Costs of Technological Frictions: Evidence from EHR (Non-)Interoperability*

Rebekah Dix Kelsey Moran Thi Mai Anh Nguyen

September 30, 2025

Click here for the latest version

Abstract

Interoperability of US hospital electronic health record systems remains limited, especially across vendors. We study how these frictions affect patients through a direct effect on information exchange and an allocative effect on patient flows. When hospitals switch to the same vendor, charges and readmissions fall 4% and 11% for transferred and referred patients, and transfers and referrals between them rise 8-10%. Patient reallocation improves outcomes when hospitals switch to vendors used by higher-quality providers and worsens outcomes otherwise. Eliminating frictions would redirect 7.5% of transfer patients and increase welfare 21%. Welfare increases from 2013–2019 achieved one-third of this potential.

*We are grateful to Amy Finkelstein, Jon Gruber, Joseph Doyle, Nikhil Agarwal, Nancy Rose, and Mike Whinston for their invaluable advice and encouragement. We also thank Jimbo Brand, Ari Bronsoler, Kristin Butcher, David Cutler, Glenn Ellison, Jeffrey Harris, Paul Joskow, Riley League, Dean Li, Maria Polyakova, Jim Poterba, Tobias Salz, Gauri Kartini Shastry, Tim Simcoe, Evan Soltas, Wilbur Townsend, John Van Reenen, Ben Vatter, and seminar participants at the MIT PF and IO lunches, MIT PF/Labor Seminar, Analysis Group, ASHEcon, BFI, CBO, Columbia, Cornerstone, Cowles, CRA, Federal Reserve Bank of Chicago, Haverford, IHS, IIOC, Miami, NBER Health and Aging Fellows Lunch, NBER Summer Institute, Purdue, Wellesley, and Yale for very helpful comments. We acknowledge support from the National Science Foundation Graduate Research Fellowship under grant number 2141064 (Dix and Moran), the National Institute on Aging under grant number T32-AG000186 to the National Bureau of Economic Research (Dix and Moran), the Jerry Hausman Graduate Dissertation Fellowship (Dix and Nguyen), and the Cowles Foundation (Dix). Dix: Yale, rebekah.dix@yale.edu; Moran: Miami, kcmoran@miami.edu; Nguyen: NYU, tmn285@nyu.edu.

1 Introduction

Interoperability—the ability of different systems to work seamlessly together—is a critical component of many product markets (FTC, 2023; European Commission, 2024). While often unnoticed, interoperability underlies many daily activities: Apple and Windows computers share files, bank cards work at most ATMs, and even trains built by different manufacturers run on the same tracks. Product compatibility often becomes noticeable only when imperfect, such as the distinctive green message bubbles in iPhone text conversations with Android users or how electric vehicle charging stations favor the vehicles of their manufacturers.[1,2] These examples illustrate the pervasive nature of product compatibility in modern life. The presence or absence of such compatibility fundamentally shapes competition between firms and may significantly affect consumer welfare (Farrell and Saloner, 1985; Katz and Shapiro, 1994).

Interoperability is particularly important in the healthcare setting as Electronic Health Record (EHR) systems form the backbone of modern medical information systems. In this paper, we study the effects of imperfect EHR system interoperability on patients, focusing on those who move between providers—e.g., referrals or inpatient transfers—for whom interoperability is most salient.[3] We examine how interoperability affects these patients through two possible channels: (1) a *direct*, technological effect of influencing health information exchange, and (2) an *allocative* effect of shifting the flow of patients across providers. Directly, improved interoperability can benefit patients by lowering hassle costs, reducing redundant care, and enhancing care coordination through better information exchange among providers. Allocatively, these advantages may lead providers to prefer interoperable systems when deciding where to send patients for additional care—a preference that may entail tradeoffs with provider quality and other attributes. Despite substantial government initiatives to promote EHR adoption and data sharing over the past two decades, interoperability remains limited, particularly across systems built by different vendors. In 2022, just 8% of primary care physicians reported it was very easy to use information from outside organizations using different vendors, compared to 38%

[1]Text messages in conversations where all members have iPhones are blue, while those with even one Android user are green. In addition to the message color, groups with Android users cannot always use the same characters or message reactions as those with only iPhone users. These frictions have received considerable media attention. One article reports that many people purchase Apple phones because, "they dread the ostracism that comes with a green text" (Higgins, 2022).

[2]Electric vehicle charging stations are often designed with proprietary connectors and communication protocols that are optimized for their own brand of vehicles. For example, Tesla's Supercharger network uses a unique connector and software that allows for seamless integration with Tesla vehicles, enabling features like automatic billing and optimal charging speeds. While adapters exist for other brands to use these stations, they may not have access to all features, creating an advantage for the station manufacturer's vehicles.

[3]These movements are common: over one-third of US patients are referred to specialists annually (Mehrotra et al., 2011), and 4% of hospitalized Medicare patients are transferred to another hospital.

for same-vendor sources (Everson et al., 2024).[4] These low interoperability levels not only limit direct benefits of information exchange but may also distort patient flows away from otherwise desirable providers due to the greater challenges of across-vendor versus within-vendor interoperability.

We quantify the effects of interoperability frictions on shared hospital patient flows and outcomes by combining three main data sources (Section 2): (1) the American Hospital Association (AHA) IT Survey, (2) the Health Information and Management Systems Survey (HIMSS), and (3) the Centers for Medicare and Medicaid Services (CMS) Medicare Claims. The hospital-level surveys provide information on hospitals' EHR vendors and interoperability experiences; the patient-level claims allow us to identify patient flows across hospitals and measure the effects of interoperability on patient outcomes. We study cross-hospital inpatient transfers and referrals separately: transfers are relatively infrequent but often involve the most complex cases and highest-risk patients (Hernandez-Boussard et al., 2017), while referrals are common but heterogeneous in type and severity. Our dataset spans 2005–2019, a period with substantial changes in hospital EHR choices.

Section 3 begins our empirical analysis by presenting causal evidence on the *direct effects* of interoperability for patients who are transferred or referred to another hospital for additional care. Conceptually, difficulties transmitting medical records across hospitals could lead to frustration for patients and providers and may result in incomplete medical histories—frictions that can cause care coordination failures, duplicative testing, or treatment delays that put patients at risk.[5] To evaluate these effects, we implement a difference-in-differences design comparing patient outcomes before and after two hospitals switch to the same EHR vendor—a proxy for higher interoperability. We find that switching to the same vendor reduces charges, imaging, testing, and readmissions for shared patients: transfers see a 4% drop in charges and a 2% drop in imaging at the receiver hospital; referrals see a 3% drop in testing at the receiver hospital and an 11% decline in 60-day readmissions. These effects are largest exactly where

[4]Even President Obama, who signed the HITECH Act, remarked, "it's proven to be harder than we expected, partly because everyone has different systems, they don't all talk to each other" (Kliff, 2017).

[5]Anecdotal evidence of imperfect interoperability undermining patient care is common. In *Fragmented: A Doctor's Quest to Piece Together American Health Care*, Dr. Ilana Yurkiewicz writes, "Without an easy way to get a patient's full medical files, I must ask where their prior doctors were located, have the patients sign a release form, fax it to other hospitals, and receive stacks of papers in return. Then I dig in… it's like opening a book to page 200 and being asked to write page 201. That can be challenging enough. But on top of that problem, the middle of the book may be mysteriously ripped out, pages 75 to 95 shuffled, and several chapters don't even seem to be part of the same story. Meanwhile, everyone is urging me to write now" (Yurkiewicz, 2023). These frictions can cause severe patient harm. In one documented case, incompatible software within the same hospital delayed care for neurological symptoms, resulting in irreversible brain damage (Schulte and Fry, 2019). In another, a lost referral between different EHR systems delayed cardiology care by three months—the patient died of heart failure on the day of her rescheduled referred appointment (Betsy Lehman Center, 2018).

improvement may be most needed: for higher-risk patients and at lower-quality hospitals.

One potential concern is that factors coinciding with hospital EHR switches—rather than changes in interoperability itself—could drive these observed patterns. Our results are robust to controlling for patient, hospital, and market characteristics, including patient composition and hospital system affiliations (e.g., ownership). Further, we implement a placebo test using vendor switches that do not result in vendor alignment, which yields null results. This suggests our main results are not driven by coincident changes such as improvements in EHR system quality. Overall, our findings indicate that higher interoperability directly improves health outcomes and decreases care costs, likely through more effective use of patient records and reduced redundant care.

Having established these direct benefits, we next examine the *allocative effects* of interoperability on transfer and referral patients by asking two questions. First, do patients and providers value interoperability when deciding where to send patients for additional care? Second, does this preference involve a tradeoff with other desirable hospital characteristics?

We address the first question in Section 4, showing that patients are more likely to be sent to a hospital when the sending and receiving hospitals become more interoperable. Beyond the direct patient benefits documented above, interoperability can also benefit patients and providers by reducing hassle costs. Thus, both patients and providers may prefer destinations where information exchange is simplest and most effective. Using the same difference-in-differences design exploiting changes in whether hospitals share the same EHR vendor, we find that when hospitals switch to the same vendor, shared inpatient transfers increase by 8% and referrals by 9-10%. These effects are largest for switches to Epic, which we later show has superior within-vendor interoperability compared to other vendors. Our estimates are again robust to controlling for hospital and market characteristics. Placebo tests of vendor switches without alignment yield null effects, suggesting results are not driven by EHR system quality changes. Due to additional concerns about endogeneity of hospital EHR choices, we employ an instrumental variables strategy exploiting pressure from out-of-market, same-system hospitals to use the same EHR as their peers. This produces larger estimates, consistent with downward bias from measurement error or endogenous vendor choice.[6] Since we find no change in the extensive margin of whether to refer or transfer, these patterns reflect a reallocation of patients across hospitals—specifically, over 100,000 Medicare transfers and 11 million referrals, or $10.4 billion in Medicare spending due to vendor switches from 2005 to 2019.

[6]Endogenous vendor choice could generate either downward or upward bias in our estimates. Hospitals may choose the same EHR vendor as others in their market to attract more patients through improved interoperability (upward bias), or they may avoid the same vendor to prevent losing patients to competitors (downward bias).

Since hospitals are imperfect substitutes (Doyle et al., 2019; Chandra et al., 2023), the overall welfare effect of interoperability-driven patient reallocation depends on whether patients are redirected to higher- or lower-quality hospitals than they would have otherwise visited. In Section 5, we test this welfare channel using a difference-in-differences design that exploits hospital-level EHR vendor switches. Specifically, we examine whether, after a sending hospital switches vendors, outcomes for patients sent to same-vendor receiving hospitals correlate with changes in the characteristics of other local same-vendor hospitals.[7] Results confirm such correlations for patient health outcomes, care costs, and distance traveled: when a hospital switches to a vendor used by higher-quality (lower-quality) hospitals in its market, patient outcomes improve (worsen). This highlights a potential tradeoff between interoperability and other hospital attributes.

In Section 6, we quantify this tradeoff using a demand model of patient flows. Our model builds on standard frameworks (Ho, 2006) by incorporating interoperability into a joint hospital-patient utility function. We estimate this model in two steps. First, we exploit hospital-level interoperability reports from the AHA IT Survey to estimate vendor-year-specific levels of interoperability along two dimensions: (1) within-vendor (e.g., Epic to Epic) and (2) across-vendor (e.g., Epic to non-Epic). Our interoperability estimates confirm that within-vendor interoperability consistently exceeds that across-vendors. Epic shows particularly high within-vendor levels, which may help explain its market dominance. Overall interoperability levels remain low, and progress is slowest in across-vendor interoperability, suggesting misaligned private and social incentives. Second, we estimate the demand model using these interoperability measures as inputs. For model estimation, we focus on transfers, whose markets are more clearly defined than referrals, and restrict the sample to 2013–2019, when the AHA IT Survey questions are available. We find that the hospital demand elasticity of substitution with respect to interoperability is approximately one-third to one-half that with respect to distance, suggesting strong preference for interoperability in transfer decisions.[8]

In Section 7, we use our model estimates to quantify the welfare gains from reducing interoperability barriers. In a first-best benchmark of perfect interoperability, joint hospital-patient utility for transfer patients would increase by 21%, equivalent to a 57-kilometer reduction in travel distance. Of this gain, 95% comes from direct utility effects and 5% comes from improved allocative efficiency—without distortive incentives to send patients to same-vendor hospitals,

[7] Using the terminology of Chandra et al. (2023), this empirical strategy tests the "predictive validity" of changes in local vendor network characteristics.

[8] Distance is a common benchmark in hospital demand models due to strong patient distaste for travel. Previous papers (e.g., Gowrisankaran et al., 2015; Lu and Lu, 2018) find that hospital demand is more elastic with respect to distance than to other factors like cost and quality. Our results imply that the effect of interoperability on hospital choice is quite comparable to that of cost and quality.

7.5% of transfer patients would be reallocated to hospitals that are, on average, farther away but lower cost and higher quality. Recognizing that technological constraints may make perfect interoperability unrealistic, we evaluate a more feasible scenario in which all hospitals adopt the vendor with the highest observed within-vendor interoperability: Epic. An Epic monopoly would achieve 61% of the first-best welfare gains. However, such concentration could entail tradeoffs like higher prices and reduced innovation. While our empirical framework does not directly capture these tradeoffs, lessons from the 2013-2019 period demonstrate how market concentration can lead to uneven welfare gains.

The 2013-2019 period featured both increasing market concentration toward Epic and interoperability improvements across all vendors. We find that welfare progress achieved during this period amounted to one-third of the first-best benchmark. Notably, 70% of this progress was driven by improvements in interoperability, compared to 30% from market concentration. Welfare progress was thus unevenly distributed: hospitals switching to Epic benefited most, particularly in markets where Epic use was widespread. Taken together, our analysis suggests that improvements in interoperability hold substantial potential for welfare gains. Achieving these improvements will require both sustained competition to incentivize private investments in within-vendor interoperability alongside government action to advance across-vendor interoperability. Recent initiatives such as the Trusted Exchange Framework and Common Agreement (TEFCA), which establish minimum interoperability standards, represent a promising step in this direction.

Related Literature This paper contributes to four strands of literature. First, we build on the economics literature on product compatibility and technological frictions. Prior work shows that compatibility affects market competition and consumer welfare (Farrell and Saloner, 1985; Katz and Shapiro, 1994), with firms often strategically limiting compatibility (Augereau et al., 2004; Knittel and Stango, 2009; Gross, 2020). Evidence of such behavior appears across sectors, including banking (Ishii, 2005; Knittel and Stango, 2008; Knittel and Stango, 2009), gaming (Park, 2004; Lee, 2013), railroads (Gross, 2020), and electric vehicles (J. Li, 2023). Our contributions are twofold. First, we extend this literature to the EHR market, showing that imperfect interoperability directly affects the health outcomes of patients, who do not choose these systems themselves.[9] Second, we develop a methodology to quantify interoperability from survey data and show that interoperability frictions exist not only across but also within vendors in the EHR setting. Eliminating these frictions promises substantial welfare gains.

[9]The EHR market has a unique three-tier structure: EHR vendors offer products, hospitals choose products, and patients experience the consequences.

Second, we contribute to the literature on hospital demand and patient flows. Prior work shows that financial structures—such as insurance networks (Ho and Pakes, 2014) and vertical integration (Walden, 2016; Brot-Goldberg and de Vaan, 2018; Cutler et al., 2020; Singh, 2022; Cuesta et al., 2024)—shape where patients receive care. Reducing costs of accessing paper medical records also affects patients' provider choices (Baker et al., 2015). We show that technological frictions, specifically EHR interoperability, also influence patient flows. Moreover, unlike studies finding no health effects from financially driven reallocation (Ho and Pakes, 2014; Cutler et al., 2020), we find that imperfect interoperability affects patient health outcomes both directly and through reallocation.

Third, we contribute to the literature on EHR systems in healthcare. Prior work has focused on EHR adoption—its determinants (e.g., Miller and Tucker, 2009; Dranove et al., 2014; Adler-Milstein and Pfeifer, 2017; J. Lin, 2023a; J. Lin, 2023b) and effects on productivity and outcomes (e.g., Agha, 2014; Miller and Tucker, 2011; S. Lin et al., 2018; Bronsoler et al., 2022; Ganju et al., 2023). In particular, J. Lin (2023a) shows that hospitals tend to adopt locally dominant EHRs, and J. Lin (2023b) finds that system-owned hospitals adopt their system's preferred vendor. Rather than focusing on the drivers of hospital EHR choices, our study examines the effects of interoperability on patient flows and outcomes given existing EHR choices. We prioritize understanding these effects as a necessary precursor to exploring broader incentives for hospitals to demand, and vendors to supply, interoperability.

Recent studies, often aligning with public policy initiatives, have examined EHR interoperability and health information exchange (HIE), highlighting both the limited state of interoperability (e.g., Holmgren et al., 2017) and the benefits of improved HIE for healthcare costs (Adjerid et al., 2018; Atasoy et al., 2021; Adler-Milstein et al., 2024), duplicative testing (Ayabakan et al., 2017; Wakefield et al., 2020), and patient outcomes (Chen et al., 2019; Bronsoler, 2022; Y. Li et al., 2022). We contribute by focusing specifically on EHR interoperability—rather than broader HIE—and by developing a unified framework capturing both its direct and allocative effects. Contemporaneous work by Xue and Meyerhoefer (2025) shows that primary care providers are more likely to share patients with specialists using the same EHR vendor. Their analysis focuses on this allocative channel and interprets reallocation as primarily distortive. Using patient-level Medicare data on transfers and referrals, we quantify welfare losses from both direct effects of low interoperability on outcomes and allocative effects that create tradeoffs with other hospital characteristics.

Finally, fourth, we contribute to the literature on the value of data in various industries (Jones and Tonetti, 2020; Bignon, 2024). We extend this work to healthcare and highlight how frictions to data sharing affect the value of data.

2 Data and Setting

2.1 Data

In this paper, we use two types of data: (1) data on hospital characteristics including EHR system interoperability, and (2) data on patient outcomes and flows. Detailed descriptions of the data and sample construction are in Appendix A.

Data on Hospitals Our primary sources for hospital and EHR system information are the American Hospital Association (AHA) Annual Survey from 2005-2019 and the AHA IT Survey from 2009-2019. These surveys target all US hospitals, with the Annual Survey capturing hospital characteristics and the IT Survey collecting vendor choices and interoperability measures for hospitals' primary inpatient EHR systems. To extend coverage of hospital EHRs back to 2005 and fill in some missing EHR vendor information for hospitals that do not respond to the AHA IT, we supplement with similar data from the Health Information and Management Systems Survey (HIMSS) from 2005-2017. Due to sharp declines in hospital EHR vendor reporting after 2017 following the end of HIMSS data, we limit our empirical analyses to 2005-2017 (transfers) or 2008-2017 (referrals).[10]

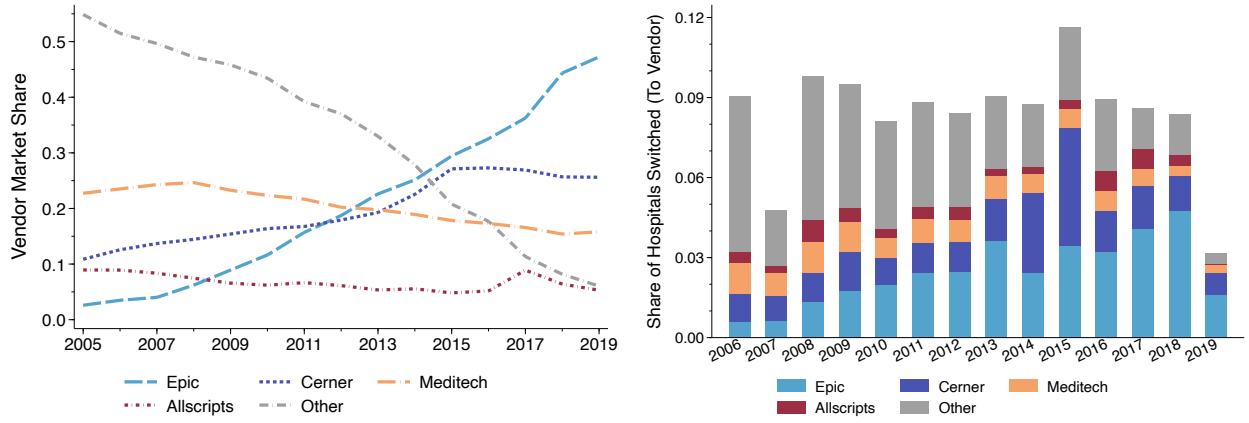
Data on Patients To study patient outcomes and flows across providers, we use Medicare Claims data from the Centers for Medicare and Medicaid Services (CMS). Medicare is the federal health insurance program for Americans aged 65 and older, and this data contains all claims submitted to CMS for healthcare services provided to the approximately 40 million individuals with Traditional Medicare coverage each year. We analyze the 100% sample (i.e., the full sample of Medicare enrollees) MedPAR files, 100% sample Outpatient files, and 20% sample Carrier files, capturing inpatient facility, outpatient facility, and professional provider service claims, respectively. These data contain detailed information about the billed medical visit including diagnoses, procedures, charges, admission and discharge dates, and provider Medicare certification numbers. We present summary statistics for these claims in Section 3.1.

2.2 EHR Market

The market for EHR systems in US hospitals has changed substantially over time. While EHR systems have existed for decades, the 2009 Health Information Technology for Economic

[10] All results are robust to extending both periods through 2019.

Figure 1: EHR Vendor Market Shares and Switches



(a) EHR Vendor Market Shares

(b) EHR Vendor Switches

Notes: Left panel plots EHR vendor market shares for major vendors in the EHR market over time, calculated with respect to hospital beds. Right panel plots the share of hospitals in each year that switch EHR vendors from the previous year, with the colors identifying the vendor to which the hospitals switched. The colors in both panels consistently represent vendors, e.g., blue is always Epic.

and Clinical Health (HITECH) Act reshaped the market by establishing "meaningful use" standards for clinical decision support and point-of-care functionality.[11] Four major vendors subsequently emerged as leaders: Epic, Cerner, Meditech, and Allscripts. Market concentration rose sharply post-HITECH, with Epic accounting for nearly 50% of hospital beds by 2019 and Cerner 25% (Figure 1a). Between 2005 and 2019, 8.4% of hospitals switched EHR vendors each year on average (Figure 1b), with 45% switching at least once (Appendix Figure B2).[12] These switches generate hospital-pair variation in interoperability over time, which we exploit in our analysis.

EHR vendor switches are major hospital decisions—installation costs alone can range from millions to billions of dollars—driven by system costs, functionality, and interoperability.[13] Cost and functionality are the most cited reasons for switching (Coustasse et al., 2018), as vendors vary in pricing (e.g., installation, licensing, customization) and features that affect billing and productivity.[14] Interoperability also influences vendor choice, as data exchange flows more

[11] The HITECH Act provided $30 billion in subsidies for EHR advancement, contingent on healthcare organizations meeting "meaningful use" thresholds. Appendix Figure B1 shows trends in EHR adoption and capabilities.

[12] Approximately 20% of EHR vendor switches occur within three years of a hospital ownership change, suggesting these switches may be driven by new ownership rather than independent hospital decisions.

[13] Examples include Massachusetts General Hospital's $1.2 billion transition to Epic in 2015 (Dayal McCluskey, 2015) and Mayo Clinic's $1.5 billion implementation in 2016 (Drees, 2020). Smaller hospitals also face substantial costs: St. Peter's Health in Montana (120 beds) spent $25 million to adopt Epic in 2023 (Diaz, 2023).

[14] Regulatory changes have also pushed hospitals to adopt more advanced systems.

smoothly between hospitals using the same vendor (Section 2.3). Supporting this, 39% of hospitals switching vendors adopt their local market's modal vendor, and 69% switch to their health system's modal vendor (conditional on system membership; Appendix Section B.2).

2.3 EHR Interoperability

Having an EHR system does not guarantee seamless data exchange with other organizations. Figure 2a shows hospital capabilities to: (1) send, (2) receive, and (3) query electronic information from other organizations, as well as (4) integrate received information without manual entry.[15] By 2019, over 80% of hospitals could send, receive, and query data, but only 60% could integrate it without manual entry. This gap is notable, as integration best reflects the US Office of the National Coordinator for Health IT's (ONC) definition of interoperability.[16] Integration is especially difficult across vendors: Figure 2b shows that 75% of hospitals surveyed from 2017–2019 reported greater challenges exchanging data with organizations using different EHR vendors, regardless of their own vendor.

This limited interoperability reflects modest public policy success over the past 15 years. The earliest major federal policy, the HITECH Act of 2009, subsidized adoption of advanced EHR systems but did not address interoperability (Bronsoler et al., 2022). Seven years later, the 21st Century Cures Act of 2016 prohibited "information blocking"—practices interfering with access, exchange, or use of electronic health data—but enforcement mechanisms were only established through subsequent agency rules in 2020 and 2024 (Turbow et al., 2021; HHS, 2024).[17] Regional health information exchanges (RHIOs) have emerged to address persistent gaps, but participation and quality vary widely (Adler-Milstein et al., 2021). Most recently, ongoing federal efforts like the Trusted Exchange Framework and Common Agreement (TEFCA) aim to build national interoperability, though effectiveness remains to be seen.[18]

Survey evidence suggests both patients and providers are acutely aware of EHR interoperability limitations and their potential consequences. In 2019, 32% of patients reported encountering information exchange gaps when visiting providers (ONC, 2019), while only 36% of physicians had electronic access to external medical records at point of care (ONC, 2022). The

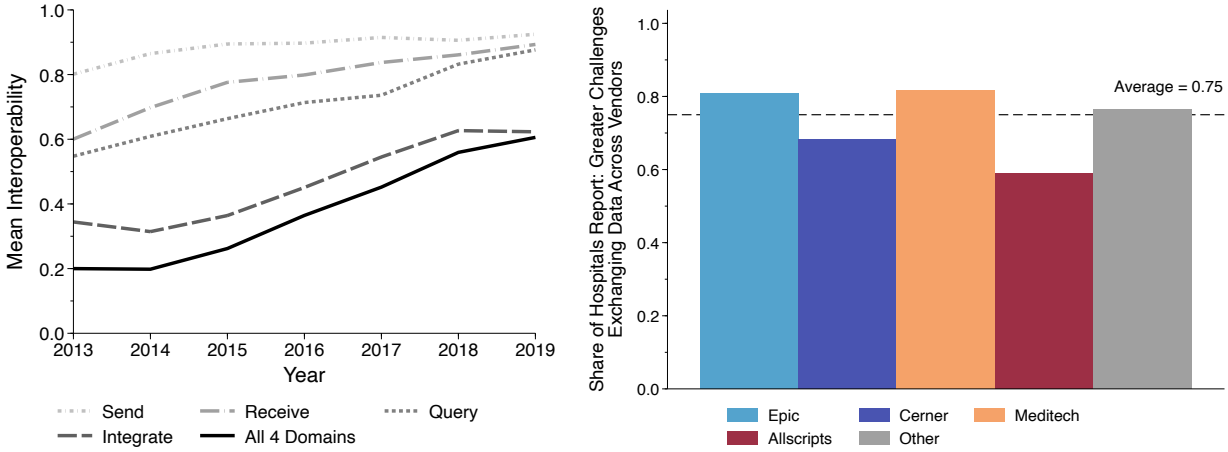
[15] For domains (1)–(3), hospitals report "yes" or "no," and the figure shows the share responding "yes." For domain (4), responses are "yes, routinely," "yes, but not routinely," or "no," coded as 1, 0.5, and 0, respectively; the average is plotted in Figure 2 as reported integration interoperability.

[16] ONC defines interoperability as "the ability of a system to exchange electronic health information with and use electronic health information from other systems without special effort on the part of the user" (Patel et al., 2016).

[17] Despite these rules, enforcement remains limited. In 2022, 32% of hospitals reported witnessing intentional data-sharing obstruction, even after information blocking regulations became enforceable in 2021 (ONC, 2023).

[18] The first organizations were approved to join TEFCA in December 2023.

Figure 2: State of EHR Interoperability



(a) Interoperability by Domain

(b) Greater Challenge Sharing Data Across Vendors

Notes: Left panel plots the average level of hospital-reported interoperability over time across four domains: (1) ability to *send* information to other organizations electronically, (2) ability to *receive* information from other organizations electronically, (3) ability to *query* for information from other organizations electronically, and (4) ability to *integrate* information received from other organizations without the need for manual entry. Solid black line represents the average for achieving all four domains. Right panel plots the share of hospitals in 2017-2019 that report experiencing greater challenges exchanging data across EHR vendors, separately by the EHR vendors used by the responding hospitals. Dotted line represents the average across all vendors.

disparity in interoperability across, versus within, EHR vendors is also stark: 85% of physicians reported difficulties exchanging information with providers using different EHR systems (ONC, 2022), and 38% found information from organizations using the same EHR vendor very easy to use compared to only 8% for different vendors (Everson et al., 2024). These data suggest that while within-vendor interoperability exceeds that across vendors, both remain imperfect. Providers and patients also perceive real consequences. About 5% of patients repeat tests due to missing data, and 20% have to physically bring prior test results to appointments (ONC, 2019). Over 80% of physicians believe health information exchange improves care quality and coordination while reducing duplicative testing (ONC, 2022). In the next section, we test for causal evidence of such direct interoperability effects on patients who move between hospitals.

3 Direct Effects of Interoperability on Patient Outcomes

In this section, we empirically examine the effects of interoperability on medical spending and health outcomes for patients shared between hospitals. Better interoperability may directly benefit patients moving between healthcare providers by facilitating health information

exchange, reducing hassle costs, and improving care coordination. Such improvements could in turn reduce wasteful medical spending (e.g., duplicative testing) and improve patient health through more timely and accurate care. We begin by defining two types of shared patients: inpatient transfers and referrals (Section 3.1). We then outline our difference-in-differences strategy (Section 3.2) and show that when hospitals gain the same EHR vendor, and thus higher interoperability, their shared patients experience lower hospital charges, diagnostic tests, and readmission rates (Section 3.3). These effects are largest exactly where improvement may be most needed: at lower-quality hospitals and for higher-risk patients. We conclude by demonstrating robustness to potential changes in patient composition as well as other hospital and market characteristics.

3.1 Definitions of Shared Patients

Using the Medicare Claims data, we analyze outcomes for two types of patients who are shared between hospitals: (1) patients who are transferred between inpatient hospital facilities and (2) patients who are referred from one provider to another.

Inpatient Transfers We define a cross-hospital inpatient transfer as when a patient is discharged from one hospital and admitted to another within a single day,[19] and we use the 100% sample MedPAR files to identify them. Under this definition, 5.1% of Medicare hospital inpatient stays—about 600,000 stays per year—result in a transfer to inpatient treatment at another hospital. Table 1 contains summary statistics for transfers in our analysis sample as defined in Section 3.2. Transfers are often costly, with average receiver charges exceeding $85,000, and involve high-risk patients, with 30-day readmission rates of 32% and on average 5 baseline chronic conditions. While most patients are transferred for care under common diagnosis categories (circulatory, respiratory, injury), many others are transferred for aftercare following initial treatment.

Referrals We define a cross-hospital referral as when a patient is directly referred from one clinician to another for a medical service that takes place at a hospital and when the main hospital affiliations of the two clinicians differ. This service can take place in an inpatient or an outpatient setting and can range widely from physical therapy to x-rays to heart transplants. We identify these referrals using the 20% sample Carrier files, which contain the identities of both the providing and the referring providers for the billed medical service as well as the type

[19] Since we do not have the exact time of discharge or admission, we operationalize this definition as a hospital admission occurring on the same day as—or on the day following—a hospital discharge (Hsuan et al., 2024).

of place of service (e.g., hospital). For services taking place at a hospital, we identify the exact facility providing the referred service using either a crosswalk between the Inpatient, Outpatient, and Carrier claims or the primary hospital affiliation of the providing clinician which is derived from the same data. The Carrier files do not define the place of service for the event that led to each referral; we thus make the simplifying assumption that this setting is also a hospital.[20] We assign the identity of that first hospital as the primary hospital affiliation of the referring clinician. Appendix A contains more details on this data construction. Approximately 6.8% of all Medicare referrals in the Carrier files—about 19 million per year—meet this definition of cross-hospital referrals. Table 1 contains summary statistics for cross-hospital referrals in our analysis sample as defined in Section 3.2. Referrals are common but heterogeneous in type and severity, unlike transfers which are less frequent but often involve the most complex cases and highest-risk patients.

3.2 Empirical Strategy

To examine the direct effect of interoperability on patient outcomes, we run a difference-in-differences regression at the patient level around changes in whether hospital sender-receiver pairs sharing each patient have the same EHR vendor:

$$Y_{i(hh')t} = \beta * \mathbf{1}_{\left[e_{ht}=e_{h't}\right]} + \alpha_{(hh')} + \gamma_t + \delta X_{it,(hh')t,mt} + \epsilon_{i(hh')t}. \tag{3.1}$$

Here, $Y_{i(hh')t}$ denotes the outcome for patient i sent from hospital h to hospital h' in year t.[21] We examine four outcomes: (1) total charges (2019 dollars) for medical services at the receiver hospital, (2) medical images taken at the receiver hospital, (3) medical tests given at the receiver hospital, and (4) whether the patient was admitted to another hospital within 30 or 60 days following receiver treatment. Parameters $\alpha_{(hh')}$ and γ_t are hospital sender-receiver pair and year fixed effects, respectively. Our main independent variable $\mathbf{1}_{\left[e_{ht}=e_{h't}\right]}$ indicates whether the hospitals sharing patient i have the same EHR vendor e in year t, proxying for interoperability since within-vendor interoperability exceeds that across-vendors. In our baseline specification, $X_{it,(hh')t,mt}$ includes time-varying controls for patient age, sex, race, baseline chronic conditions, and baseline Charlson Comorbidity Index plus fixed effects for patient diagnosis categories. We cluster standard errors at the hospital sender-receiver pair level.

[20] While technically possible to verify referral origins by examining prior claims from referring physicians, this would be prohibitively difficult given that patients often have multiple claims from the same physician and highly variable delays between referrals and referred services.

[21] Some patients are shared between the same sender-receiver pair multiple times per year. Each transfer and referral counts as a separate observation.

Table 1: Patient Summary Statistics

	Transfers			Referrals		
	Mean	Median	SD	Mean	Median	SD
Charges	85336.4	47594.3	123019.9	143.5	72.8	298.3
# Images	1.38	1	1.28	1.25	1	1.85
# Tests	3.79	4	2.31	0.62	0	1.55
Readmit within 30 (60) days	0.32			0.056		
Age	72.1	73	12.9	72.2	73	12.5
Female	0.52			0.57		
White	0.81			0.82		
# Chronic Conditions	4.98	5	3.50	6.16	6	3.61
Charlson Comorbidity Index	4.97	5	2.55	5.70	6	2.84
Diagnosis Codes						
Circulatory	0.24			0.22		
Aftercare	0.19			0.097		
Respiratory	0.13			0.13		
Injury	0.099			0.053		
Infection	0.063			0.012		
Other	0.27			0.52		
BETOS Codes						
Imaging				0.44		
Evaluation & Management				0.34		
Procedures				0.17		
Tests				0.12		
Other				0.066		
Same EHR Vendor	0.083			0.12		
Same Hospital System	0.082			0.091		
AMC to AMC	0.24			0.38		
AMC to Non-AMC	0.27			0.19		
Non-AMC to AMC	0.28			0.23		
Non-AMC to Non-AMC	0.22			0.21		
N	2,418,727			26,536,868		

Notes: Summary statistics for cross-hospital transfers (Columns 1-3) and referrals (Columns 4-6) in our analysis sample as defined in Section 3.2, using data from 2005-2017 and 2008-2017, respectively. First four rows are: (1) total charges (2019 dollars) for medical services at receiver hospital, (2) medical images taken at receiver hospital, (3) medical tests given at receiver hospital, (4) hospital admission within 30 (60) days following receiver treatment. Patient characteristics are measured at baseline. Diagnosis and BETOS codes refer to receiver hospital service. "Same EHR Vendor": sending and receiving hospitals have EHR systems from the same vendor. "Same Hospital System": sending and receiving hospitals belong to the same hospital system. "AMC to AMC": both hospitals are Academic Medical Centers (AMCs); "AMC to Non-AMC": sending hospital is AMC, receiving hospital is not; etc.

Our main identification assumption for interpreting estimates as causal relationships between same-vendor status and patient outcomes is that no time-varying hospital pair-specific factors correlate with both same-vendor status and our outcomes. Absent a hospital pair switching to the same EHR vendor, we assume patients shared between that treated pair would experience similar trends in outcomes as those shared between control pairs that never change

same-vendor status. To support this assumption, we estimate the following dynamic event study version of Equation 3.1:

$$Y_{i(hh')t} = \left(\sum_{r=-6}^{6} \beta_r \mathbf{1}_{\left[t=s_{(hh')}+r\right]} \right) + \alpha_{(hh')} + \gamma_t + \delta X_{it,(hh')t,mt} + \epsilon_{i(hh')t}, \tag{3.2}$$

where r is the relative year since the sender-receiver pair switched to having the same vendor (occurring in year $s_{(hh')}$), we bin relative year endpoints ($r \geq 6$ and $r \leq -6$), and we exclude $r = -1$. All other parameters remain the same. To reduce bias from heterogeneous treatment effects when using always-treated observations as controls or including multiply-treated observations, we limit analyses with Equations 3.1 and 3.2 to patients shared between the following hospital sender-receiver pairs: the 64% that never have the same vendor over the sample period ("never treated") and the 11% that start with different vendors and switch to the same vendor only once.[22] This event study specification allows visual analysis for pre-trends that may suggest violations of our parallel trends assumption. Even without visual violations, identification challenges may remain including potential concurrent changes at hospital, pair, or market levels and possibly endogenous vendor choices. We address these concerns extensively in Section 3.4.

3.3 Main Results

Table 2 presents results from estimating Equation 3.1 for our four patient outcomes. Panel A shows that when hospitals gain the same EHR vendor, transfer patients shared between them experience a $2,798 decrease in charges (3.7% decrease from the mean) and a 0.031 decrease in images (2.1% decrease) at the receiving hospital. While transfer patients show no changes in tests or hospital readmission rates, Panel B shows that referral patients undergo 2.9% fewer tests and experience 11.2% lower 60-day readmission rates after their hospitals gain the same vendor. These results are robust to clustering standard errors at the HRR-level (Appendix Table D1).[23] The reductions in charges, images, and tests at receiving hospitals suggest decreased duplicative care following improved interoperability. Providers may access and utilize existing patient information more effectively, reducing redundant procedures and costs. This interpretation aligns with prior research on redundant procedures in other settings

[22] All results are robust to including all patients from the full hospital pair sample.

