately by years, we show in Figure 6 a stronger modal-hospital effect emerges after the passage of the HITECH Act of 2009, which led to a rapid rise in electronic medical record systems. The modal-hospital effect is close to 0 and stable prior to 2009; following 2009, the modal-hospital effect grows to about half the size of the VA effect on mortality.

In Table 6, we examine how the modal-hospital effect relates to dates of hospital health IT or ACO adoption.[37] We estimate the modal-hospital effect in four subsamples defined by whether or not each veteran's modal hospital, at the time of his ride, had adopted health IT and similarly by whether or not the hospital had joined an ACO. We also use a control function approach to estimate separate modal-hospital effects, depending on health IT or ACO adoption, in the overall sample and with hospital group fixed effects (see Appendix A.7 for details). The results in the table provide suggestive evidence that the growth in the modal-hospital effect is associated with health IT adoption, holding hospitals fixed; the relationship with ACO adoption appears similar but is imprecise. While estimates in the table control for hospital or hospital group fixed effects, we find that results are essentially unchanged regardless of their inclusion.

6 Conclusion

The structure of health care delivery to US veterans provides a distinctive research opportunity, allowing us to study fundamentally different systems of health care that coexist for a large patient population. Specifically, millions of older veterans (those at least aged 65) are dually eligible for care

[35] Analogously to Figure 1, this figure presents binned scatterplots of the first-stage regression in Equation (10), the reduced-form regression in Equation (11), and a balance regression with predicted mortality as the outcome variable and the same design matrix.

[36] Analogously to Figure 2 and Appendix Figure A.2 in the benchmark analysis, Appendix Figure A.12 shows stability in OLS and two-stage least squares estimates with increasing controls, and Appendix Figure A.13 shows robustness of two-stage least squares estimates under an exhaustive set of control combinations.

[37] We measure health IT adoption from a dataset from the Office of the National Coordinator of Health Information Technology (ONC), coding any certified product on the Certified Health IT Product List (CHPL) as health IT adoption. We measure ACO participation from the Medicare Shared Savings Program (MSSP) Accountable Care Organizations (ACO) dataset. Consistent with other research, only 11% of non-VA hospitals participated in ACOs by the last year of our sample (Colla et al. 2016).

in a public system, operated by the Veterans Health Administration, or in private-sector hospitals, financed by Medicare. The ambulance setting provides plausible quasi-experimental assignment of veterans to these health care systems. Our work has current policy relevance, as the Department of Veterans Affairs is now considering whether to bolster its existing public delivery system or to replace it, either partially or fully, with a system of financing private care. Our work has implications more broadly for understanding the impact of public vs. private health care on mortality and spending.

We find a significant VA advantage: Our preferred instrumental variables estimate, based on veterans who are induced by their ambulance company to use the Veterans Health Administration (VHA), shows a 4.5 p.p. survival gain at 28 days (95% confidence interval 1.1 to 8.0 p.p.), implying about a 46% reduction in mortality relative to the overall average. In a novel survival analysis of this quasi-experiment, we show that these survival gains occur in the first week following the ambulance ride and appear to be long-lasting. We further use this survival analysis framework to validate our IV quasi-experiment and to demonstrate differences in long-term mortality hazards between VA and non-VA users who are non-compliers. Our analysis of long-term hazards provides a compelling explanation for the difference in magnitude between IV and OLS estimates of the VA effect on 28-day mortality. Although we find some intuitive margins of heterogeneity in the VA advantage, the VA outperforms the non-VA alternative in a wide variety of locations with different types of non-VA hospitals and for all types of patients we consider, not only for patients with stereotypical medical conditions.

Importantly, we also find that the VA reduces total spending by 21% relative to non-VA providers, which points to higher productivity in the VHA than in the private sector. Using our quasi-experiment, we shed light on mechanisms, many of which have been raised more descriptively or qualitatively (e.g., Jha et al. 2003). We interpret our findings as consistent with the idea that the VA advantage arises from continuity of care, health IT, and organization. For example, we find that compliers are more likely to have prior VA care and have larger survival gains from VA assignment than average; in a selection model that rationalizes this finding, we show veterans who are more likely to use the VA also have larger survival gains. Interestingly, we tentatively find a similar effect in the private sector for veterans who primarily use a private hospital system. These veterans also experience reduced mortality when quasi-experimentally assigned to their modal private hospital, but only in a period following adoption of health IT due to government incentives, and even then on a smaller scale (approximately half the size) than the overall VA advantage. This suggests a sizeable residual VA advantage, even when accounting for private efforts to adopt health IT and reorganize care.

Our results contribute more broadly to two streams of literature on the efficiency of production. First, we contribute to the descriptive analysis that compares the performance of the US health care system to systems in other developed countries (Blank et al. 2017). By almost all accounts, comparisons of US health outcomes and health care spending are unfavorable with those of other developed countries (Garber and Skinner 2008; Rice et al. 2013). Our analysis points to a potentially large source of inefficiency in the US context: its version of private provision of health care, in which well-known information and coordination gaps may be fatal, at least for veterans in emergencies. Although several developed countries that outperform the US also feature private provision of care, the US system arguably has the most complex configuration of financing and delivery, with high levels of uninsurance, administrative costs, and fragmentation (Cebul et al. 2008).

Second, we provide empirical support in the context of health care for the general idea of production complementarities among three innovations in production: information technology (IT), workplace reorganization, and products and services (Bresnahan et al. 2002). The VHA adopted a comprehensive health IT system almost two decades before the vast majority of private hospitals in the US. This reform was accompanied by an integration of care involving both reorganizing the delivery system and redefining services involved in patient care. For private hospitals, redefining health care products and services is limited by fee-for-service payment systems and the difficulty of measuring quality (Cutler 2010). Hospitals without a broad network of clinics and an overarching mandate for a population's health may find it difficult to reorganize and redefine its services. Our result that health IT in private hospitals may improve survival—but to a muted extent and only for patients that the hospitals have previously treated—is consistent with these production complementarities. Complementarities in health care production may pose barriers for replicating the VA advantage in the fragmented private landscape of US health care.

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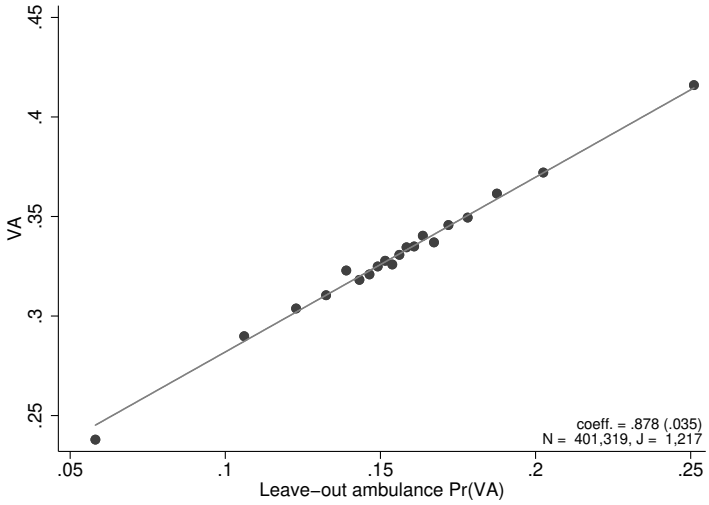
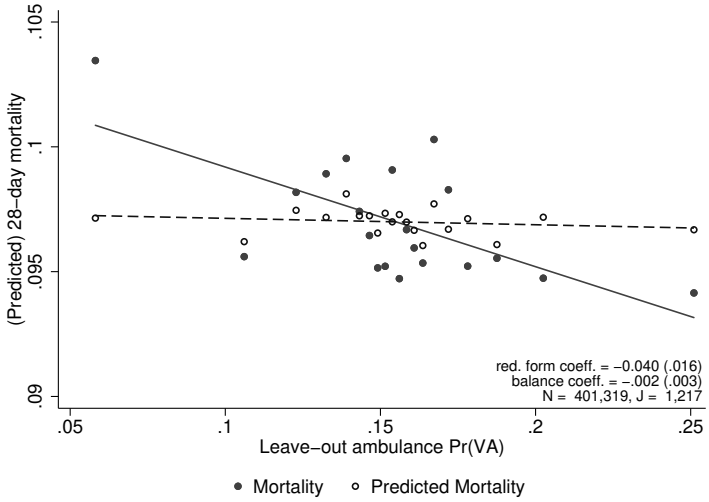
Figure 1: First Stage, Balance, and Reduced Form

A: First Stage

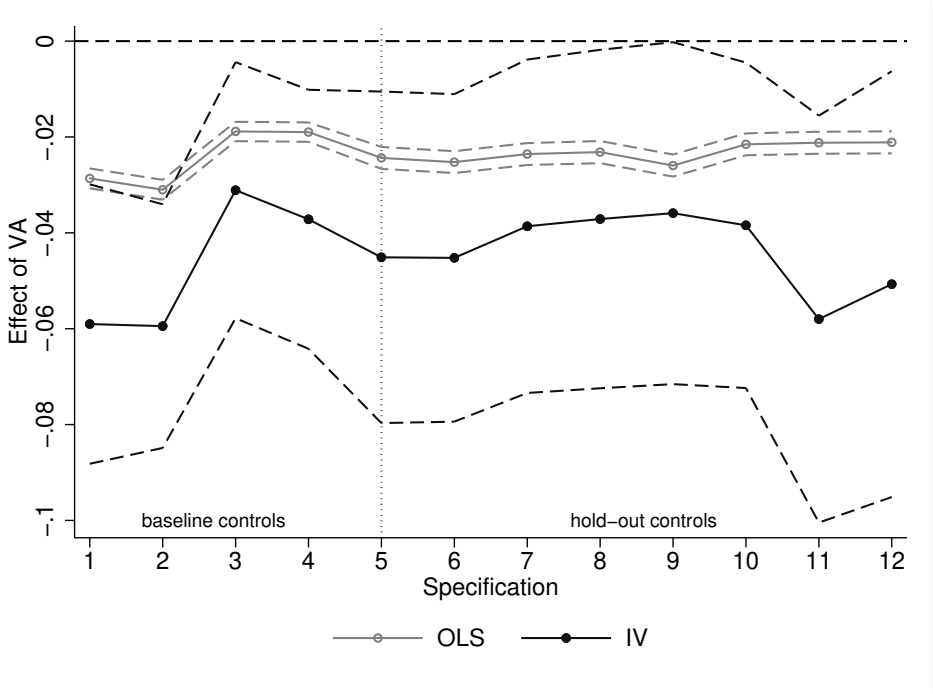


B: Balance and Reduced Form



Note: Panel A shows a binned scatterplot of arrival at a VA hospital on the *y*-axis against the ambulance leave-out propensity to arrive at a VA hospital on the *x*-axis. The figure is a graphical representation of the first-stage regression in Equation (3). Panel B shows binned scatterplots of 28-day mortality and predicted 28-day mortality on the *y*-axis against the ambulance leave-out propensity to arrive at a VA hospital on the *x*-axis. Mortality bin means are shown in solid circles, while predicted mortality bin means are shown in hollow circles. The figure represents the reduced-form regression in Equation (4) and the corresponding balance regression replacing mortality with predicted mortality. The sample includes 401,319 ambulance rides and 1,217 combinations of ambulance company identifiers and Dartmouth Atlas Hospital Referral Regions (HRRs). The sample selection is given in Appendix Table A.1. Baseline controls are detailed in Appendix Table A.2 and include patient zip code dummies, ALS/BLS dummies, source of the ambulance ride, time categories, and patient prior utilization.

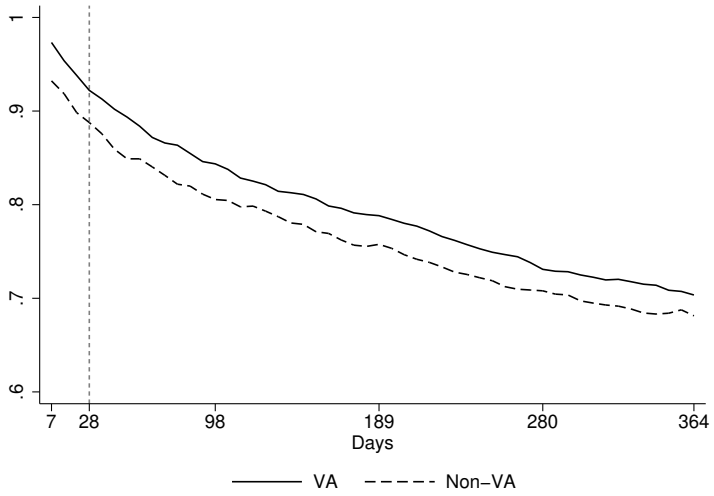
Figure 2: OLS and IV Specifications



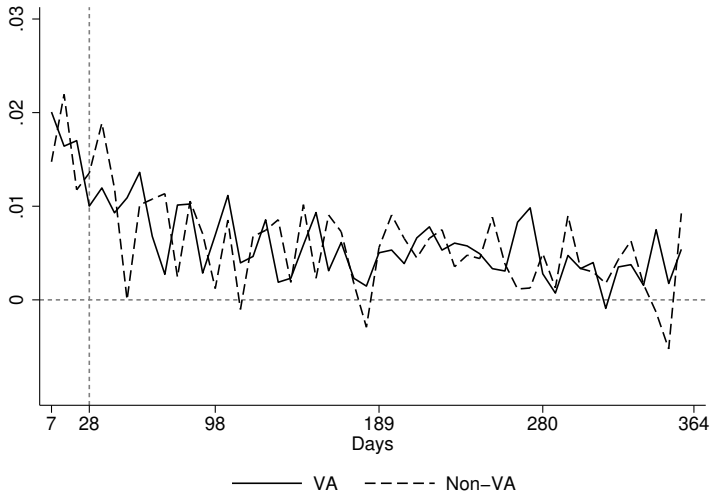
Note: This figure shows OLS and IV estimates of the effect of the VA on 28-day mortality, represented in Equation (2) as β, with progressive sets of controls. Numbered incremental controls correspond to categories or subcategories of variables that are presented in order in Appendix Tables A.2 and A.3. Estimates are shown along solid lines, while 95% confidence intervals are shown in dashed lines. All specifications use the baseline sample, given in Appendix Table A.1.

Figure 3: Complier Potential Outcomes

A: Survival



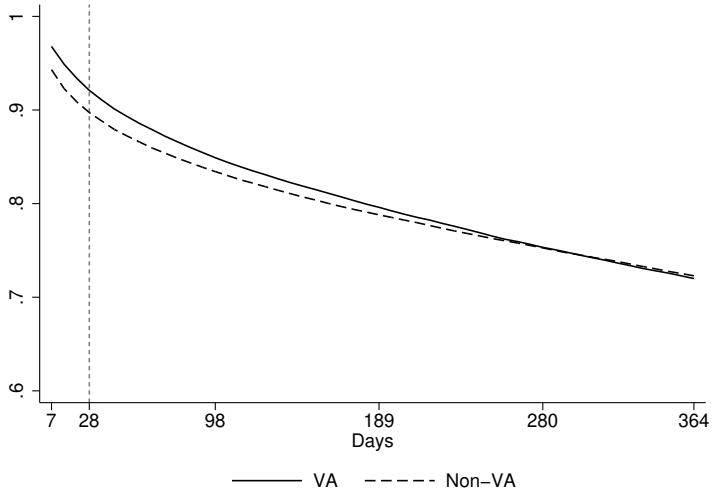
B: Mortality Hazard Rates



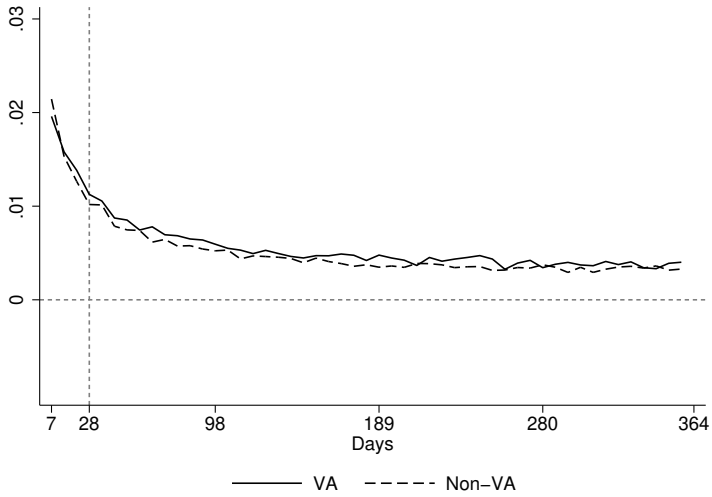
Note: This figure shows potential outcomes for ambulance compliers who arrive at a VA hospital and those who arrive at a non-VA hospital. Panel A shows survival outcomes as a function of days from the ambulance ride. "Days" indicate one-week intervals from the ambulance ride. Denote $S_i(t;d) \in \{0,1\}$ as an indicator for whether patient i survives up to time t after the ambulance ride, depending on whether the patient arrives at the VA ($d=1$) or a non-VA hospital ($d=0$). Observed survival is $S_i(t) = D_i S_i(t;1) + (1-D_i) S_i(t;0)$. We estimate complier VA survival, or $E[S_i(t;1)|i \in C]$, by an IV regression with a dependent variable of $S_i(t) D_i$, the endogenous VA treatment D_i, and the same first-stage and reduced-form design matrix implied by Equations (3) and (4). We estimate complier non-VA survival, or $E[S_i(t;0)|i \in C]$, by a similar IV regression with a dependent variable of $S_i(t)(D_i - 1)$. All regressions use a sample of ambulance rides with no prior ride in the last year and the same baseline controls as described in Figure 1. Panel B presents implied weekly mortality hazard rates, as given by Equation (5).

