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

Rebekah Dix Kelsey Moran Thi Mai Anh Nguyen

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Abstract

Interoperability—the ability of different systems to work together—is an increasingly vital component of product markets. We study the impact of interoperability frictions in the context of US hospital Electronic Health Record (EHR) systems. While use of EHR systems is widespread, interoperability of these systems remains low, particularly across those produced by different EHR vendors. We examine how interoperability affects patients by considering both a direct, technological effect of influencing health information exchange and an allocative effect of shifting the flow of patients across providers. Using an event study design in which interoperability between hospital pairs changes when one changes EHR vendors, we find evidence for both channels. When two hospitals switch to having the same EHR vendor, charges and readmissions rates for patients who are transferred and referred between them decrease by 4% and 11%, respectively. In addition, these hospitals now share 8% more inpatient transfers and 9-10% more referrals. This change in patient flows further affects patient outcomes: patient health improves when their sending hospitals switch to EHR vendors used by higher-quality hospitals in the market and worsens when the opposite occurs. To quantify the welfare gain from reducing interoperability frictions, we estimate a demand model of how patients and providers trade-off interoperability with other receiving hospital characteristics when choosing where to send patients. The model is identified by changes in patient flows following changes in hospital EHR vendors and interoperability levels. We show that eliminating all interoperability frictions would redirect 7.5% of patients to different hospitals and increase joint hospital-patient welfare by 21%, the equivalent of a 57-kilometer reduction in travel distance.

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1 Introduction

Interoperability—the ability for two different systems to work seamlessly together—is a vital component of product markets that has become increasingly important in the economy over time due to the proliferation of information and communications technologies (FTC, 2023; European Commission, 2024). While often unnoticed, interoperability underpins many daily activities: Apple and Windows computers can share files and hardware; messages can traverse across service providers; bank cards can function at almost any ATM; even trains built by different manufacturers can operate 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 can 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. We focus on patients who move between healthcare providers, for whom interoperability is most salient and consequential. These movements are common: over one-third of patients in the US are referred to specialists each year (Mehrotra, Forrest, and Lin, 2011), and 4% of those admitted to US hospitals are subsequently transferred directly to another. 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 may benefit patients by reducing hassle costs, decreasing redundant care, and enhancing care coordination and continuity through better health information exchange between providers. Patients and providers, aware of these benefits, may then prioritize interoperability in their decisions of where to send pa-

[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.

tients for additional medical care. Allocatively, interoperability may thus affect the direction of patient flows between providers. Given that providers are imperfect substitutes (Doyle, Graves, and Gruber, 2019; Chandra, Dalton, and Staiger, 2023), this interoperability-driven reallocation of patients may subsequently affect patient outcomes by, for example, sending them to different quality healthcare facilities than they would have visited absent this technological friction. By analyzing these two channels, we aim to better understand the critical role that EHR interoperability plays in shaping healthcare delivery and patient welfare.

Despite various government initiatives aimed at promoting both the adoption of EHR systems as well as improved interoperability between those systems, the ability to share patient data across different EHR systems remains low, particularly across systems built by different EHR vendors. In 2019, 32% of patients reported experiencing gaps in information exchange when visiting a provider (ONC, 2019), and only 36% of physicians reported having patients' medical information electronically available from outside sources at the point of care (ONC, 2022). The market for EHR systems is fragmented, and exchanging patient data is particularly challenging across organizations using different EHR vendors: only 8% of primary care physicians in 2022 found it very easy to use information from outside organizations with a different EHR vendor, compared to 38% from organizations with the same vendor (Everson et al., 2024).[3] While these survey responses indicate that within-vendor interoperability is better than that across different vendors, they also suggest that even within-vendor interoperability remains imperfect in this setting. Unlike much of the prior literature that has focused on EHR adoption, we examine the effects of these low (and heterogeneous) EHR interoperability levels on patients.

Low levels of EHR interoperability may have quite negative direct consequences. Difficulties sending and receiving medical records can be frustrating for patients and providers, increasing workloads by requiring them to fax records by hand or track down gaps in medical histories.[4] Low interoperability may also increase the probability of care coordination and continuity failures, potentially leading to wasteful medical spending (e.g., duplicate testing)

[3]Further, 70% of hospitals in the American Hospital Association IT Survey in 2019 reported experiencing greater challenges exchanging data across organizations with different vendors. Even President Obama, the signatory of the HITECH Act of 2009, once stated, "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).

[4]In her book, *Fragmented: A Doctor's Quest to Piece Together American Health Care*, Dr. Ilana Yurkiewicz writes that, "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).

or even patient harm due to delays in timely and accurate care.[5,6] These direct consequences would be most salient for patients who move between providers, such as those who are referred for additional care or transferred between inpatient facilities.

In this paper, we find causal evidence of such direct effects of interoperability on patients who move between hospitals. Combining data on hospital EHR vendors from the American Hospital Association (AHA) IT Survey and the Health Information and Management Systems Survey (HIMSS) with data on patient flows and outcomes from the Centers for Medicare and Medicaid Services (CMS) Medicare Claims from 2005 to 2019, we analyze whether outcomes improve for patients who are shared between two hospitals when those two hospitals become more interoperable. In particular, we use a difference-in-differences strategy to look at whether shared patient outcomes change after two hospitals switch to using the same EHR vendor, which is a proxy for higher interoperability. We analyze outcomes at the patient-level for transfer and referral patients separately. While referrals are quite common but heterogeneous in type and severity, transfers are less frequent but often involve the most complex cases and the highest-risk patients (Hernandez-Boussard, Davies, McDonald, and Wang, 2017).

We find that when two hospitals switch to having the same EHR vendor, charges, images, tests, and hospital readmission rates decrease for the patients that they share. In particular, transfer patients experience a 3.7% decrease in charges and a 2.1% decrease in images, while referral patients experience a 2.8% decrease in tests and a 11.3% decrease in 60-day hospital admission rates. These effects are largest for higher-risk patients and when the receiving hospital is of lower quality. These results are robust to various specification checks controlling for patient, hospital, and market characteristics. They are also robust to controlling for changes in hospital ownership. These decreases suggest that higher interoperability improves health outcomes and decreases care costs, likely due to more effective use of patient records and possibly reduced redundant care. Our findings are consistent with previous studies on the direct benefits of health information exchange in other settings.

We next show that patients and providers value interoperability in their decisions of where

[5]Shrank, Rogstad, and Parekh (2019) estimate that care coordination failures result in $27-78 billion of wasteful medical spending annually from unnecessary hospital admissions, avoidable complications, and redundant procedures. In this paper, we examine how imperfect EHR system interoperability contributes to these failures.

[6]Anecdotal evidence of imperfect interoperability causing patient harm is common. In one incident, a hospital lab's software was not interoperable with the same hospital's emergency department software. This caused care for a patient with neurological symptoms to be delayed, which ultimately led to him suffering irreversible brain damage (Schulte and Fry, 2019). In another, a patient's referral from a primary care physician to a cardiologist was lost between the two different EHR systems used by the medical offices, preventing her from seeing a cardiologist for three additional months. She then died of heart failure on the exact day of her referred appointment (Betsy Lehman Center, 2018). These examples underscore the potentially critical consequences of interoperability challenges in healthcare settings.

to send patients for additional healthcare. Given the above direct benefits of improved interoperability on patient outcomes—in addition to other possible direct benefits for providers—both patients and providers may prefer to send patients to other facilities with whom information exchange is simplest and most effective.[7] To examine this potential allocative effect of interoperability on patients, we use a similar difference-in-differences strategy exploiting changes in whether any two hospitals have the same EHR vendor and thus higher interoperability. We find that when two hospitals switch to using the same vendor, their shared inpatient transfers increase by 8% and their shared referrals increase by 9-10%. These effects are most pronounced for hospital pairs with baseline patient-sharing relationships and those switching to Epic EHR systems. The latter is consistent with our later finding that Epic has superior internal, within-vendor, interoperability compared to other EHR vendors. We find no evidence that switching EHR vendors affects the extensive margin of whether to transfer or refer patients. We thus conclude that these changes in patient flows represent a reallocation of patients across hospitals; more specifically, a reallocation of almost 100,000 Medicare transfers and 15 million Medicare referrals—or $7.3 billion of Medicare spending—due to hospital EHR vendor switches from 2005 to 2019.

This result that hospitals send more patients to other hospitals with which they are more interoperable is robust to various specification checks, such as controlling for hospital and market characteristics and excluding hospitals that change hospital systems (i.e., owners). Furthermore, a placebo test using vendor switches that do not result in hospital pair vendor alignment yields null estimates, suggesting that our main results are not driven by unobserved changes coinciding with vendor switches. Additionally, an instrumental variable strategy—in which we instrument for hospital vendors with a measure based on the vendors of other out-of-market, same-hospital-system hospitals—produces larger effects. This suggests OLS estimates may be downward biased due to measurement error or endogeneity in vendor choice.

Since hospitals are imperfect substitutes (Doyle et al., 2019; Chandra et al., 2023), this interoperability-driven reallocation of patients across hospitals may affect patient outcomes by—for example—sending them to better or worse quality hospitals than they would have gone to in the absence of this technological friction. To test for such allocative effects on patient outcomes, we use a patient-level difference-in-differences strategy leveraging sending hospital-level EHR vendor switches. We examine whether—after their sending hospitals switch

[7]Better interoperability may directly benefit providers by reducing the hassle costs of exchanging patient records. Providers likely prioritize patient welfare, but reduced hassle costs may misalign incentives between patients and providers and potentially incentivize providers to favor more interoperable facilities over their patients' best interests. Our model of patient flows will ultimately estimate joint sending hospital and patient welfare. We unfortunately will not be able to decompose this welfare into the separate provider and patient components.

vendors—changes in outcomes for patients sent to same-vendor receiving hospitals correlate with changes in characteristics of other local same-vendor hospitals.[8] Our findings confirm this correlation for patient health outcomes, cost of care, and distance traveled. That is, if a hospital switches to a vendor that is used by better (worse) quality hospitals in its market than before, then the outcomes of its patients that it sends to those same-vendor hospitals improve (worsen). This allocative effect on outcomes suggests a potential trade-off between interoperability and other receiving hospital characteristics.

To quantify the welfare gain from reducing interoperability frictions, we next develop and estimate a demand model of how patients and providers trade-off interoperability with other traditional hospital demand factors (e.g., cost, quality, and distance) when choosing where to transfer and refer patients. Our model builds on standard hospital demand frameworks (e.g., Ho, 2006) by adding interoperability to the utility function. While we model the sending hospital as the decision-maker, we recognize that the choice of where to send patients for additional care is jointly made by the sending hospital and the patient. Our utility measure is thus joint between the sending hospital and the patient.

We estimate this model in two steps. First, we directly estimate vendor-year-specific levels of interoperability along two dimensions: (1) internal, within-vendor (e.g., Epic to Epic) and (2) external, across-vendor (e.g., Epic to non-Epic). This estimation exploits hospital-level reports of interoperability from the AHA IT Survey as well as variation in EHR vendor choices across markets. We find that within-vendor interoperability is indeed higher than across-vendor interoperability, but these levels vary considerably across vendors and time periods. In particular, Epic has the highest within-vendor interoperability among major EHR vendors and thus the largest gap between within- and across-vendor interoperability.[9] Second, we incorporate these interoperability measures into our demand model and estimate the remaining parameters using nested logit. We find that the hospital demand elasticity of substitution with respect to interoperability is approximately one-third to one-half the magnitude of that with respect to distance.

Using our model estimates, we next evaluate the welfare gains from reducing interoperability frictions. For each counterfactual in which we increase interoperability levels, we decompose the change in joint hospital-patient welfare into two channels: (1) the *allocative*

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

[9]These findings are in contrast with the common assumption in the product compatibility literature that within-vendor interoperability is perfect (1 on a scale of 0 to 1) and across-vendor interoperability is nonexistent (0). This simplification often stems from data limitations that prevent direct estimation of interoperability levels in other settings.

effect from patients choosing different hospitals, and (2) the *direct* effect from interoperability entering the utility function directly.

Completely eliminating the technological friction in this setting yields substantial welfare gains. For an average transfer patient, joint hospital-patient welfare under the first-best full interoperability counterfactual increases 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. Eliminating the wedge between within- and across-vendor interoperability, and thus removing the distortive incentive to send patients to same-vendor hospitals, results in 7.5% of patients being sent to different recipient hospitals. On average, these patients are reallocated to hospitals that are slightly farther away but also lower cost and much higher quality. Patient reallocation and allocative efficiency gains are largest in fragmented markets with more variation in EHR vendors across hospitals. Reallocation is also largest from sending hospitals using Epic EHR systems due to Epic's large wedge between within- and across-vendor interoperability and thus its larger incentive to send patients to other Epic hospitals. Smaller, non-Epic recipient hospitals benefit from this reallocation by receiving additional transfer patients and thus additional revenue.