[23] We cannot implement Callaway and Sant'Anna (2021) estimation for patient outcomes because the analysis is at the patient level while treatment variation occurs at the hospital pair level, making Callaway and Sant'Anna (2021) algorithmically unsuitable. However, our patient flows analysis using the same source of variation yields nearly identical results with Callaway and Sant'Anna (2021) estimation, suggesting that bias from heterogeneous treatment effects is not a major concern in this setting.

Table 2: Direct Effect of Gaining Same Vendor on Shared Patient Outcomes

(a) Transfers

	Outcomes for Transfer Patients			
	(1) Charges	(2) # Images	(3) # Tests	(4) 30-Day Readmit
Same EHR Vendor	-2797.7***	-0.0305***	-0.00952	0.00163
	(917.2)	(0.00934)	(0.0308)	(0.00240)
Observations	2347783	2347783	2347783	1881237
Mean of Outcome	75314	1.425	3.830	0.3136

(b) Referrals

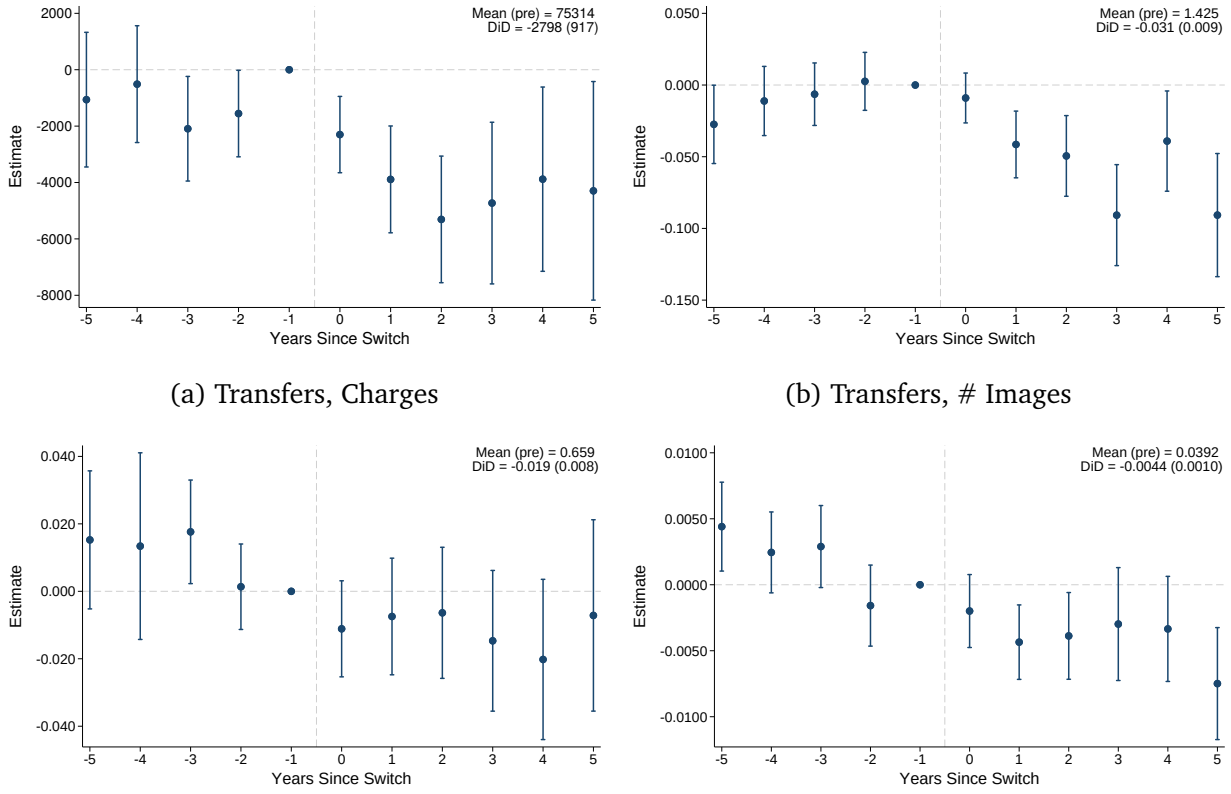
	Outcomes for Referral Patients			
	(1) Charges	(2) # Images	(3) # Tests	(4) 60-Day Readmit
Same EHR Vendor	-1.254	-0.000627	-0.0188**	-0.00440***
	(1.191)	(0.0130)	(0.00778)	(0.000956)
Observations	26393910	26393910	26393910	22048773
Mean of Outcome	146	1.314	0.659	0.0392

Notes: Each column is a separate patient-level difference-in-differences regression of the effect of the patient's sending and receiving hospitals gaining the same EHR vendor on patient outcomes as described in Equation 3.1. Panel A shows transfer patient outcomes, while Panel B shows referral patient outcomes. All regressions include hospital pair fixed effects and year fixed effects as well as fixed effects for patient diagnostic code categories, indicators for 27 patient baseline chronic conditions, and controls for patient Charlson Comorbidity Index, patient age, patient sex, and patient race. Standard errors are clustered at the hospital sender-receiver pair level and displayed in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

(e.g., Ayabakan et al., 2017; Wakefield et al., 2020). The lack of significant changes in health outcomes for transfer patients may reflect insufficient statistical power to detect rare catastrophic events. However, the larger referral patient sample provides greater power, and their decreased readmission rates suggest that enhanced interoperability may improve care quality and patient health outcomes.

To evaluate the validity of our parallel trends assumption, Figure 3 plots corresponding event study estimates of the effect of gaining the same EHR vendor on transfer patient charges (Panel A), transfer patient images (Panel B), referral patient tests (Panel C), and referral patient readmission rates (Panel D) from Equation 3.2. Despite wide 95% confidence intervals, all four outcomes show significant decreases after hospital pairs gain the same EHR vendor. Transfer patient outcomes exhibit no significant pre-trends. Referral patient outcomes show slight declines from 3-5 years before treatment to 1-2 years before treatment. However, both

Figure 3: Event Studies of Direct Effect of Gaining Same Vendor on Shared Patient Outcomes



(a) Transfers, Charges

(b) Transfers, # Images

(c) Referrals, # Tests

(d) Referrals, 60-Day Readmit

Notes: Figures plot patient-level event study estimates of the effect of the patient's sending and receiving hospitals gaining the same EHR vendor on patient outcomes as described in Equation 3.2. Panel A shows transfer patient charges, Panel B transfer patient images, Panel C referral patient tests, and Panel D referral patient 60-day hospital admissions rates. All regressions include hospital pair fixed effects and year fixed effects as well as fixed effects for patient diagnostic code categories, indicators for 27 patient baseline chronic conditions, and controls for patient Charlson Comorbidity Index, patient age, patient sex, and patient race. Standard errors are clustered at the hospital sender-receiver pair level. Bars show 95% confidence intervals. The mean of each dependent variable among treated observations in the pre-treatment period is given as is the static difference-in-differences estimate and corresponding standard error.

outcomes stabilize in the two years preceding treatment, showing no significant pre-trends, and then decrease immediately after hospital pairs switch to using the same vendor.

The event studies show immediate decreases in patient charges, tests, images, and readmission rates when hospital pairs switch to the same EHR vendor, with magnitudes growing in subsequent years. These growing magnitudes reflect two key factors. First, hospitals often take several years to fully implement new EHR systems. Our primary data source, the AHA IT Survey, reports each hospital's EHR vendor as that used by the largest number of its patients or representing its largest financial investment. Consequently, many hospitals may still be rolling

out new systems for several years after the reported switch.[24] This gradual implementation may also explain why referral patient outcome estimates in the two years before treatment are smaller than in previous pre-treatment years: new vendor rollout may start slightly before the AHA IT records the vendor switch. Second, hospitals may need time to fully learn and leverage improved health information exchange benefits with newly interoperable hospitals. This explanation aligns with the EHR adoption literature, which typically finds initial hospital productivity dips followed by gradual patient outcome improvements as providers learn to use new systems efficiently (Bronsoler et al., 2022).

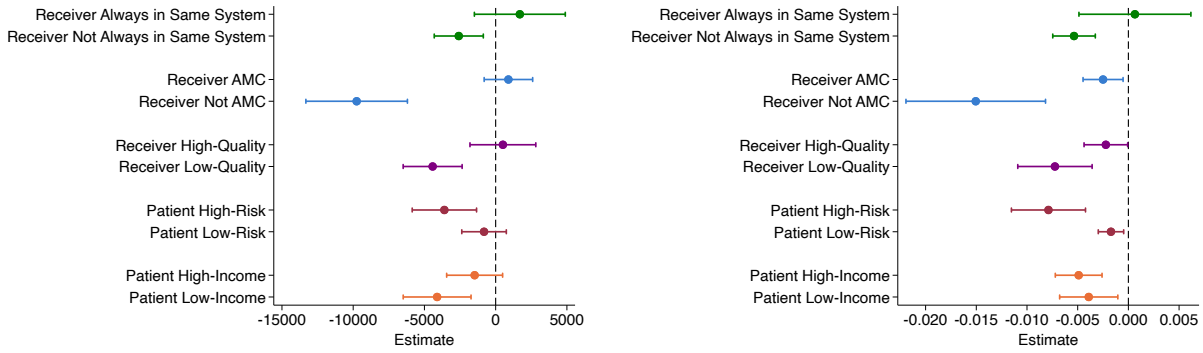
Heterogeneity To assess the credibility of our findings and identify which hospital and patient populations would benefit most from interoperability improvements, Figure 4 shows heterogeneity in the direct effect of gaining the same EHR vendor on transfer patient charges (Panel A) and referral patient 60-day hospital readmissions (Panel B) across hospital and patient characteristics. We find that charge and readmission decreases are largest when the receiving hospital is not part of the same health system as the sender, is not an academic medical center, and is of lower baseline quality. This heterogeneity is notable, as patients sent to these receiver hospitals often have worse outcomes absent treatment. We also find that health effects (readmission reductions in Panel B) are larger for higher-risk patients, while financial effects (charge reductions in Panel A) are larger for patients in lower-income areas. These patterns highlight that sharing EHR vendors yields the largest benefits exactly where improvement may be most needed: at lower-quality hospitals and for higher-risk, lower-income patients. Appendix Figure D1 shows similar patterns for other outcomes.

3.4 Threats to Validity

We now address potential confounding factors in using hospital pair-level variation in same-vendor status to identify direct interoperability effects on patient outcomes. First, we examine whether results could be driven by changes in shared patient composition following hospital EHR vendor switches. Second, we assess whether our findings reflect other concurrent hospital or market-level changes rather than interoperability improvements. Third, we conduct placebo tests to distinguish effects of switching to the same vendor from effects of simply switching vendors and potentially improving EHR system quality. While endogeneity concerns may remain, this is likely a more significant issue for allocative effects; Section 4 thus

[24] To corroborate gradual implementation, we compared EHR vendor switch years reported in the AHA IT Survey (when the new system serves the majority of patients) and the HIMSS (when the system is "live and operational"). Among hospitals switching vendors once in both sources, 78% switched later in HIMSS than AHA IT. The modal difference is one year, with HIMSS typically recording the switch one year after AHA IT.

Figure 4: Direct Effect Heterogeneity by Hospital and Patient Characteristics



(a) Transfers, Charges

(b) Referrals, 60-Day Readmit

Notes: Figures plot difference-in-differences estimates (dots) and 95% confidence intervals (bars) of the effect of a hospital sender-receiver pair gaining the same EHR vendor on total charges (2019 dollars) incurred at the receiver hospital by transfer patients (Panel A) and 60-day hospital readmission rates for referral patients (Panel B) who are shared between that pair (as described in Equation 3.1). "AMC" stands for Academic Medical Center. Receiver hospital quality is measured as the baseline risk-adjusted mortality rate for transfer (referral) patients received by the hospital in the first two years of our data samples. "High-quality" is the half of receiver hospitals that have the lowest risk-adjusted mortality rates, while "low-quality" is the half with the highest risk-adjusted mortality rates. Patient risk is measured as predicted mortality using baseline measures of patient age, sex, race, and chronic conditions as well as patient diagnosis codes. "High-risk" patients are those with the top half of such predicted mortality rates, while "low-risk" patients are those with the bottom half. "High-income" patients are those residing in zipcodes with the top half of median household incomes in 2019, while "low-income" patients are those residing in zipcodes with the bottom half.

presents an instrumental variables specification using health system-level pressure to adopt specific vendors, which yields consistent results for both patient flows and patient outcomes.

Patient Composition Changes in shared patient composition after hospitals switch to the same EHR vendor could bias our estimates. For example, providers might send more high-risk patients to newly same-vendor hospitals if they believe these patients would benefit most from improved interoperability, which would bias upward our estimated negative effects. While Section 4 shows that shared patient flows increase when hospitals gain the same vendor, we detect no significant composition changes. Appendix Table D1 demonstrates our main results' robustness across various specification checks, including adding controls for observable patient characteristics (demographics, baseline health measures, diagnosis codes) and patient-level fixed effects. Estimates remain stable with patient controls, and while precision and sample sizes decrease for images and tests, effects on transfer patient charges and referral patient readmission rates persist even with patient fixed effects. This robustness suggests that selective patient reallocation after vendor switches does not drive our estimated direct effects of

interoperability on patient outcomes.

Other Changes in Hospital and Market Another potential concern is that EHR vendor changes may coincide with other changes at the hospital, hospital pair, or market level. While our main empirical strategy may not account for such time-varying factors, our estimates demonstrate stability across robustness checks controlling for many such factors. We compare estimates from Equation 3.1 without time-varying controls to those from additional specifications: (1) adding market-by-year fixed effects, (2) adding hospital sender and receiver controls for beds, employees, ownership, and capabilities, and (3) restricting the sample to patients shared between hospital pairs that never belong to the same hospital system (i.e., never have the same owner) over the entire sample period.[25] We find negative and significant treatment effects on all patient outcomes across nearly all specifications (Appendix Table D2).[26]

Switching Vendors vs. Gaining Same Vendor Another concern is that our estimates result from hospitals switching EHR vendors, and perhaps to higher quality EHR systems, rather than switching to the *same* EHR vendor. To investigate this concern, Table 3 presents results of a placebo test on control hospital pairs that change vendors without gaining or losing same-vendor status (details are provided in Appendix Section D.1.4). Across most outcomes, this test yields statistically insignificant and small estimates less than one-fifth the magnitude of our same-vendor effects. The two exceptions are images and tests for transfer patients, which actually increase in the placebo in contrast to the decreases we observe when hospital pairs gain the same vendor. These positive placebo results may reflect vendor switches incentivizing more billing or upcoding, thus making our negative same-vendor results a potential lower bound. Alternatively, vendor switches may cause operational disruptions affecting all patients or disrupt existing pair-specific relationships that had developed over time to facilitate data exchange, even between non-interoperable systems. When hospitals switch to different vendors, these established workarounds and informal coordination mechanisms may be lost, increasing testing and imaging. Overall, the placebo results suggest that our same-vendor estimates reflect changes in same-vendor status rather than unobserved hospital- or hospital-pair-specific changes from vendor switches.

[25] Market-by-year fixed effects control for market-level time-varying factors. Hospital characteristic controls address potential internal hospital changes occurring simultaneously with EHR vendor switches. Sample restriction to hospital pairs that never belong to the same system addresses concerns that hospitals change EHR vendors simultaneously with system changes, making system switches rather than vendor switches drive our results.

[26] Restricting the sample to hospital pairs that never belong to the same system decreases the sample size and results in a loss of significance for number of images at the receiver hospital for transfer patients.

Table 3: Placebo Test of Vendor Switches on Shared Patient Outcomes

(a) Transfers

	Outcomes for Transfer Patients			
	(1) Charges	(2) # Images	(3) # Tests	(4) 30-Day Readmit
Post-Placebo Switch	-233.5	0.0190**	0.0518*	-0.00391
	(1049.7)	(0.00933)	(0.0311)	(0.00273)
Observations	950905	950905	950905	754216
Mean of Outcome	86159	1.403	3.811	0.3345

(b) Referrals

	Outcomes for Referral Patients			
	(1) Charges	(2) # Images	(3) # Tests	(4) 60-Day Readmit
Post-Placebo Switch	0.140	-0.00670	-0.00219	-0.000651
	(1.170)	(0.0107)	(0.00823)	(0.00166)
Observations	8473144	8473144	8473144	7094088
Mean of Outcome	150	1.253	0.625	0.0648

Notes: Each column is a separate difference-in-differences regression of the placebo effect of the sending or the receiving hospital switching EHR vendors but without the pair gaining the same vendor from said switch as described in Equation D.1. Panel A shows transfer patient outcomes, while Panel B shows referral patient outcomes. All regressions include hospital pair fixed effects and year fixed effects as well as fixed effects for patient diagnostic code categories, indicators for 27 patient baseline chronic conditions, and controls for patient Charlson Comorbidity Index, patient age, patient sex, and patient race. Sample is limited to patients shared by hospital pairs that never have the same vendor (i.e., control hospital pairs in the main analysis). Standard errors are clustered at the hospital sender-receiver pair level and displayed in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

4 Allocative Effect of Interoperability on Patient Flows

Given the direct benefits of improved interoperability on patient outcomes, as well as potential hassle cost reductions,[27] patients and providers may prioritize interoperability when deciding where to send patients for additional care. This section presents empirical evidence on how EHR system compatibility affects patient flows between hospitals, showing that gaining the same EHR vendor—and thus higher interoperability—causes hospitals to share more patients. We outline our event study empirical strategy in Section 4.1 and present our main results in Section 4.2. We then address identification threats, including those shared with

[27]For example, Epic's Care Everywhere network provides searchable provider directories that are more complete for Epic users, enabling providers to easily select and send patient records to other Epic providers compared to the manual entry required for non-Epic providers.

the direct effects analysis due to both analyses relying on variation in same-vendor status for identification, in Section 4.3.

4.1 Empirical Strategy

To estimate the effect of EHR interoperability on cross-hospital patient flows, we use the same dynamic event study strategy as in Section 3 around changes in whether hospital sender-receiver pairs have the same EHR vendor, but now aggregated to the hospital pair level:

$$Y_{(hh')t} = \left(\sum_{r=-6}^{6} \beta_r \mathbf{1}_{\left[t=s_{(hh')}+r\right]} \right) + \alpha_{(hh')} + \gamma_t + \delta X_{(hh')t,mt} + \epsilon_{(hh')t}. \tag{4.1}$$

The main difference from Equation 3.2 is the hospital pair-level rather than patient-level analysis. Both use the same source of variation: changes in whether hospital pairs have the same EHR vendor. Here, $Y_{(hh')t}$ denotes either the number or fraction of patients transferred or referred from hospital h to hospital h' in year t (where the denominator for fractions is the total number of transfers or referrals sent from hospital h that year). Since this equation is at the hospital pair level, we cannot include patient-level controls but can control for hospital-level and market-level covariates like bed sizes and market by year fixed effects. We weight observations by combined sender and receiver hospital beds in 2005. All other variables, standard error clustering, and sample restrictions match those in Equation 3.1.[28] Each hospital pair enters our sample twice, once for each direction of patient flows. We limit the sample to hospital pairs either in the same Health Referral Region (HRR) (transfers) or sharing patients at baseline (referrals), filling in zeros for pairs with no flows in any given year.[29] As in Section 3, we limit analyses to hospital pairs that never have the same vendor plus those starting with different vendors and switching to the same vendor only once.[30] Hospital pairs in our sample share an average of 4 transfers and 45 referrals annually (Appendix Table A1).

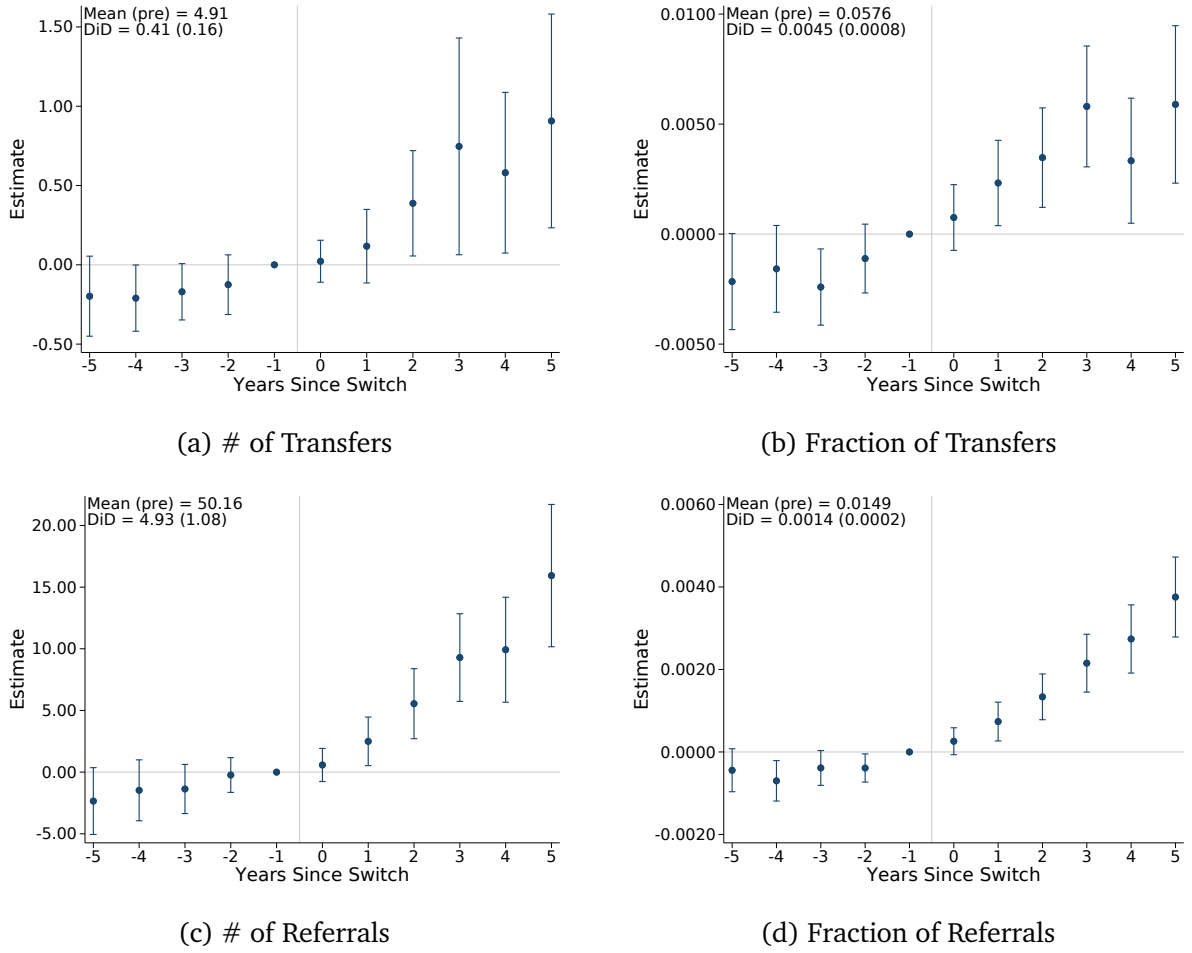
Our identification assumption mirrors that in the previous section: no time-varying hospital pair-specific factors correlate with both same-vendor status timing and patient flows. Absent switching to the same EHR vendor, we assume treated hospital pairs would experience similar

[28] Parameter r is the relative year since the sender-receiver pair switched to having the same vendor (occurring in year $s_{(hh')}$), we bin relative year endpoints ($r \geq 6$ and $r \leq -6$), and we exclude $r = -1$. Parameters $\alpha_{(hh')}$ and γ_t are hospital sender-receiver pair and year fixed effects, respectively. We cluster our standard errors at the hospital sender-receiver pair level and limit our samples to 2005-2017 (transfers) or 2008-2017 (referrals).

[29] These samples differ because referrals travel farther than transfers on average, requiring different market definitions. See Appendix A for more details.

[30] This sample restriction reduces bias from heterogeneous treatment effects due to including always-treated controls or multiply-treated observations. Results are robust to using all hospital pairs.

Figure 5: Event Studies of Allocative Effect of Gaining Same Vendor on Shared Patient Flows



(a) # of Transfers

(b) Fraction of Transfers

(c) # of Referrals

(d) Fraction of Referrals

Notes: Figures plot event study estimates of the effect of gaining the same EHR vendor on number of transfers (Panel A), fraction of transfers (Panel B), number of referrals (Panel C), and fraction of referrals (Panel D) shared between a hospital pair as described in Equation 4.1, including controls for hospital bed sizes and market by year fixed effects. The denominator for fraction of transfers (referrals) is the total number of transfers (referrals) sent from the sending hospital that year. Bars show 95% confidence intervals. The mean of each dependent variable among treated observations in the pre-treatment period is given as is the static difference-in-differences estimate and corresponding standard error.

patient flow trends as control pairs that never change same-vendor status. The event study allows visual inspection for pre-trends that may violate this parallel trends assumption.

4.2 Main Results

Figure 5 plots event study estimates of the effect of gaining the same EHR vendor on the number of transfers (Panel A), fraction of transfers (Panel B), number of referrals (Panel C), and fraction of referrals (Panel D) shared between a hospital pair as described in Equation 4.1.

Overall, there is a positive and statistically significant relationship between hospitals having the same EHR vendor and their shared patient flows. When hospitals gain the same vendor, their shared transfers increase by 0.41 (8% increase from the mean) and the fraction of all transfers from the sending hospital sent to the receiving hospital increases by 0.0045 (0.45 percentage points; 8% increase). Shared referrals increase by 4.9 (10% increase) and their fraction increases by 0.0014 (9% increase).[31] While these coefficients appear small, they are economically significant given their means.[32] There is no evidence of pre-trends for transfer or referral levels. While slight pre-trends appear for fractions, pre-treatment relative time estimates are not jointly significant and clear trend deviations occur at treatment for fractions of referrals. These results are robust to estimation following Callaway and Sant'Anna (2021) (Appendix Figure D6), to not weighting observations (Appendix Figure D7), and to clustering standard errors at the HRR-level (Appendix Table D5). We also find treatment effects of similar magnitudes but opposite signs when looking at the effect of hospital pairs *losing* the same EHR vendor (Appendix Section D.2.4).

Similar to our direct effect event studies in Section 3, these patient flow event studies display increasing treatment effects over relative time. As before, this pattern likely reflects multi-year EHR rollouts and gradual learning of interoperability benefits with new same-vendor hospitals. Notably, while direct effect event studies show immediate changes in patient outcomes, patient flow estimates only become statistically significant one to two years post-treatment. This difference in timing provides further support that changes in patient composition do not drive the direct effect results from the previous section.

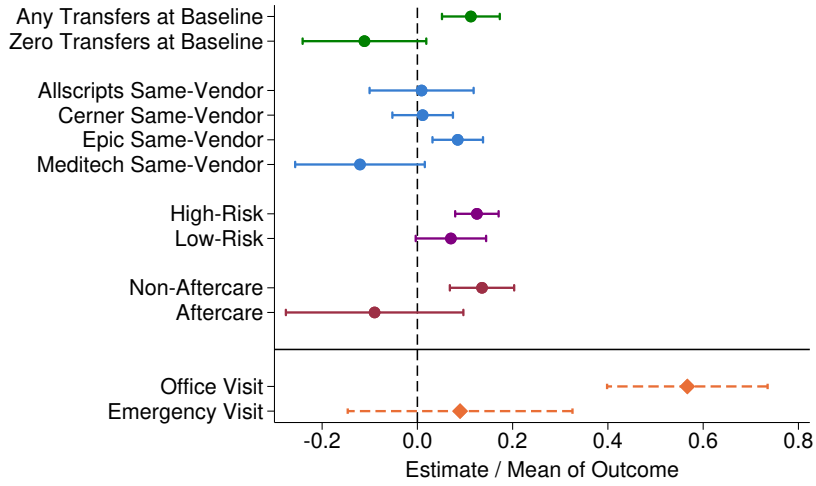
Heterogeneity To better understand the mechanisms driving patient flow responses and to identify contexts where interoperability matters most for hospital choice decisions, Figure 6 presents treatment effect heterogeneity across hospital, EHR vendor, and patient characteristics. The effect of interoperability on shared transfers is entirely driven by a strengthening of pre-existing hospital relationships rather than the start of new ones (green dots).[33] Effects are also largest and most precise for hospital pairs using Epic as their shared vendor (blue dots),

[31] Minor variations in magnitudes across levels and fractions stem from two factors: slight sample differences (fractions have fewer observations because some hospitals send zero total flows) and different parallel trends assumptions (levels assume patient flow numbers would continue on similar trends; fractions assume shares would continue on similar trends).

[32] Means are low because many hospital pairs share no flows annually. Nearly 80% of transfer hospital pair observations and 52% of referral hospital pair observations share zero flows. Excluding zeros would increase average transfers to 11.6, fraction of transfers to 0.131, referrals to 94.2, and fraction of referrals to 0.0279, but doing so would require selecting on outcomes.

[33] This intensive-margin-only result is robust to different market definitions (e.g., HSA, within 100 km, within 50 km, within 15 km).

Figure 6: Allocative Effect Heterogeneity by Hospital, Vendor, and Patient Characteristics



Notes: Figure plots difference-in-differences estimates (dots/diamonds) and 95% confidence intervals (bars) of the effect of gaining the same EHR vendor on number of transfers (dots) and number of referrals (diamonds), divided by the baseline mean to convert the estimates to percentages, for different groups of hospital sender-receiver pairs, patient types, and visit types (as described in Equation D.4). Estimates above the horizontal line are for transfers; those below the horizontal line are for referrals. "Epic Same-Vendor" indicates that both hospitals have Epic when they gain the same vendor. Patient risk is measured as predicted mortality using baseline measures of patient risk such as age, sex, race, and chronic conditions. "High-risk" patients are transfer patients with the top half of such predicted mortality rates, while "low-risk" patients are those with the bottom half.

consistent with Epic's superior within-vendor interoperability documented in Section 6.[34] Effects are modestly larger for higher-risk transfer patients (purple dots), though confidence intervals overlap and the difference is not statistically significant. By service type, we find much larger effects for patients transferred to receive advanced care than for aftercare (red dots). We also find large effects for referred office visits but not emergency visits (orange diamonds), suggesting emergency referral decisions prioritize other factors like availability and distance over interoperability.[35]

Total Flows The treatment effects may represent either a reallocation of patient flows across hospitals or an increase in total patient flows following hospital EHR vendor switches. To differentiate between these explanations, we run a hospital-level difference-in-differences regression around hospital EHR vendor switches where outcomes are total patient flows to and from the switching hospital.[36] Table 4 displays these estimates. We find that switching EHR

[34]This superior interoperability may help explain Epic's growing market share (Figure 1a) and prevalence among large hospital systems (Appendix Table B1).

[35]We further find larger effects for outpatient than inpatient referrals.

[36]Additional specification details are in Appendix Section D.2.1.

Table 4: Hospital Vendor Switches on Total Patient Flows

	Total Patient Flows			
	(1) Transfers Sent	(2) Transfers Received	(3) Referrals Sent	(4) Referrals Received
Post-EHR Switch	0.793	-2.314	14.33	425.2*
	(4.461)	(4.205)	(101.8)	(239.3)
Observations	34,542	34,542	29,805	29,805
Mean of Outcome	249.08	210.64	4,587.27	4,839.17

Notes: Each column is a separate hospital-year-level difference-in-differences regression of the effect of switching EHR vendors on total patient flows to/from the switching hospital, with hospital fixed effects and year fixed effects: $Y_{ht} = \beta * \mathbf{1}_{[\text{Post-Switch}_{ht}]} + \alpha_h + \gamma_t + \epsilon_{ht}$ (Equation D.2). Standard errors are clustered at the hospital level. Observations are weighted by bed size in 2005. Sample is limited to hospitals that switch vendors once or never. Standard errors are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

vendors does not affect total transfers sent or received, nor total referrals sent. Switching vendors does increase total referrals received by 8.8% of the mean, a marginally significant effect entirely driven by hospitals switching to Epic (Appendix Figure D5). These results are robust to log transformations and additional control variables, and event study versions show no pre-trends. Overall, we interpret these findings as evidence that our hospital pair-level results are largely due to patient reallocation across hospitals rather than increases in the extensive margin of transfers or referrals.

Magnitudes To comprehend the magnitude of this reallocation of patients across hospitals, we conduct back-of-the-envelope calculations of the number of patient flows reallocated due to EHR vendor switches in our sample period.[37] After a hospital switches EHR vendors, it has on average 7.3 other same-vendor hospitals in its HRR and 21.7 other same-vendor hospitals with shared baseline referrals. Multiplying these by our treatment effect estimates suggests that when hospitals switch EHR vendors, they send approximately 3% more transfers and 3% more referrals to other newly same-vendor hospitals. With over 7.8 million Medicare cross-hospital transfers during 2005-2019, over 930 million Medicare cross-hospital referrals during 2008-2019, and 40% of hospitals switching EHR vendors at least once, these estimates imply that over 100,000 Medicare transfers and 11 million Medicare referrals were reallocated across hospitals due to EHR vendor switches. Considering average costs of cross-hospital transfers and referrals, these patient flow changes reallocated $10.4 billion of Medicare spending across

[37] See Appendix Section D.2.2 for details.

hospitals during this 15-year period, or approximately $700 million annually.[38]

4.3 Threats to Validity

We now address potential confounding factors in using hospital pair-level variation in same-vendor status to identify interoperability effects on patient flows. As in our direct patient outcomes analysis (Section 3.4), our patient flow results are robust to controlling for various hospital and market characteristics, including changes in hospital sender and receiver beds, capabilities, and ownership, and even adding hospital sender-by-year and hospital receiver-by-year fixed effects (Appendix Table D5).[39] Additionally, placebo tests of vendor switches without alignment yield null effects, confirming again that results are not driven by EHR system quality changes (Appendix Table D8). Finally, hospitals may strategically choose to have the same EHR vendors as other hospitals in order to facilitate or discourage patient sharing, raising additional concerns about endogeneity of EHR choices. We next explain how we address these concerns with an instrumental variables approach.

Endogeneity, Measurement Error, and 2SLS Endogeneity and measurement error may bias our estimates. First, whether hospitals share the same EHR vendor results from hospital choices that could introduce selection bias and challenge external validity. Hospitals may intentionally switch to the same vendor as others to improve shared patient outcomes or flows, or conversely avoid same vendors to prevent loss of patients if records become more interoperable.[40] The direction of this bias is unpredictable, and while several robustness findings may mitigate these concerns, endogeneity could still bias our estimates in undetectable ways.[41] Second, measurement error may introduce bias because the exact year hospitals switch vendors is difficult to

[38] These calculations are likely underestimates. First, they assume constant treatment effects across hospitals of different sizes. Allowing larger hospitals that send more transfers and referrals to have larger treatment effects would increase total reallocated flows. Second, calculations are limited to Traditional Medicare patients only. If treatment effects are similar for patients with other insurance types, the number of affected flows would rise.

[39] Hospital characteristics control for internal hospital changes during EHR vendor switches, while hospital-by-year fixed effects for both sender and receiver more directly address this concern. The specification with hospital-by-year fixed effects resembles gravity equations common in the trade literature (Anderson and Van Wincoop, 2003). Following this literature, Appendix Table D6 shows flow results are robust to Poisson Pseudo Maximum Likelihood estimation (Silva and Tenreyro, 2006) addressing frequent zeros in bilateral patient flows.

[40] The latter concern is empirically supported: Baker et al. (2015) finds that when states cap the fees doctors can charge patients for copies of their paper medical records—reducing the cost of HIE as interoperability does—patients become significantly more likely to switch doctors.

[41] These findings include robustness to additional controls, absence of significant pre-trends, and interoperability's secondary role in hospital vendor decisions. Further, Appendix Section D.2.6 shows that when hospital h' switches to have the same vendor as hospital h, h starts sending more patients to h'. The receiver switching may be less endogenous than the sender switching, as the sender is not making the EHR vendor choice.

define. Our primary data source (AHA IT) defines switches as when new systems serve the majority of patients, while our secondary source (HIMSS) defines switches as when systems are live and operational. Instrumenting for same-vendor status in one survey with same-vendor status in the other yields 2SLS estimates averaging twice the magnitude of their OLS counterparts, suggesting measurement error significantly attenuates our estimates of the effects of interoperability on patients (Appendix Section D.2.7).

To address these concerns, we construct an instrument for hospital EHR vendor choice in the spirit of Dranove et al. (2014), exploiting that hospitals face pressure to use the same EHR vendor as other hospitals in their systems even when those hospitals are located outside of their local markets. For each hospital, we identify other hospitals in the same system but outside the focal hospital's market and calculate the shares of beds among these hospitals using each EHR vendor. These shares represent "system pressure" for the focal hospital to adopt each vendor. Limiting these leave-out calculations to hospitals outside each focal hospital's market eliminates endogeneity in vendor choice from local market pressure. To reduce reverse causality in the first stage, we limit the sample to hospital pairs where neither sender nor receiver is the largest hospital in their respective systems. We then instrument for hospital pair same-vendor status using interactions of system pressure between sender and receiver hospitals for each of the four major vendors: Epic, Cerner, Meditech, and Allscripts.[42] Results show a large, positive, and statistically significant relationship between same-vendor status and shared patient flows. 2SLS estimates are approximately 2.4 (3.7) times larger than OLS estimates for fraction of transfers (referrals) and approximately 3.1 (6.1) times larger for transfer patient charges (referral patient readmission rates) (Appendix Tables D13, D14, D17, and D18). These larger 2SLS estimates provide further evidence of a positive causal relationship between same-vendor status and shared patient flows and outcomes.