Figure 4: Observed Risk-Adjusted Outcomes

A: Survival

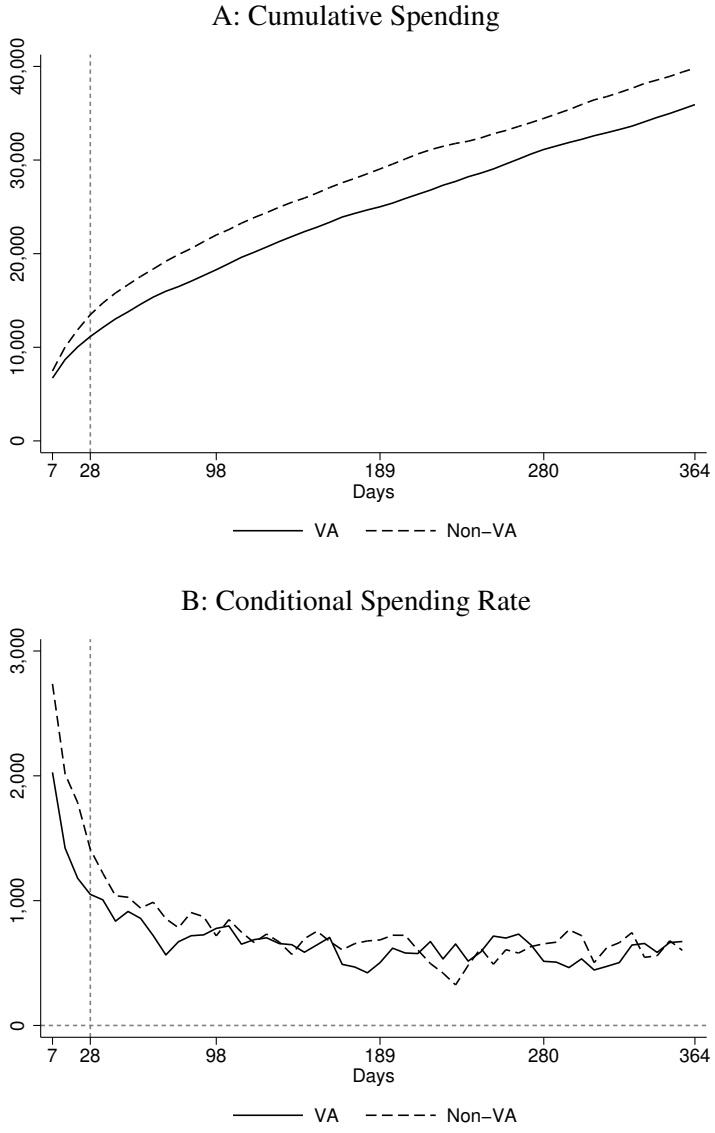


B: Mortality Hazard Rates



Note: This figure shows observed risk-adjusted outcomes for patients who arrive at a VA hospital and those who arrive at a non-VA hospital. Panel A shows survival outcomes as a function of days from the ambulance ride. "Days" indicate one-week intervals from the ambulance ride. Denote $S_i(t;d) \in \{0,1\}$ as an indicator for whether patient i survives up to time t after the ambulance ride, depending on whether the patient arrives at the VA ($d = 1$) or a non-VA hospital ($d = 0$). Observed survival is $S_i(t) = D_i S_i(t;1) + (1 - D_i) S_i(t;0)$. We estimate VA survival, or $E[S_i(t) | D_i = 1]$, by an OLS regression with a dependent variable of $S_i(t) D_i$ and the same design matrix implied by Equation (2); we estimate non-VA survival, or $E[S_i(t) | D_i = 0]$, by a similar OLS regression with a dependent variable of $S_i(t)(D_i - 1)$. All regressions use a sample of ambulance rides with no prior ride in the last year and the same baseline controls as described in Figure 1. Panel B presents implied weekly mortality hazard rates, as given by Equation (6).

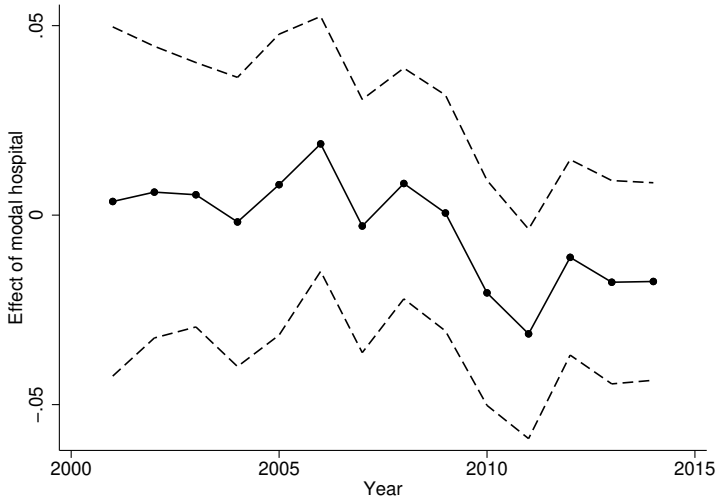
Figure 5: Complier Spending



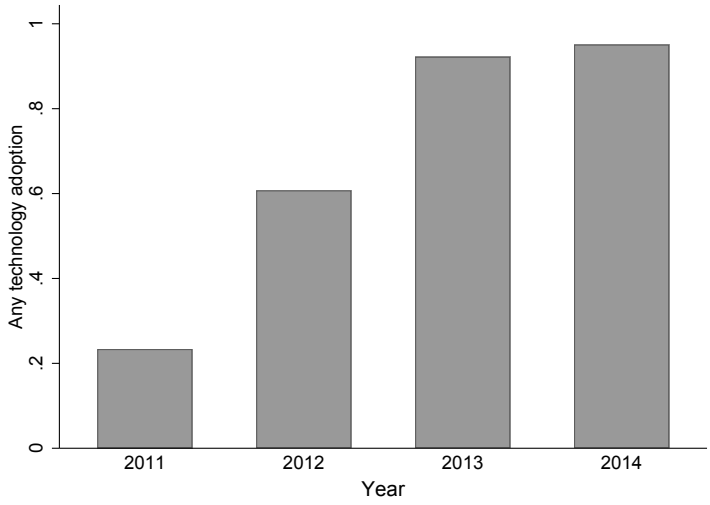
Note: This figure shows potential spending outcomes for ambulance compliers who arrive at a VA hospital and those who arrive at a non-VA hospital. Denote $\text{Spending}_i(t;d)$ as the potential cumulative spending function for patient i up to time t after the ambulance ride, depending on whether the patient arrives at the VA ($d=1$) or a non-VA hospital ($d=0$). If a veteran i dies at $\underline{t}$, $\text{Spending}_i(t;d)$ will be constant for all $t \geq \underline{t}$. Panel A shows cumulative spending per patient as a function of days from the ambulance ride. We estimate cumulative spending for compliers who arrive at a VA hospital, or $E\left[\text{Spending}_i(t;1) \middle| i \in C\right]$, by an IV regression with a dependent variable of $\text{Spending}_i(t) \times D_i$, the endogenous VA treatment D_i, and the same first-stage and reduced-form design matrix implied by Equations (3) and (4). We estimate complier non-VA cumulative spending, or $E\left[\text{Spending}_i(t;0) \middle| i \in C\right]$, by a similar IV regression with a dependent variable of $\text{Spending}_i(t)(D_i - 1)$. All regressions use a sample of ambulance rides with no prior ride in the last year and the same baseline controls as described in Figure 1. Panel B presents implied weekly spending rates for compliers, conditional on survival.

Figure 6: Modal Hospital Effect and Health IT Adoption

A: Modal Hospital Effect by Year



B: Health IT Adoption by Year



Note: Panel A of this figure shows the IV estimate of the modal non-VA hospital effect on 28-day mortality by calendar year. The first-stage and reduced-form equations are given in Equations (10) and (11). The overall sample is the same alternative sample designed to study choice among non-VA hospitals for patients with only non-VA utilization in the prior year. Results for the overall IV estimates are shown in Appendix Figure A.10. Details of the sample selection are given in Appendix Table A.13. Estimates are shown in connected dots, while 95% confidence intervals are shown in dashed lines. Panel B of the figure shows the percent of rides going to hospitals after health IT adoption in our analytic sample. Health IT adoption is defined from a dataset from the Office of the National Coordinator of Health Information Technology (ONC). This dataset merges hospital attestation data from the Medicare EHR Incentive Program with certified EHR product information from ONC's Certified Health IT Product List (CHPL), and we code the use of any certified product as health IT adoption.

Table 1: Characteristics of Baseline Sample

	Sample characteristics				
Restrictions	Dually eligible	Add zip × hospital	Add zip × ambulance	Add VA prior utilization	Add no ride in prior month
Male	0.899	0.883	0.863	0.962	0.963
Age	77.04	76.89	76.13	75.62	76.03
Black	0.111	0.163	0.187	0.200	0.194
Income	$21,724	$21,453	$20,874	$20,243	$20,905
Rural zip code	0.255	0.043	0.045	0.050	0.051
Residential source	0.610	0.600	0.652	0.685	0.705
Comorbidity count	6.53	6.69	6.44	6.54	6.14
Prior VA ED visit	0.136	0.197	0.264	0.565	0.529
Prior Medicare ED visit	0.695	0.675	0.626	0.539	0.482
Ambulance rides in prior year	2.77	3.05	3.25	3.12	2.16
Advanced Life Support	0.696	0.655	0.655	0.674	0.684
Weekend rate	0.272	0.269	0.270	0.270	0.269
28-day mortality	0.115	0.109	0.104	0.100	0.097
Present at VA	0.044	0.088	0.166	0.336	0.330
Number of patients	2,862,557	1,118,302	365,163	188,299	188,299
Number of ambulance rides	8,828,997	3,465,588	1,051,093	491,193	401,319

Note: This table presents characteristics of observations remaining at each step of creating the baseline sample, detailed in Appendix Table A.1.

Table 2: Effect of VA Hospitals on Mortality

	Dependent variable: 28-day mortality				
	(1)	(2)	(3)	(4)	(5)
	A: OLS				
VA hospital	-0.024	-0.023	-0.026	-0.022	-0.021
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Outcome mean	0.097	0.097	0.097	0.097	0.097
Observations	401,319	401,319	401,319	401,319	401,319
	B: IV				
First stage	0.878	0.853	0.839	0.837	0.860
	(0.035)	(0.034)	(0.034)	(0.034)	(0.043)
IV estimate	-0.045	-0.037	-0.036	-0.038	-0.049
	(0.018)	(0.018)	(0.018)	(0.017)	(0.023)
Outcome mean	0.097	0.097	0.097	0.097	0.097
Observations	401,319	401,319	401,319	401,319	401,319
Patient background controls	No	Yes	Yes	Yes	Yes
Comorbidity controls	No	No	Yes	Yes	Yes
Ambulance diagnosis controls	No	No	No	Yes	Yes
Ambulance and co-rider controls	No	No	No	No	Yes

Note: This table shows OLS and IV estimates of the effect of VA hospitals on 28-day mortality. Panel A gives OLS estimates, $\hat{\beta}_{OLS}$, for β in Equation (2). Panel B gives IV estimates, $\hat{\beta}_{IV}$, as well as the first stage coefficient, $\hat{\pi}_1$ in Equation (3), with respect to the leave-out probability of the assigned ambulance company to transport patients to the VA. Baseline controls in all specifications are described in Appendix Table A.2 and include patient zip code dummies, ALS/BLS dummies, source of the ambulance ride, time categories, and patient prior utilization. Patient background controls include demographics, socioeconomic status, combat history, eligibility for benefits, and counts of prior utilization. All additional controls are described in further detail in Appendix Table A.3. The estimation sample is described in Appendix Table A.1.

Table 3: Effect of VA Hospitals on Other Outcomes

	Dependent variable				
	Admission	Hospital days	ED revisits	Outpatient visits	Spending
	(1)	(2)	(3)	(4)	(5)
			A: OLS		
VA hospital	-0.004	0.514	-0.036	0.200	932
	(0.003)	(0.045)	(0.007)	(0.017)	(87)
Outcome mean	0.589	4.380	0.318	1.443	12,173
Observations	401,319	401,319	401,319	401,319	401,319
			B: IV		
IV estimate	-0.090	-0.468	0.029	0.379	-2,548
	(0.032)	(0.434)	(0.044)	(0.174)	(822)
Outcome mean	0.589	4.380	0.318	1.443	12,173
Observations	401,319	401,319	401,319	401,319	401,319

Note: This table shows OLS and IV estimates of the effect of VA hospitals on various outcomes. Hospital days is defined as the number of inpatient days immediately following the ED visit; if the patient is not admitted from the visit, then this equals 0 for that visit. Outpatient visits is defined as the number of VA and non-VA outpatient visits within one month of the ride. ED revisits is defined as subsequent ED visits up to 14 days following the ride. Spending is defined as total spending over the 28 days following the ambulance ride. Panel A gives OLS estimates, $\hat{\beta}_{OLS}$, for β in Equation (2). Panel B gives IV estimates, $\hat{\beta}_{IV}$, as well as the first stage coefficient, $\hat{\pi}_1$ in Equation (3), with respect to the leave-out probability of the assigned ambulance company to transport patients to the VA. All regressions use baseline controls, which are described in Appendix Table A.2 and include patient zip code dummies, ALS/BLS dummies, source of the ambulance ride, time categories, and patient prior utilization. The estimation sample is described in Appendix Table A.1.

Table 4: Complier Characteristics

	Overall	Compliers	Ratio
Male	0.963	0.952	0.99
		(0.006)	[0.98 - 1.00]
Age	76.0	74.9	0.99
		(0.433)	[0.97 - 1.00]
Black	0.194	0.257	1.33
		(0.028)	[1.05 - 1.61]
Income	$20,905	$16,972	0.81
		($611)	[0.75 - 0.87]
Rural zip code	0.051	0.091	1.78
		(0.025)	[0.82 - 2.75]
Residential source	0.705	0.647	0.92
		(0.033)	[0.83 - 1.01]
Comorbidity count	6.143	5.447	0.89
		(0.113)	[0.85 - 0.92]
Mental illness	0.427	0.444	1.04
		(0.015)	[0.97 - 1.11]
Substance abuse	0.144	0.163	1.13
		(0.011)	[0.97 - 1.28]
Prior VA ED visit	0.529	0.712	1.35
		(0.012)	[1.30 - 1.39]
Prior Medicare ED visit	0.482	0.336	0.70
		(0.014)	[0.64 - 0.75]
Ambulance rides in prior year	2.156	2.178	1.01
		(0.084)	[0.93 - 1.09]
Advanced Life Support	0.684	0.600	0.88
		(0.024)	[0.81 - 0.95]
Predicted VA user	0.847	0.939	1.11
		(0.004)	[1.10 - 1.12]
Predicted mortality	0.097	0.095	0.98
		(0.004)	[0.90 - 1.06]

Note: This table presents average complier characteristics and the ratio between this average and the average among all veterans in the sample. Average complier characteristics and standard errors are calculated by performing two-stage least squares using the first stage Equation (3) and a reduced-form equation replacing the outcome variable in Equation (4) with $X_i D_i$, where X_i is the characteristic corresponding to ride i. Regressions use baseline controls described in Appendix Table A.2; the regression sample is the baseline sample described in Appendix Table A.1. Standard errors for each average are presented in parentheses. The corresponding 95% confidence intervals for each ratio are presented in brackets.

Table 5: Means of Hospital Characteristics

	Hospital Sample		
	VA	Non-VA	
	Baseline sample	Baseline sample	National average
Volume, Size, and Capabilities			
ED visits	17,780	41,600	38,416
Admissions	6,310	13,676	12,445
Average daily census	227	200	182
Total staffed beds	322	283	258
Teaching hospital	0.59	0.27	0.22
Urban location	0.90	0.97	0.89
Trama center	0.11	0.64	0.61
Advanced cardiac care	0.74	0.70	0.58
Staffing			
ED staff per 1,000 ED visits	0.78	0.55	0.73
Nurses per 1,000 patient-days	6.20	5.45	5.63
Physicians per 1,000 patient-days	5.31	7.75	8.73
Hospitalists per 1,000 patient-days	0.17	0.29	0.40
Intensivists per 1,000 patient-days	0.08	0.16	0.20
Spending and Relative Outcomes			
Relative spending	1.13	1.01	1.00
Mortality rate	7.68	12.27	12.23
Readmissions rate	12.33	18.08	18.14
Payment and Organization			
Capitated lives covered		8,087	11,399
Network participant		0.51	0.46
Hospital system		0.75	0.62
HMO		0.19	0.20
PPO		0.21	0.19
ACO		0.04	0.09
Health IT Adoption			
Adoption by 2011		0.25	0.20
Adoption by 2012		0.61	0.56
Adoption by 2013		0.87	0.83
Adoption by 2014		0.93	0.87

Note: This table presents average characteristics of VA and non-VA hospitals. Non-VA hospital characteristics are further presented for the baseline sample and for the national average. The national average weights hospital characteristics by their yearly admissions in the American Hospital Association (AHA) Annual Survey. The average in the baseline sample weights hospital characteristics by rides in that sample, described in Appendix Table A.1. Hospital characteristics are described in further detail in Appendix A.5.

Table 6: Modal Hospital Mechanisms

	Dependent variable: 28-day mortality					
	(1)	(2)	(3)	(4)	(5)	(6)
	A: OLS					
Modal hospital	-0.005	-0.006		-0.012	-0.006	
	(0.001)	(0.001)		(0.005)	(0.001)	
× Adoption			-0.006			-0.008
			(0.001)			(0.003)
× No adoption			-0.006			-0.006
			(0.001)			(0.001)
	B: IV					
First stage	0.745	0.689		0.506	0.703	
	(0.011)	(0.008)		(0.026)	(0.007)	
Modal hospital	-0.015	-0.004		-0.011	-0.006	
	(0.009)	(0.006)		(0.034)	(0.005)	
× Adoption			-0.015			-0.015
			(0.006)			(0.019)
× No adoption			-0.005			-0.006
			(0.005)			(0.005)
Outcome mean	0.106	0.113	0.112	0.107	0.112	0.112
Observations	338,313	1,075,528	1,414,197	58,968	1,354,196	1,413,573
Fixed effects						
Hospital identities	Yes	Yes	No	Yes	Yes	No
Hospital ever adopted	N/A	N/A	Yes	N/A	N/A	Yes
Sample	IT adoption	No IT adoption	Full	ACO adoption	No ACO adoption	Full

Note: This table shows OLS and IV estimates of the effect of presenting to a veteran's modal hospital on 28-day mortality, depending on whether the modal hospital has adopted health IT or whether the modal hospital has joined an Accountable Care Organization (ACO). Columns 1 and 2 show results estimated in subsamples defined by whether the modal hospital has adopted health IT or not. Columns 5 and 6 show results estimated in subsamples defined by whether the modal hospital has joined an ACO or not. The first-stage and reduced-form equations for the IV estimation (Panel B) are given in Equations (10) and (11); while this table presents results with hospital fixed effects, results do not qualitatively depend on the inclusion of hospital fixed effects. Columns 3 and 6 present results estimated on the overall sample with interactions for adoption status; these specifications are described in detail in Appendix A.7. We include baseline controls defined in Appendix Table A.2. The overall sample is the same alternative sample designed to study choice among non-VA hospitals for patients with only non-VA utilization in the prior year. Details of the sample selection are given in Appendix Table A.13, Panel B.

Appendix

A.1 IV Validity

A.1.1 Exclusion Restriction

Under the standard assumptions for IV validity in Imbens and Angrist (1994), ambulance companies would be subject to the exclusion restriction, in Condition 1(ii), that they only affect outcomes by whether they transport patients to the VA, and not by other treatments that they may administer during the ambulance ride or by their choice of non-VA hospitals. Following Kolesar et al. (2015), we relax this assumption to allow for differences in potential treatments and non-VA hospital choices across ambulance companies but require that such differences that may affect outcomes are not systematically related to ambulance propensity to transport to the VA.