Perfect interoperability may be unrealistic given current technological constraints. Across-vendor interoperability, in particular, is difficult to improve as it requires coordination across EHR vendors. To simulate this constraint, we perfect only within-vendor interoperability levels while keeping across-vendor levels constant.[10] This achieves just 34% of first-best welfare gains due to persistently low across-vendor levels and worsened allocative distortions. We next fully incorporate current technological capabilities for interoperability by simulating a scenario in which all hospitals use Epic EHR systems, thus enjoying the highest (within-vendor) interoperability levels observed in our data.[11] This Epic monopoly yields substantial welfare gains, equal to 61% of the first-best. However, such a monopoly may have unintended consequences in the EHR market (e.g., higher prices) not captured in our current framework.[12] Public policies could achieve some of these welfare gains without hampering market competition by directly providing or enforcing minimum interoperability standards. Such a policy aligns with recent initiatives like the Trusted Exchange Framework and Common Agreement (TEFCA), which aims to facilitate patient record exchange across different EHR systems through a voluntary

[10]This scenario would also continue an observed trend of improving within-vendor but stagnating across-vendor interoperability levels.

[11]This scenario would continue the trend of increasing Epic market share.

[12]Investigating these potential trade-offs between competition and interoperability-driven welfare gains is a promising direction for future research. When such a trade-off exists, public enforcement of interoperability standards may be a potential solution.

nationwide health information exchange network.[13] Simulating this type of policy, we find that setting a minimum standard at the current average interoperability level would increase joint hospital-patient welfare by 34%. Higher minimum standards yield even greater welfare gains, underscoring the potential impact of interoperability policies on healthcare system efficiency and hospital-patient welfare.

Related Literature This paper contributes to three strands of literature. First, we build upon the broad economics literature on product compatibility and technological frictions. Prior work has demonstrated that compatibility issues affect market competition and harm consumers (Farrell and Saloner, 1985; Katz and Shapiro, 1994), with firms often strategically reducing product compatibility (Augereau, Greenstein, and Rysman, 2004; Knittel and Stango, 2009; Gross, 2020). Evidence of such anti-competitive behavior has been found in various industries, including banking (Ishii, 2005; Knittel and Stango, 2008; Knittel and Stango, 2009), video and gaming technology (Park, 2004; Lee, 2013), railroads (Gross, 2020), and electric cars (J. Li, 2023). We contribute to this literature by examining the market for EHR systems and how EHR interoperability affects patients, who do not choose these systems but may be affected by them nonetheless.[14] This setting is particularly welfare-relevant, as imperfect interoperability may have consequences for patient health and well-being. Further, we demonstrate how to quantify interoperability from survey data, showing that interoperability is neither perfect within vendors nor nonexistent across vendors, with substantial variation over time and across vendors. This variation is both directly welfare-relevant and useful for linking interoperability levels to welfare.

Second, we extend the literature on hospital demand and patient flows between providers. Previous studies have shown that financial structures—such as insurance networks (Ho and Pakes, 2014) and vertical integration (Walden, 2016; Brot-Goldberg and de Vaan, 2018; Cutler, Dafny, Grabowski, Lee, and Ody, 2020; Singh, 2022; Cuesta, Noton, and Vatter, 2024)—influence patient flows. We contribute by demonstrating that technological frictions can also do so. Moreover, unlike prior work that finds no effects of financially-induced patient reallocation on health outcomes (Ho and Pakes, 2014; Cutler et al., 2020), we show that imperfect EHR interoperability can impact health outcomes both directly and allocatively.

Third, we contribute to the literature on EHR systems in healthcare. Previous research has

[13] As this policy example illustrates, the federal government is currently considering direct provision to improve interoperability. This approach marks a shift from past policies such as the HITECH Act of 2009, which gave financial incentives to hospitals to encourage EHR adoption and more advanced EHR capabilities.

[14] The EHR market has a unique three-tier structure, with EHR vendors offering products, hospitals choosing products, and patients experiencing the consequences.

primarily focused on EHR adoption, examining influencing factors (e.g., Miller and Tucker, 2009; Dranove, Forman, Goldfarb, and Greenstein, 2014; Adler-Milstein and Pfeifer, 2017; J. Lin, 2023a; J. Lin, 2023b) and its effects on productivity and patient outcomes (e.g., Agha, 2014; Miller and Tucker, 2011; S. Lin, Jha, and Adler-Milstein, 2018; Bronsoler, Doyle, and Van Reenen, 2022; Ganju, Atasoy, and Pavlou, 2023). Particularly relevant to our work, J. Lin (2023a) finds that hospitals are incentivized to adopt locally dominant EHR systems, while J. Lin (2023b) shows that hospitals owned by multi-hospital systems tend to adopt their system's preferred EHR vendors. Rather than focusing on the drivers of hospital EHR choices, our study focuses on the effects of interoperability on patient flows and outcomes given existing EHR choices. We prioritize understanding these effects as a necessary precursor to exploring the broader incentives for hospitals to demand, and vendors to supply, interoperability.

More recent studies, aligning with public policy initiatives, have looked at EHR interoperability and health information exchange (HIE), highlighting both the poor state of interoperability (e.g., Holmgren, Patel, and Adler-Milstein, 2017) and demonstrating the direct benefits of improved HIE on healthcare costs (Adjerid, Adler-Milstein, and Angst, 2018; Atasoy, Demirezen, and Chen, 2021; Adler-Milstein, Linden, Hsia, and Everson, 2024), duplicative testing (Ayabakan, Bardhan, Zheng, and Kirksey, 2017; Wakefield et al., 2020), and patient health outcomes (Chen, Guo, and Tan, 2019; Bronsoler, 2022). We contribute by focusing specifically on the effect of EHR interoperability rather than broader HIE. While Y. Li, Lu, Lu, and Chen (2022) examines the direct effects of interoperability on transferred heart attack patients in New York, we look more broadly at all Medicare patient transfers and referrals. Further, we uniquely demonstrate the allocative effect of interoperability on patient flows and outcomes, in addition to the direct effect. We thereby offer a more comprehensive picture of how interoperability impacts patient welfare.

Outline The remainder of the paper is structured as follows: Section 2 describes our data sources as well as the EHR market and the state of interoperability. Section 3 shows that EHR system interoperability directly affects patients by reducing care costs and improving health outcomes. Section 4 demonstrates that interoperability also influences patient flows between hospitals. Section 5 examines the impact of this patient reallocation on patient outcomes. Section 6 develops a model quantifying the trade-offs between interoperability and other hospital characteristics in patient transfer and referral decisions. Section 7 then conducts counterfactuals to assess the welfare benefits of improved interoperability. Section 8 concludes.

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.

Data on Hospitals Our primary data sources for information on hospitals and their EHR systems are the American Hospital Association (AHA) Annual Survey from 2005-2019 and the AHA IT Survey from 2009-2019.[15] These two surveys are sent to all hospitals in the US, with the Annual Survey containing detailed information on hospital characteristics and the IT Survey collecting information on hospitals' EHR vendor choices as well as characteristics of their EHR systems including various measures of interoperability. To extend our data on hospital EHRs back through 2005, and to fill in some missing EHR vendor information for hospitals that do not respond to the AHA IT, we supplement using similar data from the Health Information and Management Systems Survey (HIMSS) from 2005-2017.[16,17] Concordance between the EHR vendor information contained in the AHA IT Survey and that contained in the HIMSS is quite high; 87% of hospitals that respond to both surveys in any given year report using the same EHR vendor in both. However, this alignment is imperfect, which suggests the possibility of measurement error in our records of hospital EHR vendor choices. We discuss how this measurement error may affect our empirical results in Section 4.

Data on Patients To study patient outcomes and patient flows across healthcare 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.[18] We analyze claims from

[15]Our analysis of the AHA IT Survey is restricted to these years because 2009 is the first year in which EHR vendor information is available in the survey and 2019 is the last year of the survey currently available to us.

[16]The AHA IT response rate is approximately 50-60% each year.

[17]Appendix Figure A1 shows the annual shares of hospitals and hospital beds for which we have data on EHR vendors. The share of beds rises quickly from just under 60% to 90% over the period from 2005 to 2017. Following the end of the HIMSS in 2017, however, this share drops steeply. Given this drop, we limit our main empirical analysis to using data only from 2005-2017. The remainder of this section, though, will describe the setting using all available years of data.

[18]We have data on claims submitted only for patients covered by Traditional Medicare. As of 2023, more than half of eligible Medicare beneficiaries opted for Medicare Advantage plans—which are administered by private health-insurers—over Traditional Medicare. This share has risen consistently over time (e.g., the share of eligible beneficiaries that selected Medicare Advantage was 19% in 2007 (Ochieng, Biniek, Freed, Damico, and Neuman,

the 100% sample MedPAR files (the full sample of Medicare enrollees), the 100% sample Outpatient files, and the 20% sample Carrier files (a 20% random sample of Medicare enrollees), which report inpatient facility claims, outpatient facility claims, and professional provider service claims, respectively. These data contain detailed information about the billed medical visit, including diagnosis codes, procedure codes, charge amounts, admission and discharge dates, and any providers' Medicare certification numbers.

We supplement the Medicare claims with comprehensive data on patient sharing relationships across healthcare providers from DocGraph Hop Teaming from 2013 to 2019 for the sole purpose of estimating interoperability levels for the model in Section 6.

2.2 EHR Market

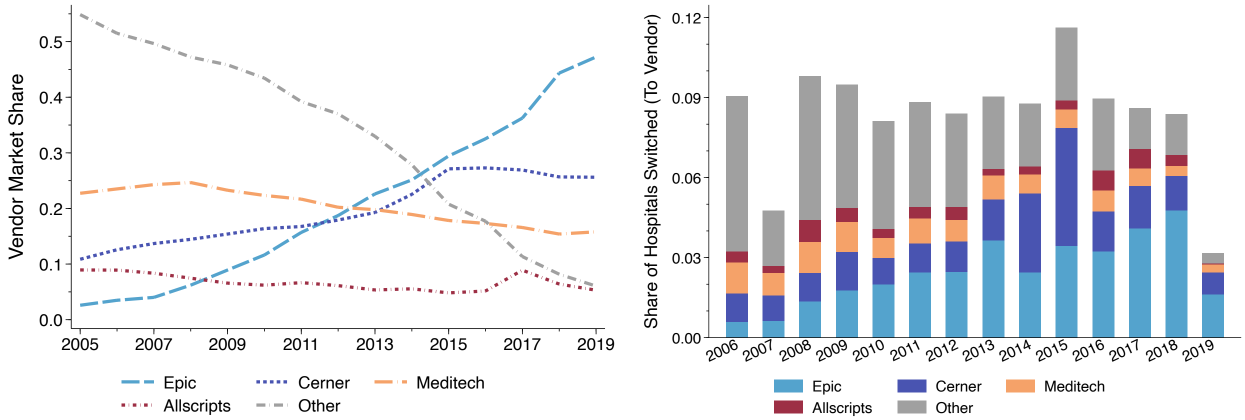
The role of EHR systems in US hospitals as well as the market for these electronic systems have changed substantially over time. While many hospitals have had some form of EHR system in place for decades, the federal Health Information Technology for Economic and Clinical Health (HITECH) Act of 2009—which allocated $30 billion to subsidize health organization adoption of more advanced EHR systems—led to significant increases in the capabilities of these systems as well as substantial changes to the EHR market.[19] By the end of our sample period in 2019, nearly all hospitals have an EHR system in place with capabilities for electronic clinician documentation and computerized physician order entry and the market for EHR systems has become quite concentrated.[20] Figure 1a plots the share of hospital beds that report using different EHR vendors in each year. The four largest vendors in 2019—in decreasing order—are Epic, Cerner, Meditech, and Allscripts. Epic has experienced a dramatic rise in market dominance over time, from representing less than 5% of hospital beds in 2005 to nearly 50% in 2019, while Cerner's market share has stagnated at around 25% for the last five years, and other vendors (e.g., Meditech, Allscripts) have witnessed steady market share declines. Epic's market share growth over the past decade is predominantly due to hospitals switching from using other EHR vendors to using Epic. Figure 1b shows that on average 8.4% of hospitals switch EHR vendors each year, with the majority of switches in the latter half of our sample period favoring Epic and Cerner.

2023). Unlike those in Medicare Advantage plans, beneficiaries enrolled in Traditional Medicare face no network restrictions. This is an advantage for our analysis, as we do not need to consider the role of insurance coverage on patient flows for these patients.

[19]The HITECH Act emphasized adoption of EHR systems with decision support capabilities as well as utilization of these systems at the point of care. Providers that met these thresholds satisfied the "meaningful use" requirement of the Act and were eligible for subsidies.