5 Allocative Effect of Interoperability on Patient Outcomes

Given the allocative effect of interoperability on patient flows, we now examine whether patient reallocation across hospitals due to EHR vendor switches consequently affects patient outcomes. Prior research establishes that hospital quality causally affects patient outcomes (Doyle et al., 2019; Chandra et al., 2023), and—while not yet explicitly shown—this relationship likely extends to transferred and referred patients. EHR vendor-driven reallocation could meaningfully impact patient outcomes provided it affects patients receiving non-routine care

[42] Of note, our instruments are defined for only the 32.6% of hospital pair-year observations where both hospitals belong to multi-hospital systems spanning more than one HRR. Additional details are available in Appendix Section D.2.8.

and occurs between hospitals that are not close substitutes.[43] In this section, we present evidence that interoperability-driven patient reallocation affects outcomes by sending patients to—for example—higher or lower quality hospitals than before. We begin with presenting our empirical strategy (Section 5.1) and main results (Section 5.2), followed by robustness checks (Section 5.3).

5.1 Empirical Strategy

Since EHR vendor switches reallocate patients away from previously same-vendor hospitals toward newly same-vendor hospitals, any allocative effects on patient outcomes depend on the characteristic differences between these gaining and losing hospitals. Some hospitals may switch to vendors used by better quality or closer hospitals in their markets while others switch to vendors used by worse quality or farther hospitals. We thus analyze allocative effects using a difference-in-differences specification that interacts an indicator for whether the sending hospital has switched EHR vendors with the change in characteristics of other local same-vendor hospitals from pre- to post-switch periods. This design allows us to test whether, conditional on EHR vendor switches, changes in local same-vendor EHR network quality pass through to changes in patient health outcomes—that is, to determine the "predictive validity" of changes in local vendor network characteristics (Chandra et al., 2023).

Specifically, we estimate:

$$Y_{iht} = \beta_1 * \mathbf{1}_{[\text{Post-Switch}_{ht}]} + \beta_2 * \left(\mathbf{1}_{[\text{Post-Switch}_{ht}]} * \Delta\text{Network}_{Yh}\right) + \alpha_h + \gamma_t + \delta X_{it,ht,mt} + \epsilon_{iht}, \quad (5.1)$$

where Y_{iht} is the outcome for patient i transferred or referred from hospital h in year t, $\mathbf{1}_{[\text{Post-Switch}_{ht}]}$ indicates whether hospital h has yet switched EHR vendors, and α_h and γ_t are hospital and year fixed effects, respectively. Baseline controls $X_{it,ht,mt}$ include market-by-year fixed effects, with results robust to adding patient characteristics from Section 3 (further discussed in Section 5.3). We cluster standard errors at the hospital level and restrict our sample to patients sent from hospitals that switch vendors once or never. To focus on patients most likely to experience reallocation due to interoperability, we further limit our sample to patients transferred or referred to hospitals using the same EHR vendor as their sending hospital. For example, if hospital h switches from Cerner to Epic, we analyze outcomes for patients sent to Cerner hospitals pre-switch and to Epic hospitals post-switch.

$\Delta\text{Network}_{Yh}$ represents the change in average baseline characteristics of other same-vendor

[43] Figure 6 shows reallocation effects for patients of many types and procedures, providing support for the former condition.

hospitals in h's market (HRR_h) after h switches vendors, where characteristic Y matches the outcome variable (e.g., if the outcome is distance, this term is the change in average distance to other same-vendor hospitals after a vendor switch). We calculate these averages using 2005 data and weighting other local same-vendor hospitals by beds. We limit the set of other local same-vendor hospitals to those with which h has shared patient flows at baseline, given that heterogeneity in Figure 6 suggests this group is the relevant choice set for h.[44]

Altogether, in Equation 5.1, β_2 captures whether changes in local vendor network characteristics pass through to corresponding changes in experienced outcomes for patients sent to same-vendor hospitals. A statistically significant $\beta_2 > 0$ would signify such predictive validity. The coefficient's magnitude elucidates how closely predicted changes align with actual changes. For example, a coefficient of 1 would imply that when a hospital switches to a vendor with a local network of hospitals located 1 km farther away on average, the distance traveled by that hospital's same-vendor transfer and referral patients increases by 1 km on average. We expect our β_2 estimates to be a lower bound on total pass-through due to potential measurement error in constructing local EHR network measures. The other coefficient, β_1, indicates how experienced outcomes of patients sent to other same-vendor hospitals change when average baseline characteristics of those hospitals remain the same after the sending hospital switches vendors. Our main identification assumption is that patients sent from hospitals that switch vendors (e.g., treated hospitals) to other same-vendor hospitals would have experienced similar trends in outcomes before versus after the switch as those sent from hospitals that never switch vendors (e.g., control hospitals), in the absence of such treatment.

5.2 Main Results

Table 5 shows results from estimating Equation 5.1 for four outcomes: distance traveled (kilometers), total charges (2019 dollars), 30-day readmission rates, and 60-day mortality rates. Most importantly, all estimates of β_2 are statistically significantly greater than zero, implying that baseline distance, charges, readmission, and mortality rates of local EHR vendor networks have predictive validity. Hospitals switching to local vendor networks comprised of farther, more expensive, or lower readmission/mortality rate hospitals than before see larger

[44] Appendix Figure D11 shows correlations between local EHR vendor network changes ($\Delta\text{Network}_{Yh}$) and patient outcome changes (ΔY_{ih}) across switching hospitals for four outcomes: distance to receiver hospitals, patient charges, 30-day readmission rates, and 60-day mortality rates. These correlations are linear, motivating our linear functional form for the interaction term in Equation 5.1. Both $\Delta\text{Network}_{Yh}$ and ΔY_{ih} have wide distributions centered near zero, indicating both that hospitals are equally likely to switch to vendors used by closer versus farther hospitals and that their patients experience both increases and decreases in outcomes post-switch. The wide distributions of outcome changes are also evident in large standard errors when estimating Equation 5.1 without the interaction term (Appendix Table D20).

effects on their patients' distances, charges, readmissions, or mortality, respectively. For example, Column 2 of Panel A shows that when a hospital switches to a vendor with a local network comprised of hospitals with mean baseline transfer patient charges $1,000 higher than those in its old network, same-vendor transfer patients sent from that hospital experience an additional $425 in charges compared to patients from hospitals whose networks remain price-stable.[45] The magnitudes of these effects also vary across contexts. For hospitals switching to higher-quality vendor networks, benefits to patients are larger when the vendor is more prevalent in the local market or health system (Appendix Figure D12), suggesting that both network quality and breadth matter for outcomes. Higher-risk patients also experience particularly large gains from these network improvements (Appendix Figures D12 and D13).

5.3 Threats to Validity

Our empirical strategy faces two main identification concerns: (1) changes in the composition of patients sent to same-vendor hospitals after vendor switches, and (2) other concurrent hospital-level changes that could affect patient outcomes. We now address these concerns.

Patient Composition Estimates of β_2 could be biased if the composition of patients sent to same-vendor hospitals changes after hospitals switch EHR vendors. However, this bias will only be positive if patient selection varies systematically with changes in local EHR vendor network quality—specifically, if hospitals that switch to better-quality networks start sending healthier patients to same-vendor hospitals, while those switching to worse-quality networks start sending sicker patients. We find no evidence of this as our estimates are robust to controlling for extensive patient characteristics, including age, chronic conditions, and Charlson Comorbidity Index (Appendix Table D21). Such robustness to observable patient characteristics provides some reassurance that our results are not driven by changes in unobservable ones.

[45] Several factors may explain why estimated interaction coefficients are not equal to 1. First, our baseline measures for charges, readmissions, and mortality are not risk-adjusted, and Chandra et al. (2023) show this attenuates predictive validity estimates. Second, using baseline characteristics does not allow for hospital quality changes over time. Third, ΔNetwork compares bed-weighted average characteristics of new- versus old-vendor hospitals, but switching hospitals may not equally redistribute patients across new-vendor hospitals. For example, a hospital may send all transfers to the highest-quality new-vendor hospital, thus improving experienced patient outcomes even if average network quality is unchanged. This may also explain why some β_1 estimates are significant.

Table 5: Predictive Validity of Changes in Local EHR Network Characteristics

(a) Transfers

	Outcomes for Same-Vendor Transfer Patients			
	(1) Distance (km)	(2) Mean Charges	(3) 30 Day Readmit	(4) 60 Day Death
Post-EHR Switch	4.440***	1106.4	0.00129	0.00295
	(1.664)	(5981.7)	(0.00634)	(0.00545)
× ΔNetwork	0.255**	0.425***	0.156**	0.222***
	(0.106)	(0.0987)	(0.0622)	(0.0573)
Observations	571377	620903	512363	620903
Mean of Outcome	35.94	78190.73	0.30	0.14

(b) Referrals

	Outcomes for Same-Vendor Referral Patients			
	(1) Distance (km)	(2) Mean Charges	(3) 60 Day Readmit	(4) 60 Day Death
Post-EHR Switch	-0.443	0.606	0.00535**	-0.00381
	(0.988)	(1.339)	(0.00242)	(0.00255)
× ΔNetwork	0.313***	0.297***	0.104*	0.189***
	(0.0518)	(0.0725)	(0.0568)	(0.0697)
Observations	12739822	13994311	11632215	13994311
Mean of Outcome	22.82	80.27	0.05	0.09

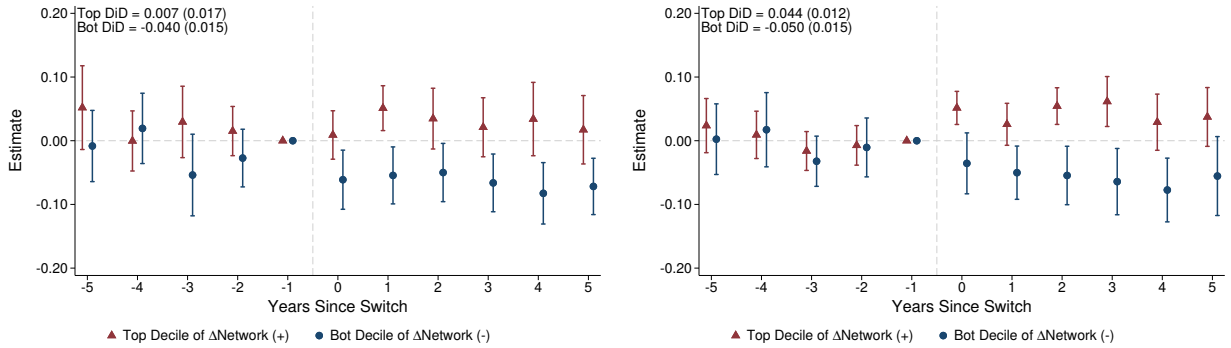
Notes: Each column is a separate patient-level difference-in-differences regression of the effect of a hospital switching EHR vendors on outcomes experienced by transfer (Panel A) and referral (Panel B) patients sent from the switching hospital to another same-vendor recipient hospital as specified in Equation 5.1. All regressions include hospital fixed effects, year fixed effects, and market-by-year fixed effects. The indicator for post-switch is interacted with the change in the average baseline characteristics of other hospitals with the same EHR vendor in the same local market as the switching hospital. Standard errors are clustered at the hospital level and displayed in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Parallel Trends To address concerns about other hospital-level changes driving our results and test our parallel trends assumption, we estimate the event study version of Equation 5.1:

$$Y_{iht} = \left(\sum_{r=-6}^{6} \beta_r \mathbf{1}_{[t=s_h+r]} \right) + \alpha_h + \gamma_t + \delta X_{it,ht,mt} + \epsilon_{iht}, \tag{5.2}$$

where r is the relative year since the sending hospital switched EHR vendors (occurring in year s_h), we bin relative year endpoints ($r \geq 6$ and $r \leq -6$), and we exclude $r = -1$. All other parameters match those in Equation 5.1. To allow for heterogeneity by $\Delta\text{Network}_{Yh}$, we split

Figure 7: Event Studies of Predictive Validity, Split by Decile of Change in Local EHR Network



(a) 30 Day Readmit for Transfers

(b) 60 Day Death for Transfers

Notes: Figures plot event study estimates from Equation 5.2. The sample in Panel A is transfer patients and the outcome is 30-day readmission rates; the sample in Panel B is transfer patients and the outcome is 60-day mortality rates. Estimates for the top decile of $\Delta\text{Network}_{Yh}$ are in red triangles; estimates for the bottom ("bot") decile of $\Delta\text{Network}_{Yh}$ are in blue circles. Bars show 95% confidence intervals. Standard difference-in-differences estimates for each group are also given (labeled "DiD").

patients into deciles of $\Delta\text{Network}_{Yh}$ and run Equation 5.2 separately by decile.

Figure 7 plots event study estimates from Equation 5.2 for the top decile group (i.e., patients sent from hospitals switching to local vendor networks with much *higher* baseline readmission and mortality rates than before; red triangles) and bottom decile group (i.e., patients sent from hospitals switching to local vendor networks with much *lower* baseline readmission and mortality rates than before; blue circles). While the estimates are slightly noisy, these figures clearly show no evidence of pre-trends and a deviation in trends for both groups at the moment of the EHR vendor switch.[46] Patients sent from hospitals switching to vendors used by much worse quality hospitals are much worse off than before, while those sent from hospitals switching to vendors used by much better quality hospitals are much better off. In other words, when a hospital switches to a better (worse) local vendor network, its patients' outcomes improve (worsen).[47]

[46] Appendix Table D22 shows that patients in these two deciles are balanced on baseline characteristics.

[47] This immediate treatment effect on patient outcomes is consistent with our earlier finding of gradual treatment effects on patient flows. This outcomes analysis limits samples to same-vendor patients, so effects should be immediate. For example, when a hospital switches to a vendor used by hospitals 1 km farther away on average, the first same-vendor patient sent after the switch should travel 1 km farther on average. As more patients are sent to new same-vendor hospitals over time, those patients should also travel 1 km farther on average, and thus the treatment effect remains constant.

6 Model of Patient Flows

Our findings in previous sections show that imperfect interoperability affects patients both directly and allocatively. These results highlight interoperability's crucial role in the decision of where to transfer and refer patients, potentially rivaling more traditional hospital demand factors like cost, quality, and distance. In this section, we quantify the above tradeoff in a simple structural model of patient flows that expands on standard hospital demand frameworks (e.g., Ho, 2006) by adding interoperability to the utility function. This model rationalizes patient flows as a function of interoperability and other hospital characteristics and offers a unified welfare framework that captures both the direct and allocative effects of changing interoperability levels—an exercise we explore in Section 7.

6.1 Set Up

In each year t, a hospital h in market m has a set $\mathcal{P}_{ht}$ of patients needing to transfer (refer). For each patient $p \in \mathcal{P}_{ht}$, we model hospital h as choosing which other hospital $h' \in m \backslash \{h\}$ to send this patient to in order to maximize the following utility function:

$$\begin{aligned} u_{hh'pt} = \theta_1 \, \iota_{e_{ht},e_{h't},t} + \theta_2 \, \mathbf{1}_{[hh' \text{ always same system}]} + \theta_3 \, d_{hh'} \\ + \xi_{h'} + \xi_h + \xi_{mt} + \nu_{hh't} + \bar{\epsilon}_{hn_h(h')pt} + (1-\rho)\bar{\epsilon}_{hh'pt}. \end{aligned} \tag{6.1}$$

Here, $\iota_{e_{ht},e_{h't},t}$ is the level of interoperability between the EHR vendor e_{ht} used by hospital h and $e_{h't}$ used by hospital h' in year t; $\mathbf{1}_{[hh' \text{ always same system}]}$ is an indicator for whether hospitals h and h' are always in the same hospital system, which captures potential financial incentives to keep patient flows within hospital systems; $d_{hh'}$ is the distance between hospitals h and h', included to capture a distaste for travel.[48] We proxy for hospital quality with receiving hospital fixed effects $\xi_{h'}$. Thus, the tradeoff in the transfer (referral) problem is between interoperability, same-system status, distance, and hospital quality.

Furthermore, we assume that the outside option in this problem is to send the patient to a hospital located outside of hospital h's market. We adopt this approach—rather than defining the outside option as keeping the patient at hospital h—because of our earlier findings that total flows sent by a hospital remained unchanged when it switched to a different EHR system.[49] Under this assumption, the sending-hospital fixed effects ξ_h and market-by-year fixed

[48] Changes in hospital system status are relatively infrequent, which makes it challenging to identify a coefficient on an indicator of whether two hospitals are in the same system using variation in system membership.

[49] Table 4 shows this result for both the number of transfers sent and the number of referrals sent. However, we also find that hospitals that switched their EHR system to Epic received more referrals after the switch (Appendix

effects ξ_{mt} capture variation in the tendency to send patients outside the market across sending hospitals, markets, and years. Moreover, recognizing that the decision to send a patient outside their market differs fundamentally from the decision to send a patient to another hospital within the same market, we specify a nested logit structure for the error term. We allow for two nests: (1) hospitals located inside the same market m as the inside option, and (2) hospitals located outside market m as the outside option. Parameter ρ characterizes the correlation of utilities that a patient experiences from the same nest.[50]

Two points clarify our demand model's assumptions and interpretation. First, by specifying a demand model, we implicitly assume that recipient hospitals act as passive receivers. That is, all hospitals always accept transfers (referrals). In practice, although patient transfers often involve discussions between sending and receiving hospitals, rejections are uncommon.[51] Second, while we attribute the transfer (referral) decision to the sending hospital in our exposition, this decision should be interpreted as a joint decision between the patient and the hospital, with utility reflecting a combination of patient and hospital welfare. Accordingly, the coefficient θ_1 on interoperability captures both the direct benefits of interoperability for patient outcomes, as shown in Section 3, as well as the potential hassle costs borne by the patient and the sending hospital due to a lack of interoperability. Our framework, however, cannot disentangle these components.

Define $\sigma_{hh't} = \theta_1 \iota_{e_{ht},e_{h't},t} + \theta_2 \mathbf{1}_{[hh' \text{ always same system}]} + \theta_3 d_{hh'} + \xi_{h'} + \xi_h + \nu_{hh't}$ as the mean utility in year t of hospital h sending a patient to hospital h'. Integrating over the logit errors yields the share $s_{hh't}$ of transfer (referral) patients that hospital h sends to hospital h':

$$s_{hh't} = \frac{\exp\left([\delta_{hh't}/(1-\rho)]\right)}{\exp\left(\left[\delta_{hn_h(h')t}/(1-\rho)\right]\right)} \times \frac{\exp\left(\delta_{hn_h(h')t}\right)}{1+\sum_{n_h}\exp\left(\delta_{hn_ht}\right)}. \tag{6.2}$$

We estimate this model in two steps. First, we use hospital-level survey reports on interoperability to estimate vendor-to-vendor and year-specific measures of interoperability. To our knowledge, no direct measures of interoperability at this level exist. Second, we incorporate

Figure D5). This pattern would be predicted by our model if two conditions hold: first, higher interoperability is preferred in the transfer (referral) decision (i.e., $\theta_1 > 0$), and second, Epic offers superior within-vendor interoperability. Both of these conditions are later confirmed by our model estimates.

[50] More specifically, $n_h(h')$ represents the nest to which the receiver hospital h' belongs. The error term $\bar{\epsilon}_{hh'pt}$ is independently and identically distributed according to the Type I Extreme Value distribution, and the idiosyncratic group preference $\bar{\epsilon}_{hn_h(h')pt}$ follows the unique distribution such that $\bar{\epsilon}_{hn_h(h')pt} + (1-\rho)\bar{\epsilon}_{hh'pt}$ is also distributed according to the Type I Extreme Value distribution. When $\rho = 1$, all patients stay within the same chosen nest; when $\rho = 0$, the nested logit simply collapses to standard logit.

[51] Our passive-receiver assumption is particularly appropriate in emergency cases covered by the Emergency Medical Treatment and Labor Act (EMTALA). Even for non-EMTALA cases, these is evidence that hospitals usually accept transferred patients, despite some instances of "patient dumping" (Kindermann et al., 2014).

our interoperability estimates into our nested logit demand model. We employ an instrumental variables approach in this demand estimation to address potential endogeneity and measurement error in our interoperability measures. The following sections provide further details on our estimation procedure and present our model estimates of interoperability levels (Section 6.2) and demand parameters (Section 6.3).

6.2 Estimation Part One: Interoperability Levels

In this section, we estimate the interoperability levels that enter our demand Equation 6.1. Using hospital-level reports of interoperability from the AHA IT Survey, we construct measures that distinguish between within-vendor and across-vendor interoperability, allowing for heterogeneity over time and across vendors. These measures not only provide greater precision than relying on a same-vendor indicator as a proxy for interoperability, but also offer a richer understanding of the interoperability landscape. Consistent with our finding that hospitals prefer to transfer patients to others using the same vendor, we find that within-vendor interoperability is higher than across-vendor interoperability for all vendors. Moreover, there is substantial variation across vendors, with Epic exhibiting particularly strong within-vendor interoperability.

Estimation Strategy Our estimation of vendor-to-vendor interoperability exploits survey responses on hospitals' interoperability experiences collected annually by the AHA IT Survey beginning in 2013. Of particular relevance for our study, the survey asks each hospital whether it can integrate data received electronically from outside sources without the need for manual entry, with three possible responses: "yes, routinely," "yes, but not routinely," or "no."[52]

Our approach exploits these hospital-level responses, together with variation in how hospitals interact with others in their market and in the EHR systems these hospitals use. For tractability, we characterize interoperability for each vendor along two dimensions: (1) within-vendor interoperability (e.g., Epic to Epic) and (2) across-vendor interoperability (e.g., Epic to non-Epic). Intuitively, if an Epic-using hospital only interacts with other Epic hospitals, then its reported interoperability level entirely reflects Epic's within-vendor interoperability. In the opposite case, if that hospital only interacts with non-Epic hospitals, its reported interoperability entirely reflects Epic's across-vendor interoperability. More generally, the greater the share

[52]Figure 2a plots the average progress on various dimensions of interoperability, showing that integration has been the most challenging to achieve. Moreover, integration is most consistent with the ONC's definition of interoperability, which is "the ability of a system to exchange electronic health information with and use electronic health information from other systems without special effort on the part of the user" (Patel et al., 2016). For the remainder of the paper, we use the word "interoperability" exclusively to mean the ability to integrate.

of that hospital's interactions with Epic hospitals relative to non-Epic hospitals, the more informative its reported interoperability is about Epic's within-vendor interoperability rather than its across-vendor interoperability.

Formally, let ι_{ht} denote hospital h's reported ability to integrate data in year t. For each EHR vendor e, let $\iota_{e,e,t}$ and $\iota_{e,-e,t}$ denote the within-vendor and across-vendor interoperability in year t of vendor e's EHR system. We recover vendor- and time-specific interoperability levels $\{\iota_{e,e,t}, \iota_{e,-e,t}\}_{e,t}$ from hospitals' surveyed responses $(\iota_{ht})_{h,t}$ via the following decomposition:

$$\iota_{ht} = f\left(\iota_{e_{ht},e_{ht},t} \times r(ht, e_{ht}) + \iota_{e_{ht},-e_{ht},t} \times (1 - r(ht, e_{ht})\right) + \epsilon_{ht}). \tag{6.3}$$

Here, $r(ht, e_{ht})$ is the fraction of patients hospital h receives in year t that come from other hospitals using the same EHR vendor e_{ht} as h does;[53] f represents an ordered probit that allows for the three ordinal outcomes described above.

In estimating Equation 6.3, there is a potential endogeneity concern: hospitals that frequently receive patients from others using the same EHR vendor may invest more in within-interoperability.[54] This endogeneity would upward bias the contribution of within-vendor interoperability to reported hospital-level interoperability. To address this concern, we instrument for $r(ht, e_{ht})$ using the share of other hospital beds in hospital h's local market whose hospitals use the same EHR vendor as h in year t—that is, what $r(ht, e_{ht})$ would be if hospital h equally shared patients with all other hospital beds in its market. Since f is non-linear, we adopt a control function approach (Wooldridge, 2015) and apply an additional transformation to scale the resulting interoperability estimates between 0 and 1. To improve precision, we estimate interoperability levels in three bins: 2013-2015, 2016-2017, and 2018-2019. Appendix Section C.2 provides further details of this estimation procedure.[55]

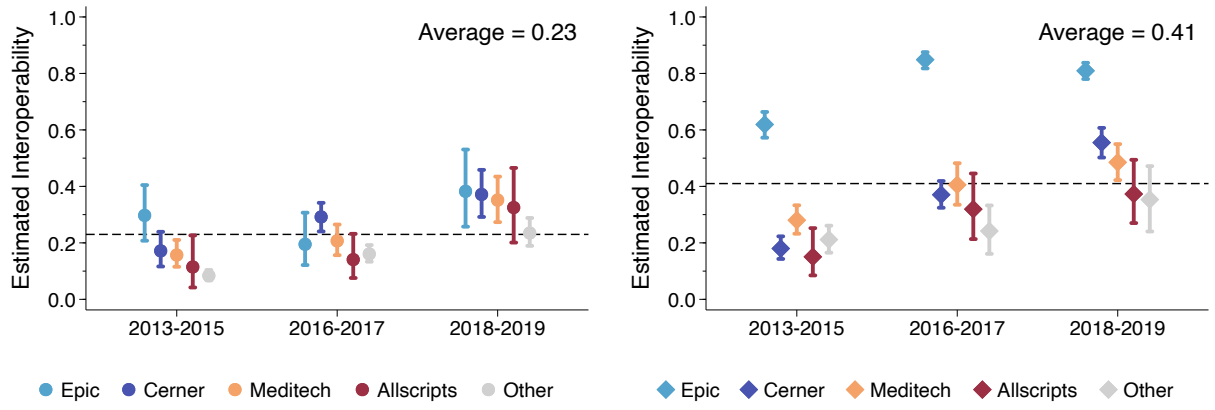
Estimation Results Figure 8 presents estimates of across-vendor (Panel A) and within-vendor (Panel B) interoperability separately by vendor and time period. Three main insights emerge from these estimates. First, across-vendor interoperability is significantly and consistently lower than within-vendor interoperability for all EHR vendors and time periods. This result supports our use of same-vendor status as a proxy for interoperability between hospital pairs

[53] To measure $r(ht, e_{ht})$ we supplement the Medicare claims with DocGraph Hop Teaming data from 2013-2019, which captures comprehensive patient sharing relationships (including transfers, referrals, and other types of shared patients) between healthcare providers constructed from the underlying claims data.

[54] EHR system functionalities are typically customized to hospital needs and could affect interoperability.

[55] Our interoperability estimates are robust to alternative specifications of f, to including additional controls, such as whether hospital h participates in a regional health information exchange organization, and to whether we define $r(ht, e_{ht})$ based on patients received or patients sent.

Figure 8: Estimated Across- and Within-Vendor Interoperability Levels



(a) Across-Vendor

(b) Within-Vendor

Notes: Figures plot estimates of across-vendor (Panel A) and within-vendor (Panel B) interoperability, normalized to be between 0 and 1, and separately by EHR vendor and time period. Dots and diamonds represent point estimates while bars show 95% confidence intervals, which are calculated using 1000 bootstrapped samples. Data is pooled for 2013-2015, 2016-2017, and 2018-2019. Dotted lines represent averages across all vendors and time periods.

in the previous sections. Second, the gap between these two dimensions varies considerably across vendors. Epic exhibits markedly higher within-vendor interoperability compared to the other vendors and is the sole vendor for which such interoperability approaches the maximum of one. This is consistent with Epic hospitals reporting the highest utilization of a within-vendor data exchange network (Epic Care Everywhere) which may facilitate better within-vendor interoperability (Appendix Figure B5).[56] Thus, while it is easier for all vendors to exchange health information internally rather than externally, high internal interoperability is not a given for everyone. Third, while both within- and across-vendor interoperability have improved over time, within-vendor interoperability gains have been more substantial. This progress likely reflects vendors' incentives to enhance within-vendor interoperability, along with some limited success of government pressure to improve across-vendor interoperability.[57]

[56]After Epic rolled out its Care Everywhere network, several competitors (Cerner, McKesson, Allscripts, athenahealth, Greenway, and RelayHealth) formed the CommonWell Health Alliance to establish common interoperability standards and facilitate data exchange across vendors. This multi-vendor network was founded in 2013 but rolled out slowly across health centers. A breakdown by year shows that the share of Cerner hospitals reporting multi-vendor network participation increased from under 30% in 2016 to over 70% in 2019 (Appendix Figure B5). This growth reflects the increase in CommonWell participation among Cerner hospitals and likely contributes to Cerner's rising levels of both across- and within-vendor interoperability over time. Appendix Section B provides additional background on these vendor strategies and their implications for interoperability.

[57]With these estimates, we can re-estimate our analyses in Sections 3 and 4 using interoperability levels directly instead of the same-vendor proxy. Doing so yields very similar estimates of the effect of interoperability on shared patient outcomes and shared patient flows. See Appendix C.2 for more details.

Table 6: Model Parameter Estimates

	(1) **Baseline**	(2) **Pair FEs**
θ_1 (Interoperability)	0.283	0.788
	(0.071)	(0.112)
θ_2 (Always Same System)	0.384	
	(0.023)	
θ_3 (Distance)	-0.0043	
	(0.0002)	
ρ (Nest)	0.6	0.27
	(0.02)	(0.03)
Mean Interoperability Elasticity	0.141	0.23
Mean Distance Elasticity	-0.41	
Sender FE	Yes	
Receiver FE	Yes	
HRR × Year FE	Yes	Yes
Sender × Receiver FE		Yes

Notes: Model parameter estimates as defined in Equation 6.1. Standard errors are in parentheses.

6.3 Estimation Part Two: Hospital Demand

Estimation Strategy Using these estimates of within- and across-vendor interoperability, we now estimate our hospital choice model using transfer data from 2013-2019, the period for which we measure interoperability. We focus solely on transfers, as their markets are more clearly defined by smaller geographic regions (HRRs).[58] Thus, the outside option is defined as transferring the patient to a hospital outside the sending hospital's HRR. We use three instruments in estimation. The first two target interoperability, and are the system-pressure instrument described in Section 4 (following Dranove et al., 2014) as well as an indicator for whether the hospital-pair is in a hospital system, since the instrument is only defined for hospitals in a system. The third instrument addresses the endogenous within-nest share of each receiving hospital and is given by the number of receiving hospitals in the nest, as is common in the literature.[59] The interoperability coefficient is identified from hospitals switching EHR vendors and from changes in interoperability levels over time.

[58] We can estimate the model on referrals, but defining the market is more challenging. Appendix Table D25 shows the results when defining the market for referrals as receiving hospitals with which the sender has a baseline referral relationship. Our estimated coefficient on interoperability is smaller than in the transfers sample.

[59] We implement this estimation using the PyBLP software (Conlon and Gortmaker, 2020).

Estimation Results Table 6 presents the model parameter estimates for our baseline specification in Column 1. We also estimate an alternative specification in Column 2 where we include hospital sender-receiver pair fixed effects $\xi_{hh'}$ instead of the separate sender and receiver fixed effects, which better control for match quality between the sending and receiving hospitals. Consistent with the prior hospital demand literature, we find that sending hospitals and patients have a strong distaste for travel (e.g., Ho, 2006; Gowrisankaran et al., 2015; Raval and Rosenbaum, 2021) and a strong preference for staying within the same multi-hospital system (e.g., Cutler et al., 2020; Singh, 2022; Cuesta et al., 2024). We also find that they strongly prefer interoperability. Further, hospital receiver fixed effects are positively correlated with measures of hospital quality, suggesting our model captures meaningful variation in hospital characteristics (Appendix Table D24).

Converting the estimated coefficients to substitution elasticities, we find a mean hospital demand elasticity of between 0.14 and 0.23 with respect to interoperability and -0.41 with respect to distance.[60,61,62] The magnitude of the interoperability elasticity is quite substantial at approximately one-third to one-half of that of distance. To put this in perspective, Gowrisankaran et al. (2015) estimates that increasing travel time by one minute (or 7%) reduces consumer surplus by approximately $167. Additionally, Lu and Lu (2018) finds that increasing travel distance by one mile has a similar effect on hospital demand as increasing the hospital's readmission rates by one percentage point. Our results imply that the effect of interoperability on hospital choice could be quite comparable to those of quality and cost.

[60] With hospital pair fixed effects, we cannot estimate a coefficient on distance (nor always same system) since the distance between hospital pairs does not vary over time. However, we can regress the estimated hospital pair fixed effects on distance, always same system, sender fixed effects, and receiver fixed effects to get a sense of how much each component matters in this utility function. This yields a coefficient of -0.00768 on distance, suggesting a similar tradeoff between interoperability and distance in the pair-FEs to that in the baseline specification.

[61] Our estimated distance elasticity aligns with previous findings in the hospital demand literature, albeit at the lower end of the range. For example, Gowrisankaran et al. (2015) reports distance elasticities from -1.56 to -0.33 but also finds that willingness to travel increases with illness severity. Our sample of Medicare transfer patients are often high-risk and complex cases; thus, it is not surprising that our sample of patients express less distaste for travel than a more general sample of all patients.

[62] We also find similar results when estimating the model with receiver hospital-by-year fixed effects, which account for changes in receiver hospital characteristics (e.g., quality) over time. This model gives similar average elasticities of substitution with respect to distance (-0.46) and interoperability (0.14). This robustness check addresses the possibility that patients prefer certain receiver hospitals that are using certain EHR vendors (e.g., those using Epic) due to the effect of the vendor on hospital quality rather than sender-receiver pair-specific interoperability.

7 Equilibrium Effects of Reducing Interoperability Frictions

Using our estimates of the interoperability levels and model parameters from Section 6, we quantify (1) the potential welfare gains from improved interoperability and (2) the welfare progress achieved between 2013 and 2019. We find that eliminating all interoperability barriers would generate substantial welfare gains, equivalent to reducing travel distances by up to 57 kilometers. However, the welfare progress between 2013 and 2019—driven by both increased market concentration in the EHR market and improvements in interoperability levels—amounts to only 34% of this first-best benchmark. Moreover, this welfare progress, and reallocation of patients, is not equally enjoyed by all hospitals. The majority of benefits have accrued to hospitals that switched to Epic, especially those in local markets that also converged toward Epic.

7.1 Reducing Interoperability Frictions

In our first set of counterfactuals, we calculate potential welfare gains from improved interoperability. Our first-best benchmark is perfect interoperability, i.e., $\iota^{CF} = \mathbf{1}$. In recognition that perfect interoperability may be unrealistic due to current technological constraints, we consider a second benchmark in which all hospitals adopt the EHR system with the highest within-vendor interoperability, that is, an Epic monopoly across all years. As in our earlier analysis, the welfare gains from these benchmarks arise from two channels: a direct channel and an allocative channel. The direct channel captures the impact of improved interoperability on patient and hospital utility, such as reduced hassle costs and health benefits from enhanced information exchange; the allocative channel reflects patient reallocation toward better hospitals when interoperability no longer varies across potential receiving hospitals. We express this decomposition as:

$$\begin{aligned}\Delta\text{Total Welfare} &= \text{W}(\text{Allocation}^{CF}, \boldsymbol{\iota}^{CF}) - \text{W}(\text{Allocation}, \boldsymbol{\iota}) \\ &= \underbrace{\text{W}(\text{Allocation}^{CF}, \boldsymbol{\iota}^{CF}) - \text{W}(\text{Allocation}, \boldsymbol{\iota}^{CF})}_{\Delta \text{ Allocative Welfare}} + \underbrace{\text{W}(\text{Allocation}, \boldsymbol{\iota}^{CF}) - \text{W}(\text{Allocation}, \boldsymbol{\iota})}_{\Delta \text{ Direct Welfare}}.\end{aligned} \tag{7.1}$$

In this equation, Allocation^{CF} represents the allocation of patients across hospitals in the counterfactual (CF), while $\boldsymbol{\iota}^{CF}$ denotes the interoperability levels experienced by patients in the counterfactual (i.e., the counterfactual interoperability levels of the hospitals patients choose in the counterfactual). Variables without superscripts refer to the corresponding inputs from the observed data.

Table 7: Summary of Counterfactuals

Counterfactual	%Δ Total Welfare	Δ Total Welfare (-km)	=	Δ Direct Welfare (-km)	+	Δ Allocative Welfare (-km)	Share Switch
First-Best Full Interop. (All = 1)	21.2%	56.5		53.8		2.7	0.075
Epic Monopoly (All Epic)	12.9%	34.4		33.1		1.4	0.053

Notes: Table displays the results of two counterfactual scenarios: (1) increase all interoperability levels to the maximum of 1; (2) increase all interoperability levels to Epic's within-vendor level in each time period, thereby in essence have all hospitals switch to Epic. "%Δ Total Welfare" is the average percent change in welfare for all patients in the counterfactual compared to the data. "Δ Total Welfare" is the average level change in welfare for all patients in the counterfactual compared to the data, divided by the recovered coefficient on distance (-0.00768) from the pair fixed effects version of the hospital demand model, multiplied by negative one; this welfare level is thus in negative distance units. "Δ Total Welfare" = "Δ Allocative Welfare" + "Δ Direct Welfare", as described in Equation 7.1. "Share Switch" is the share of patients that choose different hospitals in the counterfactual than in the data.

Table 7 presents the welfare effects of two benchmark scenarios. The first row shows the welfare effects of an infeasible first-best that assumes *perfect interoperability* across all vendors. The second row shows the welfare effects of a technologically-feasible first-best, in which all hospitals adopt the EHR system with the highest within-vendor interoperability; this is effectively an *Epic monopoly* scenario.[63]

We find that increasing all interoperability levels to the maximum of one increases joint hospital-patient welfare for the average transfer patient by 21%, equivalent to a 57-kilometer reduction in travel distance. 95% of this increase stems from the direct positive effect of higher interoperability on utility, while the remaining 5% results from improved allocative efficiency. Full interoperability eliminates the wedge between within- and across-vendor interoperability, thus removing the distortive incentive to send patients to same-vendor hospitals and resulting in 7.5% of patients being reallocated to different hospitals. On average, these patients are sent to hospitals that are slightly farther away but also lower cost and much higher quality.[64]

In addition, we find that an *Epic monopoly* throughout the 2013-2019 period would have achieved 61% of this infeasible first-best benchmark. While an Epic monopoly would substantially improve welfare in our framework—which focuses on the effects of interoperability on

[63]These scenarios not only affect patients initially sent to hospitals within the market but also those initially sent to the outside option by weakly improving the market choice set through increased interoperability. Appendix Tables D26 and D27 separately report welfare changes for patients who choose in-market hospitals under the data conditions ("intensive patients") and for those who choose the outside option ("extensive patients").