Specifically, we include controls $\mathbf{C}_i$ that are related to actions by the ambulance after pickup in the first-stage and reduced-form relationships:

$$\begin{aligned} D_i &= \pi_1 Z_i + \mathbf{X}_i^0 \delta_1 + \mathbf{C}_i \eta_1 + \zeta_{1,z(i)} + \varepsilon_{1,i}; \\ Y_i &= \pi_2 Z_i + \mathbf{X}_i^0 \delta_2 + \mathbf{C}_i \eta_2 + \zeta_{2,z(i)} + \varepsilon_{2,i}. \end{aligned}$$

Under each set of ambulance-related controls, we examine the stability of $\hat{\beta}_{IV} = \hat{\pi}_2 / \hat{\pi}_1$.

We consider four sets of controls in $\mathbf{C}_i$. First, we control for splines of ambulance charges reflected in their Medicare claims. Consistent with the health economics literature on productivity and the returns to spending (Doyle et al. 2015; Chandra et al. 2016), we consider charges incurred by the ambulance company as a sufficient statistic for the intensity of treatment during the ride.[38] Second, we control for splines of the mileage of the ride. Third, we control for indicators of the number of non-VA hospitals to which the ambulance company transports patients from a zip code.

Fourth, we control for average measures of non-VA hospitals who which the ambulance company delivers its patients. For each non-VA hospital h, we measure average mortality and spending outcomes $\overline{Y}_h$, among veterans outside of our benchmark analytic sample who *only* have non-VA prior utilization (Panel B of Appendix Table A.13). We also measure the share, w_{jh}, that each ambulance company j delivers patients to each non-VA hospital h, also among veterans with non-VA-only prior utilization. For each ride i, we then control for average non-VA hospital measures of mortality and spending, calculated as $\sum_h w_{j(i),h} \overline{Y}_h$, weighted by the hospital-specific shares of the assigned ambulance $j(i)$. As in Section 5.4, we use information on Medicare claims to infer non-VA hospital spending.

Appendix Table A.4 shows estimates of the VA effect on mortality and on spending, using the

[38] In principle, we also observe detailed CPT procedure codes for services rendered during the ambulance ride (e.g., supplemental oxygen, medications, or intravenous fluids). However, in 2002, Medicare changed to a simple payment arrangement that depended only on a few characteristics of the ride, such as ALS vs. BLS level, mileage, and the use of lights and sirens (Centers for Medicare & Medicaid Services 2002). Consistent with this payment policy, detailed CPT codes for extra services are usually missing in the claims data.

same baseline controls as in our benchmark analyses in Section 3 with the addition of various ambulance related controls. We find that results are highly robust to the addition of these controls.

A.1.2 Monotonicity

We test the monotonicity condition in Condition 1(iii) by tests standard in the judges-design literature that demonstrate a positive first-stage relationship across subgroups of observations (Arnold et al. 2018; Bhuller et al. 2020). We define eight pairs of subsamples based on several important patient characteristics: (i) age $\leq$ 80 years vs. age $>$ 80 years; (ii) white vs. non-white race; (iii) comorbidity count above vs. below median; (iv) either vs. neither mental illness or substance abuse present; (v) VA visits in the prior year above vs. below median; (vi) Advanced Life Support vs. Basic Life Support; (vii) prediction of VA user above vs. below median; and (viii) prediction of mortality above vs. below median.

Under monotonicity, we expect that an ambulance that has a higher propensity to transport veterans to the VA should weakly increase the probability of transport to the VA for any set of veterans. Specifically, using the set of observations $\mathcal{I}_m$ for each subsample m, we estimate a first-stage regression with respect to our baseline instrument, Z_i, from Equation (1):

$$D_i = \pi_1^m Z_i + \mathbf{X}_i^0 \delta_1^m + \zeta_{1,z(i)}^m + \varepsilon_{1,i}^m, \tag{A.1}$$

and we assess whether $\hat{\pi}_1^m \geq 0$.

We further assess monotonicity in each subsample m by constructing a "reverse-sample" instrument that only uses observations in the analytical sample (Step 6 in Appendix Table A.1) that are not in $\mathcal{I}_m$:

$$\tilde{Z}_i^{-m} = \frac{1}{\tilde{K}_{j(i)}^{-m}} \sum_{i' \in \tilde{\mathcal{I}}_{j(i)} \setminus \mathcal{I}_m} \frac{\mathbf{1}\left(k\left(i'\right) \neq k\left(i\right)\right) D_{i'}}{\tilde{N}_{k(i'),j(i)}}. \tag{A.2}$$

Within the *analytical* sample, $\tilde{\mathcal{I}}_j$ denotes the set of rides assigned to j, $\tilde{K}_j^{-m}$ is the number of patients assigned to ambulance j without characteristic m, and $\tilde{N}_{k,j}$ is the number of rides by patient k with ambulance j.[39] In each subsample m, we also perform first-stage regressions of the form in Equation (A.1) that use $\tilde{Z}_i^{-m}$ instead of Z_i as the instrument.

Recall that the baseline instrument, Z_i, is computed in the much larger sample of dually eligible veterans (Step 1 in Appendix Table A.1). Since the reverse-sample instruments are based on much smaller patient populations, they may be weaker predictors of underlying ambulance propensities to transport to the VA.

In Appendix Table A.5, we demonstrate a positive and statistically significant first-stage coefficient in every subsample and for both the baseline instrument and the reverse-sample instrument. Coefficient sizes are generally smaller for the reverse-sample instruments. In Appendix Table A.6,

[39] We use the analytical sample construct the reverse-sample instruments, so that the samples used to construct instruments are roughly the same between pairs of characteristics (e.g., subsamples for comorbidity count above vs. below median).

we show first-stage relationships using two other instruments that are both based on the smaller analytical sample. Specifically, we construct a "baseline" instrument, $\tilde{Z}_i$, and an "in-sample" instrument, $\tilde{Z}_i^m$, from the analytical sample:

$$\tilde{Z}_i = \frac{1}{\tilde{K}_{j(i)} - 1} \sum_{i' \in \tilde{\mathcal{I}}_{j(i)}} \frac{\mathbf{1}\left(k\left(i'\right) \neq k\left(i\right)\right) D_{i'}}{\tilde{N}_{k(i'),j(i)}}, \text{ and} \tag{A.3}$$

$$\tilde{Z}_i^m = \frac{1}{\tilde{K}_{j(i)}^m - 1} \sum_{i' \in \tilde{\mathcal{I}}_{j(i)} \cap \mathcal{I}_m} \frac{\mathbf{1}\left(k\left(i'\right) \neq k\left(i\right)\right) D_{i'}}{\tilde{N}_{k(i'),j(i)}}. \tag{A.4}$$

First-stage coefficients for these instruments are also all positive and statistically significant. They are similar in magnitude to the coefficients for the reverse-sample instruments, which suggests that lower signal-to-noise ratios due to smaller sample sizes explain much of decrease in coefficient magnitude for the reverse-sample instruments, compared to the baseline (overall-sample) instrument.

A.2 Statistical Tests of Hazard Functions

A.2.1 Potential Survival Rates and Hazard Rates

Following the notation in Section 4, let $s_{IV}\left(t;d\right) \equiv E\left[S_i\left(t;d\right) | i \in C\right]$ denote the IV estimands of the potential survival rates among compliers, where $d \in \{0,1\}$ indicates outcomes under VA care ($d = 1$) or non-VA care ($d = 0$), for each week $t \in \{0,1,\ldots,52\}$. We then define the corresponding estimands of the potential mortality *hazards* as follows:

$$h_{IV}\left(t;d\right) \equiv \frac{s_{IV}\left(t-1;d\right) - s_{IV}\left(t;d\right)}{s_{IV}\left(t-1;d\right)}.$$

We use two-stage least squares to construct estimates of the potential survivor fractions at each time horizon, $\hat{s}_{IV}\left(t;d\right)$ and then construct the corresponding potential hazard functions, $\hat{h}_{IV}\left(t;d\right)$. We also construct a set of 250 block bootstrap samples (selecting samples by zip code, with replacement), and for replication sample $r \in \{1,\ldots,R\}$, we construct $\hat{s}_{IV}^r\left(t;d\right)$ and $\hat{h}_{IV}^r\left(t;d\right)$. Using these samples we construct the mean estimated potential hazard for each week across the replications:

$$\overline{h}_{IV}^B\left(t;d\right) = \frac{1}{R}\sum_r \hat{h}_{IV}^r\left(t;d\right). \tag{A.5}$$

We also construct the standard deviation of the bootstrap-estimated potential hazard for each week:

$$\hat{\sigma}_{IV}^B\left(t;d\right) = \sqrt{\frac{1}{R-1}\sum_r \left[\hat{h}_{IV}^r\left(t;d\right) - \overline{h}_{IV}^B\left(t;d\right)\right]^2}. \tag{A.6}$$

We construct similar objects for potential survival and hazard rates under OLS: $\hat{s}_{OLS}\left(t;d\right)$ and $\hat{h}_{OLS}\left(t;d\right)$, respectively. Using the same set of block bootstrap samples, we compute $\hat{s}_{OLS}^r\left(t;d\right)$ and $\hat{h}_{OLS}^r\left(t;d\right)$ in each bootstrap replication sample r.

A.2.2 Test of Mortality Displacement

To detect "mortality displacement" (Schwartz 2000), in which deaths of VA patients are simply delayed, we test the joint null hypothesis that $h_{IV}(t;1) \leq h_{IV}(t;0)$ for all $t \geq 1$. This null hypothesis states that the mortality hazard under the VA never overtakes the mortality hazard under non-VA hospitals, even in later periods, and it is consistent with no mortality displacement.

Restating the null hypothesis as

$$H_{0,1} : h_{IV}(t;0) - h_{IV}(t;1) \geq 0, \text{ for all } t \geq 1, \tag{A.7}$$

we use estimates $\hat{h}_{IV}(t;0) - \hat{h}_{IV}(t;1)$ and consider the following test statistic of the null, based on Wolak (1987):

$$Q_1 \equiv \sum_{t=1}^{52} w_{1,t} \mathbf{1}\left(\hat{h}_{IV}(t;0) - \hat{h}_{IV}(t;1) < 0\right)\left(\hat{h}_{IV}(t;0) - \hat{h}_{IV}(t;1)(t)\right)^2, \tag{A.8}$$

where $w_{1,t}$ is a strictly positive weight. This test statistic penalizes only negative differences $\hat{h}_{IV}(t;0) - \hat{h}_{IV}(t;1) < 0$ that can be consistent with the null hypothesis that $\hat{h}_{IV}(t;0) - \hat{h}_{IV}(t;1) \geq 0$, for all $t \geq 1$, only by statistical noise.

To derive a critical value for Q_1, we use our bootstrap sample to form a set of recentered bootstrap estimates of the potential hazards at each week:

$$\begin{aligned} \tilde{h}^r_{IV}(t;0) &= \hat{h}^r_{IV}(t;0) - \overline{h}^B_{IV}(t;0); \\ \tilde{h}^r_{IV}(t;1) &= \hat{h}^r_{IV}(t;1) - \overline{h}^B_{IV}(t;1). \end{aligned}$$

We then construct the empirical distribution of the test statistic, in Equation (A.8), under the recentered bootstrap deviations:

$$Q^r_1 \equiv \sum_{t=1}^{52} w_{1,t} \mathbf{1}\left(\tilde{h}^r_{IV}(t;0) - \tilde{h}^r_{IV}(t;1) < 0\right)\left(\tilde{h}^r_{IV}(t;0) - \tilde{h}^r_{IV}(t;1)\right)^2. \tag{A.9}$$

We take the 95th percentile of this distribution as the critical value above which our test statistic Q_1 can reject the null hypothesis $H_{0,1}$, in Equation (A.7).

Following Wolak (1987), this distribution is formed under the data generating process implied by the "least favorable null" for testing joint inequality constraints (Perlman 1969). Specifically, we consider the least favorable data generating process that satisfies the null hypothesis H_0, in Equation (A.7), which is

$$\underline{H}_{0,1} : h_{IV}(t;0) - h_{IV}(t;1) = 0, \text{ for all } t \geq 1. \tag{A.10}$$

If we obtain a test statistic Q_1 with improbable negative deviations that reject the least favorable null hypothesis $\underline{H}_{0,1}$ in Equation (A.10), then we can also reject the null hypothesis $H_{0,1}$ in Equation (A.7).

We use the same weights $w_{1,t}$ in Equations (A.8) and (A.9) and set them as the inverse of the estimated sampling variance of the recentered deviations:

$$w_{1,t}^{-1} = \frac{1}{R-1} \sum_{r} \left(\tilde{h}_{IV}^{r}(t;0) - \tilde{h}_{IV}^{r}(t;1) \right)^2 . \tag{A.11}$$

These weights standardize the statistical distribution of $\hat{h}_{IV}(t;0) - \hat{h}_{IV}(t;1)$, so that the test statistic distribution can be considered as chi-squared. Although we use critical values derived from the bootstrap distribution, we find the scale of our test statistic to be more intuitive with this normalization.[40]

We show results in Panel A of Appendix Figure A.4. We find that Q_1 is within the distribution of bootstrapped values of Q_1^r. We therefore cannot reject the null of no mortality displacement.

A.2.3 Extended Test of IV Validity

In addition to standard tests of IV validity that are based on observable characteristics—including tests of balance in Section 3.2 and monotonicity in Appendix A.1.2—we develop a tractable extended test of IV validity using the insights in Balke and Pearl (1997) and Heckman and Vytlacil (2005, Proposition A.5) that are based on *potential outcomes*.

Kitagawa (2015) summarizes these insights as follows for a binary instrument $Z \in \{0,1\}$, a binary treatment $D \in \{0,1\}$ (increasing in probability with Z), and an outcome $Y \in \mathcal{Y}$. For any Borel set B in $\mathcal{Y}$, IV validity in Condition 1 implies that

$$\Pr(Y \in B, D = 1 | Z = 1) - \Pr(Y \in B, D = 1 | Z = 0) \geq 0; \tag{A.12}$$

$$\Pr(Y \in B, D = 0 | Z = 0) - \Pr(Y \in B, D = 0 | Z = 1) \geq 0. \tag{A.13}$$

Kitagawa (2015, Proposition 1.1) further states that tests of Equations (A.12) and (A.13) constitute the strongest possible tests of IV validity in the sense that no other feature of the data can contribute further to screening out invalid instruments.[41]

We note that, given the approach in Abadie (2002), testing Equations (A.12) and (A.13) is algebraically equivalent to testing, for all $B \subset \mathcal{Y}$,

$$\Pr(Y_i(0) \in B | i \in C) \geq 0; \tag{A.14}$$

$$\Pr(Y_i(1) \in B | i \in C) \geq 0. \tag{A.15}$$

[40] Wolak (1987) proposes to use an optimal minimum distance test statistic that would use the full covariance matrix of $\delta(t)$. We avoid this formulation due to finite-sample issues that would cause this covariance matrix to be poorly estimated by the full covariance matrix of $\tilde{\delta}^r(t)$, noted by Altonji and Segal (1996). Results are qualitatively similar when we choose a weight of $w_t = 1$ for all t, but we find that using w_t from Equation (A.11)—i.e., normalizing each $\delta(t)$ by its bootstrap standard error—affords greater power in rejecting the null. This approach is equivalent to our best estimate of a diagonal covariance matrix in place of the full covariance matrix.

[41] Chan et al. (2019) provides an applied example, in the setting of radiologists, in which standard monotonicity tests in Appendix A.1.2 are satisfied but a simple version of this extended test of validity is not satisfied. They find that radiologists who diagnose more cases with pneumonia do so in a wide range of subgroups of patients defined by observable characteristics (i.e., standard tests of monotonicity) but that the same radiologists who diagnose more cases with pneumonia are more likely to miss cases of pneumonia (i.e., $\Pr(|Y \in B, D = 0\ Z = 0) - \Pr(|Y \in B, D = 0\ Z = 1) < 0$).

Thus we use the Abadie (2002) approach to define a partition of mortality outcomes $\mathcal{Y}$ in terms of weekly hazard rates, by the date of death (if any) following the ambulance ride. Such a partition implies that potential hazard rates among compliers, $h_{IV}(t;d)$, are non-negative in every week $t \in \{1,\ldots,52\}$ under both VA assignment ($d=1$) and non-VA assignment ($d=0$).

That is, our extended test of IV validity amounts to testing the following joint null hypothesis of inequality constraints:

$$H_{0,2}: h_{IV}(t;d) \geq 0, \text{ for all } t \geq 1, d \in \{0,1\}. \tag{A.16}$$

Following a similar approach as for mortality displacement in Appendix A.2.2, our test statistic is

$$Q_2 \equiv \sum_{d=0}^{1}\sum_{t=1}^{52} w_{2,t}\mathbf{1}\left(\hat{h}_{IV}(t;d) < 0\right)\left(\hat{h}_{IV}(t;d)\right)^2,$$

where $w_{2,t}^{-1} = \left(\hat{\sigma}_{IV}^{B}(t;d)\right)^2$. We obtain the critical value for our test statistic by the distribution of recentered bootstrapped estimates, defined above. For the rth bootstrap replication, the test statistic is

$$Q_2^r \equiv \sum_{d=0}^{1}\sum_{t=1}^{52} w_{2,t}\mathbf{1}\left(\tilde{h}_{IV}^{r}(t;d) < 0\right)\left(\tilde{h}_{IV}^{r}(t;d)\right)^2.$$

We take the 95th percentile of the distribution of Q_2^r across replications $r \in \{1,\ldots,R\}$ as the critical value for Q_2. As above, this test of inequality constraints is based upon a least favorable null hypothesis. In this case, the least favorable null hypothesis is

$$\underline{H}_{0,2}: h_{IV}(t;d) = 0, \text{ for all } t \geq 1, d \in \{0,1\}. \tag{A.17}$$

We show results in Panel B of Appendix Figure A.4. We find that Q_2 is lower than any bootstrapped value of Q_2^r. This suggests not only that we cannot reject the null hypothesis $H_{0,2}$ in Equation (A.16), but also that the realized data are significantly more favorable than the least favorable null hypothesis $\underline{H}_{0,2}$ in Equation (A.17). In other words we can strongly reject the null that $h_{IV}(t;d) = 0$, for all $t \geq 1, d \in \{0,1\}$, which means that $h_{IV}(t;d) > 0$ for at least some $t \geq 1, d \in \{0,1\}$.

A.2.4 Tests of Hazard Rate Equality

We finally perform tests of the equality of hazard rates after the first week after the ambulance ride. Comparing hazard rates across different groups of veterans, we aim to shed light on heterogeneity in longer-term mortality risk across these groups. To define these tests generally, consider two sets of hazard rates, $h_1(t)$ and $h_2(t)$, for $t \geq 2$. We consider two types of null hypothesis.