[20]Trends in EHR system capabilities are shown in Appendix Figure B1.

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.

Switching to a new EHR system is a costly endeavor for hospitals, often amounting to millions or even billions of dollars in installation costs alone.[21] Nevertheless, 49% of hospitals switch EHR vendors at least once during our sample period.[22] When asked why they switch, hospitals predominately cite costs and system functionalities (Coustasse, Andresen, Schussler, Sowards, and Kimble, 2018). Different EHR vendors offer varying prices (e.g., upfront installation fees, annual licensing costs, add-on customization charges, etc.) and diverse functionalities and customizations, which can significantly affect a hospital's billing capabilities and productivity.[23]

Alongside costs and functionalities, hospitals cite EHR system interoperability as a factor in their EHR vendor decision. As we discuss in more detail in the following section, interoperability in this setting is imperfect, with data exchange typically being more seamless between hospitals using the same EHR vendor than hospitals using different vendors. Many hospitals' EHR vendor choices suggest a desire for improved interoperability with other hospitals in both their local markets and their hospital systems. For example, among hospitals that switch ven-

[21]For example, Massachusetts General Hospital spent \$1.2 billion to switch to Epic in 2015 (Dayal McCluskey, 2015), while the Mayo Clinic was reported to spend over \$1.5 billion to switch to Epic in 2016 (Drees, 2020). A smaller hospital can still spend millions—St. Peter's Health in Helena, Montana has approximately 120 beds and spent \$25 million to switch to Epic in 2023 (Diaz, 2023).

[22]Shown in Appendix Figure B2.

[23]Shifting regulatory standards over the past two decades have also pushed many hospitals to switch to new EHR vendors in order to comply with federal requirements for more advanced EHR systems.

dors once during our sample period, 39% switch to the modal vendor in their local market, and 69% switch to the modal vendor in their health system (conditional on belonging to one; see Appendix Section B.5 for details).[24]

2.3 EHR Interoperability

Having an EHR system unfortunately does not guarantee that healthcare organizations are able to seamlessly exchange patient data with others; in fact, this is often not the case. Figure 2a illustrates the imperfect state of hospital EHR interoperability, depicting the share of hospitals capable of achieving three key domains of interoperability: (1) sending information electronically to other organizations, (2) receiving information electronically from other organizations, and (3) querying for information electronically from other organizations. The figure also shows the average hospital's ability to achieve a crucial fourth domain: (4) integrating information received electronically from other organizations without the need for manual entry into own system.[25] While over 80% of hospitals can send, receive, and query for data by 2019, the average reported ability to integrate data without manual entry remains at only 60%. Figure 2b reveals that exchanging patient data with organizations that use different EHR vendors is particularly challenging, with 75% of hospitals surveyed between 2017-2019 acknowledging this difficulty and relatively little variation by the EHR vendor of the reporting hospital.

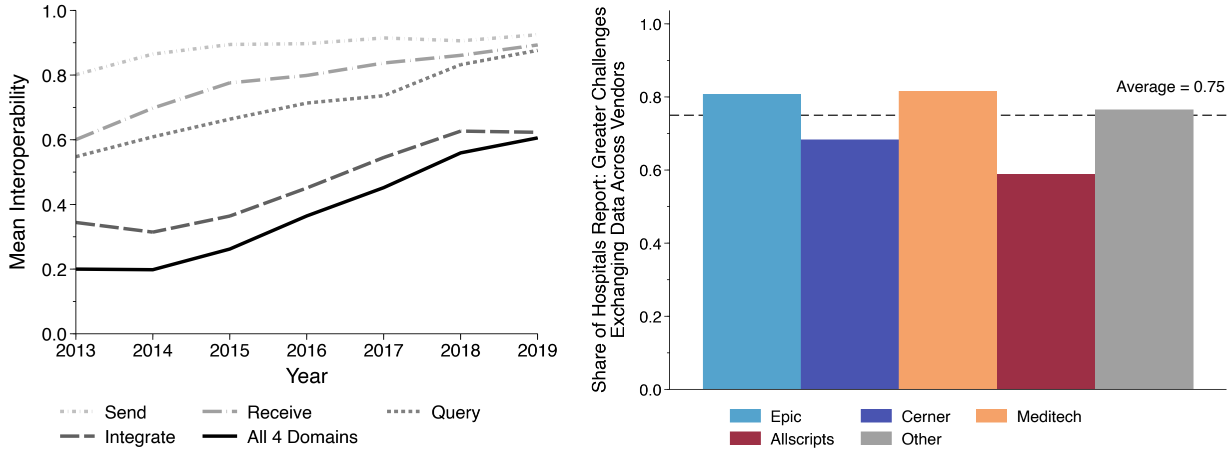
Improving interoperability is a major public policy goal, but various initiatives over the past 15 years have failed to bring about substantial change. While the HITECH Act of 2009 subsidized adoption of advanced EHR systems, the law did not promote interoperability (Bronsoler et al., 2022). And while the 21st Century Cures Act of 2016 prohibited information blocking—defined as "a practice that interferes with, prevents, or materially discourages access, exchange, or use of electronic health information"—enforcement mechanisms were only established in 2020 and 2024 through subsequent federal agency rules (Turbow, Hollberg, and Ali, 2021; HHS, 2024).[26] Public and private regional health information exchange organi-

[24]These patterns are consistent with interoperability mattering to hospitals, but not conclusive. For example, it is possible that the modal vendor in a market or health system generally happens to be good for other dimensions of hospital productivity that we do not measure.

[25]For domains (1)-(3), hospitals respond with either "yes" or "no;" thus, the lines for these domains in Figure 2 represent the share of hospitals that can achieve the domain. For domain (4), hospitals respond with "yes, routinely," "yes, but not routinely," or "no." We code these answers respectively as 1, 0.5, and 0 for the calculation of the integration line in Figure 2. The average of these coded answers is then what we report as the average reported integration interoperability in the figure.

[26]Enforcement has still been infrequent following these rules despite continued evidence of widespread information blocking. For example, 32% of hospitals reported encountering instances of intentional obstruction in information exchange by EHR vendors, healthcare providers, or other healthcare actors in 2022 after the information blocking regulations became enforceable in April 2021 (ONC, 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 across all four domains. Right panel plots the share of hospitals in 2017-2019 that report experiencing greater challenges exchanging data across EHR vendors than within EHR vendors, separately by the EHR vendors used by the responding hospitals. Dotted line represents the average across all vendors.

zations (RHIOs) have emerged nationwide to address persistent interoperability barriers, but their qualities, services, and participation rates vary widely (Adler-Milstein, Garg, Zhao, and Patel, 2021). Ongoing public initiatives like the federal Trusted Exchange Framework and Common Agreement (TEFCA), which aims to establish a nationwide health information exchange network, underscore the issue's persistence.[27]

Both patients and healthcare providers are acutely aware of the state of imperfect EHR interoperability. In 2019, 32% of patients reported experiencing gaps in information exchange when visiting a provider (ONC, 2019), and only 36% of physicians reported having patients' medical information electronically available from outside sources at the point of care (ONC, 2022). The disparity in interoperability across versus within EHR vendors is also visible to providers: 85% of office-based physicians reported encountering difficulties in electronically exchanging information with other providers who were using different EHR systems (ONC, 2022), while 38% of primary care physicians found information from organizations using the same EHR vendor very easy to use compared to only 8% for information from organizations with different EHR vendors (Everson et al., 2024). These survey responses suggest that while

[27]With the first organizations approved to join TEFCA in December 2023, its impact on improving interoperability remains to be seen.

within-vendor interoperability is better than that across different vendors, even within-vendor interoperability is imperfect in this setting. Patients and providers further believe that imperfect interoperability can have negative consequences for patient care and provider efficiency. About one in twenty patients reported having to redo a test or procedure because their prior medical data was unavailable, while one in five had to physically bring prior test results with them to appointments (ONC, 2019). And over 80% of physicians report that health information exchange improves care quality, enhances care coordination, and reduces duplicate test ordering (ONC, 2022). In the next section, we test for causal evidence of such direct effects of interoperability on patients who move between hospitals.

3 Evidence of Direct Effect on Patient Outcomes

By facilitating faster and more efficient health information exchange between providers, better interoperability may directly benefit patients who move between healthcare providers by reducing the hassle costs involved with information exchange and improving care coordination and care continuity. Such improvements could in turn reduce wasteful medical spending (e.g., from duplicate testing) or even improve patient health by supporting the provision of more timely and accurate care. In this section, we empirically examine the effects of higher interoperability on medical spending and health outcomes for patients who are shared between hospitals. Specifically, we show that when patients are shared between two hospitals that gain the same EHR vendor—and thus gain higher interoperability—patient charges and hospital readmissions rates fall.

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. Transfers are less frequent but often involve the most complex cases and the highest-risk patients, while referrals are quite common but heterogeneous in type and severity.

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,[28] and we use the

[28]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. This definition of a transfer is common in the health policy literature (e.g., Hsuan et al., 2024). While alternative definitions for

100% sample MedPAR files from 2005 to 2017 to identify them. Under this definition, 5.1% of hospital inpatient stays—about 600,000 stays per year—result in a transfer to inpatient treatment at another hospital. Table 1a contains summary statistics for transfers at the patient level, comparing the sample of transfer patients to the full sample of inpatient hospital visits. On average, transfer patients are hospitalized for longer, incur higher charges, and experience higher death rates than all patients. While most patients are transferred across hospitals to receive care that falls under common diagnosis categories (e.g., circulatory, respiratory, injury), many others are transferred across facilities for aftercare following medical 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 providers 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.[29] We identify these referrals using the 20% sample Carrier files from 2008 to 2017, which contain the identities of both the providing and the referring providers for the billed medical service as well as the category of the place of service (e.g., hospital, either inpatient or outpatient). We start our referrals sample in 2008 due to a data change in the prior year in the way the Medicare claims report provider identifiers.[30] 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. Unfortunately 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.[31] We then assign the identity of that first hospital facility as

transfers do exist, such as combining the timing of inpatient stays with disposition codes (e.g., Mueller, Zheng, Orav, and Schnipper, 2019; Shannon, Zheng, Orav, Schnipper, and Mueller, 2021), we choose to define transfers based solely on timing in order to both reduce sampling bias and include "indirect referrals" in which patients are advised at discharge to visit another hospital rather than formally transferred.

[29]Some—but not all—inpatient transfers will also be classified as referrals. If inpatient transfer patients receive inpatient services that also explicitly list the referring provider in the Carrier files, then they will also be classified as referrals. However, this is not the case for all inpatient transfers and thus not all are classified as referrals. Our measure of inpatient transfers is based solely on patient movement across facilities rather than explicit CMS claims codes, while our measure of referrals relies on codes for tractability.

[30]Beginning in 2007, National Provider Identifiers (NPIs) officially replaced Unique Physician Identification Numbers (UPINs) as the standard provider identifiers in the Medicare claims data. 2007 appears to be a transition year, with some providers reporting an NPI, some reporting an UPIN, and some reporting both in the Carrier claims. 2008 is the first year in which almost all Carrier claims lines contain a non-missing NPI. To maintain consistency across years, we thus start our referrals sample in 2008.

[31]While it is technically possible for us to check the place of service for the event that led to each referral by looking for claims submitted by the referring physician for the same patient prior to the referral, we believe that it would be insurmountably difficult for us to do this convincingly. Many patients have many claims in the Carrier files from the same physician, and the length of time between receiving a referral and receiving the referred

the primary hospital affiliation of the referring clinician. Appendix A contains more details on this data construction. Since each line item in the Carrier files contains a separate referring physician field, and the contents of that field can differ across line items contained within the same claim, we count each line item that meets our definition of cross-hospital referral as a separate cross-hospital referral.[32] Approximately 6.8% of all line items in the Carrier files—about 95 million per year—meet this definition of cross-hospital referrals.[33]

Table 1b shows summary statistics of these cross-hospital referrals at the patient level, comparing this sample of referrals to the full sample of referrals that take place in a hospital setting. On average, patients who are referred across hospitals have similar diagnoses and procedures as the general sample of referred patients. However, patients referred across hospitals are much more likely to be referred for imaging services and less likely to be referred for evaluation and management.

3.2 Empirical Strategy

To examine the direct effect of improved interoperability on transfer and referral patient outcomes, we run a difference-in-differences regression at the patient level around changes in whether the hospital sender-receiver pair that shares each patient has 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 who is sent from hospital h to hospital h' in year t. We look at four different outcomes: (1) total charges (in real 2019 dollars) for medical services incurred at the receiver hospital, (2) number of medical images taken at the receiver hospital, (3) number of medical tests given at the receiver hospital, and (4) an indicator of whether the patient was admitted to another hospital within 30 or 60 days following treatment at the receiver hospital. Parameters $\alpha_{(hh')}$ and γ_t are hospital sender-receiver pair fixed effects and year fixed effects, respectively. Our main independent variable $\mathbf{1}_{\left(e_{ht}=e_{h't}\right)}$ is an indicator

service will likely vary greatly.