[64]Specifically, we find that patients who switch hospitals under *perfect interoperability* would experience an increase of 0.536 km in distance traveled but go to hospitals with $961.4 fewer average charges, 0.028 fewer 30-day readmission rates, and 0.016 fewer 30-day mortality rates (Appendix Table D28).

patient transfers—it may also have unintended consequences in the EHR market that we do not model. The classic tradeoff in markets with interoperability concerns is that while monopoly can substantially improve interoperability, reduced competition might lead to higher prices and reduced incentives for long-term technological progress. There is suggestive evidence that these concerns are warranted in the EHR market. First, Epic systems are more likely to be adopted by hospitals that are part of larger hospital systems and by academic hospitals (Appendix Table B1)—a pattern that is potentially due to the high costs of Epic systems.[65] Second, there has been substantial progress in within-interoperability of Epic systems as well as that of other vendors over time (Figure 8). One could argue that such progress is driven in part by these vendors' incentives to compete for market shares. While evaluating the tradeoff between interoperability and competition of EHR systems is beyond the scope of this paper, our next exercise will shed light on how the dynamics within the EHR market have shaped welfare progress between 2013 and 2019.

7.2 Welfare Progress Between 2013 and 2019

We now evaluate the welfare progress achieved between 2013 and 2019. As shown in Figure 1, this period witnessed the rise of Epic as the dominant EHR vendor. We therefore expect some of the welfare progress to come from an increasing number of hospitals capitalizing on Epic's superior within-vendor interoperability. In addition, Figure 8 shows interoperability improvements across the board, though to various degrees. Such improvements would generate welfare progress even if all hospitals had kept their EHR systems as in 2013. Let $W(e, \iota)$ be total welfare given the vector of EHR choices e and interoperability levels ι. We isolate the share of total welfare progress attributable to either the rise in EHR market concentration or the improvements in interoperability levels via the following decomposition:

$$\begin{aligned} \text{Welfare Progress}_{2013\text{–}2019} &= W(e_{2019}, \iota_{2019}) - W(e_{2013}, \iota_{2013}) \\ &= \underbrace{W(e_{2019}, \iota_{2019}) - W(e_{2013}, \iota_{2019})}_{\text{market concentration}} + \underbrace{W(e_{2013}, \iota_{2019}) - W(e_{2013}, \iota_{2013})}_{\text{interoperability progress}}. \end{aligned} \tag{7.2}$$

To isolate out other changes in the hospital market and data availability, we keep hospital characteristics as in the baseline year of 2013, changing only the EHR choices and interoperability levels in the counterfactual scenarios to those of the year 2019. We then compare this progress against the welfare gains from the infeasible first-best scenario for the year 2013.

[65] While EHR vendors do not publicly disclose pricing information, industry reports on hospital contracts suggest that Epic is among the most expensive vendors (Brown, 2023).

Using Equation 7.2, we find that welfare progress between 2013 and 2019 achieved 34% of the *perfect interoperability* benchmark. Of this progress, 30% is attributable to increasing concentration in the EHR market, while 70% is due to improvements in interoperability levels.

A closer look at the EHR market structure between 2013 and 2019 reveals two notable trends. First is the sharp rise in Epic's market share, from one-quarter in 2013 to one-half in 2019 (Figure 1a). This is an important driver for welfare progress, given Epic's superior within-vendor interoperability. Second, hospitals within the same local market tend to converge to the same EHR vendor, though possibly to one other than Epic (Appendix Figure B3). This pattern highlights a key challenge to achieving interoperability progress through EHR choices: the need for coordination around the same superior EHR system. Moreover, EHR systems have high switching costs, which are likely to create winners and losers in the transition.

Recognizing these, we next examine how welfare progress varies across hospitals and local markets. Our model allows us to consider two components of hospital h's welfare gains from interoperability: (1) the utility gains from transferring patients to other hospitals (as a sending hospital) and (2) the implied revenue gains from receiving more transferred patients (as a receiving hospital). We run regressions of the following form:

$$\begin{aligned} Y_h =& \beta_0 + \beta_1 \text{Epic-switcher}_h + \beta_2 \text{Epic-share}_{2013,m-h} + \beta_3 \Delta\text{Epic-share}_{m-h} \\ &+ \beta_{1,2} \text{Epic-switcher}_h \times \text{Epic-share}_{2013,m-h} + \beta_{1,3} \text{Epic-switcher}_h \times \Delta\text{Epic-share}_{m-h} + \epsilon_h, \end{aligned} \tag{7.3}$$

where Y_h is either the welfare progress of hospital h (as in Equation 7.2) or the percent change in transfers that h received between 2013 and 2019. Epic-switcher_h indicates whether hospital h switched to Epic between 2013 and 2019; $\text{Epic-share}_{2013,m-h}$ is the bed-weighted share of other hospitals in h's local market m with Epic systems in 2013; $\Delta\text{Epic-share}_{m-h}$ is the change in Epic's local market share between 2013 and 2019. Evidence that $\beta_{1,2} > 0$ and $\beta_{1,3} > 0$ would support the presence of coordination effects: a hospital benefits more from switching to Epic when other hospitals in its local market have already adopted Epic or are also switching to Epic. Table 8 presents our estimates of Equation 7.3, showing strong evidence of coordination effects. We estimate that in a market where Epic's share increased from one-quarter to one-half, a hospital that switched to Epic would experience additional welfare gains equivalent to an 18-km reduction in travel distance and receive 14% more transferred patients compared to a hospital in the same market that did not switch to Epic.

In contrast to interoperability progress driven by EHR market concentration, direct improvements in interoperability levels have the potential to benefit all hospitals. In fact, we find that improvements in interoperability levels played a larger role than rising market concentra-

Table 8: Heterogeneity in Welfare Progress and Revenue Gains (2013-2019)

	(1) Welfare Progress (-km)	(2) %ΔTransfers Received
Epic Switcher	2.266**	-0.0483***
	(1.102)	(0.00836)
Epic Share (2013)	-2.503***	-0.0585***
	(0.455)	(0.00520)
ΔEpic Share	-1.722***	-0.0944***
	(0.647)	(0.0113)
Epic Switcher x Epic Share (2013)	32.71***	0.391***
	(2.298)	(0.0224)
Epic Switcher x ΔEpic Share	33.79***	0.368***
	(2.384)	(0.0258)
Observations	3574	3547
Mean of Outcome	19.617	0.052

Notes: Each column is a separate hospital-level regression of how switching to an Epic EHR system and how baseline levels as well as changes in Epic market share affect welfare and revenue gains, as specified in Equation 7.3. The outcome in Column 1 is the change in joint sending hospital-transfer patient welfare from 2013 to 2019, expressed as the equivalent reduction in transfer distance (kilometers) per patient. The outcome in Column 2 is the percent change in transfers received from 2013 to 2019. Column 1 additionally controls for the number of receiver hospitals in each sending hospital's market. Robust standard errors are displayed in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

tion in driving the welfare progress between 2013 and 2019. Figure 8 shows that most of the progress during this period occurred in within-vendor interoperability, with all major vendors exhibiting an increase of approximately 0.2 points. In contrast, progress in across-vendor interoperability was slower, reflecting a lack of aligned private incentives and coordination across vendors. This is an area where government intervention may be necessary. Most recently, the Trusted Exchange Framework and Common Agreement (TEFCA) initiative aims to address this gap by providing a unified framework to facilitate interoperability across different EHR systems.[66] In our counterfactual framework, this policy can be proxied by enforcing a floor level for both within- and across-vendor interoperability.

[66] J. Lin (2023a) argues that the HITECH Act—which subsidized hospitals to adopt EHR systems—missed substantial welfare gains due to its limited emphasis on interoperability and, consequently, its failure to promote local coordination on a common platform. The result was a fragmented EHR market following the HITECH era. The TEFCA initiative represents a recent effort as part of a broader, long-term policy shift aimed at directly improving interoperability across systems.

8 Conclusion

In an increasingly interconnected world, the ability for different systems to work together can significantly shape market dynamics and consumer welfare. This paper demonstrates that technological frictions—specifically, the imperfect interoperability of EHR systems—have both direct and allocative effects on healthcare outcomes. Using an event study design, we show that when hospitals switch to using the same EHR vendor, charges, diagnostic tests, and hospital readmission rates for their shared transferred and referred patients decrease. Simultaneously, these hospitals now share more transfers and referrals. This change in patient flows further affects patient outcomes: when hospitals switch to EHR vendors used by closer, cheaper, or higher-quality local hospitals, their transfer and referral patients travel shorter distances, incur fewer charges, and experience better health outcomes, respectively.

To quantify welfare gains from reducing interoperability frictions, we develop and estimate a model of how patients and providers trade off interoperability with other hospital characteristics when choosing where to send patients for additional care. Our counterfactual analyses reveal that eliminating all interoperability barriers would reallocate 7.5% of transfer patients to different hospitals and increase joint hospital-patient welfare by 21%, equivalent to a 57-kilometer reduction in travel distance per patient. From 2013 to 2019, actual welfare progress achieved one-third of this first-best benchmark, with 70% of progress driven by interoperability improvements and 30% by market concentration increases. These gains were unevenly distributed, with hospitals switching to Epic—particularly in markets with widespread Epic adoption—capturing the largest welfare benefits and additional transfer patient revenue. These findings highlight the possibility for public policies enforcing minimum interoperability standards to capture substantial welfare gains for all hospitals while avoiding the potential downsides of additional market concentration.

Our findings suggest several directions for future research. On the supply side, important open questions concern EHR vendor incentives to promote or impede interoperability, and the strategic adoption decisions of hospitals weighing the benefits of interoperability against the competitive implications of aligning with particular vendors. Further work is also needed to assess the equilibrium consequences of mandating interoperability standards, including their effects on market structure and innovation. More broadly, our results illustrate how barriers to integration across technologies can reduce welfare, a theme with relevance well beyond healthcare.

References

Adjerid, I., Adler-Milstein, J., & Angst, C. (2018). Reducing medicare spending through electronic health information exchange: The role of incentives and exchange maturity. *Information Systems Research*, *29*(2), 341–361.

Adler-Milstein, J., Garg, A., Zhao, W., & Patel, V. (2021). A survey of health information exchange organizations in advance of a nationwide connectivity framework. *Health Affairs*, *40*(5), 736–744.

Adler-Milstein, J., Linden, A., Hsia, R. Y., & Everson, J. (2024). Electronic connectivity between hospital pairs: Impact on emergency department-related utilization. *Journal of the American Medical Informatics Association*, *31*(1), 15–23.

Adler-Milstein, J., & Pfeifer, E. (2017). Information blocking: Is it occurring and what policy strategies can address it? *The Milbank Quarterly*, *95*(1), 117–135.

Agha, L. (2014). The effects of health information technology on the costs and quality of medical care. *Journal of Health Economics*, *34*, 19–30.

Anderson, J. E., & Van Wincoop, E. (2003). Gravity with gravitas: A solution to the border puzzle. *American Economic Review*, *93*(1), 170–192.

Atasoy, H., Demirezen, E. M., & Chen, P.-Y. (2021). Impacts of patient characteristics and care fragmentation on the value of HIEs. *Production and Operations Management*, *30*(2), 563–583.

Augereau, A., Greenstein, S. M., & Rysman, M. (2004). Differentiation across standards and adoption failure in 56K modems. *SSRN Electronic Journal*, (NET Institute Working Paper No. 03-12). https://doi.org/10.2139/ssrn.584801

Ayabakan, S., Bardhan, I., Zheng, Z., & Kirksey, K. (2017). The impact of health information sharing on duplicate testing. *MIS Quarterly*, *41*(4), 1083–1104.

Baker, L. C., Bundorf, M. K., & Kessler, D. P. (2015). Expanding patients' property rights in their medical records. *American Journal of Health Economics*, *1*(1), 82–100.

Betsy Lehman Center. (2018). To avoid missed referrals in ambulatory settings, close the loop [Accessed: January 25, 2024]. *Betsy Lehman Center for Patient Safety*. https://betsylehmancenterma.gov/news/year-end-to-avoid-missed-referrals-in-ambulatory-settings-close-the-loop

Bignon, L. (2024). Pharmaceuticals and digital health: How data-driven insights may reshape the insulin market. (Working Paper). https://leabignon.github.io/papers/Bignon_JMP.pdf

Bronsoler, A. (2022). *Essays on Healthcare Delivery innovation: The role of Information and Communication Technology* [Doctoral dissertation, Massachusetts Institute of Technology]. https://dspace.mit.edu/handle/1721.1/144956

Bronsoler, A., Doyle, J., & Van Reenen, J. (2022). The impact of health information and communication technology on clinical quality, productivity, and workers. *Annual Review of Economics*, *14*(1), 23–46.

Brot-Goldberg, Z. C., & de Vaan, M. (2018). Intermediation and vertical integration in the market for surgeons. (Working Paper). https://zarekcb.github.io/Referrals_Draft_2018_11_13.pdf

Brown, F. (2023). Cerner vs Epic: Top EHR Vendors [Accessed: July 15, 2025]. *TechnologyAdvice*. https://technologyadvice.com/blog/healthcare/cerner-vs-epic/

Callaway, B., & Sant'Anna, P. H. (2021). Difference-in-Differences with multiple time periods. *Journal of Econometrics*, *225*(2), 200–230.

Chandra, A., Dalton, M., & Staiger, D. (2023). Are hospital quality indicators causal? *National Bureau of Economic Research*, (Working Paper No. 31789). https://doi.org/10.3386/w31789

Chen, M., Guo, S., & Tan, X. (2019). Does health information exchange improve patient outcomes? Empirical evidence from Florida hospitals. *Health Affairs*, *38*(2), 197–204.

Conlon, C., & Gortmaker, J. (2020). Best practices for differentiated products demand estimation with PyBLP. *The RAND Journal of Economics*, *51*(4), 1108–1161.

Coustasse, A., Andresen, P., Schussler, M., Sowards, K., & Kimble, C. (2018). Why physicians switch electronic health record vendors. *Perspectives in Health Information Management*, 1–13.

Cuesta, J. I., Noton, C. E., & Vatter, B. (2024). Vertical integration and plan design in healthcare markets. *National Bureau of Economic Research*, (Working Paper No. 32833). https://doi.org/10.3386/w32833

Cutler, D. M., Dafny, L., Grabowski, D. C., Lee, S., & Ody, C. (2020). Vertical integration of healthcare providers increases self-referrals and can reduce downstream competition: The case of hospital-owned skilled nursing facilities. *National Bureau of Economic Research*, (Working Paper No. 28305). https://doi.org/10.3386/w28305

Dayal McCluskey, P. (2015). Partners' $1.2b patient data system seen as key to future [Accessed: May 6, 2024]. *The Boston Globe*. https://www.bostonglobe.com/business/2015/05/31/partners-launches-billion-electronic-health-records-system/oo4nJJW2rQyfWUWQlvydkK/story.html

Diaz, N. (2023). What health systems are paying for EHRs [Accessed: January 25, 2024]. *Becker's Hospital Review*. https://www.beckershospitalreview.com/ehrs/what-health-systems-are-paying-for-ehrs.html

Doyle, J., Graves, J., & Gruber, J. (2019). Evaluating measures of hospital quality: Evidence from ambulance referral patterns. *Review of Economics and Statistics*, *101*(5), 841–852.

Dranove, D., Forman, C., Goldfarb, A., & Greenstein, S. (2014). The trillion dollar conundrum: Complementarities and health information technology. *American Economic Journal: Economic Policy*, *6*(4), 239–270.

Drees, J. (2020). 12 EHR implementations that cost over $100M [Accessed: January 25, 2024]. *Becker's Hospital Review*. https://www.beckershospitalreview.com/ehrs/12-ehr-implementations-that-cost-over-100m.html

European Commission. (2024). *Rolling plan for ICT Standardisation 2024* (Report). European Commission. Brussels. Retrieved September 5, 2024, from https://digital-strategy.ec.europa.eu/en/policies/rolling-plan-ict-standardisation

Everson, J., Hendrix, N., Phillips, R. L., Adler-Milstein, J., Bazemore, A., & Patel, V. (2024). Primary care physicians' satisfaction with interoperable health information technology. *JAMA Network Open*, *7*(3), e243793–e243793.

Farrell, J., & Saloner, G. (1985). Standardization, compatibility, and innovation. *The RAND Journal of Economics*, *16*(1), 70–83.

FTC. (2023, December 21). *Interoperability, Privacy, and Security*. Federal Trade Commission. Retrieved September 5, 2024, from https://www.ftc.gov/policy/advocacy-research/tech-at-ftc/2023/12/interoperability-privacy-security#ftn1

Ganju, K. K., Atasoy, H., & Pavlou, P. A. (2023). Where to, Doc? Electronic health record systems and the mobility of chronic care patients. *SSRN Electronic Journal*, (Working Paper). https://doi.org/10.2139/ssrn.3012966

Gowrisankaran, G., Nevo, A., & Town, R. (2015). Mergers when prices are negotiated: Evidence from the hospital industry. *American Economic Review*, *105*(1), 172–203.

Gross, D. (2020). Collusive investments in technological compatibility: Lessons from US railroads in the late 19th century. *Management Science*, *66*(12), 5683–5700.

Hernandez-Boussard, T., Davies, S., McDonald, K., & Wang, N. E. (2017). Interhospital facility transfers in the United States: A nationwide outcomes study. *Journal of Patient Safety*, *13*(4), 187–191.

HHS. (2024, June). *HHS Finalizes Rule Establishing Disincentives for Health Care Providers That Have Committed Information Blocking* (tech. rep.) (Accessed: May 12, 2025). U.S. Department of Health and Human Services. https://www.hhs.gov/about/news/2024/06/24/hhs-finalizes-rule-establishing-disincentives-health-care-providers-that-have-committed-information-blocking.html

Higgins, T. (2022). Why Apple's iMessage Is Winning: Teens Dread the Green Text Bubble [Accessed: May 12, 2022]. *The Wall Street Journal*. https://www.wsj.com/tech/why-apples-imessage-is-winning-teens-dread-the-green-text-bubble-11641618009

Ho, K. (2006). The welfare effects of restricted hospital choice in the US medical care market. *Journal of Applied Econometrics*, *21*(7), 1039–1079.

Ho, K., & Pakes, A. (2014). Physician payment reform and hospital referrals. *American Economic Review*, *104*(5), 200–205.

Holmgren, A. J., Patel, V., & Adler-Milstein, J. (2017). Progress in interoperability: Measuring US hospitals' engagement in sharing patient data. *Health Affairs*, *36*(10), 1820–1827.

Hsuan, C., Vanness, D. J., Zebrowski, A., Carr, B. G., Norton, E. C., Buckler, D. G., Wang, Y., Leslie, D. L., Dunham, E. F., & Rogowski, J. A. (2024). Racial and ethnic disparities in emergency department transfers to public hospitals. *Health Services Research*.

Ishii, J. (2005). Compatibility, competition, and investment in network industries: ATM networks in the banking industry. (Working Paper).

Jones, C. I., & Tonetti, C. (2020). Nonrivalry and the economics of data. *American Economic Review*, *110*(9), 2819–2858.

Katz, M., & Shapiro, C. (1994). Systems competition and network effects. *The Journal of Economic Perspectives*, *8*(2), 93–115.

Kindermann, D. R., Mutter, R. L., Cartwright-Smith, L., Rosenbaum, S., & Pines, J. M. (2014). Admit or transfer? The role of insurance in high-transfer-rate medical conditions in the emergency department. *Annals of Emergency Medicine*, *63*(5), 561–571.e8.

Kliff, S. (2017). Obama's surprising answer on which part of Obamacare has disappointed him the most [Accessed: January 25, 2024]. *Vox*. https://www.vox.com/2017/1/9/14211778/obama-electronic-medical-records

Knittel, C., & Stango, V. (2008). Incompatibility, product attributes and consumer welfare: Evidence from ATMs. *The B.E. Journal of Economic Analysis & Policy*, *8*(1).

Knittel, C., & Stango, V. (2009). How does incompatibility affect prices?: Evidence from ATM's. *The Journal of Industrial Economics*, *57*(3), 557–582.

Lee, R. (2013). Vertical integration and exclusivity in platform and two-sided markets. *American Economic Review*, *103*(7), 2960–3000.

Li, J. (2023). Compatibility and investment in the U.S. electric vehicle market. (Working Paper). https://www.mit.edu/~lijing/documents/papers/li_evcompatibility.pdf

Li, Y., Lu, L. X., Lu, S. F., & Chen, J. (2022). The value of health information technology interoperability: Evidence from interhospital transfer of heart attack patients. *Manufacturing & Service Operations Management*, *24*(2), 827–845.

Lin, J. (2023a). Strategic complements or substitutes? The case of adopting health information technology by US hospitals. *Review of Economics and Statistics*, *105*(5), 1237–1254.

Lin, J. (2023b). To follow the market or the parent system: Evidence from health IT adoption by hospital chains. *Economic Inquiry*, *61*(4), 891–910.

Lin, S., Jha, A., & Adler-Milstein, J. (2018). Electronic health records associated with lower hospital mortality after systems have time to mature. *Health Affairs*, *37*(7), 1128–1135.

Lu, L. X., & Lu, S. F. (2018). Distance, quality, or relationship? Interhospital transfer of heart attack patients. *Production and Operations Management*, *27*(12), 2251–2269.

Mehrotra, A., Forrest, C. B., & Lin, C. Y. (2011). Dropping the baton: Specialty referrals in the United States. *The Milbank Quarterly*, *89*(1), 39–68.

Miller, A., & Tucker, C. (2009). Privacy protection and technology diffusion: The case of electronic medical records. *Management Science*, *55*(7), 1077–1093.

Miller, A., & Tucker, C. (2011). Can health care information technology save babies? *Journal of Political Economy*, *119*(2), 289–324.

Mueller, S., Zheng, J., Orav, E. J., & Schnipper, J. L. (2019). Inter-hospital transfer and patient outcomes: A retrospective cohort study. *BMJ Quality & Safety*, *28*(11), e1–e1.

Ochieng, N., Biniek, J. F., Freed, M., Damico, A., & Neuman, T. (2023, August). Medicare Advantage in 2023: Enrollment Update and Key Trends [Accessed: January 25, 2024]. *KFF*. https://www.kff.org/medicare/issue-brief/medicare-advantage-in-2023-enrollment-update-and-key-trends/

ONC. (2019, June). *Gaps in Individuals' Information Exchange, Health IT Quick-Stat #56* (tech. rep.) (Accessed: May 1, 2025). Office of the National Coordinator for Health Information Technology. https://www.healthit.gov/data/quickstats/gaps-individuals-information-exchange

ONC. (2022, July). *Interoperability Among Office-Based Physicians in 2019* (tech. rep.) (Accessed: May 1, 2025). Office of the National Coordinator for Health Information Technology. https://www.healthit.gov/data/data-briefs/interoperability-among-office-based-physicians-2019

ONC. (2023, January). *Trends in Hospital Leaders' Perceptions of Information Blocking* (tech. rep.) (Accessed: May 1, 2025). Office of the National Coordinator for Health Information Technology. https://www.healthit.gov/data/quickstats/trends-hospital-leaders-perceptions-information-blocking

Park, S. (2004). Quantitative analysis of network externalities in competing technologies: The VCR case. *Review of Economics and Statistics*, *86*(4), 937–945.

Patel, V., Henry, J. W., Pylypchuk, Y., & Mpa, T. S. (2016, May). *Interoperability among U.S. Non-federal Acute Care Hospitals in 2015* (tech. rep.) (Accessed: May 1, 2025). Office of the National Coordinator for Health Information Technology. https://www.healthit.gov/sites/default/files/briefs/onc_data_brief_36_interoperability.pdf

Raval, D., & Rosenbaum, T. (2021). Why is distance important for hospital choice? Separating home bias from transport costs. *The Journal of Industrial Economics*, *69*(2), 338–368.

Schulte, F., & Fry, E. (2019). Death By 1,000 Clicks: Where Electronic Health Records Went Wrong [Accessed: January 25, 2024]. *KFF Health News*. https://kffhealthnews.org/news/death-by-a-thousand-clicks/

Shannon, E. M., Zheng, J., Orav, E. J., Schnipper, J. L., & Mueller, S. K. (2021). Racial/ethnic disparities in interhospital transfer for conditions with a mortality benefit to transfer among patients with Medicare. *JAMA Network Open*, *4*(3), e213474–e213474.

Silva, J. S., & Tenreyro, S. (2006). The log of gravity. *Review of Economics and Statistics*, *88*(4), 641–658.

Singh, Y. (2022). Can private equity buy referrals? Evidence from multispecialty physician practice acquisitions. (Working Paper). https://www.yashaswinisingh.com/files/PE_Referrals_Singh_JMP.pdf

Turbow, S., Hollberg, J. R., & Ali, M. K. (2021). Electronic health record interoperability: How did we get here and how do we move forward? *JAMA Health Forum*, *2*(3), e210253–e210253.

Wakefield, B. J., Turvey, C., Hogan, T., Shimada, S., Nazi, K., Cao, L., Stroupe, K., Martinez, R., & Smith, B. (2020). Impact of patient portal use on duplicate laboratory tests in diabetes management. *Telemedicine and e-Health*, *26*(10), 1211–1220.

Walden, E. (2016). Can hospitals buy referrals? The impact of physician group acquisitions on market-wide referral patterns. (Working Paper).

Wooldridge, J. M. (2015). Control function methods in applied econometrics. *Journal of Human Resources*, *50*(2), 420–445.

Xue, B., & Meyerhoefer, C. (2025). *The effects of switching electronic health record developer on specialty referrals* (tech. rep.). National Bureau of Economic Research.

Yurkiewicz, I. (2023, July). *Fragmented: A Doctor's Quest to Piece Together American Health Care*. W. W. Norton & Company.

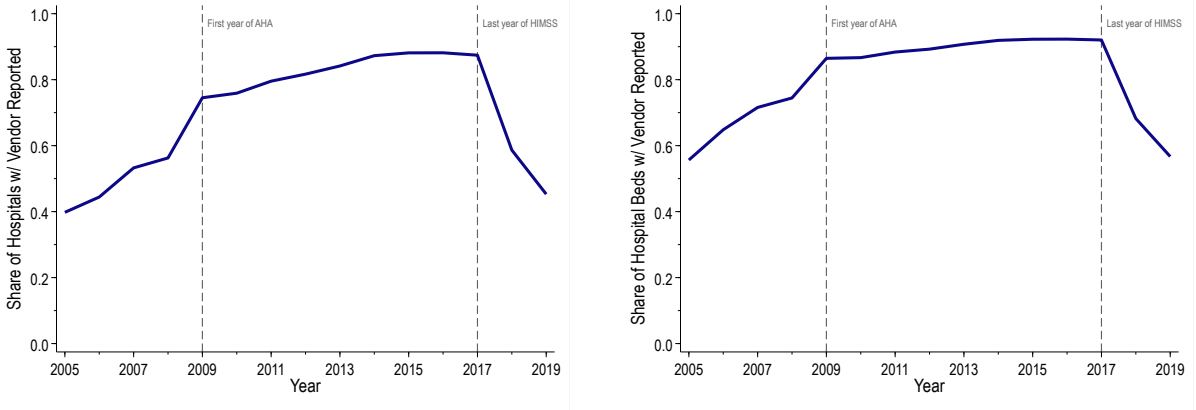
A Data Construction

A.1 Defining Hospital EHR Vendors

We combine data from the AHA IT Survey (2009-2019) and the HIMSS (2005-2017) to identify primary EHR vendors used by US hospitals from 2005 to 2019.[67] Concordance between the surveys is high—87% of hospitals that respond to both surveys in any given year report the same EHR vendor. However, this slightly imperfect alignment suggests possible measurement error in our records of hospital EHR vendor choices. We discuss this error further in Section 4. When the surveys contain different responses, we use the AHA IT response.

Appendix Figure A1 shows the shares of hospitals (Panel A) and hospital beds (Panel B) for which we have annual EHR vendor data. The dotted lines mark the first year AHA IT records EHR vendors (2009) and the last year HIMSS data is available (2017). The share of hospitals and beds with reported vendors rises quickly from under 60% in 2005 to 90% in 2017 but then drops steeply after the HIMSS data end in 2017. Given this drop, we limit our main empirical analysis to 2005-2017, though results are robust to extending through 2019.

Figure A1: Share of Hospitals with EHR Vendor Reported



(a) Hospitals (b) Beds

Notes: This figure plots the share of hospitals (Panel A) and hospital beds (Panel B) that have any EHR vendor reported across time, using data from AHA IT and HIMSS. Dotted vertical lines denote the first year in which the AHA IT data is available (2009) and the last year in which the HIMSS data is available (2017).

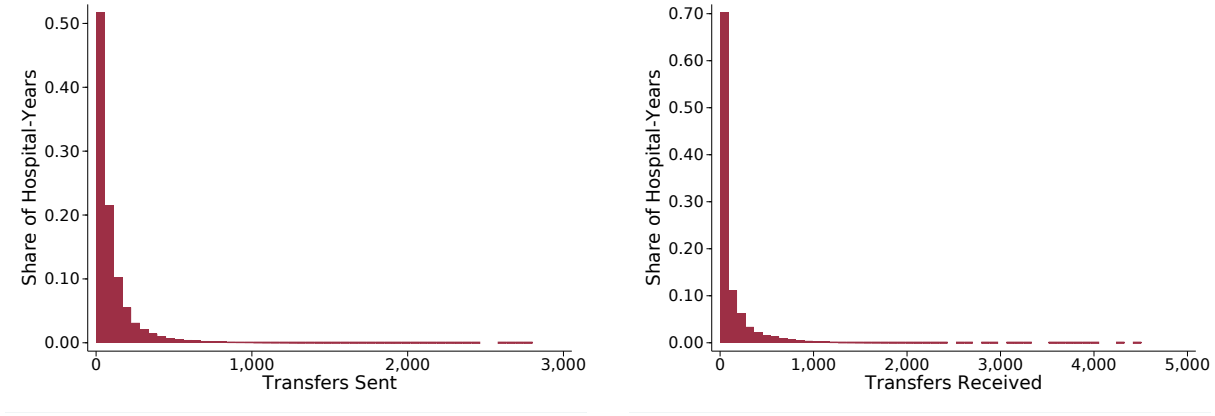
[67] 2009 is the first year in which EHR vendor information is reported in the AHA IT Survey, and 2019 is the last year of the survey available to us. Approximately 50-60% of hospitals in the US respond to the AHA IT Survey each year.

A.2 Defining Cross-Hospital Transfers

We define a *cross-hospital inpatient transfer* as when a patient is discharged from one hospital and admitted to another on the same or following day using the 100% sample MedPAR files from CMS.[68] Under this definition, 5.1% of Medicare hospital inpatient stays—about 600,000 stays per year—result in transfers to inpatient treatment at another hospital.[69]

Table 1 contains summary statistics for transfers in our analysis sample. Transfers are often costly and involve high-risk patients. Appendix Figure A2 shows the distributions of transfers sent and received by each hospital annually. These distributions have a long right tail due to some hospitals sending (receiving) no transfers in a given year while others send (receive) many.

Figure A2: Distribution of Number of Transfers Sent & Received



(a) Sent (b) Received

Notes: Figure plots the distribution of transfers sent (Panel A) and received (Panel B) by each hospital in each year (2005-2017).

[68] Since we do not have exact discharge or admission times, we operationalize the transfer definition as a hospital admission occurring on the same or following day as a hospital discharge. This definition is common in the health policy literature (Hsuan et al., 2024). While alternative definitions exist, such as combining timing with claims disposition codes (Mueller et al., 2019; Shannon et al., 2021), we define transfers based solely on timing to reduce sampling bias and to include "indirect transfers" where patients are advised at discharge to visit another hospital rather than more formally transferred.

[69] We have data on claims submitted only for Traditional Medicare patients. As of 2023, more than half of eligible Medicare beneficiaries opted for Medicare Advantage plans over Traditional Medicare, with this share rising consistently over time (e.g., 19% in 2007; Ochieng et al., 2023). Unlike Medicare Advantage beneficiaries, those enrolled in Traditional Medicare face no network restrictions, which benefits our analysis as we do not need to consider insurance coverage effects on patient outcomes and flows.

A.3 Defining Cross-Hospital Referrals

A.3.1 Overview

We define a *hospital referral* as a CMS Carrier claim where the service took place at a hospital and a referring physician is listed.[70] We then define a *cross-hospital referral* as a hospital referral in which the hospital where the service took place is different from the hospital where the referring physician is based, implicitly assuming that the place of service that led to the referral (i.e., the "sender") is also a hospital. Cross-hospital referrals thus occur when a patient is referred from one provider to another provider who works at a different hospital. Patients referred between providers at the same hospital do not meet this definition. Medicare Carrier claims list the NPIs of both performing and referring physicians but do not always list the Medicare Certification Number (MCRNUM) of the place of service and never list the MCRNUM of the place of referral.[71] We thus take several steps to identify both sending and receiving hospitals' MCRNUMs.

A.3.2 Defining place of referral (i.e., "sender")

Since place of referral is never defined in the Carrier claims and tracking down the exact prior claim that led to each referral would be quite difficult, we assume the place of referral is a hospital and define it as the referring physician's primary hospital affiliation. To calculate these affiliations, we perform the following steps for each year of data. First, we limit the 100% samples of CMS inpatient and outpatient claims to those that take place at hospitals.[72] For all claims, we retain the Medicare number of the place of service (MCRNUM) and the provider numbers (NPIs) of involved physicians. Second, for each unique site-provider (MCRNUM-NPI) pair, we count the number of claims where that provider is listed as either the attending, operating, rendering, or other physician. We count inpatient and outpatient claims separately, then sum them for each site-provider pair. Third, for each unique provider (NPI), we rank sites (MCRNUMs) by the total claims the provider participated in at that facility. Finally, we define each provider's primary affiliation as the site (MCRNUM) with the most claims. Approximately

[70] Some inpatient transfers will also be classified as referrals if transfer patients receive inpatient services that explicitly list the referring provider in Carrier files. However, this is not universal since our transfer measure is based on patient movement across facilities while our referral measure relies on codes for tractability.

[71] Beginning in 2007, National Provider Identifiers (NPIs) officially replaced Unique Physician Identification Numbers (UPINs) as standard provider identifiers in Medicare claims data. 2007 was a transition year with some providers reporting NPIs, some UPINs, and some both in the Carrier claims. 2008 is the first year almost all Carrier claims contain non-missing NPIs. To maintain consistency, we start our referrals sample in 2008.

[72] All inpatient claims and 77% of outpatient claims take place at hospitals.

40% of providers have only a single hospital affiliation (i.e., all of their hospital claims take place at a single hospital). For the average provider, 86% of claims occur at their primary hospital affiliation.[73]

A.3.3 Defining place of service (i.e., "receiver")

To define the hospitals from which patients are referred, we perform the following steps for each year of data:

(i) For the 20% sample Carrier claims, we first attempt to define the place of service using information from matching inpatient and outpatient claims with the Carrier claims:[74]

   (a) Limit the 20% sample inpatient and outpatient claims to those that take place at hospitals. Reshape the data to the patient-claim-day level (necessary for claims that take place across multiple days) and append the inpatient and outpatient data. Only keep days where the patient receives care at a single facility. The result is a dataset that shows, on date d, patient p received inpatient or outpatient care at hospital h (and only hospital h).

   (b) Limit the 20% sample Carrier claims to those that take place at hospitals. Only keep claims that span a single day.

   (c) Merge the inpatient/outpatient patient-day data onto the Carrier files.

(ii) For any remainder, we use the provider-facility crosswalk described in the previous subsection to define place of service.

A.3.4 Summary Statistics

Of all Carrier claims, 22% take place in hospitals, with 87% of those listing a referring physician, split nearly evenly between inpatient and outpatient hospital settings. Among these hospital-to-hospital referrals, 29% have the same referring and performing physician, 35.5% occur at the same hospital from which they were referred, and 35.5% occur at a different hospital. This last category meets our definition of cross-hospital referrals.

[73] For providers with multiple hospital affiliations, this average is 76%.

[74] Inpatient and outpatient claims contain place of service information. Carrier claims are provider bills, while outpatient and inpatient claims are facility bills. For example, if a patient has surgery at an outpatient center, the physician's bill appears in Carrier claims while the facility equipment bill appears in outpatient claims—same event, two files. Thus, each Carrier claim where service occurs at a hospital may be matchable to inpatient/outpatient files using patient ID and service date.

Table 1 contains summary statistics for cross-hospital referrals in our analysis sample. Referrals are common but heterogeneous in type and severity, contrasting with transfers which are less frequent but involve the most complex cases and highest-risk patients.

A.4 Defining Hospital Pair Analysis Sample

In Section 4, we analyze the allocative effect of interoperability on patient flows between hospital pairs. Each pair appears twice, once per flow direction (A to B and B to A). We restrict the sample to hospital pairs either within the same Health Referral Region (HRR) for transfers or sharing baseline patients for referrals, imputing zeros when no flows occur. These definitions differ because referral patients travel farther on average than transfer patients.[75,76] As in Section 3, we further restrict to pairs that either never share the same vendor or switch from using different vendors to using the same vendor only once.

Table A1 summarizes these hospital pairs, with transfers in Panel A and referrals in Panel B.[77] The first two columns weight observations by combined baseline bed size, representing an average hospital bed pair; the next two weight by actual transfers or referrals, representing the conditions faced by an average patient.[78] On average, each bed-weighted hospital sender-receiver pair exchanges 4 transfers and 45 referrals annually.[79] These distributions are highly skewed, with standard deviations far exceeding the means. 8% (12%) of hospitals in the transfer (referral) sample share the same EHR vendor in any given year, while 11% (14%) of transfers (referrals) occur between hospitals with the same EHR vendor—suggesting a potential effect of EHR vendors on patient flows.

[75] Transfers are limited to same-HRR pairs since 80% occur within HRRs, making this the appropriate market. For referrals, only 22% occur within HRRs; instead, 83% occur between hospitals sharing baseline referrals, making that a better market definition.

[76] An average HRR has 19.3 hospitals; 79% of same-HRR pairs share zero transfers annually. An average hospital shares baseline referrals with 74 hospitals, yet 51% of these pairs still share zero referrals annually.

[77] Appendix Table A2 reports statistics for the full sample of hospital pairs. Compared to the full sample, the analysis sample has fewer pairs sharing the same vendor and fewer pairs within the same hospital system (owned by the same company). Otherwise, the samples are largely similar.