First, we assess mean differences in hazard rates between $\{h_1(t)\}_t$ and $\{h_2(t)\}_t$, for $t \geq 1$, under the null hypothesis that the mean hazard rate is the same between the two sets:

$$H_{0,3}: \frac{1}{51}\sum_{t=2}^{52}\left(h_1(t) - h_2(t)\right) = 0. \tag{A.18}$$

We test this null hypothesis by comparing $\frac{1}{51}\sum_{t=2}^{52}\left(\hat{h}_1(t)-\hat{h}_2(t)\right)$ against the bootstrapped distribution of recentered differences. Specifically, for replication $r \in \{1,\ldots,R\}$, denote the bootstrap-estimate hazard rates of $(h_1(t), h_2(t))$ as $\left(\hat{h}_1^r(t), \hat{h}_2^r(t)\right)$. Define the recentered bootstrap hazard rate as

$$\begin{aligned}\tilde{h}_1^r(t) &\equiv \hat{h}_1^r(t) - \overline{h}_1^B(t) \text{ and}\\ \tilde{h}_2^r(t) &\equiv \hat{h}_2^r(t) - \overline{h}_2^B(t),\end{aligned}$$

where $\overline{h}_1^B(t) \equiv \frac{1}{R}\sum_r h_1(t)$ and $\overline{h}_2^B(t) \equiv \frac{1}{R}\sum_r h_2(t)$. The distribution of $\left\{\frac{1}{51}\sum_{t=2}^{52}\left(\tilde{h}_1^r(t)-\tilde{h}_2^r(t)\right)\right\}_r$ determines the two-sided critical values for the mean hazard difference. By construction, this distribution will have mean 0.

Second, we consider the joint null hypothesis that the difference between each pair of hazards is equal to 0:

$$H_{0,4}: h_1(t) - h_2(t) = 0, \text{ for all } t \geq 1. \tag{A.19}$$

Using estimates $\hat{h}_1(t) - \hat{h}_2(t)$, we construct the following test statistic:

$$Q_4(h_1(\cdot), h_2(\cdot)) \equiv \sum_{t=2}^{52} w_{4,t}\left(\hat{h}_1(t) - \hat{h}_2(t)\right)^2.$$

We compute the empirical distribution of Q_4 under the null hypothesis by using recentered differences $\tilde{h}_1^r(t) - \tilde{h}_2^r(t)$. Each bootstrap replication r yields

$$Q_4^r(h_1(\cdot), h_2(\cdot)) \equiv \sum_{t=2}^{52} w_{4,t}\left(\tilde{h}_1^r(t) - \tilde{h}_2^r(t)\right)^2.$$

We take the 95th percentile of the distribution of Q_4^r across replications $r \in \{1,\ldots,R\}$ as the critical value for Q_4. We set $w_{4,t}^{-1} = \frac{1}{R-1}\sum_r\left(\widetilde{h}_1^r(t) - \widetilde{h}_2^r(t)\right)^2$ to standardize the distribution of $\hat{h}_1(t) - \hat{h}_2(t)$.

In Appendix Figures A.5 and A.6, we consider five comparisons of hazard rates, for $t \geq 1$, under the null hypotheses of Equations (A.18) and (A.19), respectively. First, we test the null hypothesis that $h_{IV}(t;1) - h_{IV}(t;0) = 0$, for all $t \geq 1$. Under quasi-experimental assignment of compliers (Condition 1), we expect not to reject this null if longer-term hazard rates reflect underlying health. Second, we test the null hypothesis that $h_{OLS}(t;1) - h_{OLS}(t;0) = 0$, for all $t \geq 1$. While we show stability of OLS results in Figure 2, this test may reveal differences in underlying health between veterans assigned to the VA and those assigned to a non-VA hospital that are not captured by observable patient characteristics.

Third, we test the null hypothesis that $h_{IV}(t;1) - h_{OLS}(t;1) = 0$, for all $t \geq 1$. This reveals differences in underlying health between compliers and VA-assigned veterans, which includes compliers and always takers. Fourth, we similarly test the null hypothesis that $h_{IV}(t;0) - h_{OLS}(t;0) = 0$, for all $t \geq 1$. This reveals differences in underlying health between compliers and non-VA-assigned veterans, which includes compliers and never takers.

A.3 Non-Complier Characteristics

In this appendix section, we describe a simple approach to calculate characteristics of non-compliers, following Dahl et al. (2014), and we discuss results. In our approach, we first residualize the leave-out ambulance propensity to transport to the VA, Z_i, by our key controls, $\left(z\left(i\right), \mathbf{X}_i^0\right)$. Denote this residual as Z_i^*. We categorize always takers as rides with Z_i^* below the 20th percentile that still went to the VA ($D_i = 1$). We categorize never takers as rides with Z_i^* above the 80th percentile that still did not go to the VA ($D_i = 0$).

Among each group of always takers and never takers, we compute characteristics along the same dimensions as those in our compliers analysis, in Table 4. Specifically, for each characteristic, we compute mean values among the group of always takers and among the group of never takers, and we compare these means with the overall mean by a ratio. We compute standard errors of these means by drawing bootstrapped samples, blocked by zip code, and repeating this procedure with each bootstrapped sample.

As shown in Appendix Table A.7, we mostly find results that are consistent with our earlier results of complier characteristics and the fact that the majority of non-compliers are never takers: For many characteristics, those that are more common among compliers tend to be more common among always takers and less common among never takers. Compared to the overall population, always takers are more likely to be Black and have lower income. Always takers are more likely to have mental illness, and they have a slightly higher rate of substance abuse, though the latter is not statistically significant. Always takers are more likely to have prior VA ED visits and less likely to have prior non-VA ED visits. However, both always takers and never takers as defined by this methodology have slightly higher predicted mortality.

A.4 Marginal and Average Treatment Effects

Consider the probability of going to the VA as a function of our instrument Z_i and key controls $\left(z\left(i\right), \mathbf{X}_i^0\right)$: $P\left(Z_i\right)$, where we have omitted the key controls for brevity. Following Heckman and Vytlacil (2005), we can state the treatment rule as

$$D_i = \mathbf{1}\left(P\left(Z_i\right) \geq U_i\right), \tag{A.20}$$

where U_i is uniformly distributed in the interval $(0, 1)$. Individuals with low U_i relative to $\underline{p} \equiv \arg\min_i P\left(Z_i\right)$ are always takers, while individuals with high U_i relative to $\overline{p} \equiv \arg\max_i P\left(Z_i\right)$ are never takers.

In this appendix, we estimate two objects relative to selection, as defined by $U_i \sim U\left(0, 1\right)$. The marginal treatment effect (MTE) for rides with $U_i = u$ is

$$MTE\left(u\right) \equiv E\left[Y_i\left(1\right) - Y_i\left(0\right) | U_i = u\right].$$

The average treatment effect (ATE) is

$$ATE = \int_0^1 MTE\,(u)\,du.$$

We estimate $MTE(u)$, for $u \in \left[\underline{p}, \overline{p}\right]$, using variation in the propensity of ambulances to transport to the VA. We estimate the ATE by extrapolating $MTE\,(u)$ to $u \in [0,1]$ with a control function approach.

A.4.1 Marginal Treatment Effects

We first estimate marginal treatment effects using a local instrumental variables approach that exploits outcomes along the distribution of ambulance propensity to transport to the VA. The intuition for this approach is that $MTE\,(u)$ can be stated as

$$MTE\,(u) = \frac{\partial}{\partial p} E\left[\,Y_i \middle|\, P\,(Z_i) = u\right].$$

That is, if mortality decreases linearly with ambulance propensity to transport to the VA, then the data would be consistent with constant treatment effects. On the other hand, if mortality decreases at a faster rate for lower $P\,(Z_i)$, then the data would suggest "selection on gains," in which veterans who are more likely to benefit from VA care are also more likely to be transported to the VA given a set of ambulances. The visual IV relationship in Appendix Figure A.1 suggests a slightly convex shape in the relationship between mortality and $P\,(Z_i)$, which implies selection on gains.

We proceed with estimating a flexible relationship between Y_i and $P\,(Z_i)$ as follows. We compute $P\,(Z_i) = \hat{D}_i$ from the first-stage Equation (3). We then residualize $\hat{D}_i$ by baseline controls, defined in Appendix Table A.2, and denote the residual as $\hat{D}_i^*$. We similarly residualize Y_i by baseline controls and denote the residual as Y_i^*. For interpretation, we set Y_i^* and $\hat{D}_i^*$ to have the same respective means as Y_i and D_i. A regression of Y_i^* on $\hat{D}_i^*$ yields a point estimate that is numerically identical to the IV estimate $\hat{\beta}_{IV}$.[42]

Rather than fitting a straight line through points $\left(\hat{D}_i^*, Y_i^*\right)$, we fit a flexible function with Gaussian basis splines with four knots (k_1, k_2, k_3, k_4) corresponding to the 5th, 35th, 65th, and 95th percentiles of $\hat{D}_i^*$. Specifically, for each ride i, we form five basis functions

$$f_n\,(p) = \exp\left(-\,(k_n - k_{n-1})\,(p - c_n)^2\right),$$

where $c_n = \frac{1}{2}\,(k_{n-1} + k_n)$, $k_0 = \min \hat{D}_i^*$, and $k_5 = \max \hat{D}_i^*$. We regress

$$Y_i^* = \sum_{n=1}^{5} \gamma_n f_n\left(\hat{D}_i^*\right) + \varepsilon_i$$

and form a flexible prediction $\hat{Y}^*\,(p) = \sum_{n=1}^{5} \hat{\gamma}_n f_n\,(p)$.

[42] This regression corresponds to the indirect least squares version of IV and is also numerically identical to the visual IV coefficient that corresponds to the two-stage least squares version of IV.

This prediction yields a convenient analytical derivative for the MTE

$$\widehat{MTE}(u) = \sum_{n=1}^{5} \hat{\gamma}_n f_n'(u) = -\sum_{n=1}^{5} 2(k_n - k_{n-1})^2 (u - c_n) \hat{\gamma}_n f_n(u).$$

For each $p \in [0.05, 0.20]$, corresponding to the range of $\hat{D}_i^*$, we compute 95% confidence intervals of $\hat{Y}^*(p)$ by taking the standard deviations of $\hat{Y}^*(p)$ across 50 bootstrapped iterations (with samples drawn by zip code, with replacement). Similarly, for each $u \in [0.05, 0.20]$, we compute 95% confidence intervals of $\widehat{MTE}(u)$ by taking the standard deviations of $\widehat{MTE}(u)$ across these same bootstrapped iterations. We display both $\hat{Y}^*(p)$ and $\widehat{MTE}(u)$ in Appendix Figure A.7.

A.4.2 Average Treatment Effect

In order to estimate the ATE, we adopt a control function model in order to extrapolate treatment effects to non-compliers. Specifically, we model potential outcomes as

$$E[Y_i(d) | U_i = u] = \alpha_d + \gamma_d (J(u) - \mu_J) + \mathbf{X}_i^0 \delta + \zeta_{z(i)}, \tag{A.21}$$

where $d \in \{0, 1\}$ and $u \in (0, 1)$. $J(u)$ is a strictly increasing, continuous function that maps selection to potential outcomes, and $\mu_J \equiv E[J(U_i)]$. Since $E[J(u) - \mu_J] = 0$, we can interpret $\alpha_1 - \alpha_0$ as the ATE. Kline and Walters (2019) show that the control function model in Equations (A.20) and (A.21) can also rationalize the Imbens and Angrist (1994) LATE that we estimate in Section 3, regardless of the choice of $J(u)$.[43]

For our baseline specification, we adopt the linear selection function of $J(u) = u$ from Olsen (1980), which we use with Equation (A.21) to state the following expectation, conditional on the first-stage error $\varepsilon_{1,i}$ from Equation (3):[44]

$$\begin{aligned} E\left[Y_i | D_i = d, \varepsilon_{1,i} = \varepsilon\right] &= \alpha_d + \gamma_d E\left[J(u) - \mu_J | D_i = d, \varepsilon_{1,i} = \varepsilon\right] + \mathbf{X}_i^0 \delta + \zeta_{z(i)} \\ &= \alpha_d - \gamma_d \frac{\varepsilon}{2} + \mathbf{X}_i^* \delta + \zeta_{z(i)}. \end{aligned} \tag{A.22}$$

This expectation corresponds to the following regression:

$$Y_i = \alpha_\Delta D_i + \gamma_0 \left(-\frac{\hat{\varepsilon}_{1,i}}{2}\right) + \gamma_\Delta \left(-\frac{\hat{\varepsilon}_{1,i}}{2}\right) D_i + \mathbf{X}_i^0 \delta + \zeta_{z(i)} + v_i, \tag{A.23}$$

plugging in the estimated first-stage residual $\hat{\varepsilon}_{1,i}$ from Equation (3). We can compute the ATE from

[43] Kline and Walters (2019) show algebraic equivalence between the control function LATE implied by Equation (A.21), $\underline{p}$, and $\overline{p}$, when the instrument is binary and there are no controls. They also generalize their result for multivalued instruments. With controls, the equivalence may not hold in the standard regression approach in which controls are treated as additively separable but will hold under a propensity score approach.

[44] To see this, assume that the first stage regression in Equation (3) estimates a well-behaved $P(Z_i) \in (0, 1)$ such that $D_i = P(Z_i) + \varepsilon_{1,i}$. Define $\lambda_d(p) \equiv E[J(U_i) - \mu_J | D_i = d, P(Z_i) = p]$. We have $\lambda_1(p) = \frac{p}{2} - \frac{1}{2} = \frac{p-1}{2}$, and $\lambda_0(p) = \frac{p+1}{2} - \frac{1}{2} = \frac{p}{2}$. Note that $\lambda_d(p) = \frac{p-d}{2} = \frac{-\varepsilon}{2}$, where $\varepsilon \equiv d - p$. This implies that $\varepsilon_{1,i} = D_i - P(Z_i)$ is a sufficient statistic for $(D_i, P(Z_i))$, and we can state the expectation $J(U_i) - \mu_J$ conditional on $\varepsilon_{1,i}$: $E\left[J(U_i) - \mu_J | \varepsilon_{1,i} = \varepsilon\right] = -\frac{\varepsilon}{2}$.

this equation as $\alpha_\Delta = \alpha_1 - \alpha_0$. We estimate Equation (A.23) by OLS to yield $\hat{\alpha}_\Delta = -0.037$, slightly smaller in magnitude than the LATE estimate of -0.041 from Section 3. For inference on the difference between the ATE and the LATE, we recover a numerically equivalent LATE with the following control function regression:[45]

$$Y_i = \beta_{CF} D_i + \gamma \hat{\varepsilon}_{1,i} + \mathbf{X}_i^0 \delta_0 + \zeta_{0,z(i)} + v_i, \tag{A.24}$$

where $\hat{\beta}_{CF}$ is estimated by OLS and is numerically equivalent to $\hat{\beta}_{IV}$ estimated by two-stage least squares. For each bootstrapped replication, we estimate both the ATE, $\hat{\alpha}_1 - \hat{\alpha}_0$, and its difference with the LATE, $\hat{\beta}_{CF}$, in order to obtain standard errors on both the ATE and the difference.

We also examine semiparametric specifications that allow for flexible relationships between the first-stage residual and the structural error term. These alternative specifications allow nonlinear relationships of $g_d(\varepsilon) \equiv E\left[\varepsilon_{0,i} \middle| D_i = d, \varepsilon_{1,i} = \varepsilon\right]$, where $\varepsilon_{0,i}$ is the structural error term in Equation (2). Specifically, we estimate regressions of the following form:

$$Y_i = \alpha_\Delta D_i + g_0\left(\hat{\varepsilon}_{1,i}\right)(1 - D_i) + g_1\left(\hat{\varepsilon}_{1,i}\right) D_i + \mathbf{X}_i^0 \delta + \zeta_{z(i)} + v_i, \tag{A.25}$$

where $g_d\left(\hat{\varepsilon}_{1,i}\right)$, $d \in \{0,1\}$, are flexible functions of the first-stage residual that are non-zero when $D_i = 0$ and $D_i = 1$, respectively. To estimate $g_d\left(\hat{\varepsilon}_{1,i}\right)$, $d \in \{0,1\}$, we use a vector of restricted cubic spline functions or Gaussian basis functions, with three or five knots. Ensuring that $E\left[g_d\left(\hat{\varepsilon}_{1,i}\right)\right] = 0$ by demeaning each spline or basis function, we can interpret α_Δ as the ATE.

In Appendix Table A.8, we show estimates of the ATE and the ATE-LATE difference. ATE estimates are all smaller in magnitude than the LATE estimate from Section 3. We compute standard errors on this difference with 50 bootstrapped iterations (selecting samples by zip code, with replacement). The ATE-LATE difference is statistically significant in our baseline specification in Equation (A.23), though they are not statistically significant in the semiparametric specifications.

A.5 Hospital Characteristics

In this appendix, we provide further details on hospital characteristics that we use in our heterogeneity analyses in Section 5.3. For each zip code and year, we use characteristics of the closest VA hospital and a weighted average of the characteristics of associated non-VA hospitals. Weights for each non-VA hospital are proportional to the number of ambulance rides originating from a given zip code to the hospital in that year. Unless otherwise noted, characteristics are observed at the hospital-year level.

We use the American Hospital Association (AHA) Annual Survey to collect the following VA and non-VA hospital characteristics at the hospital-year level: (i) number of ED visits; (ii) number of facility admissions; (iii) number of available hospital beds; (iv) teaching hospital status; (v) trauma

[45] Blundell and Matzkin (2014) attribute the first proof of this equivalence between control function and two-stage least squares approaches to estimating the LATE to Telser (1964).

center status; (vi) number of privileged ED staff, which we use to construct ED staff per 100 ED visits given (i); (vii) number of full-time registered nurses, which we use to construct nurses per 100 admissions given (ii); (viii) number of privileged hospitalists, which we use to construct hospitalists per 100 admissions; and (ix) number of privileged intensivists, which we use to construct intensivists per 100 admissions given (ii).

We construct a measure of advanced cardiac care, which we define as either the capability to perform interventional cardiac catheterization or cardiac surgery as measured by the AHA Annual Survey (at the hospital-year level) or listing as an ST-Elevation Myocardial Infarction (STEMI) center by the American Heart Association (at the hospital level). We record whether each hospital is certified as a Primary Stroke Center according to the Joint Commission, the American Heart Association, and the American Stroke Association (at the hospital level).

For VA hospitals, we form measures of relative spending from the average cost of an inpatient-day, available from the VA Health Economics Resource Center (HERC). For non-VA hospitals, we use data from `Data.Medicare.gov` on Medicare spending per beneficiary at the hospital level. Similarly, we obtain mortality and readmission rates from `Data.Medicare.gov` for non-VA hospitals and from the VA's Strategic Analytics for Improvement and Learning (SAIL). For each hospital's mortality rate, we take the mean of all available 30-day mortality rates, including disease-specific rates such as heart attack and pneumonia; we form similar means for each hospital's readmission rate based on available 30-day readmission rates, including disease-specific rates. Because some years are missing mortality or readmission rates, for each hospital and rate, we first form averages across years at the hospital level.

For measures of non-VA hospital organization, we use AHA Annual Survey measures of network status, hospital system status, and health maintenance organization (HMO) affiliation. We also obtain whether the hospital participates in an Affordable Care Organization (ACO) from the Medicare Shared Savings Program (MSSP) ACO provider-level dataset. We measure health IT adoption for each hospital and year from electronic health record certified products measured in `healthIT.gov`. Additional characteristics in Table 5 are also obtained from the AHA Annual Survey: (i) average daily census, (ii) urban location (i.e., hospital is not classified as either "micro" or rural), (iii) capitated lives covered, and (iv) Preferred Provider Organization (PPO) affiliation.