[32]Each Medicare claim is comprised of line items for specific services rendered to that Medicare beneficiary during the medical visit for which the claim is seeking payment. For example, a claim for a visit with a primary care provider may include line items for services such as blood tests and vaccinations. By counting each line item as a separate referral, we allow for the possibility that one provider made the referral for the blood test while another separate provider made the referral for the vaccination.

[33]22% of all Carrier claim lines take place in a hospital, of which 87% list a referring physician. These lines are nearly evenly split between inpatient and outpatient hospital settings. Limiting to these hospital-to-hospital referrals, 29% have the same referring physician as the performing or operating physician, 35.5% take place at the same hospital from which they were referred, and the remaining 35.5% take place at a different hospital from which they were referred. This last bucket meets our definition of cross-hospital referrals, and we narrow our analysis to it.

Table 1: Patient Summary Statistics

(a) Transfers

	Transfer Patients			All Patients		
	Mean	Median	SD	Mean	Median	SD
Distance from Patient to 1st Hospital (km)	34.2	12.6	78.9			
Distance from Patient to 2nd Hospital (km)	52.7	25.3	86.5	31.2	11.7	74.2
Length of Stay at 1st Hospital (days)	7.88	5	24.6			
Length of Stay at 2nd Hospital (days)	13.5	10	15.4	5.96	4	14.1
Total Charges at 1st Hospital ($)	65049.7	31999	113152.6			
Total Charges at 2nd Hospital ($)	67589.5	35873	106904.5	40900.6	23754	67227.5
Died within 7 days of 2nd Admission	0.035			0.029		
Died within 30 days of 2nd Admission	0.10			0.078		
Died within 60 days of 2nd Admission	0.15			0.11		
Diagnosis Codes						
Aftercare	0.24			0.044		
Circulatory	0.22			0.24		
Respiratory	0.12			0.12		
Injury	0.096			0.096		
Infection	0.051			0.053		
Other	0.28			0.44		
N	7,804,147			8,502,571		

(b) Referrals

	Cross-Hospital Referrals			All Hospital Referrals		
	Mean	Median	SD	Mean	Median	SD
Total Allowable Charge ($)	67.0	37.9	134.5	75.2	47.5	133.5
Diagnosis Codes						
Circulatory	0.20			0.22		
Respiratory	0.12			0.12		
Musculoskeletal	0.11			0.091		
Aftercare	0.091			0.078		
Genitourinary	0.073			0.066		
Other	0.42			0.43		
BETOS Codes						
Evaluation & Management	0.28			0.38		
Imaging	0.45			0.31		
Procedures	0.11			0.13		
Tests	0.100			0.12		
Other	0.065			0.065		
N	46,818,072			206,692,287		

Notes: Summary statistics for transfers and referrals. Panel A compares transfer patient observations with a 5% sample of all inpatient hospital stays in the MedPAR files from 2005-2017. Panel B compares cross-hospital referral patient observations with a 5% sample of all claims in the Carrier files from 2008-2017.

for whether the two hospitals that share patient i have the same EHR vendor in year t, which is a proxy for interoperability given that within-vendor interoperability is higher than across-vendor interoperability. In our baseline specification, $X_{it,(hh')t,mt}$ includes time-varying controls for patient age, sex, race, baseline chronic conditions, baseline Charlson Comorbidity Index, and fixed effects for diagnosis categories. We also cluster our standard errors at the hospital sender-receiver pair level. Due to the sharp decline in the share of hospitals with EHR vendor information reported after 2017 as a result of the end of the HIMSS data, we limit our analyses to either the 2005-2017 period (transfers) or the 2008-2017 period (referrals).[34]

Our main identification assumption to interpret estimates from the above regression as identifying a casual relationship between same-vendor status and patient outcomes is that there are no time-varying hospital pair-specific factors correlated with the timing of changes in same-vendor status as well as our outcomes of interest. So, in the absence of a hospital pair switching to the same EHR vendor, patients who are shared between that treated pair would have experienced similar trends in outcomes as those shared between control hospital pairs that never change their same-vendor status. To provide support for this assumption, we estimate the following event study version of Equation 3.1:

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

where r is the relative year since the hospital sender-receiver pair switched to having the same vendor (which occurs in year $s_{hh'}$), we bin the relative year endpoints ($r \geq 6$ and $r \leq -6$), and we exclude $r = -1$. All other parameters are the same. To reduce bias from heterogeneous treatment effects that may be introduced when using always-treated observations as controls or including multiply-treated observations, we limit all of our analyses with Equation 3.1 and Equation 3.2 to patients shared between the following sample of hospital sender-receiver pairs: the 64% of pairs that never have the same vendor over the sample period (i.e., “never treated”) as well as the 11% of pairs that start with different vendors and switch to the same vendor only once over the sample period.[35] This event study specification allows us to visually analyze our results for the presence of pre-trends that may suggest a violation of our parallel trends assumption.

[34] Our results are robust to extending both sample periods through 2019.

[35] Our estimates are robust to including all patients from the full sample of hospital pairs.

3.3 Main Results

Table 2 presents the results of estimating Equation 3.1 for our four patient outcomes. Panel A shows that when two hospitals gain the same EHR vendor, transfer patients who are shared between them experience a $2,798 decrease in charges (a 3.7% decrease from the mean) and a 0.031 decrease in the number of images (a 2.1% decrease from the mean) at the receiving hospital. While transfer patients experience no changes in tests or hospital readmission rates, Panel B shows that referral patients undergo 2.8% fewer tests and experience 11.3% lower 60-day hospital readmission rates after the two hospitals they are shared between gain the same vendor. The reductions in charges, images, and tests at the receiving hospital suggest a decrease in duplicative care following improved interoperability. Providers may be accessing and utilizing existing patient information more effectively, reducing redundant procedures and their associated costs. This interpretation aligns with prior research on redundant procedures in other settings (e.g., Ayabakan et al., 2017; Wakefield et al., 2020). The lack of significant changes in health outcomes for transfer patients may be due to insufficient statistical power to detect rare catastrophic events. However, the larger sample of referral patients provides greater power, and their decreased readmission rates suggest that enhanced interoperability may lead to improved care quality and better patient health outcomes.

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) as described in Equation 3.2.[36] Despite wide 95% confidence intervals, all four outcomes show significant decreases after hospital pairs gain the same EHR vendor. Transfer patient outcomes (charges and images, Panels A and B) exhibit no significant pre-trends. Referral patient outcomes (tests and readmission rates, Panels C and D) show a slight decline from the 3-5 years before treatment to the 1-2 years before treatment. However, both outcomes stabilize in the two years preceding treatment, showing no significant pre-trends in those two years, and then decrease immediately after the hospital pair switches to using the same EHR 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 the magnitudes of these decreases growing in subsequent years. These growing magnitudes reflect two key factors. First, hospitals often require several years to fully implement a new EHR system. The AHA IT Survey, our primary data source for hospital EHR vendors, reports the vendor used by

[36] We omit the event study results for the four other outcomes (tests and readmission rates for transfer patients, charges and images for referral patients) because the static difference-in-differences estimates are insignificant and thus the event study estimates are very noisy.

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

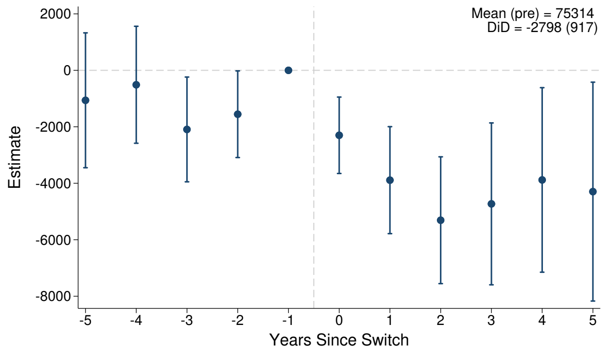
	Outcomes for Referral Patients			
	(1) Charges	(2) # Images	(3) # Tests	(4) 60-Day Readmit
Same EHR Vendor	-1.254	-0.000627	-0.0188**	-0.00440***
	(1.191)	(0.0130)	(0.00778)	(0.000956)
Observations	26393910	26393910	26393910	22048773
Mean of Outcome	146	1.314	0.659	0.0392

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.

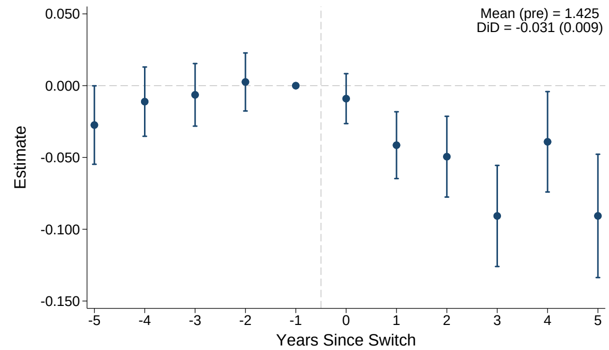
the largest number of patients or representing the largest financial investment for each hospital. Consequently, many hospitals may still be rolling out their new systems for several years after the reported switch.[37] This gradual implementation may also explain why relative time estimates for referral patient outcomes in the two years before treatment are smaller than those in the previous pre-treatment years: the new vendor rollout may start slightly before the AHA IT records the vendor switch. Second, hospitals may need time to fully learn and leverage the benefits of improved health information exchange with newly interoperable hospitals. This explanation aligns with the literature on EHR adoption, which typically shows an initial dip in hospital productivity followed by gradual improvement in patient outcomes as providers learn

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

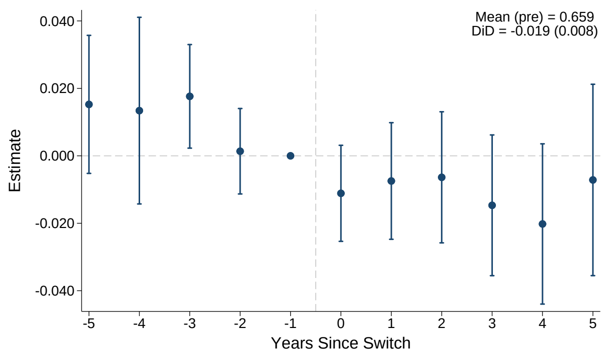
Figure 3: Event Studies of Direct Effect of Gaining Same Vendor on Shared Patient Outcomes



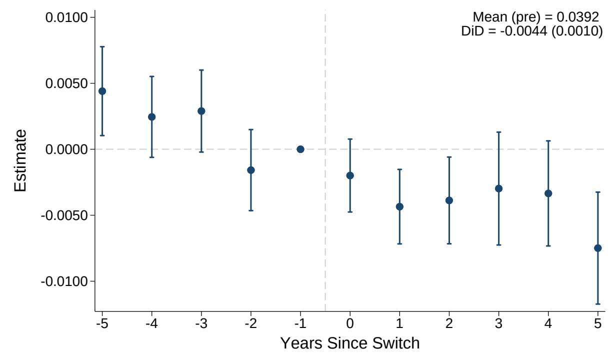
(a) Transfers, Charges



(b) Transfers, # Images



(c) Referrals, # Tests



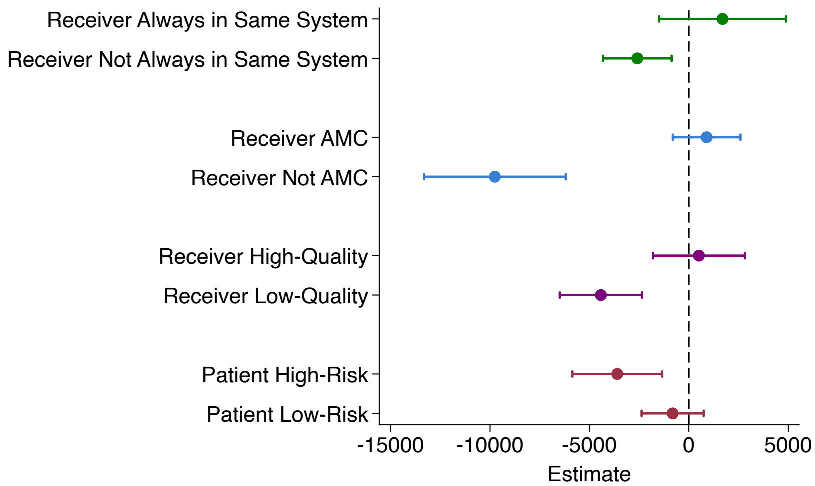
(d) Referrals, 60-Day Readmit

Notes: This figure plots 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 shows transfer patient images, Panel C shows referral patient tests, and Panel D shows 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.

how to use the systems efficiently.