[78] Weighting by combined baseline bed size better approximates conditions facing a random hospital pair and transfer patient. Weighting only by receiving hospital bed size approximates the experience of a random transfer patient conditional on destination, equating the importance of a large hospital sending many patients to another large hospital with that of a small hospital sending few patients to a large hospital.

[79] Appendix Table A3 shows unweighted averages of 2.5 transfers and 47 referrals per pair annually.

Table A1: Hospital-Pair Summary Statistics

(a) Transfers

	Weight by Beds		Weight by Transfers	
	Mean	SD	Mean	SD
# of Transfers	3.97	17.7		
Fraction of Sender Transfers	0.048	0.14		
Sender Total Transfers within HRR	143.5	205.2		
Same EHR Vendor	0.083		0.11	
Same System	0.035		0.092	
Same EHR Vendor | Different Systems	0.069		0.060	
Sender Beds (2005)	295.7	305.9	312.4	294.7
Receiver Beds (2005)	296.9	306.0	360.1	337.0
Distance between Hospitals (km)	43.4	49.7	33.3	44.0
AMC to AMC			0.28	
AMC to Non-AMC			0.25	
Non-AMC to AMC			0.29	
Non-AMC to Non-AMC			0.19	
N	1,018,541		219,485	

(b) Referrals

	Weight by Beds		Weight by Transfers	
	Mean	SD	Mean	SD
# of Referrals	45.1	244.4		
Fraction of Sender Referrals	0.016	0.068		
Sender Total Referrals within Base Pairs	5112.7	5312.9		
Same EHR Vendor	0.12		0.14	
Same System	0.018		0.089	
Same EHR Vendor | Different Systems	0.11		0.097	
Sender Beds (2005)	422.6	385.9	353.9	309.0
Receiver Beds (2005)	462.9	394.2	381.4	306.9
Distance between Hospitals (km)	223.0	235.7	44.4	78.1
AMC to AMC			0.40	
AMC to Non-AMC			0.18	
Non-AMC to AMC			0.23	
Non-AMC to Non-AMC			0.19	
N	1,561,230		744,157	

Notes: Summary statistics for hospital sender-receiver-year observations, limiting the sample to sender-receiver pairs either within an HRR (Panel A; transfers) or that share any referrals in the baseline year (Panel B; referrals) and to pairs that either never have the same EHR vendor ("never-treated" controls) or that start with different vendors and switch to the same vendor only once over the sample period (treated). Data for Panel A cover years 2005-2017 while data for Panel B cover years 2008-2017. In each panel, observations in the first two columns are weighted by combined bed size of the two hospitals in 2005. Observations in the last two columns are weighted by number of transfers (referrals) in each year. N is smaller in the last two columns than in the first two due to some hospital pairs having zero annual transfers (referrals). "Fraction of Sender Transfers (Referrals)" is the number of transfers (referrals) sent from h to h' divided by the total number of cross-hospital transfers (referrals) sent from h that year. "Same EHR Vendor | Different Systems" is the share of hospital sender-receiver pairs that have the same EHR vendor conditional on the hospitals belonging to different hospital systems (i.e., owned by different companies). "AMC to AMC" means the sending and receiving hospitals are both Academic Medical Centers (AMCs); "AMC to Non-AMC" means the sending hospital is an AMC while the receiving hospital is not; etc.

Table A2: Hospital-Pair Summary Statistics - Full Sample

(a) Transfers

	Weight by Beds		Weight by Transfers	
	Mean	SD	Mean	SD
# of Transfers	4.50	20.1		
Fraction of Sender Transfers	0.052	0.15		
Sender Total Transfers within HRR	145.7	201.3		
Same EHR Vendor	0.21		0.32	
Same System	0.079		0.23	
Same EHR Vendor | Different Systems	0.17		0.18	
Sender Beds (2005)	292.9	299.7	310.2	290.0
Receiver Beds (2005)	294.0	299.7	359.8	327.0
Distance between Hospitals (km)	41.9	48.8	31.3	42.0
AMC to AMC			0.30	
AMC to Non-AMC			0.23	
Non-AMC to AMC			0.28	
Non-AMC to Non-AMC			0.19	
N	1,377,473		306,180	

(b) Referrals

	Weight by Beds		Weight by Referrals	
	Mean	SD	Mean	SD
# of Referrals	54.1	293.4		
Fraction of Sender Referrals	0.019	0.074		
Sender Total Referrals within Base Pairs	5094.6	5270.8		
Same EHR Vendor	0.22		0.35	
Same System	0.039		0.23	
Same EHR Vendor | Different Systems	0.20		0.21	
Sender Beds (2005)	416.5	380.5	349.5	297.9
Receiver Beds (2005)	449.9	385.2	368.8	293.4
Distance between Hospitals (km)	216.8	233.6	39.8	71.5
AMC to AMC			0.41	
AMC to Non-AMC			0.18	
Non-AMC to AMC			0.22	
Non-AMC to Non-AMC			0.19	
N	2,067,664		1,009,276	

Notes: Summary statistics for hospital sender-receiver-year observations, limiting the sample to sender-receiver pairs either within an HRR (Panel A; transfers) or that share any referrals in the baseline year (Panel B; referrals). Data for Panel A cover years 2005-2017 while data for Panel B cover years 2008-2017. In each panel, observations in the first two columns are weighted by combined bed size of the two hospitals in 2005. Observations in the last two columns are weighted by number of transfers (referrals) in each year. N is smaller in the last two columns than in the first two due to some hospital pairs having zero annual transfers (referrals). "Fraction of Sender Transfers (Referrals)" is the number of transfers (referrals) sent from h to h' divided by the total number of cross-hospital transfers (referrals) sent from h that year. "Same EHR Vendor | Different Systems" is the share of hospital sender-receiver pairs that have the same EHR vendor conditional on the hospitals belonging to different hospital systems. "AMC to AMC" means the sending and receiving hospitals are both Academic Medical Centers (AMCs); "AMC to Non-AMC" means the sending hospitals is an AMC while the receiving hospital is not; etc.

Table A3: Hospital-Pair Summary Statistics - Unweighted

(a) Transfers

	Unweighted	
	Mean	SD
# of Transfers	2.50	15.2
Fraction of Sender Transfers	0.034	0.12
Sender Total Transfers within HRR	97.0	149.7
Same EHR Vendor	0.21	
Same System	0.073	
Same EHR Vendor | Different Systems	0.17	
Sender Beds (2005)	176.4	202.9
Receiver Beds (2005)	177.3	203.0
Distance between Hospitals (km)	46.1	52.0
N	1,377,473	

(b) Referrals

	Unweighted	
	Mean	SD
# of Referrals	47.0	252.4
Fraction of Sender Referrals	0.021	0.081
Sender Total Referrals within Base Pairs	3950.9	4447.9
Same EHR Vendor	0.22	
Same System	0.047	
Same EHR Vendor | Different Systems	0.19	
Sender Beds (2005)	295.1	281.8
Receiver Beds (2005)	320.5	290.3
Distance between Hospitals (km)	195.9	217.9
N	2,067,664	

Notes: Summary statistics for hospital sender-receiver-year observations, limiting the sample to sender-receiver pairs either within an HRR (Panel A; transfers) or that share any referrals in the baseline year (Panel B; referrals) and to pairs that either never have the same EHR vendor or that start with different vendors and switch to the same vendor only once over the sample period. Data for Panel A cover years 2005-2017 while data for Panel B cover years 2008-2017. In each panel, observations are unweighted. N is smaller in the last two columns than in the first two due to some hospital pairs having zero annual transfers (referrals). "Fraction of Sender Transfers (Referrals)" is the number of transfers (referrals) sent from h to h' divided by the total number of cross-hospital transfers (referrals) sent from h that year. "Same EHR Vendor | Different Systems" is the share of hospital sender-receiver pairs that have the same EHR vendor conditional on the hospitals belonging to different hospital systems. "AMC to AMC" means the sending and receiving hospitals are both Academic Medical Centers (AMCs); "AMC to Non-AMC" means the sending hospitals is an AMC while the receiving hospital is not; etc.

A.5 DocGraph Data

For our decomposition of interoperability levels across EHR vendors and time, we require comprehensive information on patient sharing relationships across healthcare providers. We obtain this information from DocGraph Hop Teaming data (2013-2019), which is derived from CMS Medicare Claims and contains information on both the number of patients shared between any two providers as well as the directionality of these flows. This includes not only direct transfers and referrals between providers, but also cases where providers interact with the same patients without formal handoffs. Additional information about the DocGraph data is available at https://careset.com/docgraph-hop-teaming-dataset/.

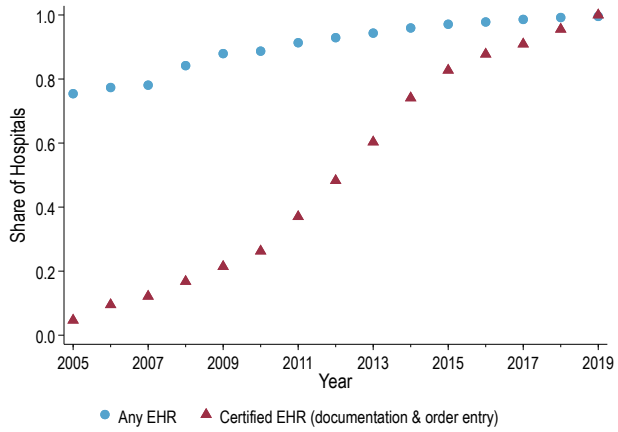
B Additional Information on EHR Choices and Interoperability

B.1 EHR Adoption versus Switches

Figure B1 shows that while most hospitals adopted some form of EHR system early in the 2005-2019 period, certified EHR adoption was more gradual, with only 60% of hospitals achieving certification by the second half of the sample period. These patterns suggest substantial variation in capabilities across EHR vendors, with changes in the EHR landscape driven more by hospitals switching between vendors than by hospitals adopting EHR systems for the first time.

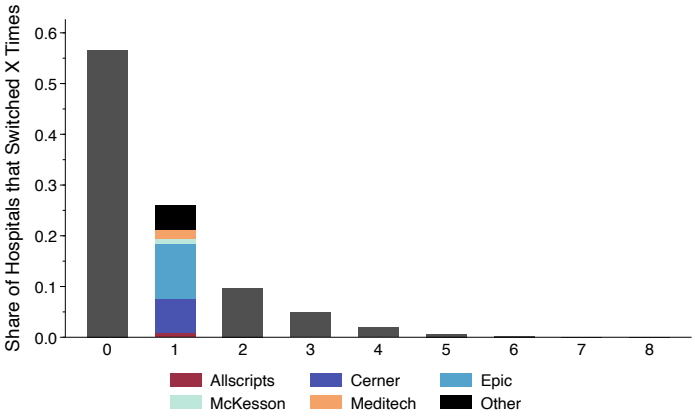
Figure B2 shows that 45% of hospitals switched EHR vendors at least once between 2005 and 2019. Most switches were toward Epic systems, explaining Epic's rise to national dominance (Figure 1a). However, evidence of coordination at the local level exists across different EHR vendors. Figure B3 plots the modal EHR vendor by market in 2019, showing that Epic's major competitors—Cerner, Meditech, and Allscripts—also enjoy dominance in many markets.

Figure B1: EHR Adoption



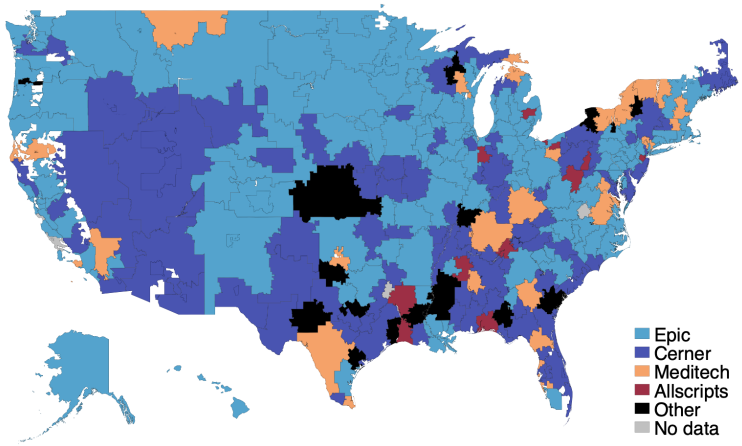
Notes: Figure plots the share of hospitals that report having any EHR vendor (blue dots) or report having a certified EHR vendor (red triangles) over time. Certified EHR is defined as an EHR system that is capable of electronic clinician documentation and computerized physician order entry.

Figure B2: EHR Switches per Hospital



Notes: Figure plots the share of hospitals that switch EHR vendors over our sample period (2005-2019) once, twice, etc. For hospitals that switch once, the figure uses colors to show which vendor the hospital switched to.

Figure B3: Modal EHR Vendor by Market (2019)



Notes: Figure plots the modal EHR vendor of each Health Referral Region (HRR) in 2019.

B.2 Why Hospitals Switch EHR Vendors

Hospitals switch EHR vendors for many reasons, including costs, system functionalities, vendor relations, and interoperability. This section provides suggestive evidence of how these factors might affect hospital EHR choices.

Pressure from Local Markets & Hospital Systems We find three pieces of evidence that hospitals prefer to switch to EHR vendors dominant in both their local markets and their hospital systems. The former—the tendency to adopt the vendor most widely used in the local

market—suggests a preference for interoperability. The latter—the tendency to adopt the vendor most widely used in the hospital system—suggests preferences for both interoperability and improved billing capabilities.[80]

First, among hospitals that switch vendors once during our sample period, 39% switch to the modal vendor in their Health Referral Region (HRR) in the year of their switch while only 9% switch away from the modal.

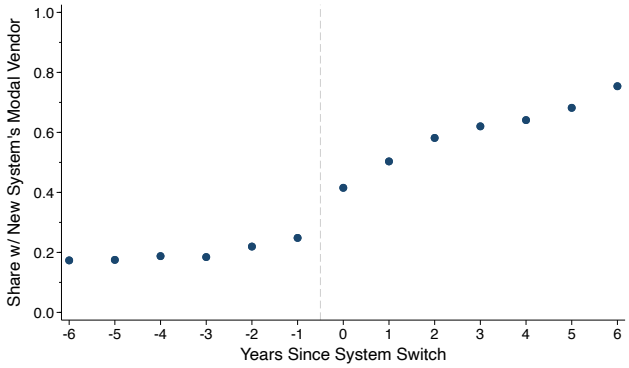
Second, among hospitals that switch vendors once during our sample period and belong to a multi-hospital system, 69% switch to the modal vendor in their hospital system in the year of their switch, while only 13% switch away from the modal. Further, 41% switch to the modal vendor in their HRR—this reflects overlap between modal in system and modal in HRR (35% switch to the modal in both).

Third, among hospitals that switch hospital systems once during our sample period (approximately 23% of hospitals), 25% use the modal vendor of their new health system the year before they switch systems, which jumps to 42% in the year they switch systems and then ultimately caps at 75% at 6 or more years after switching systems. Appendix Figure B4 shows this time pattern.

The pressure to use the EHR system used by more hospitals in the local market provides indirect evidence that interoperability may affect hospital and patient welfare. At the same time, it raises endogeneity concerns: hospitals may seek interoperability specifically with those they expect to interact with most. Beyond various robustness checks—most notably, a placebo test around EHR switches and a sample restriction excluding ownership changes—we address this concern using an instrumental variables approach that leverages hospital system pressure from other markets as an exogenous shifter of EHR choices.

[80] While these patterns suggest interoperability influences vendor choice, they are not definitive evidence because the modal vendor might excel in other dimensions like hospital productivity that we do not measure. Hospitals may benefit from complementarities of better health information exchange in choosing a widely used EHR system. However, they may also worry that choosing a popular system will lead to losing patients if patient record exchange becomes too interoperable. Overall, J. Lin (2023a) shows that hospital EHR vendor decisions are on net strategic complements, consistent with these patterns.

Figure B4: Among System Switchers, Whether Have Modal Vendor of New System



Notes: For hospitals that switch hospital systems once over our sample period, figure plots the share that have the modal vendor of their new hospital system in relative years since switching to that new system.

Characterizing Hospitals that Use Epic In Sections 6 and 7, we show that Epic offers superior interoperability and that hospitals switching to Epic can realize substantial welfare gains, particularly when other hospitals in their local market also use Epic. However, Epic systems are costly; for example, Mass General Brigham's transition to Epic reportedly cost $1.2 billion.[81] This suggests that welfare gains from EHR choices may be concentrated among hospitals with greater financial resources, especially large hospital systems.

Table B1 regresses an indicator for whether a hospital uses Epic on time-varying hospital characteristics including: system membership, academic medical center status, ownership type (non-profit or for-profit, with public as the omitted category), and number of beds. We find that system-affiliated hospitals, academic medical centers, non-profit hospitals, and larger hospitals are more likely to use Epic as their primary EHR vendor.

[81] For other examples of Epic EHR system costs, see https://www.beckershospitalreview.com/healthcare-information-technology/ehrs/most-expensive-epic-ehrs-ranked/.

Table B1: Characterizing Hospitals that use Epic

	(1) Has Epic EHR
Belongs to a Health System	0.124***
	(0.00746)
Academic Medical Center	0.0798***
	(0.00813)
Non-Profit Hospital	0.0842***
	(0.00834)
For-Profit Hospital	-0.111***
	(0.00816)
# Hospital Beds	0.0000554**
	(0.0000261)
Observations	61836
Mean of Outcome	0.15

Notes: Table shows estimates from regressing various time-varying hospital characteristics on whether the hospital-year observation has an Epic EHR system. Standard errors are clustered by hospital and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

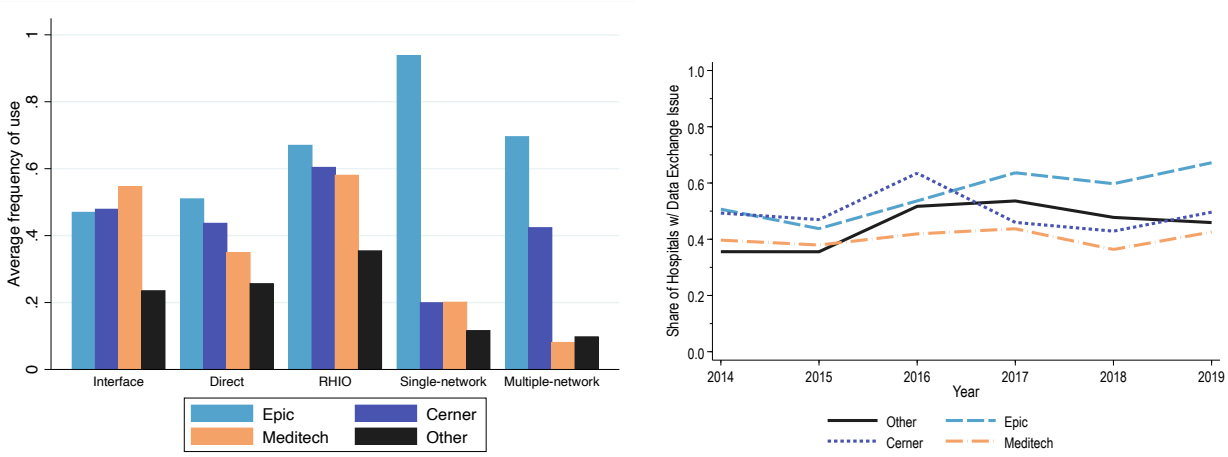
B.3 Descriptive Evidence Supporting the Interoperability Decomposition

Beyond our main measure of hospital-level interoperability—the ability to integrate patient information from other hospitals—the AHA IT surveys ask hospitals about the challenges they encounter when exchanging patient data and the technological methods they employ to do so. These measures provide additional insight into the technological landscape and further support our interoperability decomposition results in Section 6.

Panel A of Figure B5 plots the average frequency with which hospitals with different EHR vendors use electronic methods to send patient records, including (1) interface connection between EHR systems (e.g., HL7 interface), (2) direct access, (3) regional health information exchange, (4) single EHR vendor networks (e.g., Epic's Care Everywhere), and (5) multi-EHR vendor networks (e.g., Commonwealth Health Alliance). Epic hospitals report the highest use of all advanced methods. The frequent use of Epic's single network, Care Everywhere, explains why hospitals using Epic systems have such high within-vendor interoperability. Cerner hospitals have decent use of all methods and are the only ones besides Epic hospitals to use multiple-vendor networks. In contrast, Meditech, which is built on HL7 standards, relies more on interface connections between EHR systems of the same standard.

Panel B of Figure B5 shows the share of hospitals reporting difficulties exchanging data with medical facilities using different EHR vendors both over time and across vendors. This share is similar across major EHR vendors and, if anything, appears to increase over time. This pattern is consistent with our estimates of across-vendor interoperability remaining lower than within-vendor interoperability across all vendors and years.

Figure B5: Evidence Supporting Interoperability Decomposition Estimates



(a) Frequency of Using Different Advanced Methods to Send to Outside Hospital Systems (2016-2019)

(b) Difficult to Exchange Data Across Vendors

Notes: Panel A plots the average frequency at which hospitals use different types of advanced methods to send patient data to providers outside of their hospital system, separately by the hospital's EHR vendor, pooling data from 2016 to 2019. Hospitals can answer either "often," "sometimes," "rarely," or "never." We convert their answers to an ordinal variable, referred to as frequency of use, by coding "often" as 1, "sometimes" as 0.5, "rarely" as 0.25, and "never" as 0. Panel B plots the share of hospitals that report having difficulty exchanging data across EHR vendors over time, separately by the hospital's EHR vendor.

C Model Estimation Details

C.1 Measuring Hospital-Level Interoperability

We measure hospital h's reported interoperability in year t, ι_{ht}, as its answer to a survey question consistently measured across the 2013-2019 AHA IT Surveys: "Does your EHR integrate the information contained in the summary of care records received electronically (not eFax) without the need for manual entry?" Hospitals can respond with one of three answers: "yes, routinely" (coded as 1), "yes, but not routinely" (coded as 0.5), or "no" (coded as 0). In 2013-2015, this question has a precursor: "Does your EHR integrate any type of clinical information received electronically (not eFax) from providers or sources outside your hospital system/organization without the need for manual entry?" Only hospitals answering "yes, routinely" or "yes, but not routinely" to this precursor question proceed to our main question on integration interoperability. Thus, if a hospital answered "no" to the precursor question, we record its answer to the main question as "no."

C.2 Interoperability Decomposition

From hospital-level reported interoperabilities $(\iota_{ht})_{h,t}$ and the shares $(r(h,e_h,t))_{h,t}$ of patients received from same-vendor hospitals, we estimate within-vendor interoperability $\iota_{e,e,t}$ and across-vendor interoperability $\iota_{e,-e,t}$ for each EHR vendor e and each bin of years in 2013-2015, 2016-2017, and 2018-2019. We proceed in two steps: *first*, we estimate an ordered probit model of hospitals' reported interoperability, and second, we convert estimates of this model into estimates of vendors' within-vendor and across-vendor interoperabilities.

The ordered probit model is as follows:

$$\iota_{ht} = \begin{cases} 0 & \text{if score}_{ht} \leq c_1 \\ 0.5 & \text{if } c_1 < \text{score}_{ht} \leq c_2 \\ 1 & \text{if score}_{ht} > c_2, \end{cases} \tag{C.1}$$

where

$$\text{score}_{ht} = \left(\sum_e \mathbf{1}_{\{e_{ht}=e\}} \times \left(\alpha^0_{e,t} + \alpha^1_{e,t} r(h,e_{ht},t) \right) \right) + \beta \text{RHIO}_{h,m,t} + \epsilon_{ht}, \tag{C.2}$$

and $\epsilon_{ht} \sim \text{N}(0,1)$. Here, the interoperability coefficients $(\alpha^0_{e,t}, \alpha^1_{e,t})$ are imposed to be the same within each bin of years. The variable $\text{RHIO}_{h,m,t}$ is a control for the share of hospital beds in h's local market m that participate in a regional health information organization (RHIO) in year t

if h participates in a RHIO in year t, and zero otherwise.

To address potential endogeneity of $r(h, e_{ht}, t)$, we instrument for $r(h, e_{ht}, t)$ using the share $z(h, e_{ht}, t)$ of other hospital beds in hospital h's local market whose hospitals use the same EHR vendor as h in year t (i.e., what $r(h, e_{ht}, t)$ would be if hospital h equally shared its patients with all other hospital beds in its market). For estimation, we use a two-step control function approach (Wooldridge, 2015). First, we run the following regression:

$$r(h, e_{ht}, t) = \left(\sum_{e} \mathbf{1}_{\{e_{ht}=e\}} \times \left(\tilde{\alpha}^{0}_{e,t} + \tilde{\alpha}^{1}_{e,t} z(h, e_{ht}, t) \right) \right) + \tilde{\beta} \text{RHIO}_{h,m,t} + \nu_{ht}, \tag{C.3}$$

and obtain the predicted residual $\hat{\nu}_{ht}$. Second, we estimate the ordered probit model including $\hat{\nu}_{ht}$ in Equation C.2 as an additional regressor. This step yields estimates of the thresholds $\{\hat{c}_1, \hat{c}_2\}$, the interoperability coefficients $\{\hat{\alpha}^{0}_{e,t}, \hat{\alpha}^{1}_{e,t}\}_{e,t}$, and $\hat{\beta}$.

Next, we convert the ordered probit estimates into numerical estimates of within- and across-interoperability $\{\iota_{e,e,t}, \iota_{e,-e,t}\}_{e,t}$. Our procedure follows the intuition that the higher $r(h, e_{ht}, t)$ is, the more reflective the hospital's reported interoperability is of the hospital vendor's within-interoperability than across-interoperability. Specifically, we define $\hat{\iota}_{e,e,t}$ as the expected value of ι_{ht} if $r(h, e_{ht}, t) = 1$, and $\hat{\iota}_{e,-e,t}$ is the counterpart for $r(h, e_{ht}, t) = 0$. These estimates are calculated as follows:

$$\hat{\iota}_{e,-e,t} = 0.5 \left[\Phi\left(\hat{c}_2 - \hat{\alpha}^{0}_{e,t}\right) - \Phi\left(\hat{c}_1 - \hat{\alpha}^{0}_{e,t}\right) \right] + 1 \left[1 - \Phi\left(\hat{c}_2 - \hat{\alpha}^{0}_{e,t}\right) \right] \tag{C.4}$$

and

$$\hat{\iota}_{e,e,t} = 0.5 \left[\Phi\left(\hat{c}_2 - \hat{\alpha}^{0}_{e,t} - \hat{\alpha}^{1}_{e,t}\right) - \Phi\left(\hat{c}_1 - \hat{\alpha}^{0}_{e,t} - \hat{\alpha}^{1}_{e,t}\right) \right] + 1 \left[1 - \Phi\left(\hat{c}_2 - \hat{\alpha}^{0}_{e,t} - \hat{\alpha}^{1}_{e,t}\right) \right]. \tag{C.5}$$

Thus, these estimated interoperability levels are between 0 and 1.

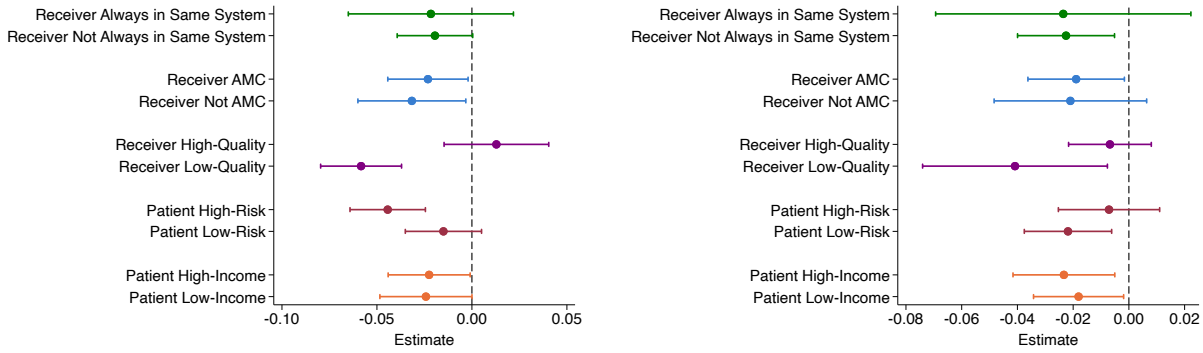
D Additional Empirical Results

D.1 Additional Evidence of Direct Effect of Interoperability on Patient Outcomes (Section 3)

D.1.1 Additional Heterogeneity

Appendix Figure D1 shows heterogeneity in the direct effect of hospitals gaining the same EHR vendor on transfer patient images (Panel A) and referral patient tests (Panel B) across hospital and patient characteristics.

Figure D1: Additional Direct Effect Heterogeneity by Hospital, Patient Characteristics



(a) Transfer # Images

(b) Referral # Tests

Notes: Figure plots difference-in-differences estimates (dots) and 95% confidence intervals (bars) of the effect of a hospital sender-receiver pair gaining the same EHR vendor on the outcomes of patients who are shared between that pair (as described in Equation 3.1). "AMC" stands for Academic Medical Center. Receiver hospital quality is measured as the baseline risk-adjusted mortality rate for transfer (referral) patients received by the hospital in the first two years of our data samples. "High-quality" is the half of receiver hospitals that have the lowest risk-adjusted mortality rates, while "low-quality" is the half with the highest risk-adjusted mortality rates. Patient risk is measured as predicted mortality using baseline measures of patient age, sex, race, and chronic conditions as well as patient diagnosis codes. "High-risk" patients are those with the top half of such predicted mortality rates, while "low-risk" patients are those with the bottom half. "High-income" patients are those residing in zipcodes with the top half of median household incomes in 2019, while "low-income" patients are those residing in zipcodes with the bottom half.

D.1.2 Robustness to Changes in Patient Composition, Hospital & Market Characteristics

Table 2 presents our main results for the direct effect of hospitals gaining the same EHR vendor on shared patient outcomes. Appendix Table D1 shows robustness to adding controls for observable patient characteristics (demographics, baseline health measures, diagnosis codes)

and patient-level fixed effects. Appendix Table D2 shows robustness to adding hospital sender and receiver controls for beds, employees, ownership, and capabilities, as well as restricting the sample to patients shared between hospital pairs that never belong to the same hospital system over the sample period.

Table D1: Direct Effect of Gaining Same Vendor on Shared Patient Outcomes - Robustness to Adding Patient Characteristics

(a) Transfers

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Charges						
Same EHR Vendor	-2653.3***	-2634.1***	-2664.9***	-2797.7***	-5810.7***	-2797.7**
	(882.8)	(888.0)	(892.0)	(917.2)	(1667.4)	(1249.3)
Observations	2406466	2394951	2347783	2347783	821655	2347783
Mean of Outcome	75314 Charges	75314 Charges	75314 Charges	75314 Charges	75314 Charges	75314 Charges
Panel B: # Images						
Same EHR Vendor	-0.0238**	-0.0235**	-0.0244***	-0.0305***	-0.0135	-0.0305***
	(0.00924)	(0.00927)	(0.00923)	(0.00934)	(0.0158)	(0.0111)
Observations	2406466	2394951	2347783	2347783	821655	2347783
Mean of Outcome	1.425	1.425	1.425	1.425	1.425	1.425
Patient Demographics		X	X	X		X
Patient Health			X	X		X
Patient Diagnosis				X		X
Patient FEs					X	
Level of Cluster	Pair	Pair	Pair	Pair	Pair	HRR

(b) Referrals

	(1)	(2)	(3)	(4)	(5)
Panel A: # Tests					
Same EHR Vendor	-0.0188**	-0.0198**	-0.0188**	-0.00649	-0.0188*
	(0.00786)	(0.00783)	(0.00778)	(0.00532)	(0.00984)
Observations	26535742	26393950	26393910	25113303	26393910
Mean of Outcome	0.659	0.659	0.659	0.659	0.659
Panel B: 60-Day Readmit					
Same EHR Vendor	-0.00484***	-0.00498***	-0.00440***	-0.00332***	-0.00440***
	(0.000995)	(0.000995)	(0.000956)	(0.000733)	(0.00107)
Observations	22164926	22048799	22048773	20785265	22048773
Mean of Outcome	0.0392	0.0392	0.0392	0.0392	0.0392
Patient Demographics		X	X		X
Patient Health			X		X
Patient FEs				X	
Level of Cluster	Pair	Pair	Pair	Pair	HRR

Notes: Each column in each panel in each table is a separate difference-in-differences regression of the effect of the patient's sending and receiving hospitals gaining the same EHR vendor on patient outcomes as described in Equation 3.1. Panel A shows transfer patient outcomes, while Panel B shows referral patient outcomes. All regressions include hospital fixed effects and year fixed effects. Column 1 has no patient-level controls. Column 2 adds controls for patient age, sex, and race. Column 3 adds controls for patient baseline Charlson Comorbidity Index as well as indicators for 27 patient baseline chronic conditions. Column 4 adds fixed effects for patient diagnostic code categories. Column 5 replaces these patient-level controls with patient fixed effects. Column 6 re-estimates Column 4 but clustering standard errors at the HRR-level. Panel B (referrals) does not include a specification with patient diagnostic code categories. Standard errors are clustered at the hospital sender-receiver pair level for all columns (except Column 6) and are reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table D2: Direct Effect of Gaining Same Vendor on Shared Patient Outcomes - Robustness to Adding Hospital Characteristics

(a) Transfers

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Charges						
Same EHR Vendor	-2797.7***	-2637.9***	-2571.0***	-2533.2***	-2532.0***	-4138.8***
	(917.2)	(906.8)	(888.4)	(878.1)	(875.8)	(1152.8)
Observations	2347783	2347783	2300977	2300977	2300977	2076227
Mean of Outcome	75314	75314	75314	75314	75314	75314
	Charges	Charges	Charges	Charges	Charges	Charges
Panel B: # Images						
Same EHR Vendor	-0.0305***	-0.0308***	-0.0325***	-0.0327***	-0.0333***	-0.0170
	(0.00934)	(0.00932)	(0.00935)	(0.00926)	(0.00923)	(0.0119)
Observations	2347783	2347783	2300977	2300977	2300977	2076227
Mean of Outcome	1.425	1.425	1.425	1.425	1.425	1.425
Hospital Beds		X	X	X	X	
Hospital Employees			X	X	X	
Hospital Ownership				X	X	
Hospital Capabilities					X	
Never Same System						X

(b) Referrals

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: # Tests						
Same EHR Vendor	-0.0188**	-0.0189**	-0.0197**	-0.0190**	-0.0186**	-0.0307***
	(0.00778)	(0.00777)	(0.00777)	(0.00773)	(0.00768)	(0.0101)
Observations	26393910	26393910	26393910	26393910	26393910	23163190
Mean of Outcome	0.659	0.659	0.659	0.659	0.659	0.659
Panel B: 60-Day Readmit						
Same EHR Vendor	-0.00440***	-0.00439***	-0.00423***	-0.00421***	-0.00425***	-0.00422***
	(0.000956)	(0.000954)	(0.000956)	(0.000957)	(0.000960)	(0.00116)
Observations	22048773	22048773	22048773	22048773	22048773	19365725
Mean of Outcome	0.0392	0.0392	0.0392	0.0392	0.0392	0.0392
Hospital Beds		X	X	X	X	
Hospital Employees			X	X	X	
Hospital Ownership				X	X	
Hospital Capabilities					X	
Never Same System						X

Notes: Each column in each panel in each table is a separate difference-in-differences regression of the effect of the patient's sending and receiving hospitals gaining the same EHR vendor on patient outcomes as described in Equation 3.1. Panel A shows transfer patient outcomes, while Panel B shows referral patient outcomes. All regressions include hospital pair fixed effects and year fixed effects as well as fixed effects for patient diagnostic code categories, indicators for 27 patient baseline chronic conditions, and controls for patient Charlson Comorbidity Index, patient age, patient sex, and patient race. Column 1 has no time-varying hospital-level controls. Columns 2-5 add sender hospital-level and receiver hospital-level controls for beds, employees, ownership, and a variety of indicators for capabilities, respectively. Column 6 re-estimates Column 1 but limits the sample to patients shared between hospital sender-receiver pairs that are never in the same hospital system over the sample period. Standard errors are clustered at the hospital sender-receiver pair for all columns and are reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

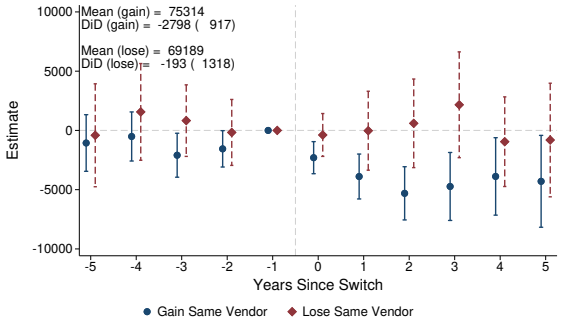
D.1.3 Direct Effect of Losing Same Vendor

When hospitals gain the same EHR vendor, charges, diagnostic tests, and readmission rates for shared transfer and referral patients decrease. We now test whether the opposite occurs when hospitals *lose* the same EHR vendor—do charges, diagnostic tests, and readmission rates for transfer and referral patients *increase*?

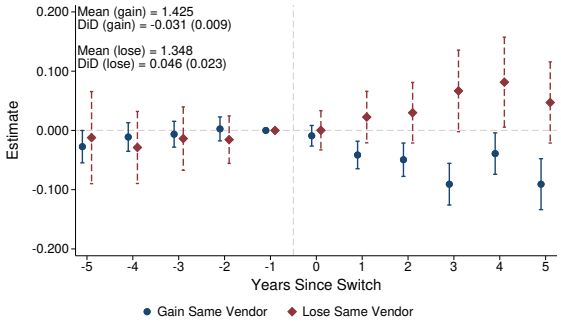
To answer this question, we use the same event study specification as Equation 3.2, but we modify the main independent variable to indicate whether the hospital pair has *different* EHR vendors rather than the same. We then limit the sample to hospital pairs that either (1) always have the same vendor over the sample period ("always treated," 7.5% of hospital pair observations) or (2) start with the same vendor and switch to different vendors only once (6.3% of observations).

Figure D2 plots event study estimates for our four main outcomes for transfer patients, while Figure D3 shows results for referral patients. We find a positive relationship between hospitals losing the same EHR vendor and the number of images and tests for shared transfer patients, suggesting duplicative healthcare procedures increase when hospitals become less interoperable. However, in contrast to when hospital pairs gain the same vendor, we see no effect on shared transfer patient charges and no significant effects for referral patient outcomes.