A.6 OLS Heterogeneity in Station-Specific VA Advantage

In this appendix, we describe analyses related to OLS heterogeneity in the station-specific VA advantage and validating this heterogeneity with our quasi-experiment. As in our heterogeneity analyses in Section 5.3, we assign each zip code z to a VA station $\ell(z)$ based on the station that the most veterans living in that zip code use. This assignment of zip codes to VA stations matches station catchment areas for 92% of zip codes.

In separate OLS regressions, we estimate the VA advantage for each station ℓ as β_ℓ in

$$Y_i = \beta^\ell_{OLS} D_i + \mathbf{X}_i^0 \delta^\ell + \zeta^\ell_{z(i)} + \varepsilon_i, \tag{A.26}$$

using ambulance rides i such that the zip code $z(i)$ maps to station ℓ (i.e., $\ell(i) \equiv \ell(z(i)) = \ell$). The ride-weighted variance of $\hat{\beta}^\ell_{OLS}$ is 3.4×10^{-4}, while the ride-weighted variance of the sampling error for each $\hat{\beta}^\ell_{OLS}$ is 2.1×10^{-4}. This implies a sampling-error-adjusted, ride-weighted variance of β^ℓ_{OLS} of $A = (3.4 - 2.1) \times 10^{-4} = 1.4 \times 10^{-4}$, or a standard deviation of β^ℓ_{OLS} of $\sqrt{A} = 0.012$.

In Appendix Figure A.8, we plot the distribution of $\hat{\beta}^\ell_{OLS}$ for 32 stations with at least 5,000 rides, forming a smple of 276,483 rides. We also plot the empirical Bayes posteriors for all stations, which we calculate as follows:

$$\tilde{\beta}^\ell_{OLS} = (1 - B_\ell)\hat{\beta}^\ell_{OLS} + B_\ell \hat{\beta}_{OLS}, \tag{A.27}$$

where $B_\ell = \frac{V_\ell}{V_\ell + A}$ is the shrinkage factor based on V_ℓ, which is the variance of the sampling error for station ℓ, and A, which is the variance of the prior distribution of β^ℓ_{OLS}. $\hat{\beta}_{OLS} = -0.024$ is the overall OLS estimate reported in Section 3.3. This figure shows that essentially all stations exhibit a VA advantage, at least when estimated by OLS.

We evaluate whether differences in $\tilde{\beta}^\ell_{OLS}$ imply differences in the treatment effects identified by our quasi-experiment. As a first analysis, we divide stations into two groups depending on whether $\tilde{\beta}^\ell_{OLS}$ is above- or below-median. We estimate by two-stage least squares $\hat{\beta}_{IV}$, based on Equations (3) and (4), separate IV estimates for ambulance rides belonging to each of these two groups. $\hat{\beta}_{IV}$ estimated for stations with below-median (i.e., larger in magnitude) $\tilde{\beta}^\ell_{OLS}$ is 0.030 larger in magnitude than the same estimate for stations with above-median (i.e., smaller in magnitude) $\tilde{\beta}^\ell_{OLS}$. However, the difference is imprecise, with a bootstrapped standard error of 0.051.

For a more systematic validation of $\tilde{\beta}^\ell_{OLS}$, in the spirit of Angrist et al. (2017), we conduct a pooled analysis by indirect least squares. Specifically, denoting demeaned $\tilde{\beta}^\ell_{OLS}$ as $\tilde{\beta}^{\ell*}_{OLS}$, we estimate

$$Y_i = \beta D_i + \gamma D_i \times \tilde{\beta}^{\ell(i)*}_{OLS} + \mathbf{X}_i^0 \delta + \zeta_{z(i)} + \varepsilon_i,$$

where we instrument D_i and $D_i \times \tilde{\beta}^{\ell(i)*}_{OLS}$ by Z_i and $Z_i \times \tilde{\beta}^{\ell(i)*}_{OLS}$. This regression reveals an imprecise and wrong-signed result of $\hat{\gamma} = -0.790$ (s.e., 1.351). The overall imprecision of these results suggests that there is little signal that we can validate in the heterogeneity across station-specific OLS measures of the VA advantage. The more precise results in Section 5.3 also suggest little meaningful heterogeneity along binary characteristics of VA and non-VA hospitals in a given zip code.

A.7 Modal-Hospital Mechanisms

In Section 5.5, we investigate the role of health IT and integrated care in improving outcomes for patients with prior care at a given non-VA hospital. As described in Appendix A.5, we measures dates of hospital health IT adoption or ACO participation. During our sample period a sizable proportion of hospitals adopted health IT and, to a much lesser extent, participated in an ACO. In Figure 6, we

show that the survival effect of a veteran being transported to his modal hospital emerges after the passage the HITECH Act of 2009. This law led to a rapid rise in electronic medical record systems in US hospitals, which had previously been close to absent among non-VA hospitals.

To investigate this further, we focused on four subsamples defined by whether or not each veteran's modal hospital had adopted health IT, at the time of his ambulance ride, and similarly by whether or not each veteran's modal hospital had joined an ACO. In each of these subsamples, we performed the same IV regression of the effect of transport to a veteran's modal hospital. Results are shown in Table 6, Columns 1, 2, 4, and 5. We obtain all of these results after adding hospital fixed effects in the first-stage and reduced-form regressions in Equations (10) and (11), respectively. Results are qualitatively unchanged regardless of their inclusion.

In Columns 3 and 6 of Table 6, we also perform regressions in the overall sample (described in Panel B of Appendix Table A.13). We maintain all of the interactions implicit in our subsample results except that we allow hospital group fixed effects to remain constant before and after adoption of health IT or an ACO. We do so with the following control function approach. First, we estimate a first-stage regression that interacts everything with adoption status, except for fixed effects for hospital groups, $g\left(h\left(i\right)\right)$, defined by whether a hospital ever adopts health IT or an ACO:

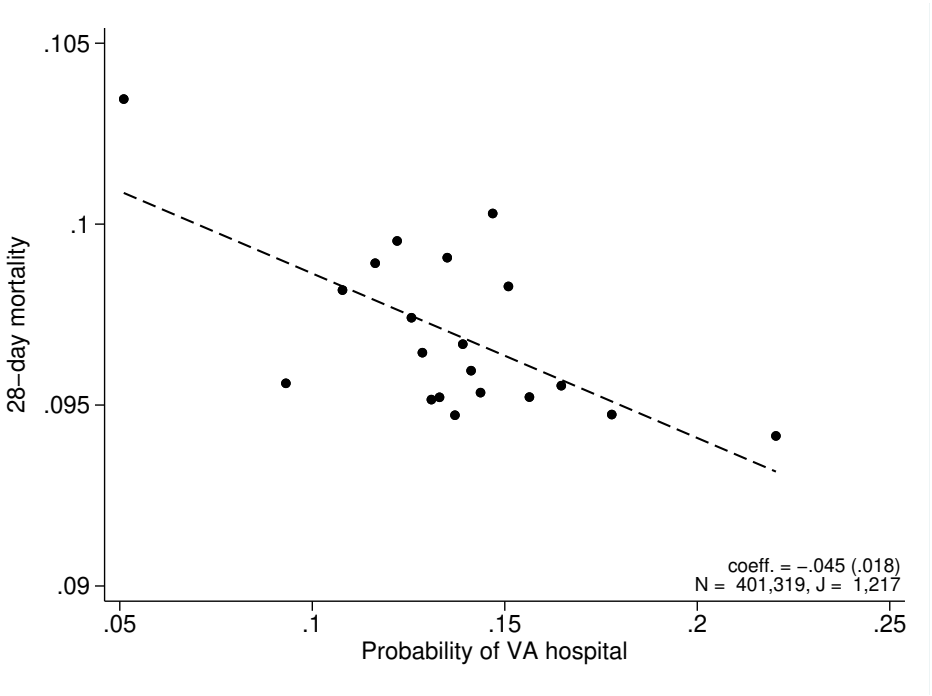
$$D_i^m = \sum_{a \in \{0,1\}} \mathbf{1}\left(\text{Adopted}_i = a\right)\left(\pi_{1,a}^m Z_i^m + \gamma_{1,a}^m \overline{Z}_i^m + \mathbf{X}_i^0 \delta_{1,a}^m + \zeta_{1,z(i),a}^m\right) + \xi_{1,g(h(i))}^m + \varepsilon_{1,i}^m. \tag{A.28}$$

We then take estimated first-stage residuals $\hat{\varepsilon}_{1,i}^m$ and include them in an interacted control-function model:

$$Y_i = \sum_{a \in \{0,1\}} \mathbf{1}\left(\text{Adopted}_i = a\right)\left(\beta_a D_i^m + \gamma_a \hat{\varepsilon}_{1,i}^m + \mathbf{X}_i^0 \delta_a + \zeta_{z(i),a}\right) + \xi_{g(h(i))} + \epsilon_i. \tag{A.29}$$

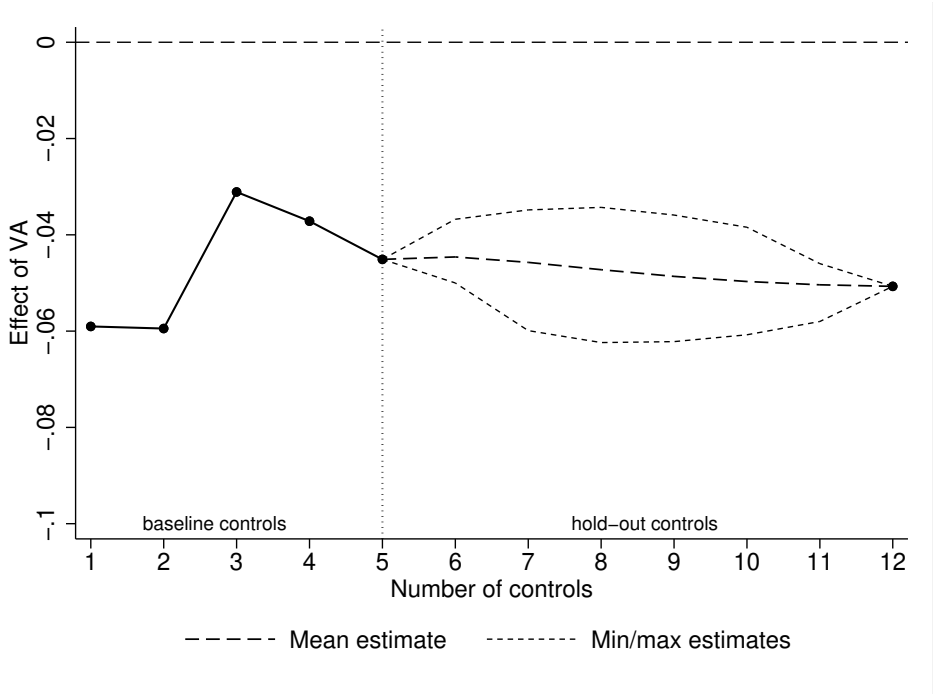
As with our other control-function regressions, we compute standard errors by 50 bootstrapped iterations, drawing samples by zip code blocks, with replacement.

Figure A.1: Visual IV



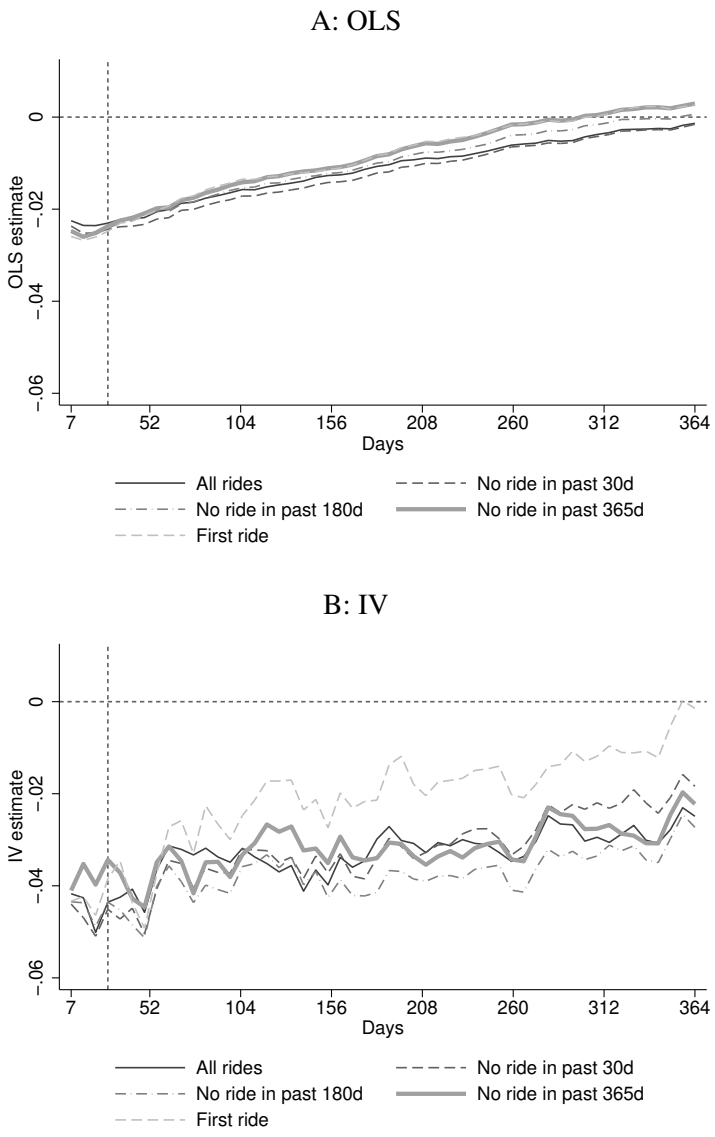
Note: This figure shows the visual IV plot corresponding to our baseline IV regression of the effect of the VA on 28-day mortality. For each bin of the instrument, which is the ambulance leave-out propensity to arrive at a VA hospital, we plot the mean 28-day mortality on the y-axis and the probability that the index patient arrives at a VA hospital on the x-axis. VA arrival predictions correspond to a first-stage regression in Equation (3), and mortality predictions correspond to a reduced-form regression in Equation (4). The best-fit line in the visual IV plot replicates the IV estimate of the effect of the VA on 28-day mortality, which we perform to obtain the standard error (in parentheses). This IV regression uses $401,319$ observations and $1,217$ combinations of ambulance company identifiers and Dartmouth Atlas Hospital Referral Regions (HRRs). The baseline sample selection is given in Appendix Table A.1. Controls include patient zip code dummies, ALS/BLS dummies, source of the ambulance ride, time categories, and patient prior utilization, which are detailed in Appendix Table A.2.

Figure A.2: Combinations of Controls



Note: This figure shows IV estimates of the VA effect on 28-day mortality on the y-axis, from Equation (2), varying the number of controls included in the IV regression. Numbered incremental controls correspond to categories or subcategories of variables that are presented in order in Appendix Tables A.2 and A.3. All specifications include the five baseline controls. Therefore, the figure represents $5 + (2^7 - 1) = 132$ specifications. For each number of controls n for $n > 5$, we consider "7 choose $n - 5$" specifications. The mean IV estimate is shown with a dashed line; the minimum and maximum IV estimates are shown with a short dashed line. We use our baseline sample, described in Appendix Table A.1.

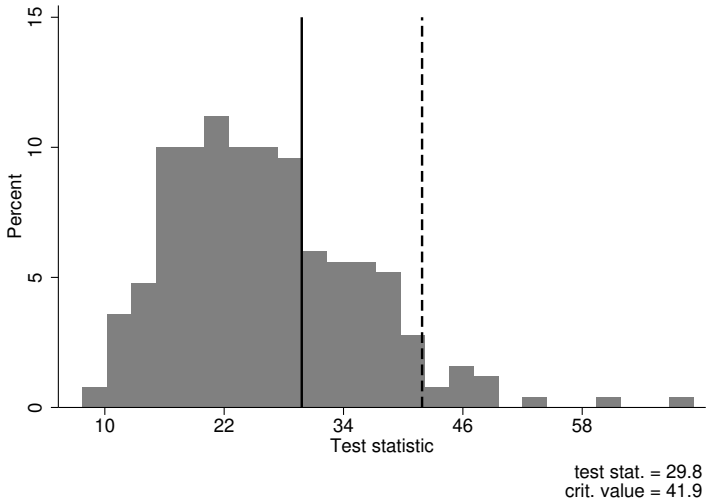
Figure A.3: Treatment Effects by Time and Sample



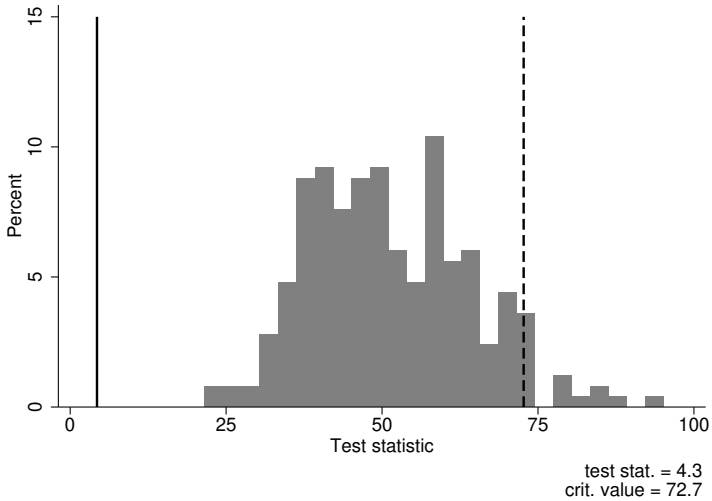
Note: This figure shows mortality treatment effects over varying days since the ambulance ride and in varying samples restricting by prior rides. "Days" indicate one-week intervals from the ambulance ride. Panel A shows OLS results corresponding to Equation (6). Panel B shows IV results corresponding to Equation (5). The vertical dashed line indicates treatment effects on 28-day mortality, our baseline outcome. All regressions use a sample of ambulance rides with no prior ride in the last year.