Some of these direct effects could stem from changes in the types of patients who are shared between two hospitals after they switch to using the same EHR vendor. In the next section, we show that while shared patient flows do increase when two hospitals gain the same vendor, there is no significant compositional change in the shared patient population that could be contributing to the results in this section. We discuss potential additional confounding factors for this empirical strategy and outline our strategies to mitigate them in Section 4.3.

Figure 4: Direct Effect Heterogeneity: Transfer Patient Charges



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 total charges (in real 2019 dollars) incurred at the receiver hospital by transfer 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 baseline risk-adjusted mortality rate for transfer patients. "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 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.

Heterogeneity Figure 4 shows heterogeneity in the direct effect of gaining the same EHR vendor on transfer patient charges across several receiver hospital and patient characteristics. We find that the decrease in charges is largest when the receiving hospital is not part of the same health system (i.e., does not have the same owner) as the sender, is not an academic medical center (AMC), and is of lower quality.[38] This heterogeneity is quite notable, as patients who are sent to these types of receiver hospitals often have worse outcomes in the absence of treatment than those sent to other hospitals. We also find that the treatment effect of gaining the same EHR vendor is larger for higher-risk patients.[39] Appendix Figure D1 shows similar patterns for other outcomes.

[38]We define baseline hospital quality as the risk-adjusted mortality rates for transfer (referral) patients who are received at the hospital in the first two years of our data samples (i.e., 2005-2006 for transfers, 2008-2009 for referrals). We then bucket receivers into two bins: "high-quality" receivers, which have below-median baseline risk-adjusted mortality rates, and "low-quality" receivers, which have above-median baseline risk-adjusted mortality rates.

[39]We measure patient risk as predicted patient mortality based on age, sex, race, chronic conditions, and diagnosis codes. We then bucket patients into two bins: "high-risk" patients who have above-median predicted mortality rates, and "low-risk" patients who have below-median predicted mortality rates.

4 Evidence of Effect on Patient Flows

Given these direct benefits of improved interoperability on patient outcomes, patients and providers may prioritize interoperability in their decisions of where to send patients for additional care. This section presents empirical evidence on how EHR system compatibility affects patient flows between hospitals. Specifically, we show that gaining the same EHR vendor—and thus higher interoperability—causes hospitals to share more patients.

4.1 Empirical Strategy

To look at the effect of EHR vendor interoperability on cross-hospital patient flows, we run a similar difference-in-differences strategy as in Section 3 around changes in whether a hospital sender-receiver pair has the same EHR vendor:

$$Y_{(hh')t} = \beta * \mathbf{1}_{\left(e_{ht}=e_{h't}\right)} + \alpha_{(hh')} + \gamma_t + \delta X_{(hh')t,mt} + \epsilon_{(hh')t}. \tag{4.1}$$

The main difference between this equation and Equation 3.1 is that this specification is run at the hospital sender-receiver pair level rather than at the patient level. Both use the same source of variation: changes in whether a hospital pair has the same EHR vendor. Here, $Y_{(hh')t}$ denotes either the number or the fraction of patients transferred or referred from hospital h to hospital h' in year t (where the denominator for the fraction calculation is the total number of transfers or referrals sent from hospital h in year t). Since this equation is at the hospital pair level, we can no longer include patient-level controls. However, we can control for hospital-level and market-level covariates like hospital bed sizes and market by year fixed effects. We weight observations by the combined number of beds at the sender and receiver hospitals in 2005. All other variables, standard error clustering, and sample restrictions are the same as those in Equation 3.1.[40]

Each hospital pair enters our sample twice, once for each direction of patient flows. We limit the sample to hospital pairs that either are in the same Health Referral Region (HRR) (transfers) or that share patients at baseline (referrals), filling in zeros for pairs that share no transfers or referrals in any given year. We allow these samples to differ because on average referrals travel farther distances than transfers, and thus these two types of patient flows require

[40]Our main independent variable $\mathbf{1}_{(e_{ht}=e_{h't})}$ is an indicator for whether the two hospitals have the same EHR vendor in year t, which is a proxy for interoperability. Parameters $\alpha_{(hh')}$ and γ_t are hospital sender-receiver pair fixed effects and year fixed effects, respectively. We cluster our standard errors at the hospital sender-receiver pair level. We limit our sample to either the 2005-2017 period (transfers) or the 2008-2017 period (referrals).

different market definitions.[41,42]

As in the previous section, our main identification assumption to interpret estimates from the above regression as identifying a casual relationship between same-vendor status and patient flows is that there are no time-varying hospital sender-receiver pair-specific factors correlated with the timing of changes in same-vendor status as well as our outcomes of interest. So, in the absence of a hospital pair switching to the same EHR vendor, such a treated hospital pair would have similar trends in patient flows as control hospital pairs that never change their same-vendor status. To provide support for this assumption, we estimate the following event study version of Equation 4.1:

$$Y_{(hh')t} = \left(\sum_{r=-6}^{6} \beta_r \mathbf{1}_{\{t=s_{(hh')}+r\}} \right) + \alpha_{(hh')} + \gamma_t + \delta X_{(hh')t,mt} + \epsilon_{(hh')t}, \tag{4.2}$$

where r is the relative year since the hospital sender-receiver pair switched to having the same vendor (which occurs in year $s_{hh'}$), we bin the relative year endpoints ($r \geq 6$ and $r \leq -6$), and we exclude $r = -1$. All other parameters are the same. As in Section 3, we limit all of our analyses with Equation 4.1 and Equation 4.2 to the sample of hospital pairs that never have the same vendor as well as those that start with different vendors and switch to the same vendor only once.[43] This event study specification allows us to visually check for the presence of pre-trends that may suggest a violation of our parallel trends assumption. We discuss potential additional confounding factors and outline our strategies to mitigate them in Section 4.3.

Table 3 shows summary statistics for our hospital pairs analysis sample, with the transfers sample in Panel A and the referrals sample in Panel B.[44] The first two columns weight hospital

[41] Approximately 80% of cross-hospital inpatient transfers are within the same HRR. An average HRR has 19.3 hospitals. 79% of hospital sender-receiver-year observations in the same HRR share zero transfers that year, indicating that many hospitals within the same HRR do not share patients annually.

[42] In our inpatient transfer analysis, we limit to same-HRR hospital pairs because approximately 80% of hospital-to-hospital transfers are contained within the same HRR and thus HRR appears to be the appropriate market definition. For cross-hospital referrals, HRR does not seem to be the appropriate market since only 22% of cross-hospital referrals are contained within the same HRR. On average, 83% of a hospital's cross-hospital referrals are with other hospitals with which they share some amount of referrals at baseline. This implies that this definition of the market is more appropriate than same-HRR. An average hospital shares baseline referrals with 74 other hospitals. Once the sample is limited to hospital pairs that have patient flow relationships at baseline, 51% of hospital sender-receiver-year observations share zero referrals that year, indicating that many hospital pairs still do not share patients annually.

[43] As before, we implement this sample restriction in order to reduce bias from heterogeneous treatment effects from using always-treated observations as controls or including multiply-treated observations. Difference-in-differences results are robust to using the full sample of hospital pairs.

[44] Appendix Table D4 shows the same summary statistics but for the full sample of hospital pairs. Compared to the full sample, the fraction of hospital pairs with the same vendor is much lower in the event study sample (as expected given the sample restrictions) and the fraction of hospital pairs in the same hospital system—meaning owned by the same company—is lower. Otherwise, the samples are quite similar.

Table 3: 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 hospitals is an AMC while the receiving hospital is not; etc.

sender-receiver-year observations by combined baseline bed size, while the second two weight by actual number of transfers or referrals shared between the pair in that year. The former weights thus represent an average hospital bed pair while the latter show the circumstances faced by an average transfer or referral patient.[45] On average, each bed-weighted hospital pair exchanges 4 transfers and 45 referrals annually.[46] The distributions of these measures are highly skewed, with standard deviations that are much larger than the means. 8% (12%) of hospitals in the transfer (referral) sample have the same EHR vendor in any given year, while 11% (14%) of transfers (referrals) are shared between two hospitals with the same EHR vendor; this difference is suggestive of an effect of EHR vendors on patient flows.

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 4.2. Overall, there is a positive and statistically significant relationship between two hospitals having the same EHR vendor and their shared patient flows. When two hospitals gain the same vendor, their number of shared transfers increases by 0.41 (given a pre-treatment period mean of 4.9, a 8% increase) and the fraction of all transfers sent from the sending hospital that are sent to the receiving hospital increases by 0.0045 (0.45 percentage points; 8% increase from the pre-treatment period mean). Further, their number of shared referrals increases by 4.9 (10% increase from mean) and their fraction of referrals increases by 0.0014 (9% increase from the mean).[47] Though the magnitudes of these estimated coefficients are small, they are economically significant given their pre-treatment means.[48] There is no evidence of pre-trends

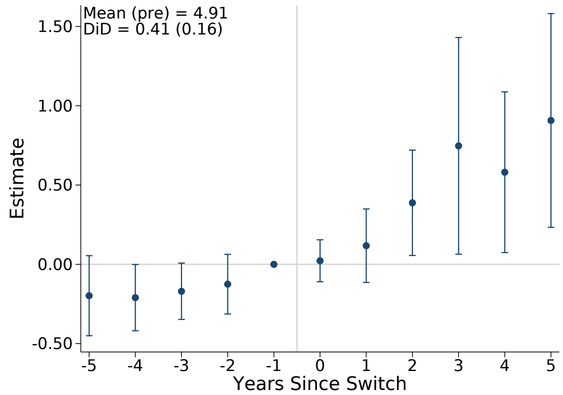
[45]We weight by combined baseline bed size of the two hospitals rather than bed size of the receiving hospital to better approximate the conditions facing a randomly selected hospital pair and a randomly selected transfer patient. Weighting by bed size of the receiving hospital only would approximate the circumstances for a random transfer patient conditional on where that transfer patient starts. It would hold the importance of a big hospital sending many patients to another big hospital equivalent to that of a small hospital sending few patients to a big hospital.

[46]Appendix Table D5 shows unweighted summary statistics. Unweighted, the average hospital pair shares 2.5 transfers and 47 referrals annually.

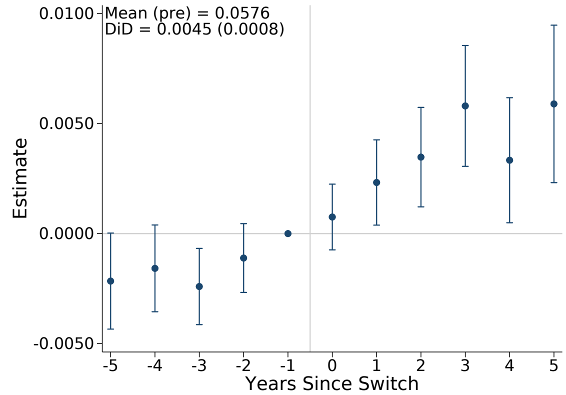
[47]The minor variations in percentage effects across the levels and fractions specifications stem from two primary factors. First, there are slight differences in the samples used for these different outcomes, with the number of observations for fractions being slightly smaller than that for levels. This discrepancy arises because some hospitals send zero total transfers/referrals in a year, thus rendering the fractions undefined. Second, the two outcomes rely on slightly different parallel trends assumptions: for levels, that the number of patient flows would have continued on similar trends over time; for fractions, that the share of patient flows sent to the receiving hospitals would have continued on similar trends over time.