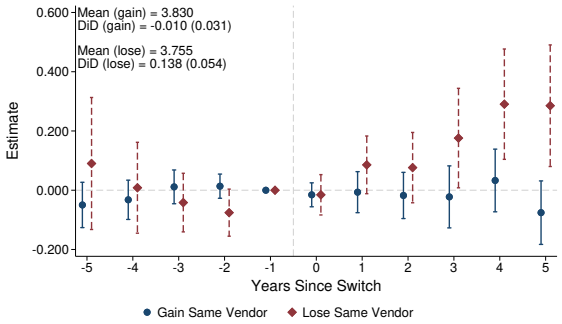
Figure D2: Event Studies of Direct Effect of Losing Same Vendor on Shared Transfer Outcomes



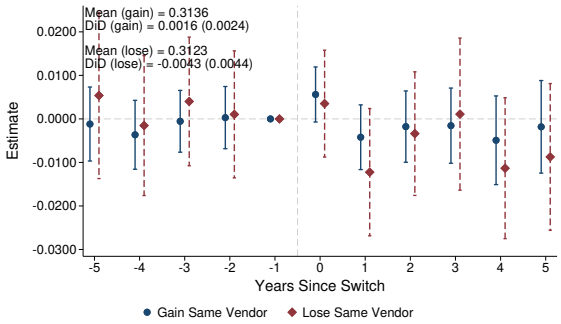
(a) Transfers, Charges



(b) Transfers, # Images



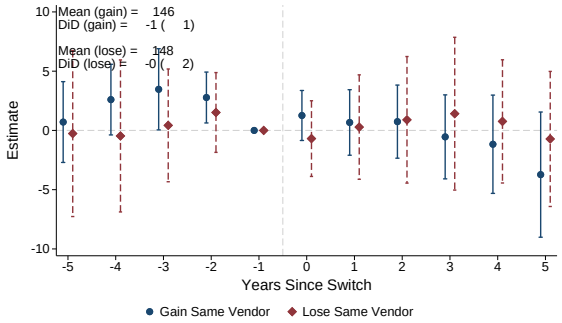
(c) Transfers, # Tests



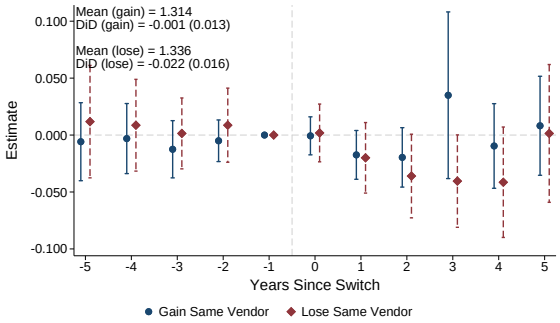
(d) Transfers, 30-Day Readmit

Notes: Figure plots patient-level event study estimates of the effect of both the patient's sending and receiving hospitals gaining (blue) and losing (red) the same EHR vendor on transfer patient outcomes as described in Equation 3.2. All regressions include hospital pair fixed effects and year fixed effects as well as fixed effects for patient diagnostic code categories, indicators for 27 patient baseline chronic conditions, and controls for patient Charlson Comorbidity Index, patient age, patient sex, and patient race. Standard errors are clustered at the hospital sender-receiver pair level. Bars show 95% confidence intervals. The mean of each dependent variable among treated observations in the pre-treatment period is given as are the standard difference-in-differences estimates and corresponding standard errors.

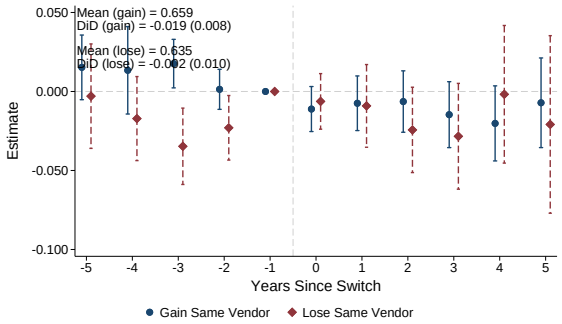
Figure D3: Event Studies of Direct Effect of Losing Same Vendor on Shared Referral Outcomes



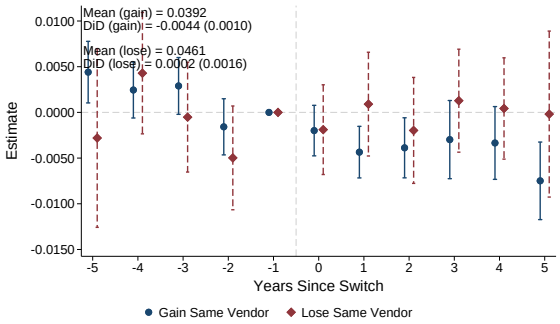
(a) Referrals, Charges



(b) Referrals, # Images



(c) Referrals, # Tests



(d) Referrals, 60-Day Readmit

Notes: Figure plots patient-level event study estimates of the effect of both the patient's sending and receiving hospitals gaining (blue) and losing (red) the same EHR vendor on referral patient outcomes as described in Equation 3.2. All regressions include hospital pair fixed effects and year fixed effects as well as fixed effects for patient diagnostic code categories, indicators for 27 patient baseline chronic conditions, and controls for patient Charlson Comorbidity Index, patient age, patient sex, and patient race. Standard errors are clustered at the hospital sender-receiver pair level. Bars show 95% confidence intervals. The mean of each dependent variable among treated observations in the pre-treatment period is given as are the standard difference-in-differences estimates and corresponding standard errors.

D.1.4 Details of Placebo Test on Patient Outcomes

To check whether our main estimates of the direct effect of interoperability on patient outcomes result from hospitals switching EHR vendors rather than switching to the same EHR vendor, we conduct a placebo test around EHR vendor changes for hospital sender-receiver pairs that never have the same vendor (i.e., we look at vendor changes when those changes do not result in the pair either gaining or losing the same vendor). We restrict the sample to control hospital pairs from our main specification (pairs that never share the same vendor) where both sender and receiver switch EHR vendors at most once over our sample period. We discard pairs where sender and receiver switch vendors in different years to simplify identification to a single event for each pair. We then run the following difference-in-differences specification around whether either sender or receiver has switched vendors:

$$Y_{i(hh')t} = \beta * \mathbf{1}_{\left[\text{Post-Switch}_{(hh')t}\right]} + \alpha_{(hh')} + \gamma_t + \delta X_{it,(hh')t,mt} + \epsilon_{i(hh')t}. \tag{D.1}$$

Here, $\mathbf{1}_{\left[\text{Post-Switch}_{(hh')t}\right]}$ indicates whether either hospital has switched vendors (but not to the same vendor as the other) by year t. Table 3 shows estimates from this specification.

D.2 Additional Evidence of Allocative Effect of Interoperability on Patient Flows (Section 4)

D.2.1 Details of Effect of Switches on Total Patient Flows

In this section, we analyze the effect of hospital EHR vendor switches on the number of total transfers and referrals sent and received. To do so, we run the following hospital-year-level regression that includes hospital fixed effects, year fixed effects, and an indicator for whether the hospital has already switched EHR vendors:

$$Y_{ht} = \beta * \mathbf{1}_{[\text{Post-Switch}_{ht}]} + \alpha_h + \gamma_t + \epsilon_{ht}. \tag{D.2}$$

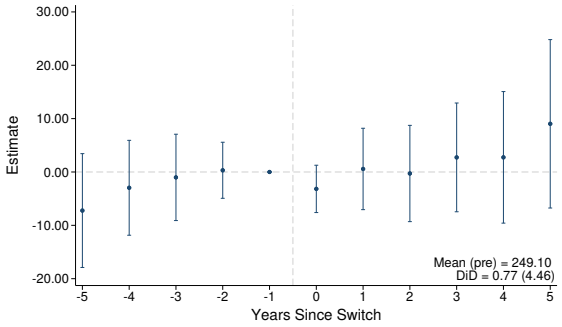
We cluster standard errors at the hospital level and weight observations by baseline hospital beds. We also estimate the following event study equation:

$$Y_{ht} = \left(\sum_{r=-5}^{5} \beta_r \mathbf{1}_{[t=s_h+r]} \right) + \alpha_h + \gamma_t + \epsilon_{ht}, \tag{D.3}$$

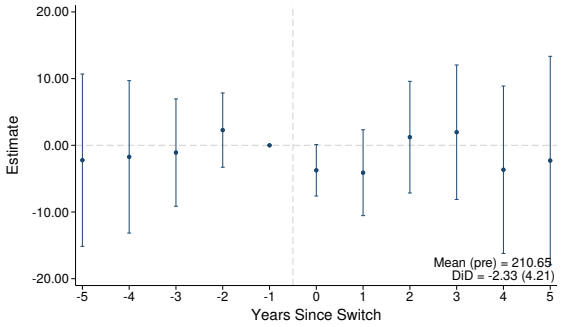
where r is the relative year since the hospital switched vendors (occurring in year s_h) and we exclude $r = -1$. We limit our analysis to hospitals that either never switch EHR vendors ("never treated") or switch only once over the sample period.

Appendix Figure D4 plots event study estimates of the effect of switching EHR vendors on transfers and referrals sent and received from Equation D.3. Table 4 shows static difference-in-differences estimates, while Appendix Tables D3 and D4 show robustness to various specification checks. Switching EHR vendors does not affect total transfers sent or received, nor total referrals sent, but does increase total referrals received by 8.8%. Figure D5 shows heterogeneity by EHR vendor, with effects on total referrals received driven by Epic.

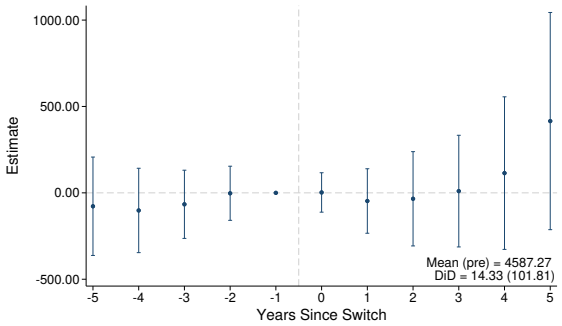
Figure D4: Hospital Vendor Switches on Total Patient Flows



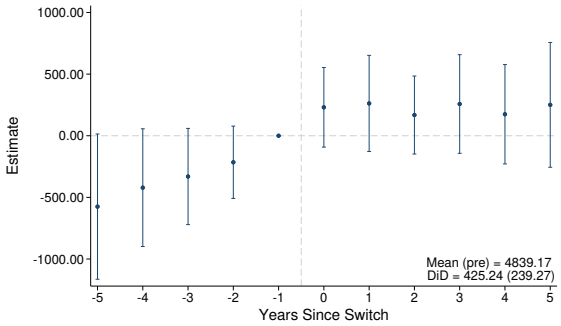
(a) Transfers Sent



(b) Transfers Received



(c) Referrals Sent



(d) Referrals Received

Notes: Figure plots event study estimates of the effect of switching EHR vendors on the number of transfers and referrals both sent from and received by the switching hospital as described in Equation D.3. Observations are weighted by bed size in 2005. Sample is limited to hospitals that switch vendors once or never. Bars show 95% confidence intervals.

Table D3: Hospital Vendor Switches on Total Patient Flows - Robustness for Transfers

(a) Transfers Sent

	# of Transfers Sent				
	(1)	(2)	(3)	(4)	(5)
Post-EHR Switch	0.769	0.820	-0.719	-0.719	7.746
	(4.461)	(4.316)	(4.061)	(3.927)	(5.073)
Observations	34542	34542	32581	32581	
Mean of Outcome	249.10	249.10	249.10	249.10	249.10
Sample	Event	Event	Event	Event	C&S
Hospital Covariates		X	X	X	
Market x Year FEs			X	X	
Level of Cluster	Hosp	Hosp	Hosp	HRR	Hosp

(b) Transfers Received

	# of Transfers Received				
	(1)	(2)	(3)	(4)	(5)
Post-EHR Switch	-2.332	-2.299	-2.175	-2.175	-1.899
	(4.205)	(4.151)	(3.952)	(3.880)	(4.608)
Observations	34542	34542	32581	32581	
Mean of Outcome	210.65	210.65	210.65	210.65	210.65
Sample	Event	Event	Event	Event	C&S
Hospital Covariates		X	X	X	
Market x Year FEs			X	X	
Level of Cluster	Hosp	Hosp	Hosp	HRR	Hosp

Notes: Each column is a separate hospital-year-level difference-in-differences regression of the effect of switching EHR vendors on total patient flows to/from the switching hospital with hospital fixed effects, year fixed effects, and standard errors clustered at the hospital (or HRR) level as described in Equation D.2. Observations are weighted by bed size in 2005. Sample is limited to hospitals that switch vendors once or never. Standard errors are in parentheses. Column 2 adds controls for hospital's current bed sizes. Column 3 adds market by year fixed effects. Column 5 follows Callaway and Sant'Anna (2021). * p<0.10, ** p<0.05, *** p<0.01.

Table D4: Hospital Vendor Switches on Total Patient Flows - Robustness for Referrals

(a) Referrals Sent

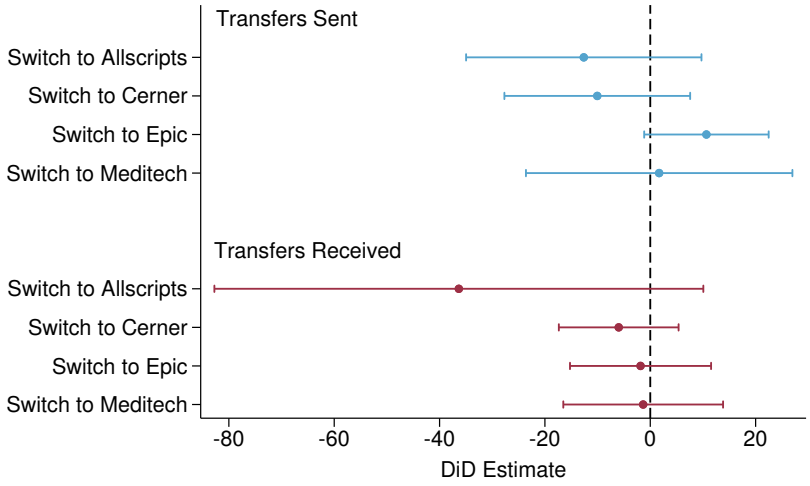
	# of Referrals Sent				
	(1)	(2)	(3)	(4)	(5)
Post-EHR Switch	14.33	26.22	-65.17	-65.17	132.7
	(101.8)	(102.2)	(91.50)	(123.9)	(185.3)
Observations	29805	29805	28319	28319	
Mean of Outcome	4587.27	4587.27	4587.27	4587.27	4587.27
Sample	Event	Event	Event	Event	C&S
Hospital Covariates		X	X	X	
Market x Year FEs			X	X	
Level of Cluster	Hosp	Hosp	Hosp	HRR	Hosp

(b) Referrals Received

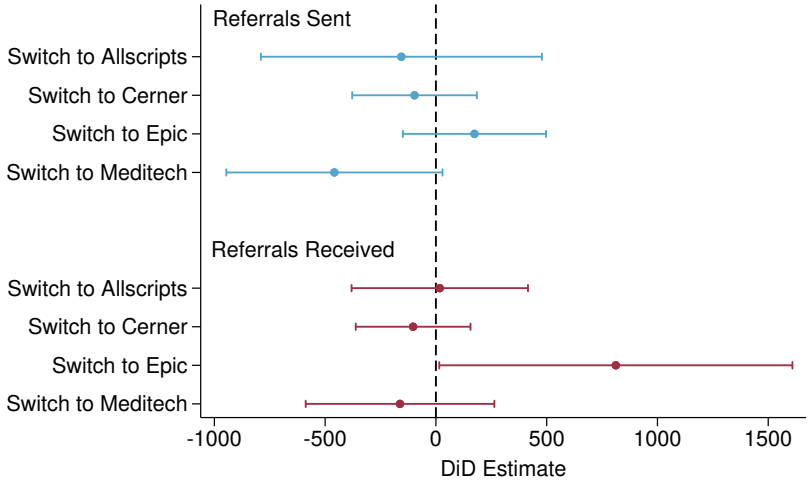
	# of Referrals Received				
	(1)	(2)	(3)	(4)	(5)
Post-EHR Switch	425.2*	417.5*	387.5*	387.5	268.9*
	(239.3)	(237.6)	(211.7)	(260.5)	(147.2)
Observations	29805	29805	28319	28319	
Mean of Outcome	4839.17	4839.17	4839.17	4839.17	4839.17
Sample	Event	Event	Event	Event	C&S
Hospital Covariates		X	X	X	
Market x Year FEs			X	X	
Level of Cluster	Hosp	Hosp	Hosp	HRR	Hosp

Notes: Each column is a separate hospital-year-level difference-in-differences regression of the effect of switching EHR vendors on total patient flows to/from the switching hospital with hospital fixed effects, year fixed effects, and standard errors clustered at the hospital (or HRR) level as described in Equation D.2. Observations are weighted by bed size in 2005. Sample is limited to hospitals that switch vendors once or never. Standard errors are in parentheses. Column 2 adds controls for hospital's current bed sizes. Column 3 adds market by year fixed effects. Column 5 runs Callaway and Sant'Anna (2021). * p<0.10, ** p<0.05, *** p<0.01.

Figure D5: Total Flows Heterogeneity by Vendor



(a) Transfers



(b) Referrals

Notes: Figure plots difference-in-differences estimates (dots) of the effect of switching EHR vendors on the number of transfers and referrals both sent and received according to Equation D.2, separately by the identity of the EHR vendor to which the switching hospital switches. Bars show 95% confidence intervals.

D.2.2 Details of Magnitudes Calculation

To assess the magnitude of interoperability-driven patient reallocation, we conduct back-of-the-envelope calculations of the number of patient flows reallocated due to EHR vendor switches in our sample period.

After hospitals switch EHR vendors, they have on average 7.3 other same-vendor hospi-

tals in their HRR (the market definition for transfers) and 21.7 other same-vendor hospitals with shared baseline referrals (the market definition for referrals). Multiplying these numbers by our treatment effect estimates suggests that when hospitals switch vendors, they send approximately 3% more transfers and 3% more referrals to newly same-vendor hospitals.[82]

Given over 7.8 million Medicare cross-hospital transfers during 2005-2019, over 930 million Medicare cross-hospital referrals during 2008-2019, and 40% of hospitals switching EHR vendors at least once, these estimates imply that over 100,000 Medicare transfers and 11 million Medicare referrals were reallocated across hospitals due to vendor switches.[83]

Considering average charges of $85,336.40 per transfer and $143.50 per referral at receiving hospitals (Table 1), these patient flow changes also reallocated $10.4 billion in Medicare spending across hospitals during this 15-year period, or approximately $700 million annually.

D.2.3 Robustness to Changes in Hospital & Market Characteristics

Figure 5 presents our main results for the allocative effect of hospitals gaining the same EHR vendor on shared patient flows. Appendix Table D5 shows robustness across various specifications: adding hospital sender and receiver controls for beds, employees, ownership, and capabilities; adding hospital sender-by-year and receiver-by-year fixed effects; limiting the sample to hospital pairs that never switch hospital systems; and clustering standard errors at the HRR level. The table reports results from the static difference-in-differences version of Equation 4.1:

$$Y_{(hh')t} = \beta * \mathbf{1}_{[e_{ht}=e_{h't}]} + \alpha_{(hh')} + \gamma_t + \delta X_{(hh')t,mt} + \epsilon_{(hh')t}. \tag{D.4}$$

Appendix Table D6 shows that results are further robust to Poisson Pseudo Maximum Likelihood estimation, which addresses frequent zeros in bilateral patient flows. Appendix Figure D6 then shows results are robust to Callaway and Sant'Anna (2021) estimation, while Appendix Figure D7 shows robustness to unweighted hospital pair observations.

[82] For transfers: 0.0045 (treatment effect on fraction of transfers) × 7.3 = 0.03285 ≈ 3.3%. For referrals: 0.0014 (treatment effect on fraction of referrals) × 21.7 = 0.03038 ≈ 3.0%.

[83] For transfers: 7.8 million × 0.4 × 0.03285 = 102,492. For referrals: 186 million (20% sample) × 5 × 0.4 × 0.03038 = 11.3 million.

Table D5: Allocative Effect of Gaining Same Vendor on Shared Patient Flows - Robustness to Adding Hospital Characteristics

(a) Transfers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: # of Transfers									
Same EHR Vendor	0.431***	0.424***	0.415***	0.503***	0.499***	0.434**	0.429***	0.415**	-0.215
	(0.151)	(0.156)	(0.156)	(0.163)	(0.163)	(0.185)	(0.138)	(0.176)	(0.286)
Observations	957657	957656	957656	912995	912995	626938	955832	957656	342336
Mean of Outcome	4.91	4.91	4.91	4.91	4.91	4.91	4.91	4.91	4.91
Panel B: Fraction of Transfers									
Same EHR Vendor	0.00395***	0.00459***	0.00449***	0.00461***	0.00456***	0.00484***	0.00503***	0.00449***	0.00241*
	(0.000880)	(0.000827)	(0.000824)	(0.000836)	(0.000835)	(0.000954)	(0.000829)	(0.00108)	(0.00141)
Observations	901423	901414	901414	880662	880662	606206	899411	901414	318930
Mean of Outcome	0.0576	0.0576	0.0576	0.0576	0.0576	0.0576	0.0576	0.0576	0.0576
Market x Year FEs		X	X	X	X	X		X	X
Hospital Beds			X	X	X	X		X	X
Hospital Employees				X	X	X			
Hospital Ownership					X	X			
Hospital Capabilities						X			
Hospital x Year FEs							X		
Drop System Switchers									X
Level of Cluster	Pair	Pair	Pair	Pair	Pair	Pair	Pair	HRR	Pair

(b) Referrals

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: # of Referrals									
Same EHR Vendor	5.996***	4.985***	4.926***	4.783***	4.697***	3.931***	2.606**	4.926***	3.991**
	(1.150)	(1.080)	(1.081)	(1.088)	(1.087)	(1.149)	(1.178)	(1.467)	(1.992)
Observations	1450814	1425179	1425179	1401970	1401970	1064102	1448065	1425179	433454
Mean of Outcome	50.16	50.16	50.16	50.16	50.16	50.16	50.16	50.16	50.16
Panel B: Fraction of Referrals									
Same EHR Vendor	0.00132***	0.00137***	0.00135***	0.00127***	0.00126***	0.00120***	0.00140***	0.00135***	0.00131***
	(0.000191)	(0.000192)	(0.000192)	(0.000192)	(0.000192)	(0.000209)	(0.000215)	(0.000273)	(0.000380)
Observations	1431659	1406422	1406422	1396069	1396069	1060813	1429309	1406422	427298
Mean of Outcome	0.0149	0.0149	0.0149	0.0149	0.0149	0.0149	0.0149	0.0149	0.0149
Market x Year FEs		X	X	X	X	X		X	X
Hospital Beds			X	X	X	X		X	X
Hospital Employees				X	X	X			
Hospital Ownership					X	X			
Hospital Capabilities						X			
Hospital x Year FEs							X		
Drop System Switchers									X
Level of Cluster	Pair	Pair	Pair	Pair	Pair	Pair	Pair	HRR	Pair

Notes: Each column in each panel in each table is a separate difference-in-differences regression of the effect of gaining the same EHR vendor with hospital sender-receiver pair fixed effects and year fixed effects as described in Equation D.4. In Table (a), the outcome in Panel A is number of transfers while that in Panel B is fraction of transfers. In Table (b), the outcome in Panel A is number of referrals while that in Panel B is fraction of referrals. Observations are weighted by combined bed size in 2005. Column 1 has no time-varying controls. Column 2 adds market by year fixed effects. Columns 3-6 further add hospital-level controls for beds, employees, ownership, and a variety of indicators for capabilities, respectively. Column 7 replaces these controls with hospital sender by year fixed effects and hospital receiver by year fixed effects. Column 8 re-estimates Column 3 but clustering standard errors at the HRR-level. Column 9 drops observations in which at least one hospital in the pair switches hospital systems over the course of the sample period. Standard errors are clustered at the hospital sender-receiver pair level for all columns (except Column 8) and are reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table D6: Allocative Effect of Gaining Same Vendor on Shared Patient Flows - Robustness to Poisson Pseudo Maximum Likelihood (PPML)

(a) Transfers

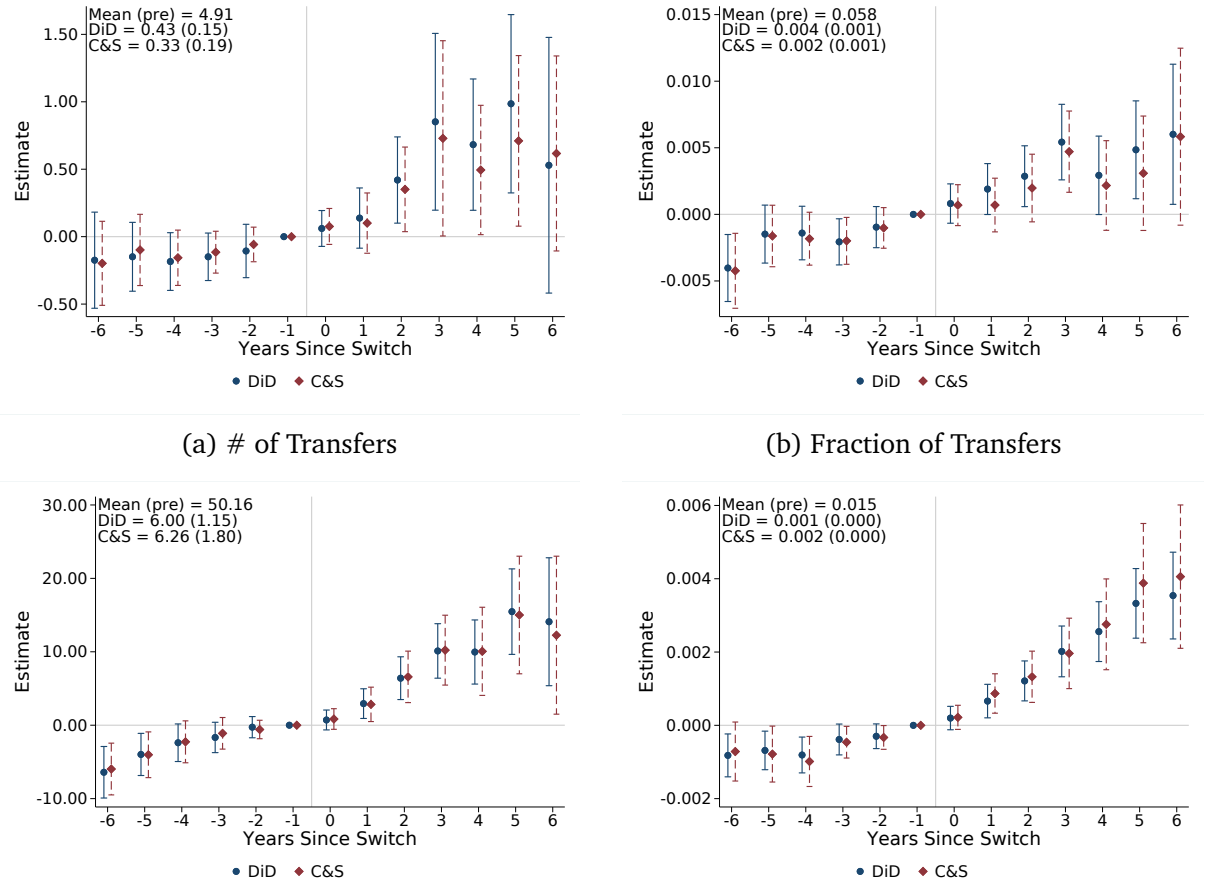
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: # of Transfers						
Same EHR Vendor	0.120***	0.113***	0.158***	0.120***	0.113***	0.158***
	(0.0321)	(0.0264)	(0.0220)	(0.0321)	(0.0264)	(0.0220)
Observations	390458	390399	342308	390458	390399	342308
Mean of Outcome	4.91	4.91	4.91	4.91	4.91	4.91
Panel B: Fraction of Transfers						
Same EHR Vendor	0.0737***	0.0885***	0.152***	0.0737***	0.0885***	0.152***
	(0.0148)	(0.0149)	(0.0226)	(0.0148)	(0.0149)	(0.0226)
Observations	380831	380773	342308	380831	380773	342308
Mean of Outcome	0.0576	0.0576	0.0576	0.0576	0.0576	0.0576
Market x Year FEs		X			X	
Hospital x Year FEs			X			X
Level of Cluster	Pair	Pair	Pair	Pair	Pair	Pair
Separation	FE, Simplex, IR	FE, Simplex, IR	FE, Simplex, IR	FE, Simplex	FE, Simplex	FE, Simplex

(b) Referrals

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: # of Referrals						
Same EHR Vendor	0.137***	0.143***	0.132***	0.137***	0.143***	0.132***
	(0.0209)	(0.0167)	(0.0126)	(0.0209)	(0.0167)	(0.0126)
Observations	1374119	1349101	1337024	1374119	1349101	1337024
Mean of Outcome	50.16	50.16	50.16	50.16	50.16	50.16
Panel B: Fraction of Referrals						
Same EHR Vendor	0.0779***	0.0853***	0.137***	0.0779***	0.0853***	0.137***
	(0.0111)	(0.0113)	(0.0133)	(0.0111)	(0.0113)	(0.0133)
Observations	1358747	1334102	1337024	1358747	1334102	1337024
Mean of Outcome	0.0149	0.0149	0.0149	0.0149	0.0149	0.0149
Market x Year FEs		X			X	
Hospital x Year FEs			X			X
Level of Cluster	Pair	Pair	Pair	Pair	Pair	Pair
Separation	FE, Simplex, IR	FE, Simplex, IR	FE, Simplex, IR	FE, Simplex	FE, Simplex	FE, Simplex

Notes: Each column in each panel in each table is a separate difference-in-differences regression of the effect of gaining the same EHR vendor with hospital sender-receiver pair fixed effects and year fixed effects as described in Equation D.4, estimated using Poisson Pseudo Maximum Likelihood (PPML). Standard errors are clustered at the hospital sender-receiver pair and reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Figure D6: Allocative Effect of Gaining Same Vendor on Shared Patient Flows - Robustness to Callaway and Sant'Anna (2021)



(a) # of Transfers

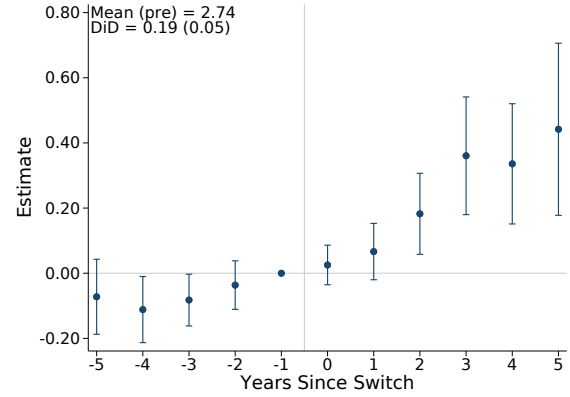
(b) Fraction of Transfers

(c) # of Referrals

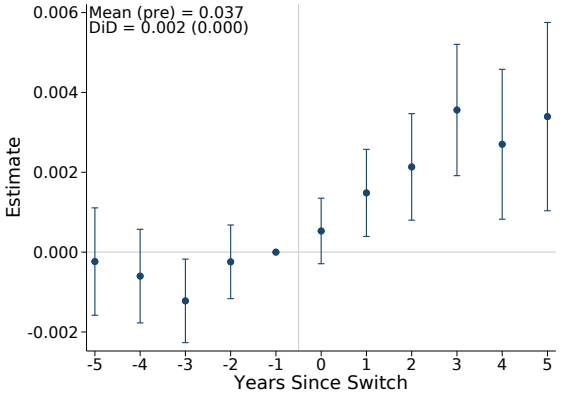
(d) Fraction of Referrals

Notes: Figure plots event study estimates of the effect of gaining the same EHR vendor on number of transfers (Panel A), fraction of transfers (Panel B), number of referrals (Panel C), and fraction of referrals (Panel D) shared between a hospital pair as described in Equation 4.1 without any time-varying controls. The denominator for fraction of transfers (referrals) is the total number of transfers (referrals) sent from the sending hospital that year. Bars show 95% confidence intervals. Standard event study estimates are in blue (labeled "DiD"), while Callaway and Sant'Anna (2021) event study estimates are in red (labeled "C&S"). We use a universal base year for Callaway and Sant'Anna (2021) event studies rather than the default varying base year to make pre-period estimates comparable to standard event studies. The mean of each dependent variable among treated observations in the pre-treatment period is given as are the static difference-in-differences estimates and corresponding standard errors.

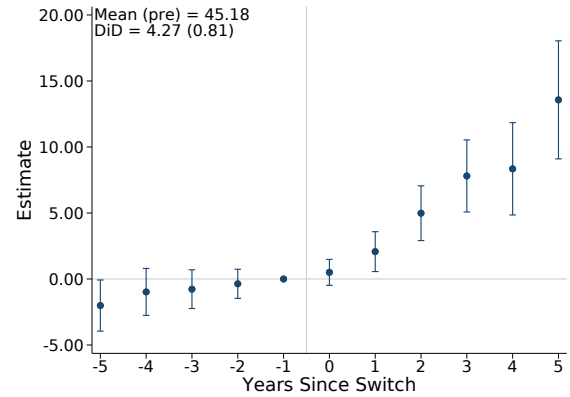
Figure D7: Allocative Effect of Gaining Same Vendor on Shared Patient Flows - Robustness to Not Weighting Observations



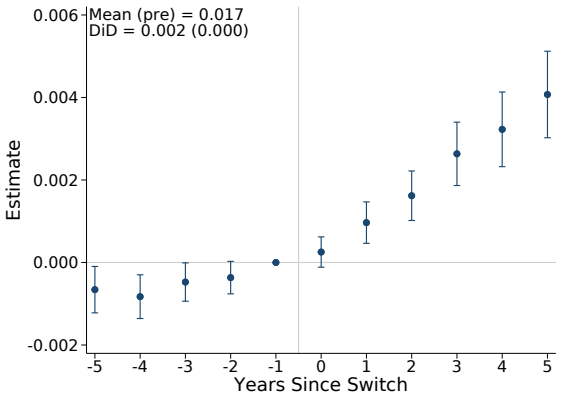
(a) # of Transfers



(b) Fraction of Transfers



(c) # of Referrals



(d) Fraction of Referrals

Notes: Figure plots event study estimates of the effect of gaining the same EHR vendor on number of transfers (Panel A), fraction of transfers (Panel B), number of referrals (Panel C), and fraction of referrals (Panel D) between a hospital pair as described in Equation 4.1 except observations are not weighted by any measure. The denominator for fraction of transfers/referrals is the total number of transfers/referrals sent from the sending hospital that year. Bars show 95% confidence intervals. Standard event study estimates are in blue (labeled "DiD"). The mean of each dependent variable among treated observations in the pre-treatment period is given as is the static difference-in-differences estimate and corresponding standard error.

D.2.4 Effect of Losing Same Vendor on Patient Flows

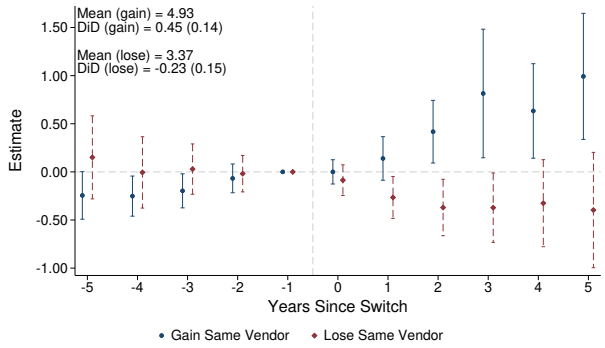
We show in the main text that patient flows between hospital pairs increase when they gain the same EHR vendor. We now ask whether the opposite is true: do patient flows decrease when pairs lose the same vendor? Asymmetric treatment effects are possible. For example, after switching EHR vendors, a hospital's extensive margin of patient flows could change—it could start sending more patients externally overall and direct all new flows to newly same-vendor hospitals without changing flows with previously same-vendor hospitals. We would then see positive treatment effects of gaining the same vendor but no effects of losing it.

We test this question using the same event study specification as Equation 4.1 but modify the main independent variable to indicate whether the hospital pair has *different* EHR vendors rather than the same. Further, we limit the sample to hospital pairs that either (1) always have the same vendor over the sample period ("always treated," 7.5% of observations) or (2) start with the same vendor and switch to different vendors only once (6.3% of observations).

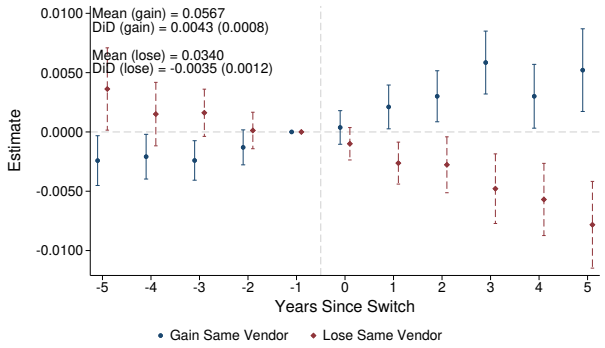
Appendix Figure D8 plots event study estimates for our four main outcomes, while Appendix Table D7 shows static difference-in-differences estimates from additional robustness checks. We find a negative relationship between hospitals losing the same EHR vendor and their shared patient flows that is statistically significant for all outcomes except number of transfers. When hospitals lose the same vendor, they experience a 10% decrease in fraction of shared transfers and an 18% decrease in fraction of shared referrals.

For fraction of transfers, the magnitude of losing the same vendor is slightly larger than gaining it (8%). For fraction of referrals, however, the losing effect is nearly double the gaining effect (9%). These differences may be partially driven by our regression specification. Hospitals that switch EHR vendors typically select a new vendor more widely adopted by other hospitals—particularly larger hospitals—in their local market than their old vendor. Since our empirical strategy operates at the hospital pair level, positive effects of gaining the same vendor are distributed more thinly across a greater number of hospital pairs than equivalent negative effects of losing the same vendor. If a hospital reallocates 10% of its patients from Cerner to Epic hospitals after switching from Cerner to Epic, and there are more Epic than Cerner hospitals locally, the average pair-level effect will be smaller in magnitude for gaining Epic hospitals than losing Cerner hospitals.