Figure A.4: Joint Inequality Constraints

A: No Mortality Displacement

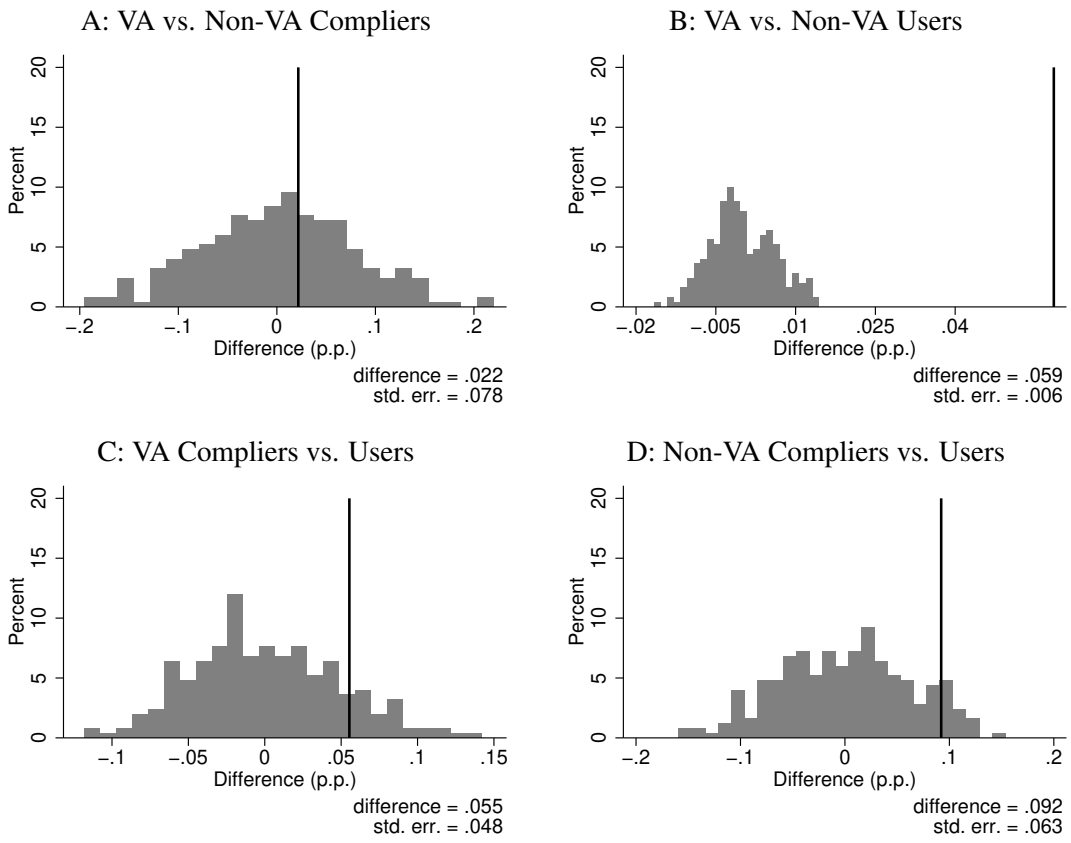


B: No Negative Hazard Rate



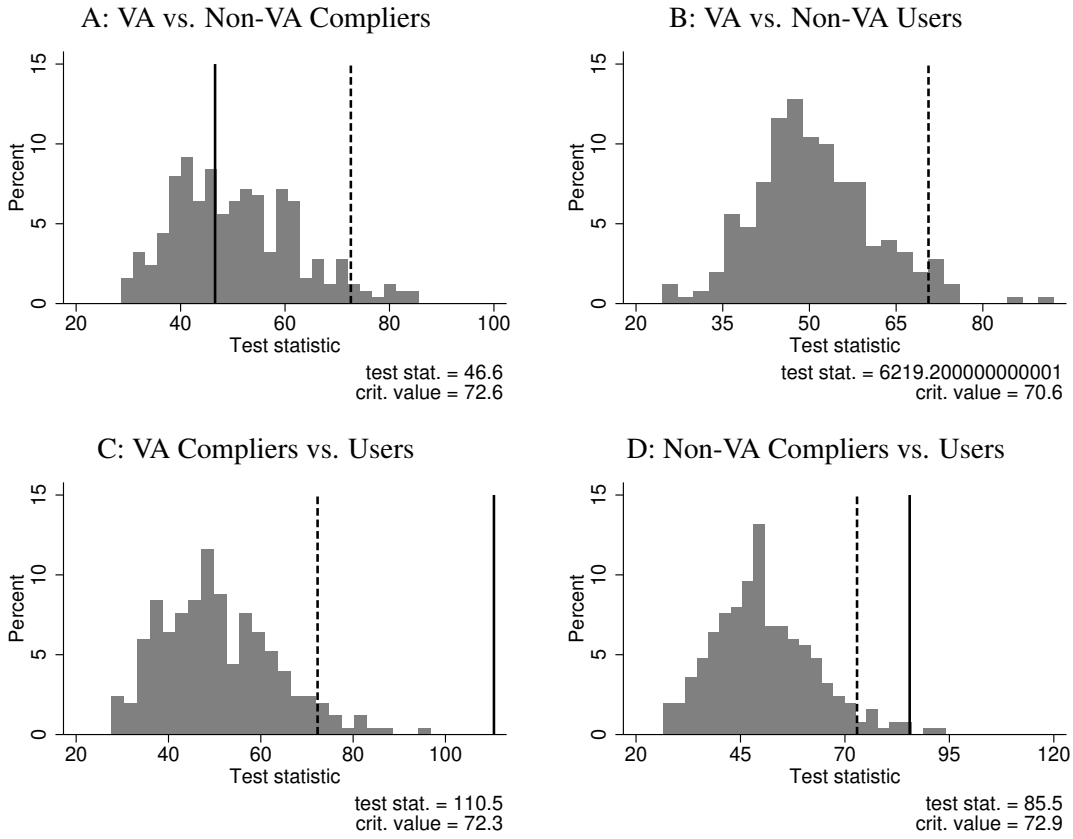
Note: This figure shows the test statistic for joint inequality constraints and bootstrapped-generated distributions of the test statistic under the least favorable version of the null hypothesis. Panel A shows the joint inequality test of no mortality displacement, as defined by null hypothesis in Equation (A.7). Panel B shows the joint inequality test of no negative hazard rates, as defined by the null hypothesis in Equation (A.16). The test statistic for both tests is shown as a solid vertical line. The one-sided critical value, or 95th percentile of the bootstrapped distribution of the test statistic under the least favorable version of the null hypothesis, is shown as a dashed vertical line. Details of the test statistic and the bootstrap procedure for Panels A and B are given in Appendices A.2.2 and A.2.3, respectively.

Figure A.5: Mean Hazard Differences



Note: This figure shows tests of equality of mean hazard rates for different sets of hazard rates, as defined by the null hypothesis in Equation (A.18). Each panel corresponds to a comparison between sets of hazards corresponding to VA or non-VA compliers or users. Details of the statistical procedure are given in Appendix A.2.4. Hazard rates for compliers are estimated by two-stage least squares and denoted in the appendix by $\hat{h}_{IV}(t;d)$, where $d = 1$ for compliers assigned to the VA and $d = 0$ for compliers assigned to a non-VA hospital. Hazard rates for users are estimated by OLS and denoted by $\hat{h}_{OLS}(t;d)$, where d similarly denotes VA users ($d = 1$) vs. non-VA users ($d = 0$). The solid black line shows the test statistic, and the histogram shows the distribution of bootstrapped test statistics under the null hypothesis. Bootstrapped standard errors are given in the caption.

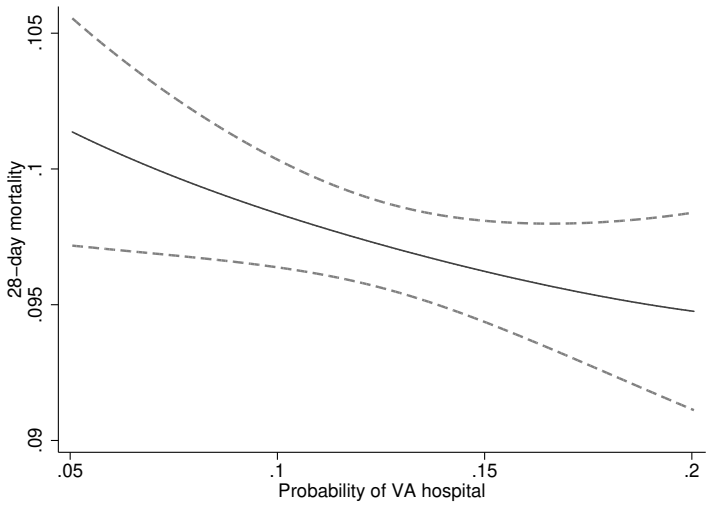
Figure A.6: Joint Equality Constraints



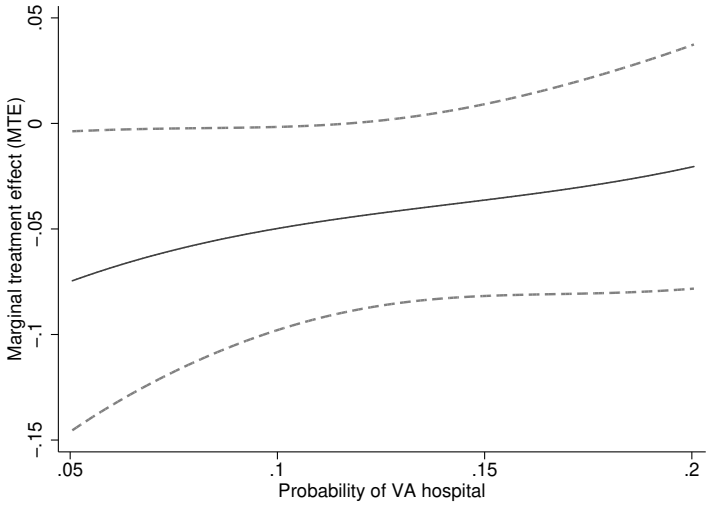
Note: This figure shows tests of joint equality of hazard rates for different sets of hazard rates, as defined by the null hypothesis in Equation (A.19). Each panel corresponds to a comparison between sets of hazards corresponding to VA or non-VA compliers or users. Details of the statistical procedure are given in Appendix A.2.4. Hazard rates for compliers are estimated by two-stage least squares and denoted in the appendix by $\hat{h}_{IV}(t;d)$, where $d = 1$ for compliers assigned to the VA and $d = 0$ for compliers assigned to a non-VA hospital. Hazard rates for users are estimated by OLS and denoted by $\hat{h}_{OLS}(t;d)$, where d similarly denotes VA users ($d = 1$) vs. non-VA users ($d = 0$). The solid line shows the test statistic, the histogram shows the distribution of bootstrapped test statistics under the null hypothesis, and the dashed line shows the one-sided 95th percentile critical value.

Figure A.7: Marginal Treatment Effects

A: Flexible Visual IV



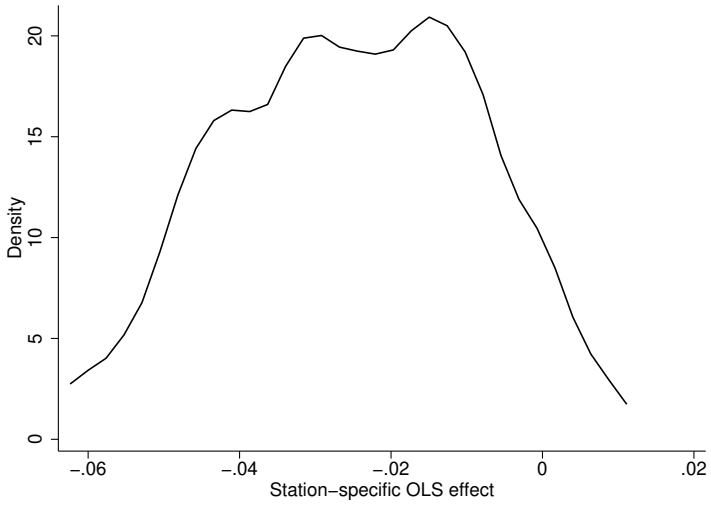
B: Marginal Treatment Effects



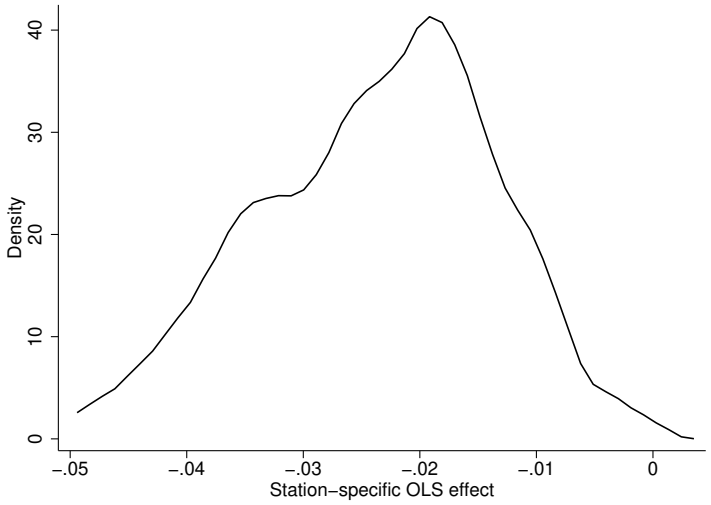
Note: This figure shows a flexible fit of the IV relationship between 28-day mortality and the ambulance propensity to transport to a VA hospital. Panel A shows the visual IV relationship with residual 28-day mortality on the *y*-axis and residual probability of being transported to a VA hospital on the *x*-axis. Both objects are residualized by baseline controls, described in Appendix Table A.2. The probability of being transported to a VA hospital is calculated from the first-stage relationship in Equation (3). The data underlying the fit in Panel A are similar to those in the linear visual IV plot in Appendix Figure A.1. The fit is based on five Gaussian basis splines. Panel B shows the implied marginal treatment effects, which are the analytical derivatives at each point on the fit in Panel A. 95% confidence intervals are calculated by 50 bootstrapped interations (drawn by zip codes, with replacement). Details are given in Appendix A.4.

Figure A.8: Station-Specific OLS Estimates of VA Advantage

A: Stations with at Least 5,000 Rides

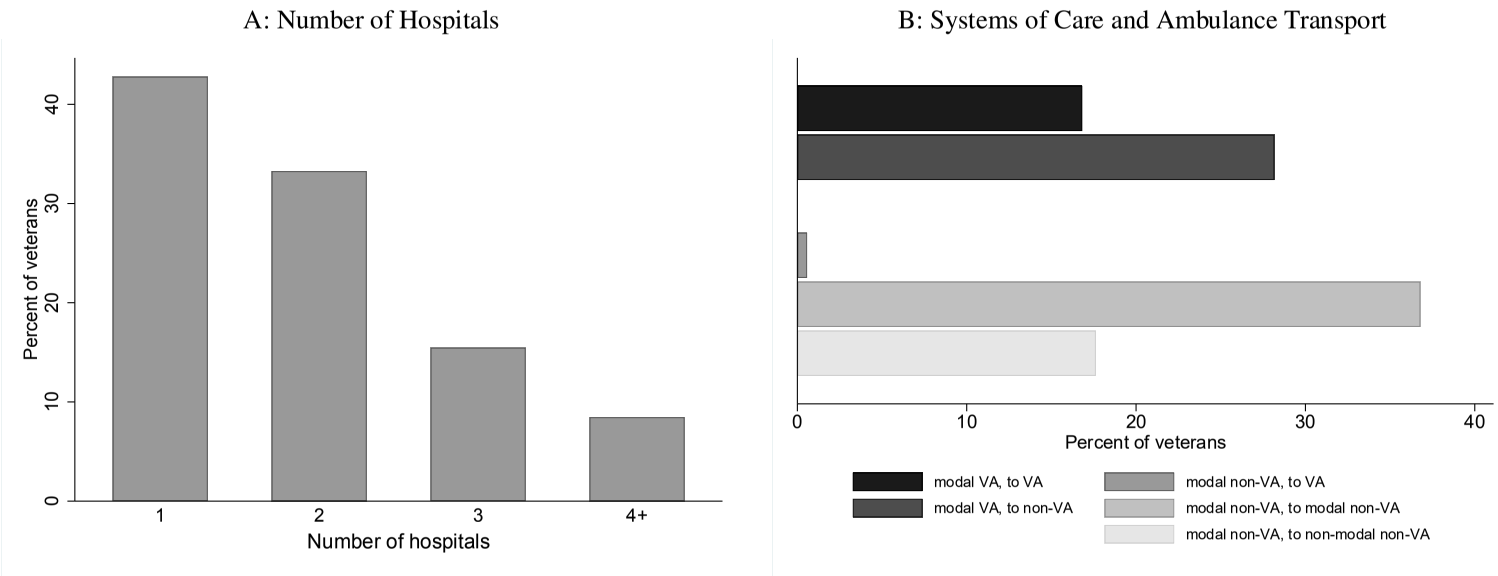


B: All Stations, Shrunken Estimates



Note: Panel A of this figure shows the kernel density distribution of station-specific OLS estimates of the VA advantage, or $\hat{\beta}^{\ell}_{OLS}$ estimated from Equation (A.26) for rides corresponding to each station. We include estimates from 32 stations with at least 5,000 rides, comprising a sample of 276,483 rides. Panel B of this figure shows the kernel density distribution of empirical Bayes posteriors of the station-specific OLS estimates of the VA advantage. These posteriors are given by $\tilde{\beta}^{\ell}_{OLS}$ in Equation (A.27). The figure displays posteriors from all 94 stations in our baseline sample in Appendix Table A.1, comprising 401,319 rides.

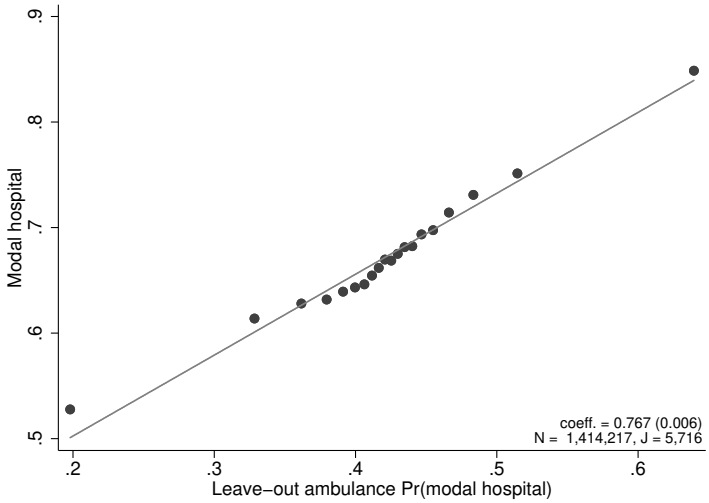
Figure A.9: Sources of Prior Utilization



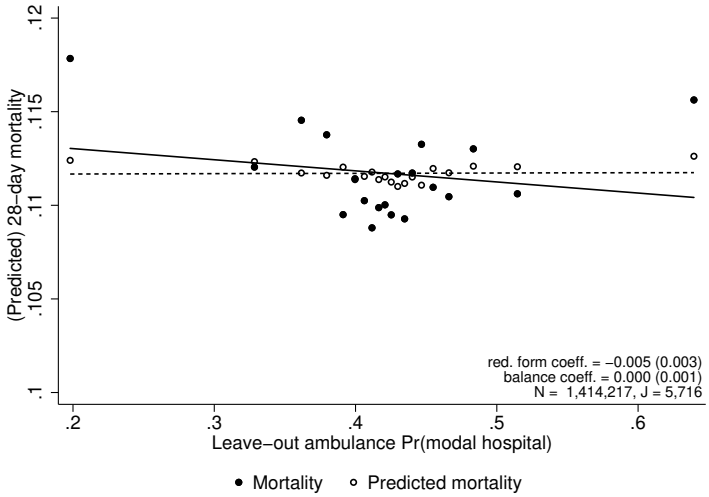
Note: This figure shows patterns of prior utilization and ambulance transport among a sample of patients who have some prior utilization either at the VA or affiliated with a non-VA hospital. Panel A shows the percentage of patients in this sample who utilize care associated with different numbers of hospitals. Panel B shows ambulance transport patterns to either the VA or a non-VA hospital depending on whether a patient's modal hospital in prior utilization was associated with the VA or with a non-VA hospital. If the patient's modal hospital utilization was at a non-VA hospital, the figure also shows the percentage of patients who were transported to their modal non-VA hospital or another non-VA hospital. The sample selection for this group of patients is given in Appendix Table A.13.