[48]These pre-period means are low largely due to the fact that many hospital pairs do not share referrals or transfers in any given year. Nearly 80% of the hospital sender-receiver-year observations for the transfers sample

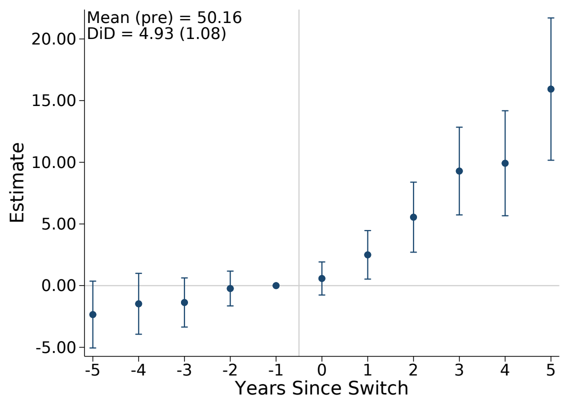
Figure 5: Event Studies of Gaining Same Vendor on Shared Patient Flows



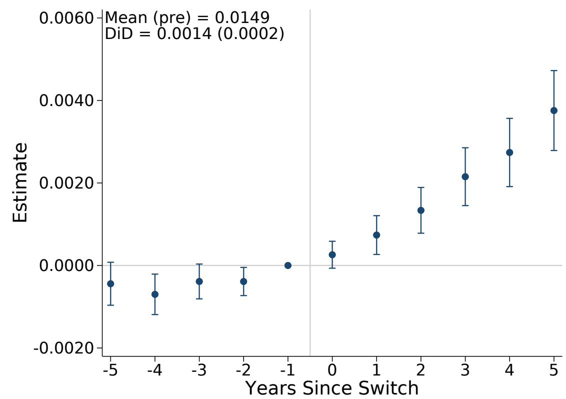
(a) # of Transfers



(b) Fraction of Transfers



(c) # of Referrals



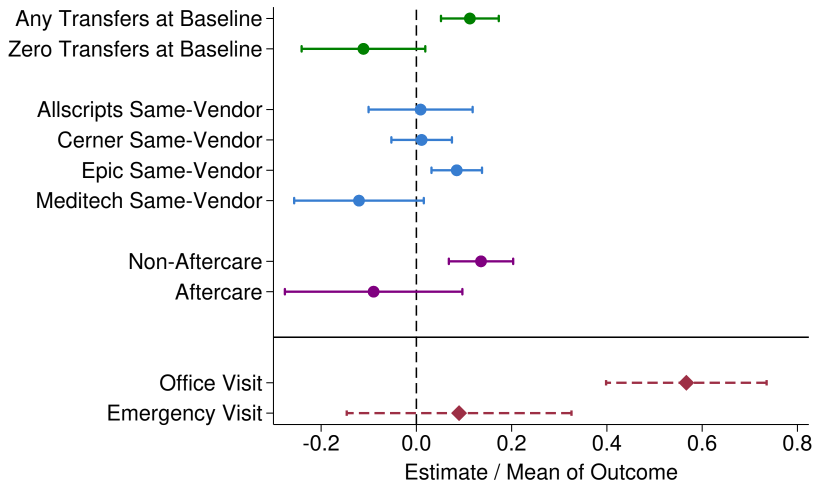
(d) Fraction of Referrals

Notes: This 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 4.2 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.

for the levels of transfers or referrals. While there appear to be slight pre-trends for the fractions specification, the pre-treatment relative time estimates for fraction of transfers are not jointly significant and there is a clear deviation in the trend at the moment of treatment for fraction of referrals.

share zero transfers that year, while 52% of the referrals sample share zero referrals. Removing these zeros from the samples would increase the average pre-period number of transfers to 11.6, fraction of transfers to 0.131, number of referrals to 94.2, and fraction of referrals to 0.0279. This sample definition, though, would require selecting on an outcome (i.e., any flows).

Figure 6: Treatment 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 4.1). 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.

Similar to our direct effect event studies, these event studies for patient flows display increasing treatment effects over relative time. As before, this pattern likely reflects the multi-year EHR vendor rollout across facilities and gradual learning of the benefits of interoperability with new same-vendor hospitals. Notably, while the direct effect event studies show immediate changes in patient outcomes, the patient flow event study estimates only become statistically significant one to two years post-treatment. This difference in treatment effect timing suggests that changes in patient composition are not driving the direct effect results observed in the previous section. We discuss this further in Section 4.3.

Heterogeneity Figure 6 presents treatment effect heterogeneity along a series of hospital, EHR vendor, and patient characteristics. This figure shows that the total effect of interoperability on shared transfers is entirely driven by a strengthening of pre-existing relationships between hospitals rather than the start of new ones (green dots).[49] Effects are also largest (and most precise) for hospital pairs that switch to both having Epic as their EHR vendor (blue dots). Further, effects are larger for transfer patients that are not transferred for aftercare ser-

[49]This heterogeneity result is robust to different market definitions (e.g., HSA, located within 100 km, located within 50 km, located within 15 km), indicating that the null result on the extensive margin of patient flow relationships is not a byproduct of using HRR as the market definition.

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, year fixed effects, and standard errors clustered at the hospital 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. * p<0.10, ** p<0.05, *** p<0.01.

vices (i.e., those transferred to receive more advanced care) than for those transferred for after-care (purple dots). For referrals, the figure shows that effects vary by type of referred service. In particular, effects are large and precise for office visit referrals but small and insignificant for emergency visits. This difference may be due to providers caring less about interoperability and more about other factors (such as availability, distance, etc.) when deciding where to send their patients for additional care in emergent situations.

Total Flows These estimated treatment effects may represent a reallocation of patient flows across hospitals or an increase in total patient flows following hospital-level EHR vendor switches. To differentiate between these two explanations, we run a hospital-level difference-in-differences regression around hospital EHR vendor switches where the outcomes of interest are total patient flows both to and from the switching hospital. Additional details of this specification are discussed in Appendix Section D.2.2. Table 4 displays the estimates from these regressions. We find that switching EHR vendors does not affect the total number of transfers a hospital either sends or receives, nor does it affect the total number of referrals a hospital sends. Switching vendors does appear to increase the total number of referrals a hospital receives by 8.8% of the mean, a marginally statistically significant effect that is entirely driven by hospitals who switch to Epic (Appendix Figure D5). These results are robust to taking the log of the outcomes and adding a variety of control variables, and the event study versions show no evidence of pre-trends. Overall, we interpret these findings as evidence that our hospital pair-level results are largely due to a reallocation of patients across hospitals instead of an increase in the extensive margin of transfers or referrals.

Magnitudes To further comprehend the magnitude of this reallocation of patients across hospitals, we conduct simple back-of-the-envelope calculations of the number of patient flows that are reallocated due to EHR vendor switches observed in our sample period. Consider that after a hospital switches EHR vendors it now has on average 7.3 other hospitals with the same vendor in its HRR and 21.7 other hospitals with the same vendor and shared referrals at baseline. Multiplying these numbers by our OLS same-vendor treatment effect estimates suggests that, when a hospital switches EHR vendors, it on average sends approximately 3% additional transfers and 3% additional referrals to other hospitals who now have the same vendor.[50] 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 almost 100,000 Medicare patient transfers and 15 million Medicare patient referrals are reallocated across hospitals over our sample due to EHR vendor switches.[51] Considering the average costs of cross-hospital transfers and referrals, these changes in patient flows also reallocated $7.3 billion of Medicare spending across hospitals during this 15 year period, or approximately $0.5 billion each year.[52,53]

4.3 Threats to Validity

We now address potential confounding factors in using hospital pair-level variation in same-vendor status for this section and the previous, describing these issues and our strategies to mitigate them.

Patient Composition The increase in shared patient flows between hospital pairs gaining the same EHR vendor raises concerns about possible changes in patient composition biasing our estimates of the direct effects of interoperability on patient outcomes in the previous sec-

[50] For transfers, we multiply the estimated treatment effect on fraction of transfers, 0.0045, by 7.3. For referrals, we multiply the estimated treatment effect on fraction of referrals, 0.0014, by 21.7.

[51] For transfers, the calculation is 7.8 million in the 100% sample multiplied by 0.4 and 0.03, which equals 93,600. For referrals, the calculation is 186 million in the 20% sample multiplied by 5, and then that resulting number multiplied by 0.4 and 0.04, which equals 14.9 million.

[52] An average transfer results in $67,589.50 in Medicare charges at the receiving hospital, while an average referral results in $67. Multiplying these averages by the total number of patients reallocated results in $6.33 billion for transfers and $0.998 million for referrals.

[53] These back-of-the-envelope calculations are limited and likely underestimate the effect of EHR system interoperability on patient flows. First, these calculations assume constant treatment effects across hospitals of different transfer sizes, which we show in the previous heterogeneity analyses is not true; instead, we find larger effects for bigger hospitals who also send larger numbers of transfers or referrals. Taking into account this heterogeneity would only increase the total number of patient flows reallocated due to EHR vendor switches. Second, the calculations are limited to patient flows for Traditional Medicare patients only. If treatment effects are similar for patients with other types of health insurance, the numbers of affected flows would rise significantly.

tion. However, we find no evidence supporting this concern. Appendix Table D1 demonstrates the robustness of our main direct effect results across various specification checks, including adding controls for observable patient characteristics (i.e., demographics, baseline health measures, and diagnosis codes) and even patient-level fixed effects. Estimates remain nearly identical with patient controls. While precision decreases for images and tests, our findings on transfer patient charges and referral patient readmission rates persist even with patient-level fixed effects. This robustness suggests that selective patient reallocation following EHR vendor switches is not biasing our estimates of the direct effects of interoperability on patient outcomes.

Other Changes in Hospital and Market A potential concern with our estimates of the effects of sharing the same EHR vendor on both shared patient outcomes and shared patient flows is that EHR vendor changes may coincide with other changes in hospital, hospital pair, or market-level characteristics. While our main empirical strategy may not account for all time-varying factors, our main estimates demonstrate remarkable stability across various robustness checks controlling for many such factors. For patient flows, Appendix Table D8 displays static difference-in-differences estimates as described in Equation 4.1 without any time-varying controls as well as those from additional specifications that test the stability of the main estimates to: (1) adding market by year fixed effects, (2) adding hospital-level controls for beds, employees, ownership, and subsequently a variety of indicators for capabilities, (3) adding hospital sender by year fixed effects and hospital receiver by year fixed effects, (4) clustering standard errors at the HRR-level, and finally (5) further restricting the sample to observations in which neither hospital in the pair switches hospital systems (i.e., changes owners) over the course of the sample period.[54] Across all specifications, we see a positive and statistically significant treatment effect. Appendix Table D2 implements similar robustness checks for our patient outcomes analyses in the previous section and shows consistently negative and statistically significant results.

The patient flows specification with hospital by year fixed effects for both the sender and

[54]Adding market by year fixed effects controls for any market-level time-varying factors that may be causing bias. Controlling for current hospital bed size deals with the concern that the number of transfers mechanically increases when beds increase and that bed size may be correlated with same-vendor status (e.g., growing hospitals are more likely to adopt Epic). Controlling for other hospital characteristics attempts to control for other factors that could be changing internally in a hospital when that hospital switches EHR vendors, and including hospital by year fixed effects for both the sender and the receiver even more directly addresses this concern. Clustering standard errors at the market level deals with concerns that our standard errors are incorrect due to potential spillovers within a market. Dropping observations in which at least one of the hospitals in the pair switches systems (i.e., changes owners) belays concerns that hospitals change EHR vendors at the same time as they change systems and thus that our results are driven by system switches rather than vendor switches.

receiver is similar to the gravity equations that are common in the trade literature (Anderson and Van Wincoop, 2003); following this literature, Appendix Table D9 shows that our flows results are further robust to estimation using Poisson Pseudo Maximum Likelihood (PPML; Silva and Tenreyro, 2006) to deal with frequent zeros in bilateral patient flows between hospital pairs.[55] Our results are also robust to estimation following Callaway and Sant'Anna (2021) (Appendix Figure D6) and to not weighting the observations (Appendix Figure D7).[56] We also find treatment effects of similar magnitude but opposite sign when looking at the effect of hospital pairs *losing* the same EHR vendor (see Appendix Section D.2.4 for more details).[57]

Switching Vendors vs. Gaining Same Vendor It is further possible that our estimates on patient outcomes and patient flows are the result of hospitals switching EHR vendors rather than hospitals switching to the same EHR vendor. To investigate this concern, Table 5 presents the results of a placebo test on control hospital pairs that change vendors without gaining or losing same-vendor status (details are discussed in Appendix Section D.2.5). Across all types of patient flows, this test yields statistically insignificant and small estimates that are less than one-tenth of the magnitude of our estimated effects from having the same vendor. Appendix Table D3 does the same placebo test for shared patient outcomes, showing statistically insignificant and small placebo estimates for most outcomes that are less than one-fifth of the magnitude of our estimated 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. In total, these placebo results further suggest that our same-vendor estimates are not driven by unobserved hospital- or hospital-pair-specific changes from vendor switches (e.g., disruptions from changing technologies) but rather by changes in same-vendor status.

Endogeneity, Measurement Error, and 2SLS Endogeneity may be biasing our event study estimates because whether any two hospitals share the same EHR vendor is the result of hospital choices. Particularly for patient flows, one hospital might intentionally switch to the same vendor as another with the aim of increased patient sharing, thereby introducing selection on gains that could challenge the external validity of our results. In the opposite direction, one hospital may choose not to have the same vendor as another because it is worried about losing

[55] Our results are also robust to using logs, but we do not prefer this method due to the abundance of zeros.

[56] When calculating the Callaway and Sant'Anna (2021) event studies, we use a universal base year rather than the default varying base year. This makes the Callaway and Sant'Anna (2021) pre-period estimates comparable to the standard event study pre-period estimates.

[57] Asymmetry in treatment effects are possible, which is why we separately evaluate the effect of gaining the same EHR vendor and losing the same EHR vendor. See Appendix Section D.2.4 for more details.