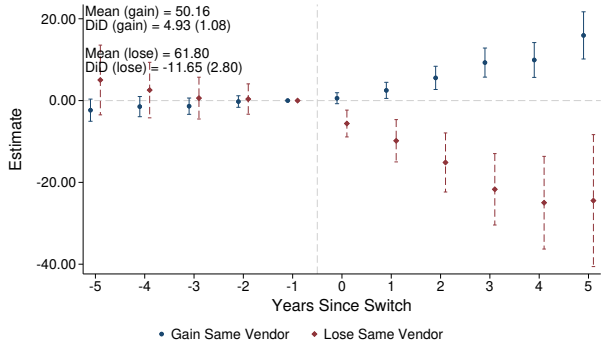
Figure D8: Event Studies of Losing Same Vendor on Shared Patient Flows



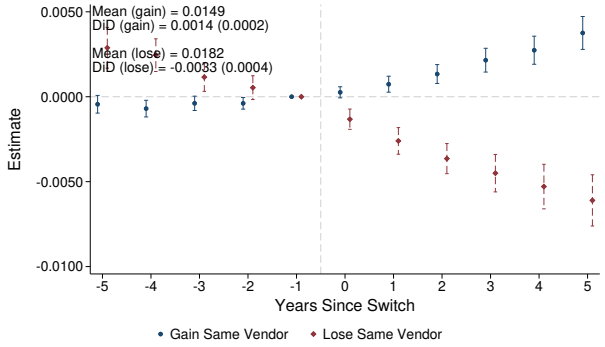
(a) # of Transfers



(b) Fraction of Transfers



(c) # of Referrals



(d) Fraction of Referrals

Notes: Figure plots event study estimates of the effect of both gaining (blue) and losing (red) the same EHR vendor on number of transfers (Panel A), fraction of transfers (Panel B), number of referrals (Panel C), and fraction of referrals (Panel D) between a hospital pair as described in Equation 4.1 including controls for hospital bed sizes and market by year fixed effects. The denominator for fraction of transfers (referrals) is the total number of transfers (referrals) sent from the sending hospital that year. Standard event study estimates are in blue (labeled "DiD"). Bars show 95% confidence intervals. The means of each dependent variable among treated observations in the pre-treatment period are given as are the static difference-in-differences estimates and corresponding standard errors.

Table D7: Allocative Effect of Losing Same Vendor on Shared Patient Flows - Robustness to Adding Hospital & Market Characteristics

(a) Transfers

	(1)	(2)	(3)	(4)	(5)
Panel A: # of Transfers					
Diff EHR Vendor	-0.0449	-0.0166	-0.268*	-0.268	-0.346
	(0.173)	(0.167)	(0.159)	(0.205)	(0.278)
Observations	185887	185887	185883	185883	78374
Mean of Outcome	3.32	3.32	3.32	3.32	3.32
Panel B: Fraction of Transfers					
Diff EHR Vendor	-0.00339***	-0.00326***	-0.00285**	-0.00285**	-0.00419
	(0.00110)	(0.00109)	(0.00120)	(0.00130)	(0.00272)
Observations	175708	175708	175636	175636	74024
Mean of Outcome	0.034	0.034	0.034	0.034	0.034
Sample	Event	Event	Event	Event	Event
Hospital Covariates		X	X	X	X
Market x Year FEs			X	X	X
Drop System Switchers					X
Level of Cluster	Pair	Pair	Pair	HRR	Pair

(b) Referrals

	(1)	(2)	(3)	(4)	(5)
Panel A: # of Referrals					
Diff EHR Vendor	-10.44***	-10.56***	-11.65***	-11.65***	-23.08***
	(2.829)	(2.909)	(2.799)	(3.899)	(6.874)
Observations	231852	231852	226395	226395	74938
Mean of Outcome	61.80	61.80	61.80	61.80	61.80
Panel B: Fraction of Referrals					
Diff EHR Vendor	-0.00333***	-0.00333***	-0.00328***	-0.00328***	-0.00418***
	(0.000374)	(0.000374)	(0.000384)	(0.000528)	(0.000818)
Observations	227704	227704	222390	222390	73747
Mean of Outcome	0.018	0.018	0.018	0.018	0.018
Sample	Event	Event	Event	Event	Event
Hospital Covariates		X	X	X	X
Market x Year FEs			X	X	X
Drop System Switchers					X
Level of Cluster	Pair	Pair	Pair	HRR	Pair

Notes: Each column in each panel in each table is a separate difference-in-differences regression of the effect of losing the same EHR vendor with hospital sender-receiver pair fixed effects and year fixed effects as described in Equation D.4. In Table (a), the outcome in Panel A is number of transfers while that in Panel B is fraction of transfers. In Table (b), the outcome in Panel A is number of referrals while that in Panel B is fraction of referrals. Observations are weighted by combined bed size in 2005. Column 1 has no time-varying controls. Column 2 adds hospital-level controls for bed sizes. Column 3 adds market by year fixed effects. Column 4 re-estimates Column 3 but clustering standard errors at the HRR-level. Column 5 drops observations in which at least one hospital in the pair switches hospital systems over the course of the sample period. Standard errors are clustered at the hospital sender-receiver pair for all columns (except Column 4) and are reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

D.2.5 Details of Placebo Test on Patient Flows

To check whether our main allocative patient flow estimates result from hospitals switching EHR vendors rather than switching to the same EHR vendor, we conduct placebo tests around EHR vendor changes for hospital pairs that never have the same vendor (i.e., we look at vendor changes when those changes do not result in the pair gaining or losing the same vendor). We restrict the sample to control hospital pairs from our main specification (pairs that never share the same vendor) where both sender and receiver switch EHR vendors at most once over our sample period. We discard pairs where sender and receiver switch vendors in different years to simplify identification to a single event for each pair. We then run the following difference-in-differences specification around whether either sender or receiver has yet switched vendors:

$$Y_{(hh')t} = \beta * \mathbf{1}_{\left[\text{Post-Switch}_{(hh')t}\right]} + \alpha_{(hh')} + \gamma_t + \delta X_{(hh')t,mt} + \epsilon_{(hh')t}. \tag{D.5}$$

Here, $\mathbf{1}_{\left[\text{Post-Switch}_{(hh')t}\right]}$ indicates whether either hospital has yet switched vendors (but not to the same vendor as the other hospital) by year t. We also run the event study version:

$$Y_{(hh')t} = \left(\sum_{r=-6}^{6} \beta_r \mathbf{1}_{\left[t=switch_{(hh')}+r\right]}\right) + \alpha_{(hh')} + \gamma_t + \delta X_{(hh')t,mt} + \epsilon_{(hh')t}, \tag{D.6}$$

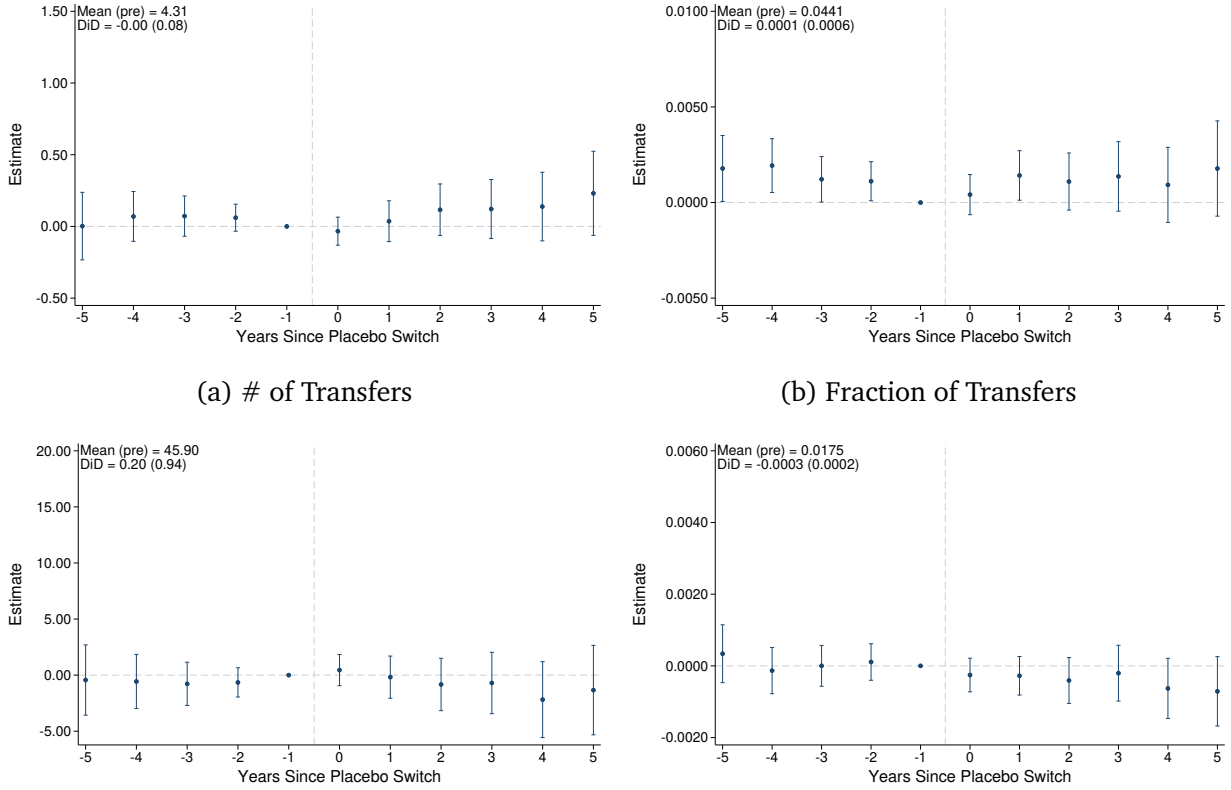
where r is the relative year since either hospital switched vendors (occurring in year $switch_{(hh')}$). Appendix Table D8 and Appendix Figure D9 show estimates from these specifications.

Table D8: Placebo Test of Vendor Switches on Shared Patient Flows

	(1) # of Transfers	(2) Fraction of Transfers	(3) # of Referrals	(4) Fraction of Referrals
Post-Placebo Switch	-0.000970	0.000118	-0.000970	0.000118
	(0.0760)	(0.000579)	(0.0760)	(0.000579)
Observations	418498	392291	418498	392291
Mean of Outcome	4.31	0.0441	45.90	0.0175

Notes: Each column is a separate difference-in-differences regression of the placebo effect of the sending or the receiving hospital switching EHR vendors but without the pair gaining the same vendor as a result of the switch, as described in Appendix Equation D.5. Outcomes are number of transfers (Column 1), fraction of transfers (Column 2), number of referrals (Column 3), and fraction of referrals (Column 4) between the hospital pair. The denominator for fraction of transfers (referrals) is the total number of transfers (referrals) sent from the sending hospital that year. Observations are weighted by combined bed size in 2005. Sample is limited to hospital pairs that never have the same vendor (i.e., the control hospital pairs in the main analysis). Standard errors are clustered at the hospital sender-receiver pair and are reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Figure D9: Event Study Placebo Test of Vendor Switches on Shared Patient Flows



(a) # of Transfers

(b) Fraction of Transfers

(c) # of Referrals

(d) Fraction of Referrals

Notes: Figure plots event study estimates of the placebo effect of the sending or the receiving hospital switching EHR vendors but without the pair gaining the same vendor from said switch, as described in Appendix Equation D.6. Outcomes are number of transfers (Column 1), fraction of transfers (Column 2), number of referrals (Column 3), and fraction of referrals (Column 4) between the hospital pair. The denominator for fraction of transfers (referrals) is the total number of transfers (referrals) sent from the sending hospital that year. Observations are weighted by combined bed size in 2005. Sample is limited to hospital pairs that never have the same vendor (i.e., the control hospital pairs in the main analysis). Standard event study estimates are in blue (labeled "DiD"). Bars show 95% confidence intervals. The mean of each dependent variable among treated observations in the pre-treatment period is given as is the static difference-in-differences estimate and corresponding standard error. * p<0.10, ** p<0.05, *** p<0.01.

D.2.6 Heterogeneity by Which Hospital Switches Vendors

A primary concern with our main results is potential endogeneity biasing the event study estimates. In particular, one hospital may explicitly switch to the same vendor as another because it wants to increase patient flows with that hospital. To mitigate this concern, we examine results separately by whether the sending or receiving hospital switches vendors. While the decision to send patients is almost entirely within a hospital's control, the decision to receive patients is not and relies more heavily on sending hospitals' decisions. Thus, it is plausible that the margin of receiving patients is less likely to be biased by the type of endogeneity described above than that of sending patients.

Appendix Table D9 tests whether treatment effects exist along both receiving and sending margins. The table displays estimates of Equation D.4 for the full sample of treated observations where only one hospital in the pair switches EHR vendors (Columns 1 and 4), the sample where pairs gain the same vendor due to the sending hospital switching (Columns 2 and 5), and the sample where pairs gain the same vendor due to the receiving hospital switching (Columns 3 and 6).[84] All columns examine patient flows from sender to receiver. Columns 2 and 5 show that when the sending hospital switches to have the same vendor as the receiving hospital, transfers from sender to receiver increase. Columns 3 and 6 show that when the receiving hospital switches, transfers from sender to receiver still increase, though typically by smaller magnitudes. Since the receiver switching is less endogenous than the sender switching (since the sender is not making the vendor choice), the significant positive effects on sender patient flow decisions suggest that the endogeneity described above does not entirely drive our main estimates (Columns 1 and 3).

[84] Of approximately 167,000 treated hospital pair observations (hospital pairs that start with different vendors and switch to the same vendor only once), 46% become same-vendor due to the sending hospital switching, 45% due to the receiving hospital switching, and 9% due to both hospitals switching. Estimates in Columns 1 and 4 differ slightly from those in Column 3 of Table D5 because we discard the 9% where both hospitals switch.

Table D9: Treatment Effect Heterogeneity by Which Hospital in the Pair Switches Vendors

(a) Transfers

	# of Transfers			Fraction of Transfers		
	(1) All	(2) Senders	(3) Receivers	(4) All	(5) Senders	(6) Receivers
Same EHR Vendor	0.311**	0.377***	0.250	0.00413***	0.00592***	0.00241**
	(0.147)	(0.144)	(0.254)	(0.000854)	(0.00135)	(0.00104)
Observations	941645	874216	873869	887041	822978	822579

(b) Referrals

	# of Referrals			Fraction of Referrals		
	(1) All	(2) Senders	(3) Receivers	(4) All	(5) Senders	(6) Receivers
Same EHR Vendor	5.106***	3.904***	6.485***	0.00141***	0.00232***	0.000644***
	(1.077)	(1.436)	(1.739)	(0.000192)	(0.000294)	(0.000239)
Observations	1505121	1328893	1320468	1486142	1310924	1303404

Notes: Each column in each table is a separate difference-in-differences regression of the effect of gaining the same EHR vendor with hospital sender-receiver pair fixed effects, year fixed effects, controls for both hospitals' current bed sizes, and market by year fixed effects, as described in Equation D.4. In Table (a), the outcome is transfers. In Table (b), the outcome is referrals. Observations are weighted by combined bed size in 2005. Columns 1 and 4 include all observations in the main event study sample minus those treated observations that had both sending and receiving hospitals switch vendors to become treated. Columns 2 and 5 restrict the treated observations to those where the sending hospital is the one who switched. Columns 3 and 6 restrict the treated observations to those where the receiving hospital is the one who switched. Standard errors are clustered at the hospital sender-receiver pair level and are reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

D.2.7 Role of Measurement Error

The exact year when hospitals switch EHR vendors is difficult to define. Our primary data source (AHA IT Survey) defines the switch year as when the new EHR system is used by the majority of a hospital's patients, while our secondary source (HIMSS) defines it as when the new system is live and operational for all patients. This section illustrates the potential for measurement error in which EHR vendor each hospital has each year to attenuate our patient flow results by instrumenting for whether a hospital pair has the same vendor in one survey (HIMSS) with whether the hospital pair has the same vendor in the other survey (AHA IT Survey).

Table D10 shows these results. The 2SLS estimates are, on average, twice the magnitude of OLS estimates, suggesting that measurement error significantly attenuates our estimates of interoperability effects on patient flows. Table D11 repeats this analysis but limits the sample to the event study hospital pair sample and creates a donut around the switch year in the endogenous dataset (i.e., discards relative years -1, 0, and 1 for switches). The donut significantly reduces the gap between 2SLS and OLS estimates, implying that measurement error stems from error in the exact year of EHR vendor switches rather than error in general vendor identity evenly distributed across relative time.

Table D10: Role of Measurement Error in Allocative Effect on Shared Patient Flows

(a) Transfers

	Fraction of Transfers					
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) 2SLS	(6) 2SLS
Same EHR Vendor - HIMSS	0.00182***		0.00145*		0.00598***	
	(0.000497)		(0.000833)		(0.00206)	
Same EHR Vendor - AHA IT		0.00291***		0.00238***		0.00365*
		(0.000745)		(0.000818)		(0.00209)
Observations	1057046	477713	375796	375796	375796	375796
Mean of Outcome	0.0511	0.0511	0.0511	0.0511	0.0511	0.0511
Restrict to Non-Miss			X	X	X	X
First Stage F-Stat					7138.2	6800.8

(b) Referrals

	Fraction of Referrals					
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) 2SLS	(6) 2SLS
Same EHR Vendor - HIMSS	0.000956***		0.000634***		0.00127***	
	(0.000135)		(0.000162)		(0.000358)	
Same EHR Vendor - AHA IT		0.000707***		0.000569***		0.00145***
		(0.000164)		(0.000160)		(0.000371)
Observations	1741987	924141	855012	855012	855012	855012
Mean of Outcome	0.0184	0.0184	0.0184	0.0184	0.0184	0.0184
Restrict to Non-Miss			X	X	X	X
First Stage F-Stat					32714.8	30462.7

Notes: Columns 1-4 are OLS difference-in-differences regression with hospital sender-receiver pair fixed effects, year fixed effects, market by year fixed effects, and controls for current bed sizes of both hospitals in the pair. Columns 1-2 estimate on the full sample; Columns 3-4 restrict the sample to hospital sender-receiver pair-year observations with non-missing same-vendor variables in both datasets (AHA IT and HIMSS). Columns 5-6 are the same but 2SLS, instrumenting for same-vendor status in one dataset with same-vendor status in another. Observations are weighted by combined bed size in 2005. Standard errors are clustered at hospital sender-receiver pair level and displayed in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table D11: Role of Measurement Error in Allocative Effect on Shared Patient Flows - Event Study Donut

(a) Transfers

	Fraction of Transfers					
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) 2SLS	(6) 2SLS
Same EHR Vendor - HIMSS	0.00539***		0.00405**		0.00586**	
	(0.00124)		(0.00183)		(0.00255)	
Same EHR Vendor - AHA IT		0.00745***		0.00538**		0.00591**
		(0.00217)		(0.00234)		(0.00268)
Observations	738458	332245	253890	253890	253890	253890
Mean of Outcome	0.0473	0.0473	0.0473	0.0473	0.0473	0.0473
Restrict to Non-Miss			X	X	X	X
First Stage F-Stat					19092.7	4308.4

(b) Referrals

	Fraction of Referrals					
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) 2SLS	(6) 2SLS
Same EHR Vendor - HIMSS	0.00180***		0.000970***		0.00153***	
	(0.000278)		(0.000332)		(0.000443)	
Same EHR Vendor - AHA IT		0.00181***		0.00141***		0.00132***
		(0.000422)		(0.000408)		(0.000453)
Observations	1238785	651155	598084	598084	598084	598084
Mean of Outcome	0.0163	0.0163	0.0163	0.0163	0.0163	0.0163
Restrict to Non-Miss			X	X	X	X
First Stage F-Stat					90616.5	22506.9

Notes: Columns 1-4 are OLS difference-in-differences regression with hospital sender-receiver pair fixed effects, year fixed effects, market by year fixed effects, and controls for current bed sizes of both hospitals in the pair. Columns 1-2 estimate on the full sample; Columns 3-4 restrict the sample to hospital sender-receiver pair-year observations with non-missing same-vendor variables in both datasets (AHA IT and HIMSS). Columns 5-6 are the same but 2SLS, instrumenting for same-vendor status in one dataset with same-vendor status in another. All columns restrict observations to those not in the event study donut hole, as described in the text. Observations are weighted by combined bed size in 2005. Standard errors are clustered at hospital sender-receiver pair level and displayed in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

D.2.8 Details of 2SLS Specification

To address concerns of endogeneity and measurement error, we construct an instrument for hospital EHR vendor choice via the following steps. For each focal hospital h_A, we:

(i) Identify all other hospitals belonging to the same hospital system as h_A but located outside of h_A's market (HRR_A). Label the set of these hospitals H_A. Note that by definition $h_A \notin H_A$.

(ii) Calculate market shares of each major EHR vendor e among $h \in H_A$ with respect to baseline hospital beds b. Denote these shares as the "System Pressure" for h_A to have vendor e:

$$\text{SP}(h_A, e) = \frac{\sum_{h \in \text{H}_A} b_h \times \mathbf{1}_{[e_h = e]}}{\sum_{h \in H_A} b_h}. \tag{D.7}$$

We perform this calculation for each focal hospital h_A in each year t. Appendix Figure D10 visualizes this calculation. Limiting these leave-out calculations to hospitals outside of each focal hospital's market eliminates endogeneity in vendor choice from local market pressure.

We instrument for the same-vendor status of hospital sender-receiver pair hh' in year t using four instruments: $\text{SP}(ht, e) \times \text{SP}(h't, e)$ for $e \in \{\text{Epic, Cerner, Meditech, Allscripts}\}$. That is, each instrument is the interaction of sender hospital h's System Pressure to have vendor e in year t and receiver hospital h''s System Pressure to have that same vendor e.[85] Table D12 shows summary statistics of these instruments and compares the sample for which instruments are defined to the full sample of hospital pair-year observations.[86]

Our first stage estimation equation is:

$$\begin{aligned}
\mathbf{1}_{[e_{ht} = e_{h't}]} = {} & \beta_1 \times \text{SP}(ht, e = \text{Epic}) \times \text{SP}(h't, e = \text{Epic}) \\
& + \beta_2 \times \text{SP}(ht, e = \text{Cerner}) \times \text{SP}(h't, e = \text{Cerner}) \\
& + \beta_3 \times \text{SP}(ht, e = \text{Meditech}) \times \text{SP}(h't, e = \text{Meditech}) \\
& + \beta_4 \times \text{SP}(ht, e = \text{Allscripts}) \times \text{SP}(h't, e = \text{Allscripts}) \\
& + \alpha_{(hh')} + \gamma_t + \delta X_{(hh')t, mt} + \epsilon_{(hh')t},
\end{aligned} \tag{D.8}$$

where $\mathbf{1}_{[e_{ht} = e_{h't}]}$ indicates whether the two hospitals have the same vendor in year t. This

[85] We use four instruments to allow for vendor heterogeneity. Results are similar when summing the four into one instrument.

[86] Over our sample period, 65.1% of hospital-year observations belong to multi-hospital systems, and 55.5% belong to systems spanning more than one HRR. As a result, 47.2% of hospital sender-receiver pair-year observations both belong to multi-hospital systems, and 32.6% both belong to systems spanning more than one HRR. This instrument requires limiting our sample to that 32.6%.

specification operates at the hospital pair-year level for our allocative analysis of patient flows (Section 4) and at the patient level for our direct analysis of patient outcomes (Section 3). To reduce potential reverse causality (e.g., hospital h using Epic pressures other hospitals in h's system outside of h's market to also use Epic), we limit the sample to hospital sender-receiver pairs where neither sender nor receiver is the largest hospital in their respective systems.

Our reduced form estimation equation is:

$$\begin{aligned} Y_{(hh')t} = \beta_1 &\times \text{SP}(ht, e = \text{Epic}) \times \text{SP}(h't, e = \text{Epic}) \\ &+ \beta_2 \times \text{SP}(ht, e = \text{Cerner}) \times \text{SP}(h't, e = \text{Cerner}) \\ &+ \beta_3 \times \text{SP}(ht, e = \text{Meditech}) \times \text{SP}(h't, e = \text{Meditech}) \\ &+ \beta_4 \times \text{SP}(ht, e = \text{Allscripts}) \times \text{SP}(h't, e = \text{Allscripts}) \\ &+ \alpha_{(hh')} + \gamma_t + \delta X_{(hh')t,mt} + \epsilon_{(hh')t}, \end{aligned} \tag{D.9}$$

where $Y_{(hh')t}$ denotes patient flows from hospital h to hospital h' in year t (following Equation D.4). For our allocative analysis on patient outcomes, we examine patient-level outcomes $Y_{i(hh')t}$ (following Equation 3.1).

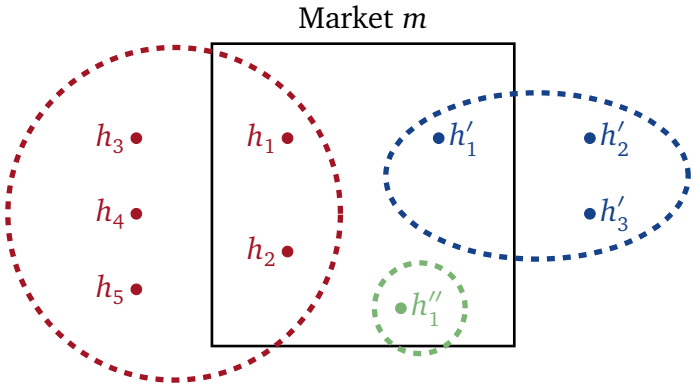
Table D13 displays 2SLS estimation results for fraction of transfers, with Column 1 showing first stage estimates, Column 2 reduced form estimates, Column 3 2SLS estimates using the four instruments, and Column 4 corresponding OLS estimates for the same sample. Similar results for fraction of referrals are in Appendix Table D14, while level results are in Appendix Tables D15 and D16. We find a large, positive, and statistically significant relationship between same-vendor status and shared patient flows when instrumenting with our system pressure instruments. 2SLS estimates are approximately 2.4 (3.7) times larger than OLS estimates for fraction of transfers (referrals). Similarly, Appendix Tables D17 and D18 show that 2SLS estimates are approximately 3.1 (6.1) times larger than OLS estimates for transfer patient charges (referral patient readmission rates).[87] These 2SLS estimates provide further evidence of a positive causal relationship between same-vendor status and shared patient flows and outcomes.

The validity of this instrumental variable strategy relies on relevance and exclusion assumptions. First stage estimates in Column 1 of Table D13 show that the four instruments are positively and significantly predictive of pair same-vendor status, with first stage F-statistics of almost 700. The magnitudes indicate that, for example, a 10 percentage point increase in the interacted System Pressure for the hospital pair to have Cerner leads to a 5.5 percentage point increase in the likelihood of that pair having the same vendor. For our exclusion restriction, we assume the four instruments only affect patient flows and outcomes through their effect on

[87] 2SLS estimates for shared patient images and tests are noisy.

whether the hospital pair has the same EHR vendor. This is plausible given the separation between the instruments and the markets where hospitals are located. Our primary endogeneity concern is that a focal hospital may switch its EHR vendor to increase shared patient flows or improve shared patient outcomes with specific other hospitals in its market. Our instruments are constructed based on information from other same-system hospitals outside the focal hospital's market. We do not believe this is likely correlated with any local market conditions for the focal hospital. One potential limitation is that while this instrumental variable strategy eliminates market-specific desires to share patients with certain hospitals, it will not necessarily eliminate a hospital's generic desire to share patients with, for example, Epic hospitals.

Figure D10: Illustration of System Pressure Instrument Calculation



Notes: Figure illustrates the calculation of the system pressure instrument. Hospitals are represented by dots. Hospitals in the same hospital system have the same color and are located in the same dotted circles. Hospitals inside of the solid box are located in the same market (HRR) m. Hospitals outside of the circle are located in different markets. In this example, system pressure for hospital h_1 to have vendor e in year t, $SP(ht, e)$, is calculated as the share of beds among hospitals $\{h_3, h_4, h_5\}$ that use vendor e in year t. System pressure for h'_1 is similarly based on the vendors used by $\{h'_2, h'_3\}$. Hospital h''_1 does not belong to a multi-hospital system containing hospitals outside of its own market and is therefore discarded from the instrumental variables sample.

Table D12: Hospital-Pair Summary Statistics - IV Sample Comparison

(a) Transfers

	Full Sample		IV Sample	
	Mean	SD	Mean	SD
# of Transfers	4.50	20.1	5.96	24.2
Fraction of Sender Transfers	0.052	0.15	0.062	0.17
Sender Total Transfers within HRR	145.7	201.3	153.9	197.9
Same EHR Vendor	0.21		0.27	
Instrument Same Epic			0.052	0.087
Instrument Same Cerner			0.037	0.054
Instrument Same Meditech			0.036	0.035
Instrument Same Allscripts			0.0043	0.015
N	1,377,473		443,153	

(b) Referrals

	Full Sample		IV Sample	
	Mean	SD	Mean	SD
# of Referrals	54.1	293.4	65.3	335.3
Fraction of Sender Referrals	0.019	0.074	0.021	0.081
Sender Total Referrals within Base Pairs	5094.6	5270.8	5292.0	5833.3
Same EHR Vendor	0.22		0.26	
Instrument Same Epic			0.057	0.094
Instrument Same Cerner			0.044	0.062
Instrument Same Meditech			0.037	0.037
Instrument Same Allscripts			0.0040	0.014
N	2,067,664		702,234	

Notes: Summary statistics for hospital sender-receiver-year observations, limiting the sample to sender-receiver pairs either within an HRR (Panel A; transfers) or that share any referrals in the baseline year (Panel B; referrals). Data for Panel A cover years 2005-2017 while data for Panel B cover years 2008-2017. Observations are weighted by combined bed size of the two hospitals in 2005. In each panel, observations in the first two columns are from the full sample, while observations in the last two columns are from the sample for which the instruments are defined. "Fraction of Sender Transfers (Referrals)" is the number of transfers (referrals) sent from h to h' divided by the total number of cross-hospital transfers (referrals) sent from h that year.

Table D13: Instrument for Same Vendor - Fraction of Transfers

	Same EHR Vendor	Fraction of Transfers		
	(1) FS	(2) RF	(3) 2SLS	(4) OLS
Instrument Same Epic	0.510***	0.000578		
	(0.0143)	(0.00173)		
Instrument Same Cerner	0.545***	0.00665***		
	(0.0169)	(0.00240)		
Instrument Same Meditech	0.435***	0.00326*		
	(0.0157)	(0.00174)		
Instrument Same Allscripts	0.149***	-0.0152**		
	(0.0230)	(0.00642)		
Same EHR Vendor			0.00547**	0.00228***
			(0.00238)	(0.000672)
Observations	218632	218632	218632	218632
Mean of Outcome	0.257	0.062		
First Stage F-Stat	683.8			

Notes: Each column is a separate difference-in-differences regression with hospital sender-receiver pair fixed effects, year fixed effects, market by year fixed effects, and controls for current bed sizes of both hospitals in the pair. Observations are weighted by combined bed size in 2005. Standard errors are clustered at hospital sender-receiver pair level and displayed in parentheses. Column 1 presents first stage results of the effect of the four instruments on whether the hospital pair has the same EHR vendor as given by Equation D.8. Column 2 presents reduced form results of the effect of the four instruments on fraction of transfers as given by Equation D.9. Column 3 presents 2SLS results of the effect of having the same vendor on fraction of transfers, instrumenting for same vendor status with the given four instruments. Column 4 presents the OLS equivalent of Column 3 as given by Equation D.4. * p<0.10, ** p<0.05, *** p<0.01.

Table D14: Instrument for Same Vendor - Fraction of Referrals

	Same EHR Vendor	Fraction of Referrals		
	(1) FS	(2) RF	(3) 2SLS	(4) OLS
Instrument Same Epic	0.592***	0.00294***		
	(0.0112)	(0.00102)		
Instrument Same Cerner	0.603***	0.00231**		
	(0.0132)	(0.000947)		
Instrument Same Meditech	0.390***	0.000524		
	(0.0134)	(0.000946)		
Instrument Same Allscripts	0.170***	-0.0149***		
	(0.0389)	(0.00387)		
Same EHR Vendor			0.00402***	0.00110***
			(0.00112)	(0.000345)
Observations	260033	260033	260033	260033
Mean of Outcome	0.243	0.021		
First Stage F-Stat	1348.4			

Notes: Each column is a separate difference-in-differences regression with hospital sender-receiver pair fixed effects, year fixed effects, market by year fixed effects, and controls for current bed sizes of both hospitals in the pair. Observations are weighted by combined bed size in 2005. Standard errors are clustered at hospital sender-receiver pair level and displayed in parentheses. Column 1 presents first stage results of the effect of the four instruments on whether the hospital pair has the same EHR vendor as given by Equation D.8. Column 2 presents reduced form results of the effect of the four instruments on fraction of referrals as given by Equation D.9. Column 3 presents 2SLS results of the effect of having the same vendor on fraction of referrals, instrumenting for same vendor status with the given four instruments. Column 4 presents the OLS equivalent of Column 3 as given by Equation D.4. * p<0.10, ** p<0.05, *** p<0.01.

Table D15: Instrument for Same Vendor - # of Transfers

	Same EHR Vendor	# of Transfers		
	(1) FS	(2) RF	(3) 2SLS	(4) OLS
Instrument Same Epic	0.509***	0.226		
	(0.0138)	(0.298)		
Instrument Same Cerner	0.535***	0.213		
	(0.0164)	(0.420)		
Instrument Same Meditech	0.433***	0.340		
	(0.0155)	(0.216)		
Instrument Same Allscripts	0.148***	-1.074**		
	(0.0228)	(0.508)		
Same EHR Vendor			0.492	0.277**
			(0.392)	(0.117)
Observations	224839	224839	224839	224839
Mean of Outcome	0.256	5.756		
First Stage F-Stat	701.7			

Notes: Each column is a separate difference-in-differences regression with hospital sender-receiver pair fixed effects, year fixed effects, market by year fixed effects, and controls for current bed sizes of both hospitals in the pair. Observations are weighted by combined bed size in 2005. Standard errors are clustered at hospital sender-receiver pair level and displayed in parentheses. Column 1 presents first stage results of the effect of the four instruments on whether the hospital pair has the same EHR vendor as given by Equation D.8. Column 2 presents reduced form results of the effect of the four instruments on number of transfers as given by Equation D.9. Column 3 presents 2SLS results of the effect of having the same vendor on number of transfers, instrumenting for same vendor status with the given four instruments. Column 4 presents the OLS equivalent of Column 3 as given by Equation D.4. * p<0.10, ** p<0.05, *** p<0.01.

Table D16: Instrument for Same Vendor - # of Referrals

	Same EHR Vendor	# of Referrals		
	(1) FS	(2) RF	(3) 2SLS	(4) OLS
Instrument Same Epic	0.592***	23.75*		
	(0.0111)	(14.02)		
Instrument Same Cerner	0.604***	-5.488		
	(0.0131)	(5.075)		
Instrument Same Meditech	0.389***	-1.820		
	(0.0133)	(3.884)		
Instrument Same Allscripts	0.169***	-101.0***		
	(0.0389)	(26.11)		
Same EHR Vendor			16.11	2.741*
			(12.71)	(1.654)
Observations	261653	261653	261653	261653
Mean of Outcome	0.243	65.278		
First Stage F-Stat	1360.6			

Notes: Each column is a separate difference-in-differences regression with hospital sender-receiver pair fixed effects, year fixed effects, market by year fixed effects, and controls for current bed sizes of both hospitals in the pair. Observations are weighted by combined bed size in 2005. Standard errors are clustered at hospital sender-receiver pair level and displayed in parentheses. Column 1 presents first stage results of the effect of the four instruments on whether the hospital pair has the same EHR vendor as given by Equation D.8. Column 2 presents reduced form results of the effect of the four instruments on number of referrals as given by Equation D.9. Column 3 presents 2SLS results of the effect of having the same vendor on number of referrals, instrumenting for same vendor status with the given four instruments. Column 4 presents the OLS equivalent of Column 3 as given by Equation D.4. * p<0.10, ** p<0.05, *** p<0.01.

Table D17: Instrument for Same Vendor - Transfer Patient Charges

	Same EHR Vendor	Charges		
	(1) FS	(2) RF	(3) 2SLS	(4) OLS
Instrument Same Epic	0.797***	-22274.0***		
	(0.0595)	(3627.0)		
Instrument Same Cerner	0.626***	-18410.8***		
	(0.0989)	(3776.4)		
Instrument Same Meditech	0.299***	1772.5		
	(0.0772)	(4368.9)		
Instrument Same Allscripts	0.237	11478.8		
	(0.160)	(15066.2)		
Same EHR Vendor			-27660.3***	-8883.4***
			(4164.1)	(1910.7)
Observations	348939	348939	348939	348939
Mean of Outcome	0.113	9.1e+04		
First Stage F-Stat	66.1			

Notes: Each column is a separate difference-in-differences regression with hospital sender-receiver pair fixed effects and year fixed effects as well as fixed effects for patient diagnostic code categories, indicators for 27 patient baseline chronic conditions, and controls for patient Charlson Comorbidity Index, patient age, patient sex, and patient race. Standard errors are clustered at hospital sender-receiver pair level and displayed in parentheses. Column 1 presents first stage results of the effect of the four instruments on whether the hospital pair has the same EHR vendor as given by Equation D.8 (but at the patient level). Column 2 presents reduced form results of the effect of the four instruments on shared transfer patient charges at the receiver hospital as given by Equation D.9 (but at the patient level). Column 3 presents 2SLS results of the effect of having the same vendor on transfer patient charges at the receiver hospital, instrumenting for same vendor status with the given four instruments. Column 4 presents the OLS equivalent of Column 3 as given by Equation 3.1. * p<0.10, ** p<0.05, *** p<0.01.