Figure A.10: Modal Hospital First Stage, Balance, and Reduced Form

A: First Stage

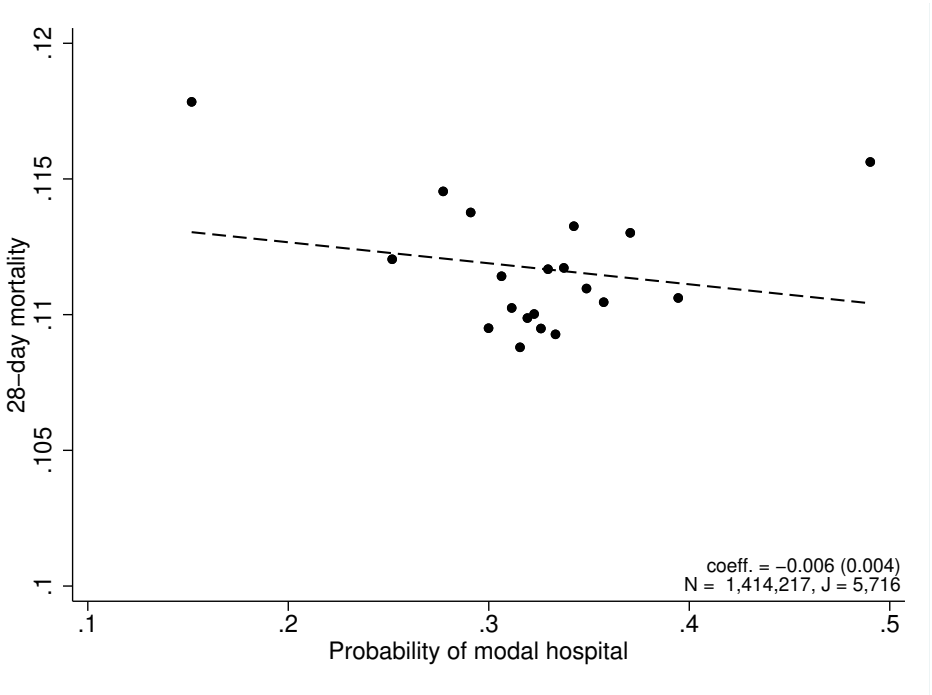


B: Balance and Reduced Form



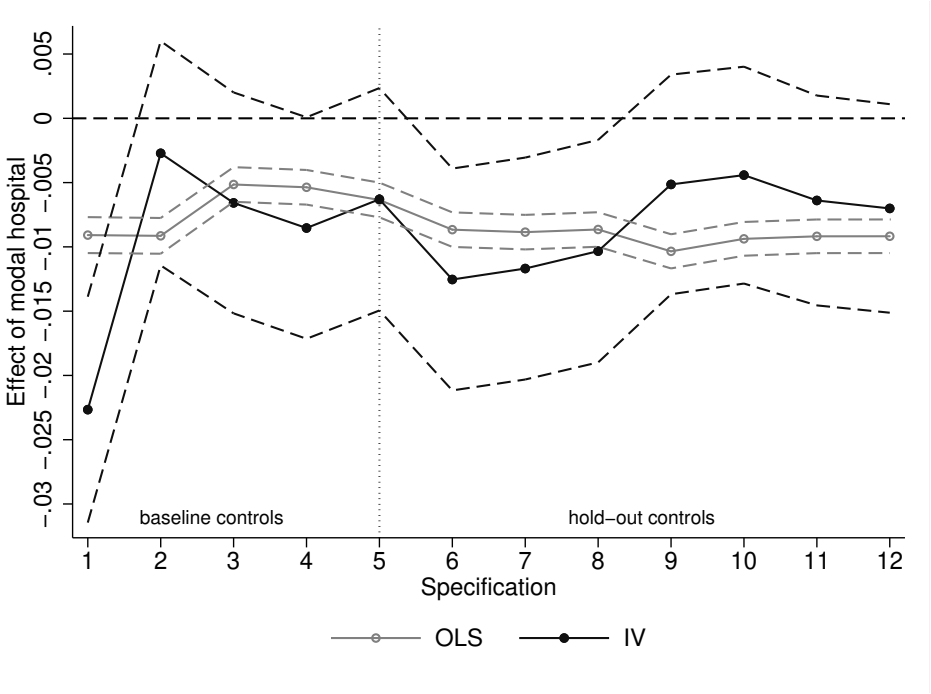
Note: Panel A shows a binned scatterplot of arrival at the veteran's modal hospital against the ambulance leave-out propensity to arrive at that hospital on the x-axis. The figure is a graphical representation of the first-stage regression in Equation (10). Panel B shows binned scatterplots of 28-day mortality and predicted 28-day mortality on the y-axis against the ambulance leave-out propensity to arrive at the veteran's modal hospital on the x-axis. Mortality bin means are shown in solid circles, while predicted mortality bin means are shown in hollow circles. The figure represents the reduced-form regression in Equation (11) and the corresponding balance regression replacing mortality with predicted mortality. The sample includes $1,414,217$ ambulance rides and $5,716$ combinations of ambulance company identifiers and Dartmouth Atlas Hospital Referral Regions (HRRs). The sample includes patients who have some utilization affiliated with a non-VA hospital and no utilization at the VA in the prior year. The selection details of this sample is given in Appendix Table A.13. Controls include patient zip code dummies, ALS/BLS dummies, source of the ambulance ride, time categories, and patient prior utilization, which are detailed in Appendix Table A.2.

Figure A.11: Modal Hospital Visual IV



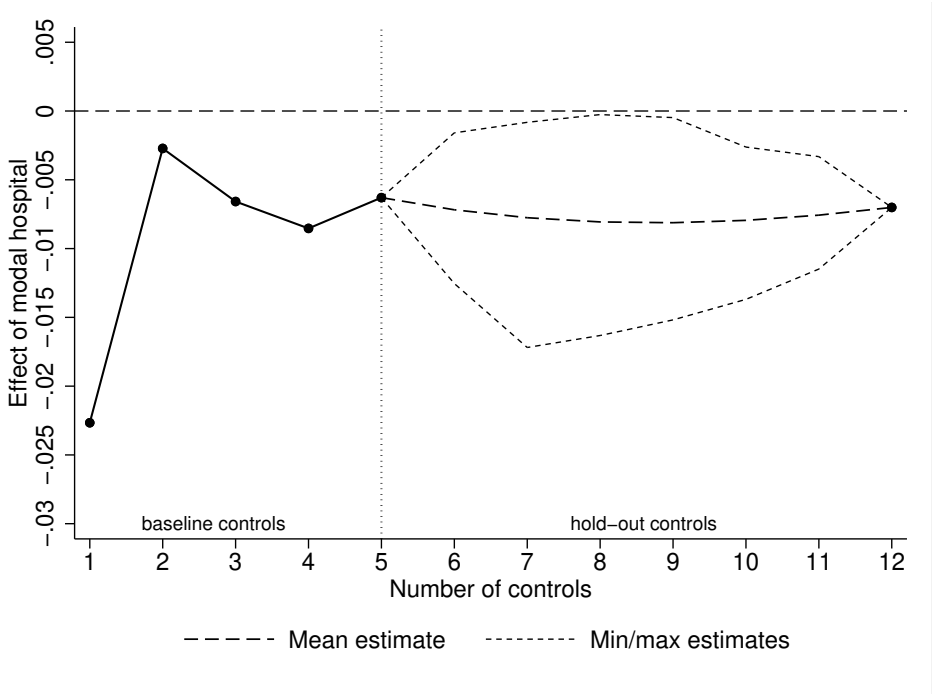
Note: This figure shows the visual IV plot corresponding to the IV regression of the effect of arrival at a patient's modal hospital on 28-day mortality. For each bin of the instrument, which is the ambulance leave-out propensity to arrive at the patient's modal hospital, we plot the mean 28-day mortality on the y-axis and the probability that the index patient arrives at his modal hospital on the x-axis. Modal hospital arrival predictions correspond to a first-stage regression in Equation (10), and mortality predictions correspond to a reduced-form regression in Equation (11). The best-fit line in the visual IV plot replicates the IV estimate of the effect of arrival at a patient's modal hospital on 28-day mortality, which we perform to obtain the standard error (in parentheses). This IV regression uses $1,414,217$ observations and $5,716$ combinations of ambulance company identifiers and Dartmouth Atlas Hospital Referral Regions (HRRs). We use the sample of non-VA-only utilizers, given in Appendix Table A.13. Controls include patient zip code dummies, ALS/BLS dummies, source of the ambulance ride, time categories, and patient prior utilization, which are detailed in Appendix Table A.2.

Figure A.12: Modal Hospital OLS and IV Specifications



Note: This figure shows the effect of arrival at a patient's modal hospital on 28-day mortality estimated from OLS and IV specifications, with progressive sets of controls. Numbered incremental controls correspond to categories or subcategories of variables that are presented in order in Appendix Tables A.2 and A.3. Estimates are shown along solid lines, while 95% confidence intervals are shown in dashed lines. All specification control for hospital identities and use the sample of non-VA-only utilizers, given in Appendix Table A.13.

Figure A.13: Modal Hospital Combinations of Controls



Note: This figure shows IV estimates of the effect of arrival at a patient's modal hospital on mortality on the y-axis, with first-stage and reduced-form Equations (10) and (11), varying the number of controls included in the IV regression. Control variables are detailed in Appendix Tables A.2 and A.3. All specifications include the five baseline controls. Therefore, the figure represents $5 + \left(2^7 - 1\right) = 132$ specifications. For each number of controls n for $n > 5$, we consider "7 choose $n-5$" specifications. The mean IV estimate is shown with a dashed line; the minimum and maximum IV estimates are shown with a short dashed line. We use the sample of non-VA-only utilizers, given in Appendix Table A.13.

Table A.1: Selection of Baseline Sample

Sample step	Description	Rides	Patients	Ambulance companies	Hospitals	
					VA	Non-VA
1. Build initial sample of ambulance rides to EDs from January 1, 2001, to December 31, 2014.	Require ED visit within 24 hours after ambulance ride, non-missing demographic data, enrollment in Medicare Parts A and B for at least one year, date of death (if non-missing) weakly after the ambulance ride.	8,952,884	2,898,667	183,693	127	7,816
2. Clean sample	Drop rides linked to more than one ED visit (i.e., visits in different hospitals), with patients younger than 20 years or older than 99 years, with missing Health Referral Region, with missing ambulance diagnosis code, or from VA New Orleans (destroyed in 2005 due to Hurricane Katrina).	8,828,997	2,862,557	180,320	125	7,744
3. Distance restrictions	Drop patients who do not live within 20 miles of a VA hospital and within 20 miles of a non-VA hospital. Drop rides to a hospital over 50 miles from the patient's home.	3,465,588	1,118,302	14,662	118	3,071
4. Ambulance restrictions	Drop rides by an ambulance company with fewer than 20 patients in a given zip code. Drop rides by an ambulance company with less than 5% of rides in a given zip code to a VA hospital. Drop rides from zip codes with only one remaining ambulance company.	1,051,093	365,163	1,217	100	1,577
5. Prior utilization restriction	Drop rides for patients with no VA utilization (inpatient, ED, or primary care) in the prior year.	491,193	188,299	1,217	99	1,404
6. Prior ride restriction	Drop rides for patients with a prior ride within 30 days.	401,319	188,299	1,217	99	1,386

Note: This table details selection steps to create the baseline sample. At each step, the table lists the number of ambulance rides, patients, ambulance companies, and VA and non-VA hospitals. Table 1 shows average patient characteristics among observations at each sample step.

Table A.2: Baseline Control Variables

Category	Subcategory	Variables
Location (1,633 indicators)	Zip code (1,630 indicators)	Zip code indicators (1,630 indicators)
	Pickup source (3 indicators)	Indicators for whether pickup is from residence, residential (including domiciliary, custodial facility), skilled nursing facility, or scene of accident (omitted category)
Ambulance service (3 indicators)		Indicators for whether ambulance is ALS special (CPT codes A0427, A0330, A0370), ALS non-special (CPT codes Q3019, A0368, A0328), ALS level 2 (CPT code A0433), or BLS (omitted category; CPT codes A0429, A0362, A0322)
Time categories (173 indicators)		Day of the week (6 indicators); Month-year interactions (167 indicators)
Prior utilization (6 indicators)		Indicators for utilization in prior year of Medicare primary care, VA primary care utilization, Medicare ED, VA ED, Medicare inpatient, and VA inpatient services

Note: This table describes baseline controls variables, denoted as $\left(z\left(i\right), \mathbf{X}_i^0\right)$ in Condition 1 and throughout the text. We consider our quasi-experiment to be conditional on these variables, and we include these variables as controls in all of our analyses. Numbers of non-collinear indicators are given in parentheses.

Table A.3: Hold-Out Control Variables

Category	Subcategory	Variables
Patient background (60 variables)	Demographics (30 indicators)	Age: 5-year age bins from 20-64 years, 2-year age bins from 65-100 years (26 indicators); Male gender; Race: indicators for white, Black, Hispanic, and Asian/other (omitted category)
	Socioeconomic status, combat history, and eligibility (22 indicators)	Terciles of income and net worth (4 indicators); Period of combat: WWII, Korean, Vietnam, other (omitted category) (3 indicators); Indicator for aid and attendance for in-home care; Priority group indicators (7 indicators); Service connection: service connected, not service connected, or non-veteran/other (omitted category) (2 indicators); 6 missing indicators for each of the above characteristics
	Extended prior utilization (8 variables)	Counts of VA primary care visits, outpatient visits, ED visits, and inpatient visits in prior year; Analogous counts of Medicare visits in prior year
Prior diagnoses (93 indicators)		31 Elixhauser indicators (dividing hypertension indicator into 2 indicators for complicated and uncomplicated hypertension), in four categories: present in VA data only, present in Medicare data only, and present in both VA and Medicare data ($31 \times 3 = 93$ indicators)
3-digit ambulance diagnosis codes (641 indicators)		3-digit ambulance diagnosis (ICD-9) codes (641 indicators)
Co-rider characteristics (33 variables)	Co-rider baseline controls (12 variables)	Co-rider pickup source proportions (3 variables); Co-rider ambulance service proportions (3 variables); Co-rider prior utilization proportions (6 variables)
	Co-rider hold-out controls (21 variables)	Co-rider average continuous age; Co-rider proportion male gender; Co-rider race proportions (3 variables); Co-rider 1-digit ambulance code proportions (15 variables); Co-rider average predicted mortality

Note: This table describes hold-out control variables. These variables are used to test robustness of our findings, particularly in Figures 2, A.2, A.13, and A.12. Numbers of non-collinear indicators or variables are given in parentheses.

Table A.4: Robustness of Exclusion Restriction

	(1)	(2)	(3)	(4)	(5)	(6)
	A: Dependent variable: 28-day mortality					
VA hospital	-0.053	-0.042	-0.045	-0.045	-0.045	-0.049
	(0.019)	(0.018)	(0.018)	(0.018)	(0.018)	(0.020)
Outcome mean	0.097	0.097	0.097	0.097	0.097	0.097
Observations	401,319	401,319	401,319	401,319	401,319	401,319
	B: Dependent variable: 28-day spending					
VA hospital	-2,245	-2,583	-2,748	-2,501	-2,569	-2,559
	(899)	(832)	(880)	(816)	(822)	(992)
Outcome mean	12,173	12,173	12,173	12,173	12,173	12,173
Observations	401,319	401,319	401,319	401,319	401,319	401,319
Ambulance charges splines	Yes	No	No	No	No	Yes
Non-VA hospitals in chosen set	No	Yes	No	No	No	Yes
Mileage splines	No	No	Yes	No	No	Yes
Out-of-sample non-VA mortality	No	No	No	Yes	No	Yes
Out-of-sample non-VA spending	No	No	No	No	Yes	Yes

Note: This table presents IV estimates of the effect of the VA on 28-day mortality (Panel A) and on 28-day spending (Panel B). In each panel, each column involves including a set of controls for ambulance actions on the specific ride (flexible functions of the charges incurred by the ambulance company, flexible functions of the mileage driven by the ambulance company) and for non-VA hospitals chosen by the ambulance company (non-VA hospitals chosen by the ambulance company in rides originating from the same zip code and "out-of-sample" averages of non-VA hospital mortality and spending). "Out-of-sample" refers to averages that are computed using patients outside of the main analytical sample (Appendix Table A.1) because they have no VA utilization in the prior year; specifically, they are computed using patients with only non-VA utilization in the prior year (Panel B of Appendix Table A.13). Regressions are run on the main analytical sample. Further details are given in Appendix A.1.1.

Table A.5: Monotonicity Tests

First stage sample	Observations	VA share	Instrument: Baseline	Instrument: Reverse-sample
Age ≤ 80	239,611	0.347	0.931	0.497
			(0.038)	(0.022)
Age > 80	161,707	0.305	0.789	0.456
			(0.041)	(0.022)
White	314,064	0.304	0.821	0.221
			(0.037)	(0.016)
Non-white	87,176	0.426	0.992	0.596
			(0.068)	(0.041)
Comorbidity count (high)	167,332	0.292	0.758	0.427
			(0.041)	(0.019)
Comorbidity count (low)	233,987	0.358	0.938	0.553
			(0.039)	(0.027)
Mental illness or substance abuse	188,961	0.354	0.931	0.514
			(0.040)	(0.024)
No mental illness or substance abuse	212,358	0.309	0.815	0.456
			(0.037)	(0.020)
VA visits in prior year (high)	183,087	0.508	1.038	0.710
			(0.050)	(0.035)
VA visits in prior year (low)	218,232	0.181	0.718	0.284
			(0.031)	(0.014)
Advanced Life Support	274,690	0.301	0.836	0.249
			(0.036)	(0.018)
No Advanced Life Support	126,616	0.393	0.840	0.301
			(0.048)	(0.032)
Predicted VA user (high)	200,659	0.543	1.113	0.865
			(0.054)	(0.055)
Predicted VA user (low)	200,660	0.117	0.559	0.218
			(0.030)	(0.011)
Predicted mortality (high)	200,659	0.328	0.835	0.368
			(0.036)	(0.019)
Predicted mortality (low)	200,660	0.333	0.898	0.502
			(0.046)	(0.024)
Instrument sample			Dual eligibles	Analytical sample

Note: This table presents first-stage coefficients on different subsamples of patients. For each subsample, we present results for two different instruments: (i) the baseline leave-out instrument, Z_i, given in Equation (1) and calculated from observations among dually eligible veterans (Step 1 of Appendix Table A.1), and (ii) a reverse-sample instrument, $\tilde{Z}_i^{-m}$, given in Equation (A.2) and calculated from observations in the analytical sample (Step 6 of Appendix Table A.1) that are outside of the regression subsample. Each regression uses baseline controls defined in Appendix Table A.2. Further details are given in Appendix A.1.2.