Table 5: 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 from said switch as described in Equation D.4. 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.

patients to that hospital if its patient records become more interoperable. Alternatively, two hospitals may seek to increase their patient flows and thus opt for a series of pair-specific changes, whereby an unobservable change serves as the catalyst for observed increases in shared patient flows rather than the simultaneous change in same-vendor status. The direction of this endogeneity bias is thus unpredictable. While several findings in the preceding sections may mitigate these concerns, it remains plausible that endogeneity could bias our event study estimates in ways that are difficult to detect.[58]

Additionally, measurement error in our setting may also be introducing bias. As discussed previously, the exact year in which a hospital switches EHR vendors is difficult to define. The AHA IT Survey—our primary data source—defines the switch year as the year in which the new EHR system is used by the majority of a hospital's patients, while HIMSS—our secondary data source—defines it as the year in which the new system is live and operational, likely for all patients. Appendix Section D.2.8 illustrates the potential for such measurement error to attenuate our results by instrumenting for whether a hospital pair has the same vendor in one survey (e.g., HIMSS) with whether the hospital pair has the same vendor in the other survey (e.g., AHA IT). These 2SLS estimates are on average twice the magnitude of the OLS estimates, suggesting that measurement error is indeed significantly attenuating our estimates

[58]These findings include the robustness of our estimates to a plethora of additional controls, the absence of statistically significant pre-trends in the event studies, and the discussed secondary role often played by interoperability in a hospital's decision to switch vendors. Further, Appendix Section D.2.6 shows that when a hospital h' switches EHR vendors to now have the same vendor as hospital h, h starts sending more patients to h'. The receiver in this pair switching is perhaps less endogenous than the sender switching, as the sender is not making the EHR vendor choice.

of the effect of interoperability on patient flows and possibly patient outcomes.

To address these two concerns, we construct an instrument for hospital EHR vendor choice. The main intuition behind our instrument is that hospitals face pressure to use the same EHR vendor as other hospitals in their hospital systems. For each hospital h, we identify all other hospitals that are in the same system as h but located outside of h's market (HRR_h), and we calculate the shares of beds among these hospitals that use each EHR vendor e in year t. We denote these shares as the "system pressure" for h to have vendor e in year t, $\text{SP}(ht, e)$. By limiting these leave-out calculations to hospitals outside of each focal hospital h's own market, these measures eliminate any potential endogeneity in EHR vendor choice due to local market pressure on the focal hospital.

We then re-estimate Equations 3.1 and 4.1 instrumenting for a hospital sender-receiver pair's same-vendor status with four instruments: $\text{SP}(ht, e) \times \text{SP}(h't, e)$ for $e \in$ {Epic, Cerner, Meditech, Allscripts}. That is, each instrument is the interaction of the sender hospital h's System Pressure to have a certain EHR vendor e in year t and the receiver hospital h''s System Pressure to have that same EHR vendor e in year t. Using four instruments allows for more flexibility in estimation, though results are similar when summing the four into one single instrument. Our instruments are only defined for the 32.6% of hospital sender-receiver pair-year observations in which both hospitals belong to multi-hospital systems that span more than one HRR.[59] Further, to reduce potential reverse causality in the first stage (e.g., focal hospital h uses Epic, which pressures other hospitals in h's system outside of h's market to also use Epic), we further limit the sample to hospital sender-receiver pairs in which both sender and receiver are not the largest hospitals in their respective systems. Appendix Section D.2.7 discusses this 2SLS strategy in further detail, including the estimating equations, the identification assumptions, and summary statistics for the instrument sample.

Table 6 displays the results of this 2SLS estimation for fraction of transfers as the second stage outcome, with Column 1 showing the first stage estimates, Column 2 the reduced form estimates, Column 3 the 2SLS estimates using the four instruments for same-vendor status, and Column 4 the corresponding OLS estimates for the same sample.[60] Overall, we find a large, positive, and statistically significant relationship between same-vendor status and shared patient flows when instrumenting for same-vendor status with our System Pressure instruments.

[59]Over our sample period, 65.1% of hospital-year observations are in a multi-hospital system, and 55.5% of hospital-year observations are in a multi-hospital system that has hospitals spanning more than one HRR. As a result, 47.2% of hospital sender-receiver pair-year observations are in a multi-hospital system, and 32.6% of hospital sender-receiver pair-year observations are in a multi-hospital system that has hospitals spanning more than one HRR. This instrument requires us to limit our sample to that 32.6%.

[60]Similar results for fraction of referrals are shown in Appendix Table D13. Results for levels are shown in Appendix Tables D14 and D15.

Table 6: Instrument for Same Vendor - Fraction of Transfers

	Same EHR Vendor	Fraction of Transfers		
	(1)	(2)	(3)	(4)
	FS	RF	2SLS	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.7. Column 2 presents reduced form results of the effect of the four instruments on fraction of transfers as given by Equation D.8. 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 4.1. * p<0.10, ** p<0.05, *** p<0.01.

The 2SLS estimates are approximately 2.4 (3.6) times the size of the OLS estimates for fraction of transfers (referrals), which is likely due to measurement error attenuating our OLS estimates as described in Appendix Section D.2.8. Similarly, the 2SLS estimates are approximately 3.1 (3.2) times the size of the OLS estimates for transfer patient charges (referral patient hospital admission rates).[61] Overall, we take these 2SLS estimates as further evidence that there is indeed a positive causal relationship between same-vendor status and shared patient flows and outcomes.

[61] See Appendix Tables D16 and D17. The 2SLS estimates for shared patient images and tests are noisy.

5 Evidence of Allocative Effect on Patient Outcomes

The previous section shows evidence that variations in EHR system interoperability lead to the reallocation of patient flows across hospitals. We now analyze whether this reallocation in turn affects patient outcomes.

5.1 Empirical Strategy

When a hospital switches to a new EHR vendor, that hospital increases its patient flows with other hospitals that have its new vendor and decreases its patient flows with those that have its previous vendor. This choice to reallocate patients from old-vendor to new-vendor facilities may affect patient outcomes by—for example—sending them to better or worse quality hospitals than they would have gone to before. The literature has long documented that hospitals are imperfect substitutes and that hospital quality measures are causal; patients admitted to worse quality hospitals do indeed experience worse health care outcomes than those admitted to better quality hospitals (e.g., Doyle et al., 2019; Chandra et al., 2023). Though to our knowledge no one has yet shown a similar causal relationship for the subset of patients who are transferred or referred from another hospital, it is quite plausible that causality holds for this group. Thus, as long as interoperability does not only reallocate patients between hospitals that are very close substitutes and does not only reallocate patients who are receiving low-risk or routine care for which hospital quality will have little impact, the reallocation of patients from old-vendor to new-vendor facilities may indeed change patient outcomes.

Such an allocative effect on patient outcomes will depend crucially on the comparative quality of recipient hospitals that receive more versus fewer patients after the sending hospital switches vendors. The average sign of such quality differences is non-obvious; some hospitals may switch to vendors used by better quality hospitals in their markets, while others may switch to vendors used by worse quality hospitals. To allow for such heterogeneity, we analyze the allocative effect of EHR vendor switches on patient outcomes by running a difference-in-differences specification that interacts an indicator for whether the sending hospital has yet switched vendors with a measure of the change in characteristics of other local same-vendor hospitals pre- versus post-switch. This design allows us to ask whether, for example, patients experience improved health outcomes when their sending hospital switches to a vendor with a local EHR vendor network comprised of higher-quality hospitals than before—i.e., following the terminology of Chandra et al. (2023), to determine the "predictive validity" of changes in local vendor network characteristics:

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

Here, Y_{iht} is the outcome for patient i who was transferred or referred from hospital h to another hospital in year t, $\mathbf{1}_{(\text{Post-Switch}_{ht})}$ is an indicator for whether the sending hospital h has yet switched EHR vendors, α_h and γ_t are hospital and year fixed effects, respectively, and $X_{it,ht,mt}$ are patient, sending hospital, and market-level controls such as a count of patient chronic conditions, the number of sender hospital beds, and market by year fixed effects. We limit the sample to patients sent from hospitals that either switch vendors once or never over the sample period, and we cluster standard errors at the hospital level.

$\Delta\text{Network}_{Yh}$ represents the raw change in the average baseline characteristics of other same-vendor hospitals in h's market (HRR_h) after h switches vendors, where the characteristic Y is the same as the outcome variable (e.g., if the outcome is distance, this term is change in average distance to other same-vendor hospitals in own market after a vendor switch). We calculate these averages by using baseline data from 2005 and weighting all other local same-vendor hospitals by their bed sizes. We further limit the set of other local same-vendor hospitals to those with which h has some baseline shared patient flows relationship, given that the heterogeneity discussed in the previous section suggests that this group is the relevant choice set for h. This change term is not time-varying and thus does not appear on its own. Appendix Figure D11 displays the wide distributions of $\Delta\text{Network}_{Yh}$ across hospitals and our four main characteristics of interest: distance to focal hospital, baseline charges for transfer/referral patients, and baseline 30-day readmission and 60-day mortality rates for transfer/referral patients. These distributions are centered around zero, meaning that switching hospitals in our sample are equally likely to switch to vendors used by, for example, closer local hospitals and farther local hospitals.

Not all patients are reallocated across receiving hospitals following a sending hospital vendor switch. To focus on the subset of patients most likely to experience reallocation, we further limit the sample to patients who are transferred or referred to hospitals that use the same EHR vendor as their sending hospital. For example, if hospital h initially uses Cerner and switches to Epic within the sample period, we look at outcomes for patients sent to other Cerner hospitals during the years in which h uses Cerner and outcomes for patients sent to other Epic hospitals during the years in which h uses Epic. Appendix Figure D12 displays the wide distribution of hospital-level changes in these patients' outcomes before versus after the sending hospital switches vendors.[62,63] Appendix Figure D13 then plots the correlation of these hospital-level patient outcome changes with $\Delta\text{Network}_{Yh}$; the relatively linear correlations motivate our choice of a linear functional form for the interaction term in Equation 5.1.

Altogether, in Equation 5.1, β_1 indicates the extent to which the experienced outcomes of transfer and referral patients who are sent to other same-vendor hospitals change when the average baseline characteristics of those other same-vendor hospitals remain the same after the sending hospital switches vendors. Meanwhile, β_2 offers insights into whether changes in local vendor network characteristics pass through to corresponding changes in the experienced outcomes for patients sent to same-vendor hospitals. A statistically significant $\beta_2 > 0$ would signify such predictive validity. The magnitude of the coefficient elucidates how closely the predicted change aligns with the actual change. For example, a coefficient of 1 would imply that when a hospital switches to a vendor with a local network comprised of hospitals located 1 km farther away on average than the hospitals in the previous vendor's network, the distance traveled by that hospital's same-vendor transfer or referral patients increases by 1 km on average.[64] We expect our estimates of β_2 to be a lower bound on total pass-through due to potential measurement error in our construction of the local EHR network quality measures.

While run at the patient level, Equation 5.1 uses hospital-level variation in EHR vendors. This varies slightly from the analysis in the previous sections, which use hospital pair-level variation in having the same EHR vendor. Our main identification assumption for this analysis is that patients who are sent from hospitals that switch vendors over the sample period (e.g., treated hospitals) to other same-vendor recipient hospitals would have experienced similar trends in outcomes before versus after the sending hospital EHR switch as those patients who are sent from hospitals that do not switch vendors over the sample period (e.g., control hospitals) in the absence of such treatment.

[62] In particular, we restrict the sample to same-vendor patients sent from hospitals that switch EHR vendors once. Then for each hospital, we calculate the average outcomes for patients sent before the switch and the average outcomes for patients sent after the switch. The change is then after minus before. Hospitals are then weighted by the number of same-vendor patients.

[63] Another way to show the wide distribution of hospital-level changes in same-vendor patient outcomes before versus after the sending hospital switches vendors is to estimate Equation 5.1 without the interaction term. Table D21 shows the results of such a specification. Generally, the estimated coefficients have large standard errors, which is suggestive of a wide distribution of changes.

[64] Chandra et al. (2023) term this interpretation of an estimated magnitude of 1 as "forecast unbiased," meaning that the gain in outcomes is exactly equal to that predicted by the change in network characteristics.

5.2 Results

Table 7 shows the results of estimating Equation 5.1 for four outcomes for transfer and referral patients: distance traveled (in kilometers), total charges (in real 2019 US 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, and readmission and mortality rates of the local EHR vendor network have predictive validity. Hospitals that switch to a local vendor network comprised of farther away, more expensive, or lower readmission or mortality rate hospitals than before see a larger effect of the switch on their patients' distances traveled, charges incurred, readmissions, or mortality, respectively. For example, the estimates in Column 2 of Panel A tell us that when a hospital switches to a vendor with a local network comprised of hospitals with mean baseline transfer patient charges $1000 higher than those of its old local vendor network, same-vendor-sent transfer patients at that hospital experience an additional $425 in charges compared to similar patients from hospitals whose local vendor networks remain price-stable after a vendor switch.[65]

Robustness There are two primary concerns with this difference-in-differences strategy. First, the characteristics of patients sent to same-vendor hospitals may change after sending hospitals switch EHR vendors. To address this concern, Appendix Table D22 demonstrates that our estimates are robust to the inclusion of patient-level controls such as age, Charlson Comorbidity Index, and indicators for 27 different chronic conditions. This robustness to observable patient characteristics provides some reassurance that our results are not driven by changes in other patient characteristics.