Table D18: Instrument for Same Vendor - Referral Patient 60-Day Readmission Rates

	Same EHR Vendor	60-Day Readmit		
	(1) FS	(2) RF	(3) 2SLS	(4) OLS
Instrument Same Epic	0.790***	-0.00986**		
	(0.0397)	(0.00404)		
Instrument Same Cerner	0.698***	-0.0114*		
	(0.0748)	(0.00589)		
Instrument Same Meditech	0.216***	-0.00921		
	(0.0644)	(0.0101)		
Instrument Same Allscripts	-0.0493	-0.0117		
	(0.0588)	(0.00741)		
Same EHR Vendor			-0.0129***	0.00210
			(0.00471)	(0.00297)
Observations	3033347	3033347	3033347	3033347
Mean of Outcome	0.165	0.071		
First Stage F-Stat	148.2			

Notes: Each column is a separate difference-in-differences regression with hospital sender-receiver pair fixed effects and year fixed effects as well as fixed effects for patient diagnostic code categories, indicators for 27 patient baseline chronic conditions, and controls for patient Charlson Comorbidity Index, patient age, patient sex, and patient race. Standard errors are clustered at hospital sender-receiver pair level and displayed in parentheses. Column 1 presents first stage results of the effect of the four instruments on whether the hospital pair has the same EHR vendor as given by Equation D.8 (but at the patient level). Column 2 presents reduced form results of the effect of the four instruments on shared transfer patient charges at the receiver hospital as given by Equation D.9 (but at the patient level). Column 3 presents 2SLS results of the effect of having the same vendor on transfer patient charges at the receiver hospital, instrumenting for same vendor status with the given four instruments. Column 4 presents the OLS equivalent of Column 3 as given by Equation 3.1. * p<0.10, ** p<0.05, *** p<0.01.

D.2.9 Evidence of Spillovers in Allocative Effect on Shared Patient Flows

Our empirical analysis of the effect of a hospital pair gaining the same EHR vendor on shared patient flows relies on the Stable Unit Treatment Value Assumption (SUTVA) for causal interpretation. However, spillovers are possible due to the network nature of the hospital industry and patient flows between providers. If hospital h switches to the same EHR vendor as hospital h', that change could affect both the number of patients h' transfers to h as well as the number of patients h' transfers to h''. The former (h' to h) would be classified as a treatment effect since that hospital pair experiences a positive change in same-vendor status and is thus a treated unit, but the latter (h' to h'') is a control unit and any change in its outcomes due to the treatment units is the definition of a spillover. More simply, the EHR vendor decision of one hospital may affect the patient flow decisions of many other hospitals and thus bias our difference-in-difference estimates.[88]

Appendix Table D19 shows evidence that spillovers exist in this setting. The table displays difference-in-differences estimates at the hospital sender-receiver-year level where outcomes are patient flows between the hospital pair and the main independent variables are (1) the leave-out market share of the vendor used by the sending hospital in that year and (2) the leave-out market share of the vendor used by the receiving hospital in that year. Market shares are calculated using hospital beds, our market definition from Equation D.4 (HRR for transfers; hospitals with baseline patient flows for referrals), and excluding the focal hospital. As in Equation D.4, hospital sender-receiver pair and year fixed effects are included, with standard errors clustered at the sender-receiver level. Crucially, we limit the sample to hospital pairs that never have the same EHR vendor over the sample period (i.e., the "never treated"). These are the control observations in our main empirical analyses.

Results indicate that the market shares of both sending and receiving hospitals' EHR vendors affect patient flows between hospital pairs. While the hospital pair never shares the same vendor, the decisions of other hospitals in their markets (which determine market shares) affect their patient flows. For example, Column 1 of Panel B shows that when the sending hospital's vendor market share increases by 10 percentage points, the fraction of referrals that the sending hospital sends to a receiving hospital with which it never shares the same vendor decreases by 0.02 percentage points (1.4% of the mean). In contrast, when the receiving hospital's vendor market share increases by 10 percentage points, the fraction of referrals increases by 0.03

[88] Ideally, to eliminate intra-market spillovers, we would match treated hospital sender-receiver pairs to control pairs in markets with no treated observations so that our estimates are not biased by potential spillovers. Unfortunately, this is not possible in this setting. Only one HRR (out of 306) contains no switching hospitals over the sample period, and that single market is likely not representative. We thus cannot match treated observations to "pure" controls.

percentage points (2% of the mean).[89] This result is consistent across multiple robustness checks. Effects are more precisely estimated for referrals than for transfers, which are omitted. The magnitudes of these spillover estimates are smaller than our estimates for the effect of gaining the same EHR vendor, suggesting that while spillovers may exist, they may not be the primary drivers of our main estimates.

While these results indicate the presence of spillovers, they do not indicate the overall direction of their bias. Estimates for sending and receiving hospital market shares have opposite signs (at least for the fraction of referrals specification). Further, while some sending and receiving hospitals experience increases in their vendor market shares over time, others experience decreases, so not all control units are affected in the same direction. A simple model can illustrate how both positive and negative spillovers are possible, depending on assumptions regarding treatment effects on treated and control pairs. Consider a market with four hospitals: A, B, C, and D, where hospital A starts with Cerner in Period 1 and switches to Epic in Period 2, while hospital B always has Epic, hospital C always has Cerner, and hospital D always has Allscripts. Our event study sample for this market consists of treated pair AB (since A and B gain the same vendor over time) and control pairs AD, BC, BD, and CD; pair AC is discarded because the two hospitals lose the same vendor over time. Assume the number of patients sent from each hospital remains constant and that A—after switching EHR vendors—reallocates some patients to B that previously went to C. If B also sends more patients to A, then mechanically the number of patients that B sends to C and D must decrease, creating a negative spillover. However, if B does not change its sending patterns, but C also sends fewer patients to A, then mechanically the number of patients that C sends to B and D must increase, creating a positive spillover. Alternatively, if both B and C react as described but in equal magnitude, then the total spillover to control pairs would be zero. The direction of bias is non-obvious.

[89] Sender's vendor market share has a mean of 0.177 and standard deviation of 0.115, so a 10 percentage point increase is approximately a 1 standard deviation increase. Receiver's vendor market share has a slightly higher mean (0.197) and standard deviation (0.148).

Table D19: Evidence of Spillovers in Allocative Effect on Shared Patient Flows

	(1)	(2)	(3)	(4)	(5)
Panel A: # of Referrals					
Sender's Vendor Market Share	2.592	3.147	4.614*	4.614	0.0949
	(2.652)	(2.572)	(2.678)	(3.981)	(4.905)
Receiver's Vendor Market Share	15.68***	15.20***	8.611***	8.611**	13.83***
	(2.216)	(2.097)	(1.966)	(3.436)	(4.103)
Observations	1131337	1131337	1109715	1109715	343094
Mean of Outcome	42.00	42.00	42.00	42.00	42.00
Panel B: Fraction of Referrals					
Sender's Vendor Market Share	-0.00223***	-0.00214***	-0.00135**	-0.00135*	-0.00182
	(0.000543)	(0.000542)	(0.000565)	(0.000716)	(0.00119)
Receiver's Vendor Market Share	0.00323***	0.00299***	0.00316***	0.00316***	0.00598***
	(0.000547)	(0.000544)	(0.000589)	(0.000999)	(0.00131)
Observations	1115280	1115280	1093982	1093982	337726
Mean of Outcome	0.016	0.016	0.016	0.016	0.016
Sample	Event	Event	Event	Event	Event
Hospital Covariates		X	X	X	X
Market x Year FEs			X	X	X
Drop System Switchers					X
Level of Cluster	Pair	Pair	Pair	HRR	Pair

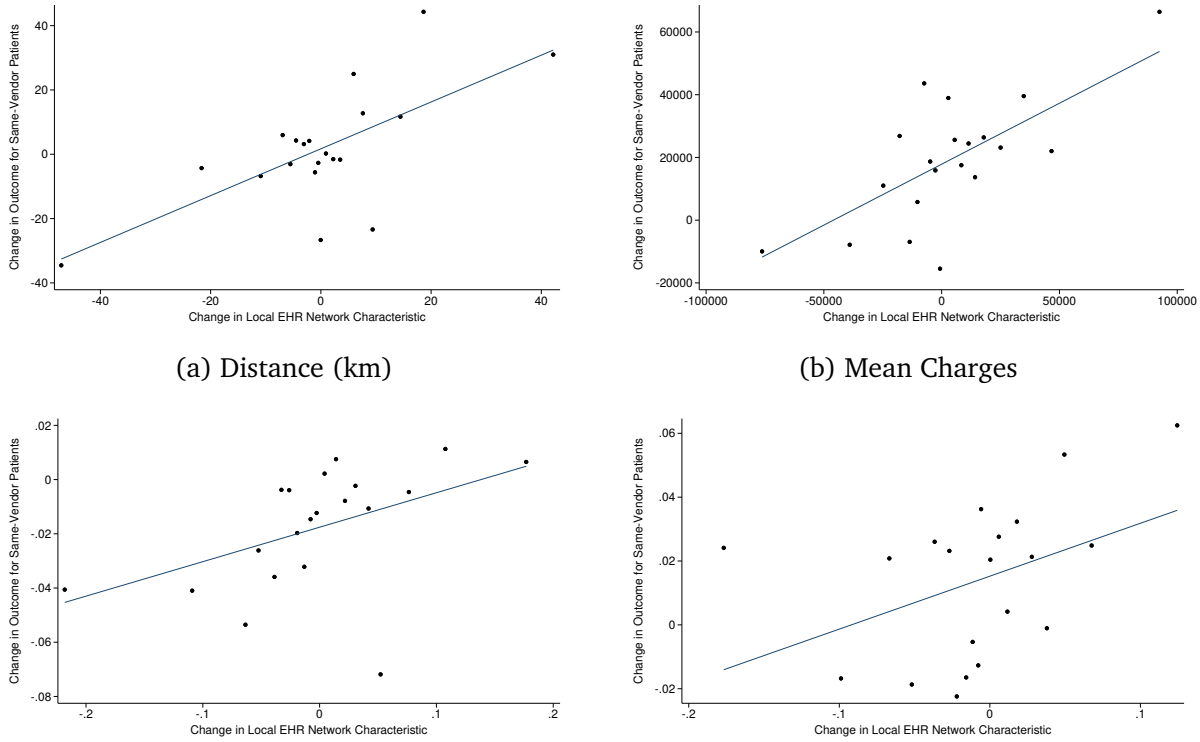
Notes: Each column is a separate regression with hospital sender-receiver pair fixed effects, year fixed effects, and standard errors clustered at the specified level (hospital sender-receiver pair for all Columns except 4, HRR for Column 4). The outcome in Panel A is number of referrals while that in Panel B is fraction of referrals. Observations are weighted by combined bed size in 2005. Column 2 adds controls for both hospitals' current bed sizes. Column 3 adds market by year fixed effects. Column 5 drops observations in which at least one hospital in the pair switches hospital systems over the course of the sample period. Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

D.3 Additional Evidence of Allocative Effect of Interoperability on Patient Outcomes (Section 5)

D.3.1 Correlation between ΔY_h and $\Delta \text{Network}_{Yh}$

Appendix Figure D11 shows the correlation between local EHR vendor network changes ($\Delta \text{Network}_{Yh}$) and patient outcome changes (ΔY_h) across EHR vendor switching hospitals for four outcomes: distance to receiver hospitals, patient charges, 30-day readmission rates, and 60-day mortality rates. The linear correlations between these measures motivate our linear functional form for the interaction term in Equation 5.1.

Figure D11: Changes in Patient Outcomes vs. Changes in Local EHR Network for Transfers



(a) Distance (km)

(b) Mean Charges

(c) Readmit 30 Days

(d) Death 60 Days

Notes: Figures plot the correlation between change in experienced patient outcomes for same-vendor-sent patients sent from a switching hospital before versus after that hospital switches vendors (y-axis; ΔY_h) and changes in baseline characteristics of a switching hospital's local EHR network from before versus after the hospital switches vendors (x-axis; $\Delta \text{Network}_{Yh}$) using binscatter. Hospital observations are weighted by baseline bed size.

D.3.2 Average Effect of Vendor Switch on Same-Vendor Patient Outcomes (Non-Interacted Version of Table 5)

Appendix Table D20 shows results from estimating Equation 5.1 without the interaction term. The large standard errors highlight the wide distributions of outcome changes (ΔY_h).

Table D20: Average Same-Vendor Patient Outcomes after Vendor Switch

(a) Transfers

	Outcomes for Same-Vendor Transfer Patients			
	(1) Distance (km)	(2) Mean Charges	(3) 30 Day Readmit	(4) 60 Day Death
Post-EHR Switch	6.589***	2723.5	0.00823*	0.00512
	(1.596)	(4592.2)	(0.00478)	(0.00467)
Observations	714532	781421	644864	781421
Mean of Outcome	35.94	78190.73	0.30	0.14

(b) Referrals

	Outcomes for Same-Vendor Referral Patients			
	(1) Distance (km)	(2) Mean Charges	(3) 60 Day Readmit	(4) 60 Day Death
Post-EHR Switch	-0.173	-0.185	0.00576***	-0.00415**
	(0.913)	(1.290)	(0.00212)	(0.00200)
Observations	13842146	15186471	12621995	15186471
Mean of Outcome	22.82	80.27	0.05	0.09

Notes: Each column is a separate patient-level difference-in-differences regression of the effect of a hospital switching EHR vendors on outcomes experienced by transfer (Panel A) and referral (Panel B) patients sent from the switching hospital to another same-vendor recipient hospital as specified in Equation 5.1 but without the interaction term. All regressions include hospital fixed effects, year fixed effects, and market-year fixed effects, and hospitals are weighted by baseline beds. Standard errors are clustered at the hospital level and displayed in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

D.3.3 Predictive Validity Robustness to Changes in Patient Composition

Appendix Table D21 shows that the predictive validity results in Table 5 are robust to controlling for patient characteristics, including age, chronic conditions, and Charlson Comorbidity Index.

Table D21: Predictive Validity of Changes in Local EHR Network Characteristics - Robustness to Adding Patient Characteristics

(a) Transfers

	Outcomes for Same-Vendor Transfer Patients			
	(1) Distance (km)	(2) Mean Charges	(3) 30 Day Readmit	(4) 60 Day Death
Post-EHR Switch	4.359***	409.8	0.00344	0.00231
	(1.647)	(5833.0)	(0.00574)	(0.00547)
x Change in Network	0.255**	0.421***	0.122**	0.218***
	(0.106)	(0.0973)	(0.0571)	(0.0572)
Observations	559433	607807	501664	607807
Mean of Outcome	35.94	78190.73	0.30	0.14

(b) Referrals

	Outcomes for Same-Vendor Referral Patients			
	(1) Distance (km)	(2) Mean Charges	(3) 60 Day Readmit	(4) 60 Day Death
Post-EHR Switch	-0.442	0.573	0.00606***	-0.00291
	(0.987)	(1.326)	(0.00220)	(0.00205)
x Change in Network	0.313***	0.284***	0.0878*	0.141***
	(0.0518)	(0.0707)	(0.0497)	(0.0532)
Observations	12739106	13993531	11631497	13993531
Mean of Outcome	22.82	80.27	0.05	0.09

Notes: Each column is a separate patient-level difference-in-differences regression of the effect of a hospital switching EHR vendors on outcomes experienced by transfer (Panel A) and referral (Panel B) patients sent from the switching hospital to another same-vendor recipient hospital as specified in Equation 5.1. All regressions include hospital fixed effects, year fixed effects, and market-year fixed effects. All regressions also include patient-level baseline controls such as age, Charlson Comorbidity Index, and indicators for 27 chronic conditions. The indicator for post-switch is interacted with the raw change in the average baseline characteristics of other hospitals with the same EHR vendor in the same local market as the switching hospital. Standard errors are clustered at the hospital level and displayed in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

D.3.4 Patient Balance Across Deciles of ΔNetwork$_{Yh}$

Appendix Table D22 shows that patients in the top decile group (patients sent from hospitals switching to local vendor networks with much *higher* baseline readmission and mortality rates) and bottom decile group (patients sent from hospitals switching to local vendor networks with much *lower* baseline readmission and mortality rates) used for the event studies in Figure 7 are balanced on baseline characteristics.

Table D22: Balance Tests for Patients in Top, Bottom Deciles of Change in Local EHR Network

(a) Transfers, 30-Day Readmit

	Characteristics for Transfer Patients					
	(1) Age	(2) # Chronic Conditions	(3) Any Cancers	(4) Any Heart Conditions	(5) Charlson Score	(6) Aftercare Transfer
Top Decile	-0.395	-0.0110	0.00135	-0.00499	-0.0265	0.0422
	(0.441)	(0.109)	(0.00267)	(0.0168)	(0.0846)	(0.0922)
Observations	394488	386236	386236	386236	394488	394488
Mean of Outcome	72.3	4.780	0.095	0.505	4.868	0.230

(b) Transfers, 60-Day Death

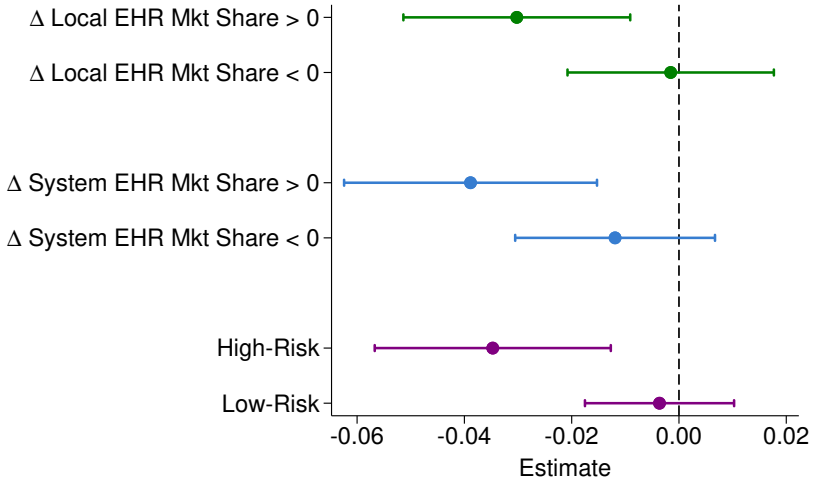
	Characteristics for Transfer Patients					
	(1) Age	(2) # Chronic Conditions	(3) Any Cancers	(4) Any Heart Conditions	(5) Charlson Score	(6) Aftercare Transfer
Top Decile	-0.625	0.191	-0.00577	0.0216**	0.106	-0.0333
	(0.479)	(0.125)	(0.00391)	(0.0109)	(0.107)	(0.0457)
Observations	391856	383654	383654	383654	391856	391856
Mean of Outcome	72.3	4.780	0.095	0.505	4.868	0.230

Notes: Each column is a separate patient-level regression of patient characteristics on whether the transfer patient is in the top decile (compared to being in the bottom decile) of change in local EHR vendor network for 30-day hospital readmission rates for transfer patients (Panel A) or 60-day death rates for transfer patients (Panel B). No controls or fixed effects are included. Standard errors are clustered at the sending hospital level and displayed in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

D.3.5 Predictive Validity Heterogeneity

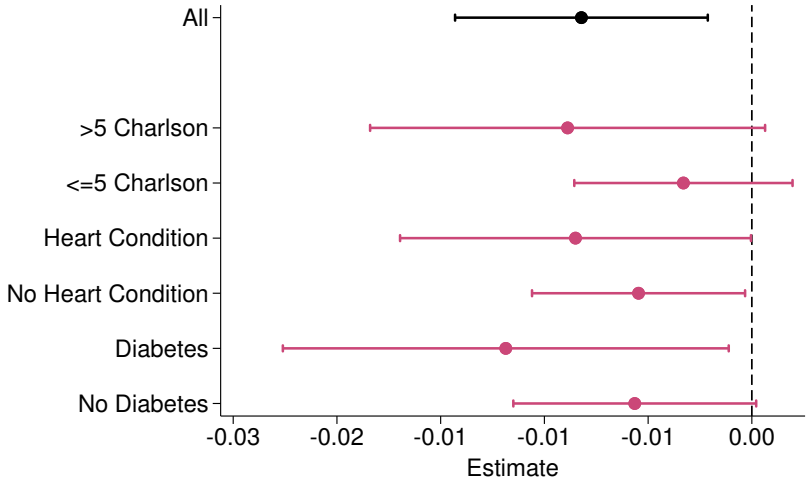
Appendix Figures D12 and D13 show heterogeneity in the predictive validity of local EHR vendor networks on experienced same-vendor transfer patient mortality and referral patient mortality, respectively. We restrict the sample to hospitals that either never switch EHR vendors or switch to a better local EHR vendor network, defined as a network of local same-vendor hospitals with lower baseline 60-day mortality rates for received patients. Appendix Figure D12 shows that benefits are larger when the vendor is more prevalent in the local market or health system, suggesting that both network quality and breadth matter for outcomes. Both figures show that higher-risk patients experience particularly large gains from these network improvements.

Figure D12: Predictive Validity Heterogeneity: Transfer Patient Mortality



Notes: Figure plots difference-in-differences estimates (dots) and 95% confidence intervals (bars) from a modified version of Equation 5.1, omitting the interaction term. The sample is restricted to patients sent from hospitals that either (1) never switch EHR vendors (controls), or (2) switch once to a "better" local EHR vendor network, defined as a network of local same-vendor hospitals with lower baseline 60-day mortality rates for received transfer patients. The estimates show the effect of this switch on same-vendor transfer patients' 60-day mortality rates, grouped by hospital or network characteristics. "Δ Local EHR Mkt Share > 0" denotes sending hospitals switching to vendors with greater local market presence. "Δ System EHR Mkt Share > 0" indicates switches to vendors more prevalent within the switching hospital's system. Patient risk is measured as predicted mortality using baseline measures of patient risk such as age, sex, race, and chronic conditions. "High-risk" patients are transfer patients with the top half of such predicted mortality rates, while "low-risk" patients are those with the bottom half.

Figure D13: Predictive Validity Heterogeneity: Referral Patient Mortality



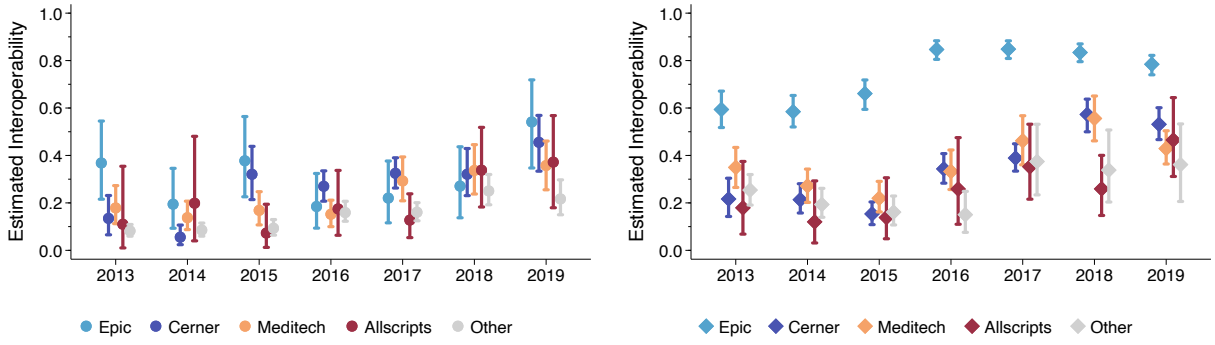
Notes: Figure plots difference-in-differences estimates (dots) and 95% confidence intervals (bars) from a modified version of Equation 5.1, omitting the interaction term. The sample is restricted to same-vendor referral patients from hospitals that either (1) never switch EHR vendors (controls), or (2) switch once to a "better" local EHR vendor network. A "better" network comprises local same-vendor hospitals with lower baseline 60-day mortality rates for received referral patients. The estimates show the effect of switching on 60-day mortality rates for same-vendor referral patients, grouped by patient characteristics. "Charlson" stands for the Charlson Comorbidity Index.

D.4 Additional Details of Model of Patient Flows (Section 6)

D.4.1 Additional Visualizations of Interoperability Estimates

Appendix Figure D14 presents estimates of across-vendor (Panel A) and within-vendor (Panel B) interoperability by vendor and year (rather than time periods). Appendix Figure D15 plots average levels of across-vendor and within-vendor interoperability by vendor, pooled across all years. Appendix Figure D16 plots estimates of across-vendor and within-vendor interoperability by time period, pooled across all vendors.

Figure D14: Estimated Across- and Within-Vendor Interoperability Levels - Each Year

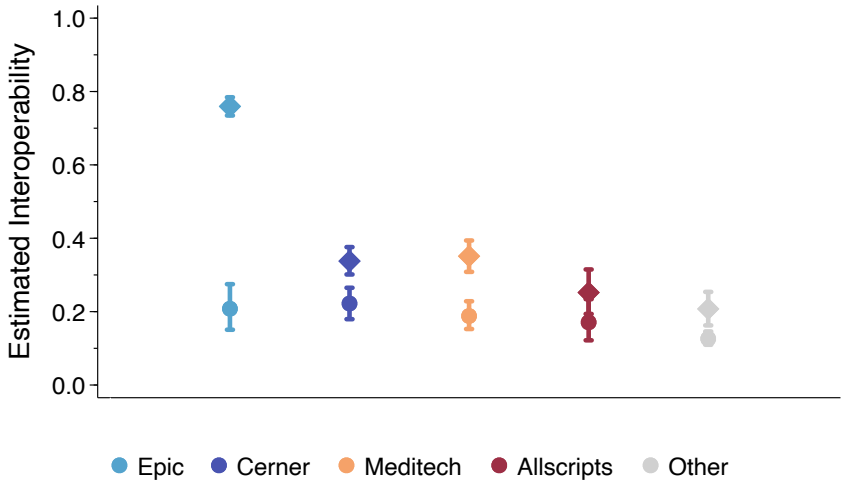


(a) Across-Vendor

(b) Within-Vendor

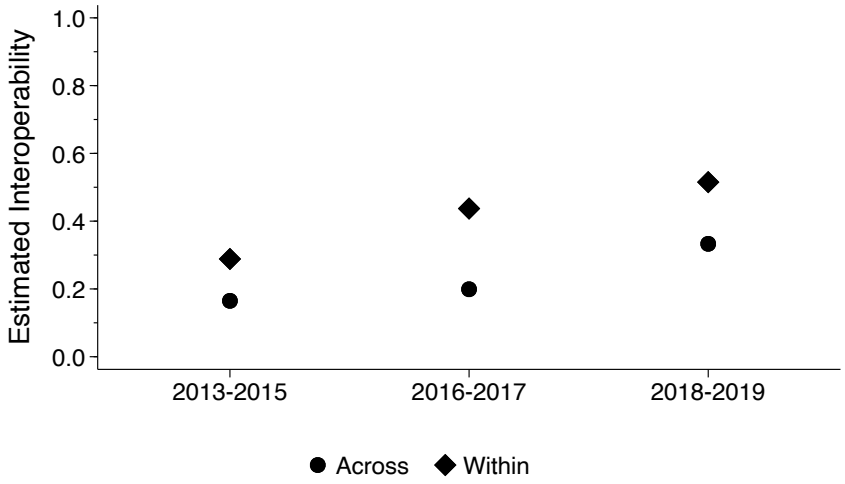
Notes: Figures plot ordered probit estimates of across-vendor (Panel A) and within-vendor (Panel B) interoperability, normalized to be between 0 and 1, and separately by EHR vendor and year. Dots and diamonds represent point estimates while bars show 95% confidence intervals, which are calculated using 1000 bootstrapped samples.

Figure D15: Estimated Across- and Within-Vendor Interoperability Levels - Average by Vendor



Notes: Figure plots ordered probit estimates of across-vendor (dots) and within-vendor (diamonds) interoperability, normalized to be between 0 and 1, and separately by EHR vendor. Dots and diamonds represent point estimates while bars show 95% confidence intervals, which are calculated using 1000 bootstrapped samples. Data is pooled across all years.

Figure D16: Estimated Across- and Within-Vendor Interoperability Levels - Average by Year



Notes: Figure plots ordered probit estimates of across-vendor (dots) and within-vendor (diamonds) interoperability, normalized to be between 0 and 1, and separately by time period. Dots and diamonds represent point estimates while bars show 95% confidence intervals, which are calculated using 1000 bootstrapped samples. Data is pooled across all vendors and for time periods 2013-2015, 2016-2017, and 2018-2019.

D.4.2 Using Interoperability Estimates in Analysis of Allocative Effect of Interoperability on Patient Flows

Same-vendor status serves as a proxy for interoperability. This proxy is useful for estimating event studies centered on a binary independent variable and for extending the analysis period to years before 2013, the first year in which interoperability can be measured directly. However, it relies on the simplifying assumption that the interoperability advantage of sharing a vendor is constant across all EHR vendors—a notion Figure 6 shows is inaccurate due to differences in within-vendor interoperability. To incorporate variation across vendors and over time, Table D23 reports static difference-in-differences estimates (as in Equation D.4, but restricted to 2013–2019) for three independent variables: the same-vendor proxy, a time-invariant measure of interoperability between the vendors used by the hospital pair, and a time-varying measure of interoperability. We present these estimates for two hospital-pair samples: the event study sample used throughout the paper and the full sample. Across nearly all outcomes and samples, estimates based on time-invariant measured interoperability are more precise than those using the proxy and allowing for time variation further improves precision. On average, the estimates are twice as large when based on measured interoperability rather than the proxy, consistent with interoperability improving by about 0.5 (on a 0–1 scale) when moving from across-vendor to within-vendor. Taken together, these results provide additional evidence that the proxy-based findings capture the impact of interoperability on patient flows and outcomes.

Table D23: Allocative Effect of Gaining Same Vendor on Shared Patient Flows - Same-Vendor Proxy vs. Interoperability Levels

(a) Transfers

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: # of Transfers						
Same EHR Vendor	0.182**			0.0531		
	(0.0826)			(0.0736)		
Time-Invariant Interoperability		0.475***			0.502***	
		(0.170)			(0.141)	
Time-Varying Interoperability			0.360***			0.561***
			(0.123)			(0.108)
Observations	550991	550991	550991	737113	737113	737113
Mean of Outcome	3.76	3.76	3.76	4.30	4.30	4.30
Panel B: Fraction of Transfers						
Same EHR Vendor	0.00142*			0.000895		
	(0.000802)			(0.000684)		
Time-Invariant Interoperability		0.00354**			0.00330***	
		(0.00167)			(0.00125)	
Time-Varying Interoperability			0.00249**			0.00348***
			(0.00121)			(0.000976)
Observations	517068	517068	517068	693838	693838	693838
Mean of Outcome	0.0477	0.0477	0.0477	0.0512	0.0512	0.0512
Sample	Event	Event	Event	Full	Full	Full

(b) Referrals

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: # of Referrals						
Same EHR Vendor	1.613**			1.481*		
	(0.812)			(0.811)		
Time-Invariant Interoperability		3.209**			4.095***	
		(1.603)			(1.476)	
Time-Varying Interoperability			2.104*			3.217***
			(1.269)			(1.228)
Observations	856812	856812	856812	1133829	1133829	1133829
Mean of Outcome	43.32	43.32	43.32	52.27	52.27	52.27
Panel B: Fraction of Referrals						
Same EHR Vendor	0.000432**			0.000220		
	(0.000193)			(0.000181)		
Time-Invariant Interoperability		0.000838**			0.000988***	
		(0.000373)			(0.000314)	
Time-Varying Interoperability			0.00100***			0.00132***
			(0.000303)			(0.000264)
Observations	841573	841573	841573	1113803	1113803	1113803
Mean of Outcome	0.0171	0.0171	0.0171	0.0192	0.0192	0.0192
Sample	Event	Event	Event	Full	Full	Full

Notes: Each column in each panel in each table is a separate difference-in-differences regression of the effect of interoperability (exact variable specified in the table) on shared patient flows with hospital sender-receiver pair fixed effects, year fixed effects, and market by year fixed effects as described in Equation D.4. In Table (a), the outcome in Panel A is number of transfers while that in Panel B is fraction of transfers. In Table (b), the outcome in Panel A is number of referrals while that in Panel B is fraction of referrals. Observations are weighted by combined bed size in 2005. The sample period is limited to years 2013-2019. Standard errors are clustered at the hospital sender-receiver pair and reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

D.4.3 Decomposition of Demand Model Receiver Hospital Fixed Effects

Appendix Table D24 shows that the receiver hospital fixed effects from the demand model in Table 6 are positively correlated with multiple measures of hospital quality.

Table D24: Decomposing Receiver Hospital Fixed Effects

	(1) Receiver FEs
Beds (SD)	0.0980***
	(0.00115)
30 Day Death (SD)	-0.0479***
	(0.000822)
30 Day Readmit (SD)	-0.0773***
	(0.000898)
Charges (SD)	0.0442***
	(0.00103)
In Any System	0.0463***
	(0.00223)
AMC	-0.0131***
	(0.00206)
Trauma Center	0.0377***
	(0.00217)
Burn Unit	0.0435***
	(0.00317)
Cardiac Intensive Care Unit	0.120***
	(0.00235)
Adult Diagnostic Cath Unit	0.0500***
	(0.00295)
Oncology Unit	0.0620***
	(0.00319)
SPECT Capable	0.0339***
	(0.00237)
PET Capable	0.0841***
	(0.00214)
CT Capable	-0.220***
	(0.00376)
Mammogram Capable	-0.156***
	(0.00346)
Observations	126294

Notes: Table shows results of a regression of receiver hospital fixed effects (recovered from estimation of Equation 6.1) on various receiver hospital characteristics. "(SD)" indicates that the characteristic has been standardized to have a mean of 0 and a standard deviation of 1. Standard errors are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

D.4.4 Hospital Demand Model Estimation Results for Referrals

Appendix Table D25 reports estimates from the hospital demand model for referrals, where the referral market is defined as the set of receiving hospitals with which the sender has a baseline referral relationship. The estimated coefficient on interoperability is smaller than in the transfers sample (Table 6).

Table D25: Model Parameter Estimates for Referrals

	Baseline	**Pair FEs**
θ_1 (Interoperability)	0.036	0.263
	(0.03)	(0.055)
θ_2 (Always Same System)	0.176	
	(0.021)	
θ_3 (Distance)	-0.0009	
	(0.0001)	
ρ (Nest)	0.87	0.57
	(0.01)	(0.05)
Mean Interoperability Elasticity	0.072	0.153
Mean Distance Elasticity	-0.66	
Sender FE	Yes	
Receiver FE	Yes	
HRR × Year FE	Yes	Yes
Sender × Receiver FE		Yes

Notes: Model parameters as defined in Equation 6.1 estimated for referrals. The market for each sending hospital is defined as the set of receiving hospitals with which the sender shares referrals at baseline (2008). All other receiving hospitals are in the outside option. Standard errors are in parentheses.

D.5 Additional Exhibits for Equilibrium Effects of Reducing Interoperability Frictions (Section 7)

D.5.1 Summary of Counterfactuals Separately by Intensive, Extensive Patients

Appendix Tables D26 and D27 separately report welfare changes under the two interoperability counterfactuals for patients who choose in-market hospitals under the data conditions ("intensive patients") and for those who choose the outside option ("extensive patients"), respectively.

Table D26: Summary of Counterfactuals: Intensive Patients

Counterfactual	%Δ Total Welfare	Δ Total Welfare (-km)	=	Δ Direct Welfare (-km)	+	Δ Allocative Welfare (-km)	Share Switch
First-Best Full Interop. (All = 1)	20.3%	71.6		71.2		0.4	0.02
Epic Monopoly (All Epic)	12.5%	44.2		43.8		0.4	0.02

Notes: Table displays the results of two counterfactual scenarios: (1) increase all interoperability levels to the maximum of 1; (2) increase all interoperability levels to Epic's within-vendor level in each time period, thereby in essence have all hospitals switch to Epic. "%Δ Total Welfare" is the average percent change in welfare for all patients in the counterfactual compared to the data. "Δ Total Welfare" is the average level change in welfare for all patients in the counterfactual compared to the data, scaled by the estimated coefficient on distance (-0.00768) multiplied by negative one; this welfare level is thus in negative distance units. "Δ Total Welfare" = "Δ Allocative Welfare" + "Δ Direct Welfare", as described in Equation 7.1. "Share Switch" is the share of patients that choose different hospitals in the counterfactual than in the data. In contrast to Table 7, this table limits the sample of patients to those who choose hospitals in the market under the data conditions ("intensive patients").

Table D27: Summary of Counterfactuals: Extensive Patients

Counterfactual	%Δ Total Welfare	Δ Total Welfare (-km)	=	Δ Direct Welfare (-km)	+	Δ Allocative Welfare (-km)	Share Switch
First-Best Full Interop. (All = 1)	∞ %	9.9		0.0		9.9	0.245
Epic Monopoly (All Epic)	∞ %	4.3		0.0		4.3	0.157

Notes: Table displays the results of two counterfactual scenarios: (1) increase all interoperability levels to the maximum of 1; (2) increase all interoperability levels to Epic's within-vendor level in each time period, thereby in essence have all hospitals switch to Epic. "%Δ Total Welfare" is the average percent change in welfare for all patients in the counterfactual compared to the data. "Δ Total Welfare" is the average level change in welfare for all patients in the counterfactual compared to the data, scaled by the estimated coefficient on distance (-0.00768) multiplied by negative one; this welfare level is thus in negative distance units. "Δ Total Welfare" = "Δ Allocative Welfare" + "Δ Direct Welfare", as described in Equation 7.1. "Share Switch" is the share of patients that choose different hospitals in the counterfactual than in the data. In contrast to Table 7, this table limits the sample of patients to those who choose hospitals outside of the market under the data conditions ("extensive patients").

D.5.2 Change in Receiver Hospital Characteristics for Patients who Switch in First-Best

Appendix Table D28 reports changes in hospital characteristics for patients who switch hospitals under the *perfect interoperability* counterfactual. On average, switchers travel 0.536 km farther but go to hospitals with $961.40 lower average charges, 0.028 lower 30-day readmission rates, and 0.016 lower 30-day mortality rates.

Table D28: Change in Hospital Characteristics for Patients who Switch

	Δ in Receiver Hospital Characteristics			
	(1) Distance (km)	(2) Charges	(3) 30 Day Readmit	(4) 30 Day Death
First-Best Full Interop. (All = 1)	0.536	-961.4	-0.028	-0.016
Mean of Outcome	27.7	63,388	0.308	0.093

Notes: Table displays average changes in receiver hospital characteristics for patients who choose different receiver hospitals in the first-best full interoperability counterfactual than in the data (i.e., patient switchers).