Table A.6: Monotonicity Tests (Continued)

First stage sample	Observations	VA share	Instrument	
			Baseline	In-sample
Age ≤ 80	239,611	0.347	0.586	0.525
			(0.021)	(0.020)
Age > 80	161,707	0.305	0.494	0.394
			(0.023)	(0.022)
White	314,064	0.304	0.504	0.513
			(0.019)	(0.020)
Non-white	87,176	0.426	0.676	0.440
			(0.032)	(0.033)
Comorbidity count (high)	167,332	0.292	0.493	0.438
			(0.020)	(0.021)
Comorbidity count (low)	233,987	0.358	0.583	0.504
			(0.022)	(0.020)
Mental illness or substance abuse	188,961	0.354	0.592	0.518
			(0.021)	(0.020)
No mental illness or substance abuse	212,358	0.309	0.501	0.426
			(0.020)	(0.020)
VA visits in prior year (high)	183,087	0.508	0.691	0.572
			(0.026)	(0.021)
VA visits in prior year (low)	218,232	0.181	0.421	0.445
			(0.018)	(0.021)
Advanced Life Support	274,690	0.301	0.523	0.518
			(0.020)	(0.021)
No Advanced Life Support	126,616	0.393	0.531	0.433
			(0.025)	(0.024)
Predicted VA user (high)	200,659	0.543	0.743	0.619
			(0.028)	(0.021)
Predicted VA user (low)	200,660	0.117	0.331	0.423
			(0.016)	(0.027)
Predicted mortality (high)	200,659	0.328	0.513	0.458
			(0.020)	(0.019)
Predicted mortality (low)	200,660	0.333	0.570	0.479
			(0.023)	(0.021)
Instrument sample			Analytical sample	Analytical sample

Note: This table presents first-stage coefficients on different subsamples of patients. For each subsample, we present results for two different instruments: (i) the baseline leave-out instrument, $\tilde{Z}_i$, given in Equation (1), and (ii) a in-sample instrument, $\tilde{Z}_i^m$, given in Equation (A.2) and calculated from leave-out observations in the same regression subsample. Both instruments are calculated using observations in the analytical sample (Step 6 of Appendix Table A.1). Each regression uses baseline controls defined in Appendix Table A.2. Further details are given in Appendix A.1.2.

Table A.7: Always-Taker and Never-Taker Characteristics

	Always takers		Never takers	
	Mean	Ratio	Mean	Ratio
Male	0.961	1.00	0.965	1.00
	(0.002)	[0.99 - 1.00]	(0.001)	[1.00 - 1.00]
Age	75.6	0.99	76.3	1.00
	(0.158)	[0.99 - 1.00]	(0.153)	[1.00 - 1.01]
Black	0.222	1.14	0.184	0.95
	(0.012)	[1.02 - 1.26]	(0.010)	[0.85 - 1.05]
Income	$18,039	0.86	$22,397	1.07
	($200)	[0.84 - 0.88]	($232)	[1.05 - 1.09]
Rural zip code	0.064	1.27	0.053	1.04
	(0.015)	[0.67 - 1.87]	(0.011)	[0.62 - 1.46]
Residential source	0.685	0.97	0.667	0.95
	(0.011)	[0.94 - 1.00]	(0.009)	[0.92 - 0.97]
Comorbidity count	5.85	0.95	6.44	1.05
	(0.046)	[0.94 - 0.97]	(0.032)	[1.04 - 1.06]
Mental illness	0.469	1.10	0.420	0.98
	(0.006)	[1.07 - 1.13]	(0.004)	[0.97 - 1.00]
Substance abuse	0.150	1.04	0.137	0.95
	(0.005)	[0.97 - 1.10]	(0.004)	[0.90 - 1.00]
Prior VA ED visit	0.823	1.56	0.383	0.72
	(0.004)	[1.54 - 1.57]	(0.006)	[0.70 - 0.75]
Prior Medicare ED visit	0.262	0.54	0.613	1.27
	(0.006)	[0.52 - 0.57]	(0.004)	[1.26 - 1.29]
Ambulance rides in prior year	2.212	1.03	2.210	1.03
	(0.030)	[1.00 - 1.05]	(0.025)	[1.00 - 1.05]
Advanced Life Support	0.576	0.84	0.707	1.03
	(0.013)	[0.81 - 0.88]	(0.010)	[1.01 - 1.06]
Predicted VA user	0.969	1.14	0.778	0.92
	(0.001)	[1.14 - 1.15]	(0.002)	[0.91 - 0.92]
Predicted mortality	0.103	1.07	0.100	1.03
	(0.002)	[1.03 - 1.10]	(0.001)	[1.01 - 1.05]

Note: This table presents average characteristics for always takers and never takers. Always takers are defined as patients who present to the VA even when they receive a residualized instrument below the 20th percentile; never takers are defined as patients who present to a non-VA hospital even when they receive a residualized instrument above the 80th percentile. To form these residualized instruments, we residualize the baseline instrument, Z_i, given in Equation (1), by baseline controls, described in Appendix Table A.2. Observations are drawn from the baseline sample described in Appendix Table A.1. For each row corresponding to a characteristic, the table presents average characteristics and the ratio between this average and the overall sample average. Overall sample means are given in Table 4. Standard errors are calculated by bootstrap, blocking observations by zip codes, and are shown in parentheses. Corresponding 95% confidence intervals of the ratio are presented in brackets. Further details are given in Appendix A.3.

Table A.8: Treatment Effects from Selection Model

	Dependent variable: 28-day mortality				
	(1)	(2)	(3)	(4)	(5)
ATE	-0.043	-0.033	-0.033	-0.033	-0.033
	(0.017)	(0.006)	(0.006)	(0.005)	(0.005)
ATE-LATE difference	0.003	0.013	0.012	0.012	0.012
	(0.001)	(0.011)	(0.011)	(0.011)	(0.011)
Control function	Linear	Cubic	Cubic	Gaussian basis	Gaussian basis
Knots		3	5	3	5
Outcome mean	0.097	0.097	0.097	0.097	0.097
Observations	401,319	401,319	401,319	401,319	401,319

Note: This table presents estimates of the average treatment effect (ATE) from the selection model in Equation (A.21), under different specifications. Column 1 presents results from a control function that is linear in the first-stage residual, corresponding to the regression in Equation (A.23). Columns 2 to 5 present results from semiparametric control functions, corresponding to regressions of the form in Equation (A.25). The columns vary in whether the spline functions are cubic functions or Gaussian basis functions and in the number of knots. In addition to the ATE, each column presents the difference between the ATE and the local average treatment effect (LATE). The LATE is estimated from Equation (A.24) and is numerically equivalent to the LATE from our benchmark analysis in Section 3. We compute standard errors (shown in parentheses) for the ATE and the ATE-LATE difference by bootstrap, blocking by zip codes. Appendix A.4 provides further details.

Table A.9: Heterogeneity by Non-VA Hospital Characteristics

	Regression estimates		Characteristic means	
	VA	VA $\times \tilde{I}_{x,i}$	$I_{x,i} = 0$	$I_{x,i} = 1$
Volume, Size, and Capabilities				
ED visits	-0.046	-0.002	28,082	53,849
	(0.016)	(0.002)		
Admissions	-0.046	-0.003	9,664	17,859
	(0.017)	(0.002)		
Total staffed beds	-0.046	-0.004	199	375
	(0.017)	(0.002)		
Teaching hospital	-0.045	-0.000	0.02	0.51
	(0.017)	(0.002)		
Trauma center	-0.045	0.004	0.28	0.93
	(0.016)	(0.002)		
Advanced cardiac care	-0.046	-0.000	0.64	1.00
	(0.017)	(0.002)		
Stroke center	-0.045	0.001	0.03	0.65
	(0.017)	(0.002)		
Staffing				
ED staff per 1,000 ED visits	-0.045	-0.001	0.30	0.75
	(0.017)	(0.002)		
Nurses per 1,000 patient-days	-0.046	0.006	4.13	6.58
	(0.016)	(0.002)		
Physicians per 1,000 patient-days	-0.045	0.002	4.36	10.79
	(0.017)	(0.002)		
Hospitalists per 1,000 patient-days	-0.045	0.003	0.12	0.39
	(0.017)	(0.002)		
Intensivists per 1,000 patient-days	-0.045	0.003	0.05	0.23
	(0.017)	(0.002)		

Note: This table presents regression results investigating heterogeneous treatment effects along binary indicators of average non-VA hospital characteristics associated with each zip code. For each zip code, hospital characteristics are averaged with weights proportional to the number of rides going to each non-VA hospital from the zip code. We then divide observations i, based on whether their zip codes $z(i)$ have below- vs. above-median averages, denoted by $I_{x,i} = 0$ and $I_{x,i} = 1$, respectively. Regression results correspond to Equation (8). The coefficient on VA represents the LATE of going to the VA, and the coefficient on VA $\times \tilde{I}_{x,i}$ represents the difference in the LATE between observations with $I_{x,i} = 1$ and observations with $I_{x,i} = 0$. Appendix Table A.10 presents results for additional characteristics. Appendix A.5 provides further details on the hospital characteristics.

Table A.10: Heterogeneity by Non-VA Hospital Characteristics (Continued)

	Regression estimates		Characteristic means	
	VA	VA $\times \tilde{I}_{x,i}$	$I_{x,i} = 0$	$I_{x,i} = 1$
Spending and Outcomes				
Relative spending	-0.045	-0.002	0.97	1.04
	(0.017)	(0.002)		
Mortality rate	-0.045	-0.003	11.62	12.89
	(0.017)	(0.002)		
Readmission rate	-0.045	-0.002	17.30	18.90
	(0.017)	(0.002)		
Organization and IT				
Network or hospital system	-0.045	-0.002	0.65	1.00
	(0.017)	(0.002)		
HMO or ACO	-0.045	-0.002	0.00	0.47
	(0.017)	(0.002)		
Health IT	-0.046	-0.002	0.00	0.80
	(0.016)	(0.002)		
Share of non-VA rides (max.)	-0.045	0.002	0.42	0.73
	(0.017)	(0.002)		

Note: This table presents regression results investigating heterogeneous treatment effects along binary indicators based on non-VA hospital characteristics associated with each zip code. For "Share of non-VA rides (max.)", we take the maximum non-VA hospital share of non-VA rides for each zip code as the zip code characteristic. For the remaining characteristics, hospital characteristics are averaged within each zip code with weights proportional to the number of rides going to each non-VA hospital from the zip code. We then divide observations i, based on whether their zip codes $z(i)$ have below- vs. above-median statistics, denoted by $I_{x,i} = 0$ and $I_{x,i} = 1$, respectively. Regression results correspond to Equation (8). The coefficient on VA represents the LATE of going to the VA, and the coefficient on VA $\times \tilde{I}_{x,i}$ represents the difference in the LATE between observations with $I_{x,i} = 1$ and observations with $I_{x,i} = 0$. Appendix Table A.9 presents results for additional characteristics. Appendix A.5 provides further details on the hospital characteristics.

Table A.11: Heterogeneity by VA Hospital Characteristics

	Regression estimates		Characteristic means	
	VA	VA $\times \tilde{I}_{x,i}$	$I_{x,i} = 0$	$I_{x,i} = 1$
Volume, Size, and Capabilities				
ED visits	-0.045	-0.001	8,625	23,111
	(0.017)	(0.002)		
Admissions	-0.044	-0.003	3,247	8,148
	(0.016)	(0.002)		
Total staffed beds	-0.044	-0.007	139	463
	(0.017)	(0.002)		
Teaching hospital	-0.045	-0.003	0.00	0.93
	(0.017)	(0.002)		
Trauma center	-0.052	0.006	0.00	1.00
	(0.018)	(0.004)		
Advanced cardiac care	-0.051	-0.004	0.00	1.00
	(0.018)	(0.002)		
Staffing				
ED staff per 1,000 ED visits	-0.050	-0.001	0.19	1.21
	(0.022)	(0.003)		
Nurses per 1,000 patient-days	-0.045	0.003	3.80	8.60
	(0.017)	(0.002)		
Physicians per 1,000 patient-days	-0.050	-0.000	1.12	7.95
	(0.022)	(0.003)		
Hospitalists per 1,000 patient-days	-0.051	0.006	0.03	0.30
	(0.022)	(0.003)		
Intensivists per 1,000 patient-days	-0.050	0.001	0.00	0.15
	(0.022)	(0.003)		
Spending and Outcomes				
Relative spending	-0.045	-0.002	0.95	1.22
	(0.016)	(0.002)		
Mortality rate	-0.045	0.005	7.11	7.98
	(0.017)	(0.003)		
Readmission rate	-0.045	-0.003	11.70	12.70
	(0.017)	(0.002)		

Note: This table presents regression results investigating heterogeneous treatment effects along characteristics of the VA hospital associated with each zip code. For each VA hospital characteristic x, we divide observations i, based on whether x is below vs. above the median, denoted by $I_{x,i} = 0$ and $I_{x,i} = 1$, respectively. Regression results correspond to Equation (8). The coefficient on VA represents the LATE of going to the VA, and the coefficient on VA $\times \tilde{I}_{x,i}$ represents the difference in the LATE between observations with $I_{x,i} = 1$ and observations with $I_{x,i} = 0$. Appendix A.5 provides further details on the hospital characteristics.

Table A.12: Heterogeneity by Patient Characteristics

	Regression estimates		Characteristic means	
	VA	VA $\times \tilde{I}_{x,i}$	$I_{x,i} = 0$	$I_{x,i} = 1$
Older than 80	-0.047	0.004	0.00	1.00
	(0.017)	(0.003)		
Black	-0.043	-0.002	0.00	1.00
	(0.017)	(0.003)		
Hispanic	-0.045	-0.008	0.00	1.00
	(0.017)	(0.008)		
Income	-0.044	0.003	$10,651	$31,159
	(0.017)	(0.002)		
Comorbidity count	-0.044	-0.014	3.90	9.28
	(0.016)	(0.002)		
Mental illness or substance abuse	-0.045	-0.005	0.00	1.00
	(0.017)	(0.002)		
VA visits in prior year	-0.044	-0.004	2.15	11.88
	(0.017)	(0.002)		
Ambulance rides in prior year	-0.043	-0.008	1.00	3.55
	(0.017)	(0.002)		
Advanced Life Support	-0.046	-0.013	0.00	1.00
	(0.017)	(0.002)		
Predicted VA user	-0.044	-0.005	0.70	1.00
	(0.017)	(0.003)		
Predicted mortality	-0.045	-0.018	0.04	0.15
	(0.016)	(0.002)		

Note: This table presents regression results investigating heterogeneous treatment effects along patient characteristics. For each VA hospital characteristic x, we divide observations i, based on whether x is below vs. above the median, denoted by $I_{x,i} = 0$ and $I_{x,i} = 1$, respectively. Regression results correspond to Equation (8). The coefficient on VA represents the LATE of going to the VA, and the coefficient on VA $\times \tilde{I}_{x,i}$ represents the difference in the LATE between observations with $I_{x,i} = 1$ and observations with $I_{x,i} = 0$.

Table A.13: Selection of Alternative Samples

Sample step	Description	Rides	Patients	Ambulance companies	Hospitals	
					VA	Non-VA
A: Sample for Descriptive Utilization Patterns						
3. Start from distance restrictions in baseline sample	See step #3 in Table A.1.	1,051,093	365,163	1,217	100	1,577
4. Prior utilization restriction	Keep rides for patients with some non-VA or VA utilization (inpatient, ED, or primary care).	977,826	340,371	1,217	100	1,565
5. Prior ride restriction	Drop rides for patients with a prior ride within 30 days.	794,940	340,371	1,217	100	1,548
B: Non-VA-Only Sample						
2. Start from clean sample	See step #2 in Table A.1.	8,828,997	2,862,557	180,320	125	7,744
3. Distance restrictions	Drop rides to a hospital over 50 miles from the patient's home. Drop zip codes without at least two non-VA hospitals within 20 miles that receive at least 5% from that zip code.	6,424,120	2,131,152	29,100	122	5,498
4. Ambulance restrictions	Drop rides by an ambulance company with fewer than 20 patients in a given zip code. Drop rides from zip codes with only one remaining ambulance company.	3,919,572	1,372,499	5,716	119	3,999
5. Prior utilization restriction	Keep only rides for patients with some non-VA utilization (inpatient, ED, or primary care) but no VA utilization in the prior year.	1,735,141	644,917	5,716	97	3,812
6. Prior ride restriction	Drop rides for patients with a prior ride within 30 days.	1,414,217	644,917	5,716	96	3,799

Note: This table details selection steps to create two alternative samples. Panel A shows selection steps for the sample that we use to study descriptive utilization patterns in Appendix Figure A.9. Panel B shows selection steps for the sample of patients with only non-VA prior utilization, which we use in Section 5.5 to study the effect of receiving care at a modal non-VA hospital. At each step, the table lists the number of ambulance rides, patients, ambulance companies, and VA and non-VA hospitals. Table A.14 shows average patient characteristics among observations at each sample step.

Table A.14: Characteristics of Non-VA-Only Sample

Restrictions	Sample characteristics				
	Dually eligible	Add zip × hospital	Add zip × ambulance	Add non-VA-only prior utilization	Add no ride in prior month
Male	0.899	0.898	0.897	0.824	0.825
Age	77.04	77.12	77.32	77.68	78.05
Black	0.111	0.124	0.129	0.125	0.118
Income	$21,724	$21,763	$22,253	$22,800	$23,393
Rural zip code	0.255	0.169	0.125	0.120	0.120
Residential source	0.610	0.619	0.657	0.614	0.636
Comorbidity count	6.53	6.62	6.60	6.96	6.57
Prior VA ED visit	0.136	0.141	0.138	0.000	0.000
Prior Medicare ED visit	0.695	0.694	0.687	0.797	0.752
Ambulance rides in prior year	2.77	2.83	2.82	3.13	2.28
Advanced Life Support	0.696	0.695	0.699	0.676	0.684
Weekend rate	0.272	0.271	0.271	0.270	0.269
28-day mortality	0.115	0.116	0.113	0.117	0.112
Present at VA	0.044	0.049	0.049	0.002	0.002
Number of patients	2,862,557	2,131,152	1,372,499	644,917	644,917
Number of ambulance rides	8,828,997	6,424,120	3,919,572	1,735,141	1,414,217

Note: This table presents characteristics of observations remaining at each step of creating the sample of patients with only non-VA prior utilization, which we use in Section 5.5 to study the effect of receiving care at a modal non-VA hospital. Each step is detailed in Panel B of Appendix Table A.13.