Second, hospital EHR vendor switches may coincide with other hospital-level changes that could affect patient outcomes. To address this concern, we test the validity of the underlying

[65]We have a few hypotheses for why the estimated coefficients on the interaction term are not equal to 1. First, for our last three outcomes of charges, readmissions, and mortality, we do not currently risk-adjust our baseline measures for each hospital and thus ΔNetwork is not risk-adjusted. Chandra et al. (2023) show that not risk-adjusting will attenuate the estimate of predictive validity. Relatedly, by using baseline characteristics we do not allow for hospitals to improve or worsen over time, meaning that patients might be encountering better/worse conditions than our baseline measures capture. Finally, ΔNetwork simply compares average (bed-weighted) characteristics of new-vendor hospitals to those of old-vendor hospitals. Since switching hospitals may not equally redistribute patients across new-vendor hospitals, such averages may not fully incorporate the changes in hospital conditions that patients face when redistributed. For example, a switching hospital may send all of its transfer patients to the best-quality new-vendor hospital in its choice set. Then, even if average quality of the local EHR vendor network as a whole does not change, if quality measures are predictive then these patients' outcomes will improve. This may explain why some of the estimates of β_1 are statistically significant in Table 7.

Table 7: 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)
x Change in Network	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

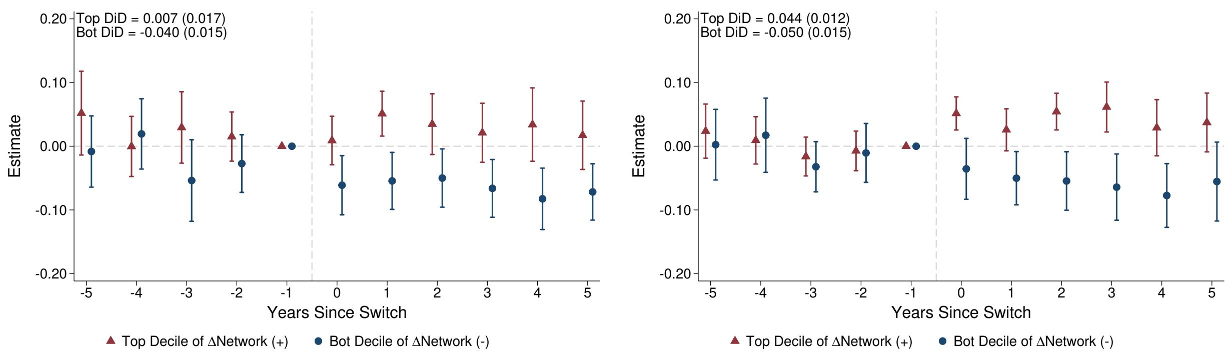
Notes: Each column is a separate patient-level difference-in-differences regression of the effect of a hospital switching EHR vendors on outcomes experienced by transfer (Panel A) and referral (Panel B) patients sent from the switching hospital to another same-vendor recipient hospital as specified in Equation 5.1. All regressions include hospital fixed effects, year fixed effects, and market-year fixed effects. 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.

parallel trends assumption by estimating an 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 (which occurs in year s_h), we bin the relative year endpoints ($r \geq 6$ and $r \leq -6$), and we exclude $r = -1$. All other parameters are the same as in Equation 5.1. This event study specification allows us to visually check for the presence of pre-trends that may suggest a violation of our parallel

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: These 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 referral patients and the outcome is 60-day mortality rates. Estimates for the top decile of $\Delta\text{Network}_{Yh}$ are in red; estimates for the bottom ("bot") decile of $\Delta\text{Network}_{Yh}$ are in blue. Bars show 95% confidence intervals. Standard difference-in-differences estimates for each group are also given (labeled "DiD").

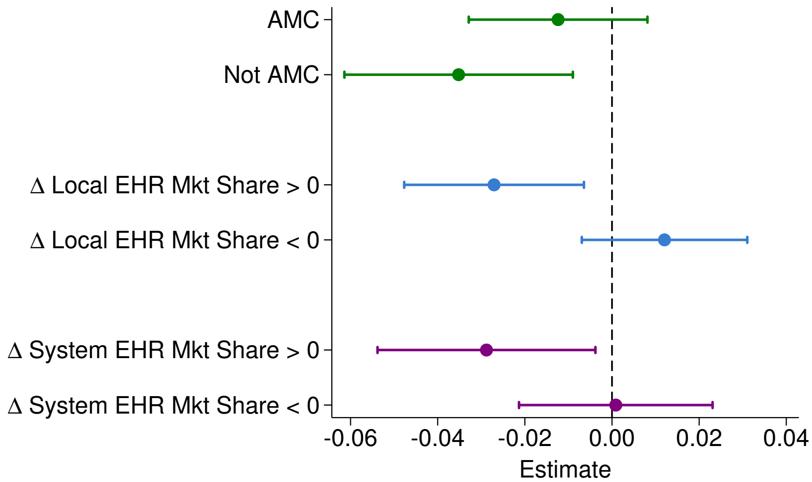
trends assumption. To allow for heterogeneity by $\Delta\text{Network}_{Yh}$, we split patients into deciles of $\Delta\text{Network}_{Yh}$ and run Equation 5.2 separately by decile group.

Figure 7 plots event study estimates from Equation 5.2 for the top decile group (red; e.g., patients sent from hospitals that switch to local vendor networks comprised of hospitals with much *higher* baseline readmission and mortality rates than before) and for the bottom decile group (blue; e.g., patients sent from hospitals that switch to local vendor networks comprised of hospitals with much *lower* baseline readmission and mortality rates than before). 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.[66] Patients sent from hospitals that switch to vendors used by much worse quality hospitals in their local areas are much worse off than before, while those sent from hospitals that switch to vendors used by much better quality hospitals in their local areas are much better off. In other words, when a hospital switches to a better local vendor network, its patients' outcomes improve; when a hospitals switches to a worse local vendor network, its patients' outcomes worsen.[67]

[66] Appendix Table D23 also shows that patients in these two deciles are balanced on baseline characteristics.

[67] Recall that we found the treatment effect of gaining the same EHR vendor on patient flows increases over relative time after a change in same-vendor status for a hospital pair. Our finding here of an immediate and stable treatment effect of switching EHR vendors on patients' outcomes is not inconsistent with that previous result because the patient outcomes specifications limit the sample of patients to same-vendor patients. Thus, the effect on patient outcomes should be immediate. For example, a hospital switches to a vendor used by hospitals on average located 1 km farther away than hospitals using their old vendor, the first patient who is sent to a same-vendor hospital after the switch should travel 1 km farther away on average (anything different for distance is due

Figure 8: Predictive Validity Heterogeneity: Transfer Patient Readmissions



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 outcome is 30-day hospital readmissions for same-vendor transfer patients. 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 30-day readmission rates for transfer patients. The estimates show the effect of this switch on same-vendor transfer patients' 30-day readmission rates, grouped by hospital or network characteristics. "AMC" indicates the sending hospital is an Academic Medical Center. "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.

Heterogeneity Figure 8 shows heterogeneity in the effect of switching to a higher-quality local EHR vendor network across hospital and network characteristics. Among hospitals that switch to vendors used by higher-quality hospitals (defined as those with lower baseline readmission rates for transfer patients), we find greater switch effects for patients transferred from non-academic medical centers (non-AMCs) compared to those from AMCs. These allocative benefits are more pronounced when hospitals switch to EHR vendors that are more widely used in either their local market or health system. Thus, patients gain most from reallocation when hospitals align their vendor choice with surrounding institutions. Appendix Figure D14 also suggests that switching to a better local EHR vendor network is particularly beneficial for patients with poorer baseline health (e.g., higher Charlson Comorbidity Index, heart conditions, or diabetes).

to the sending hospital not choosing an "average" hospital in their same-vendor choice set; anything different for other measures could also be due to patient characteristics or a lack of current relevance for baseline measures). As more patients are also sent to new same-vendor hospitals over relative time, those patients should also travel 1 km farther away on average and thus the average treatment effect is still 1 km.

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. To quantify these trade-offs, in this section we develop and estimate a structural model of patient flows between providers. Our model expands on standard hospital demand frameworks (e.g., Ho, 2006; for a broader review, see Handel and Ho, 2021) by adding interoperability to the utility function. This approach allows us to quantify interoperability's relative importance compared to other factors in transfer and referral decisions. We then use these model estimates to quantify the welfare gains from reducing interoperability frictions in the next section.

6.1 Setup

Consider a hospital h in market (HRR) m that has a patient p who requires a transfer (referral) to another hospital h' in year t. We assume that both the timing and necessity of the transfer (referral) are exogenous, meaning the patient must be sent to a different hospital and this is not a decision of whether to transfer (refer). We also assume that all hospitals always accept transfers and referrals (i.e., recipient hospitals are passive receivers).[68] In each year t, hospital h has a set of patients $\mathcal{P}_{ht}$ needing to transfer (refer). Hospital h will choose which other hospital $h' \in m \setminus \{h\}$ to send each patient $p \in \mathcal{P}_{ht}$ in order to maximize expected utility. The outside option is to send the patient to a hospital located outside of m. Expected utility is given by:

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

While modeled as the sending hospital's choice, the transfer (referral) decision is in practice a joint decision between patients and hospitals and thus utility is a combination of hospital and patient welfare.[69] Here, $\iota_{e_{ht},e_{h't},t}$ is the level of interoperability between the EHR vendors used

[68] While patient transfers often involve joint decisions between sending and receiving hospitals, our passive receiver assumption is particularly appropriate in emergency cases covered by Emergency Medicine Treatment and Labor Act (EMTALA). Evidence suggests hospitals usually accept transferred patients even in non-EMTALA cases, despite some instances of "patient-dumping" (Kindermann, Mutter, Cartwright-Smith, Rosenbaum, and Pines, 2014).

[69] For simplicity of notation, we attribute the decision to the hospital. Our model does not take a position on the nature of the joint decision-making process involved.

by hospital h and hospital h' in year t. Interoperability—or the lack thereof—may directly affect utility as both a hassle cost and a direct effect on patient health outcomes. $\mathbf{1}_{hh'\text{always same system}}$ is an indicator for whether hospitals h and h' are always in the same hospital system, which we include in the utility function to capture financial incentives to keep patient flows within systems.[70] $d_{hh'}$ is the distance between hospitals h and h', included to capture a distaste for travel. We proxy for quality with receiving hospital fixed effects $\xi_{h'}$. To capture variation in the tendency to send patients outside of the market across sending hospitals, markets, and years, we also include sending hospital fixed effects ξ_h and market-by-year fixed effects ξ_{mt}. $\nu_{hh't}$ is an unobservable structural error.

The error term $\bar{\epsilon}_{hh'pt}$ is independent and identically distributed according to the Type I Extreme Value (i.e., Gumbel) distribution. $n_h(h')$ represents the group (or "nest") to which the receiver hospital h' belongs, 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 (i.e., Gumbel) distribution. Parameter ρ characterizes the correlation of utilities that a patient experiences among the receiving hospitals in the same nest.[71] We allow for two nests of receivers: hospitals located inside the same market as the sending hospital (i.e., an inside option) and hospitals located outside the market (i.e., the outside option). We choose this nesting structure because we believe the decision to send a patient outside of the market is quite different from the decision to send a patient to a hospital within the same market. In sum, this utility function allows transfer (referral) utility to depend on interoperability, same-system status, distance, and hospital quality.

Define $\delta_{hh't} = \theta_1 \iota_{e_{ht},e_{h't},t} + \theta_2 \mathbf{1}_{hh'\text{always same system}} + \theta_3 d_{hh'} + \xi_h + \xi_{h'} + \xi_{mt} + \nu_{hh't}$ as the mean utility in year t of hospital h sending a patient to hospital h'. For each t, integrating over the Gumbel error gives the share $s_{hh't}$ of transfer (referral) patients that hospital h sends to hospital h' as:

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

This ultimately yields a linear estimating equation. We estimate this model in two steps. First, we directly estimate vendor-year specific levels of interoperability. Second, we incorporate these interoperability measures into our demand model and estimate the remaining parameters with nested logit using the PyBLP program (Conlon and Gortmaker, 2020).

[70] Changes in hospital system status are relatively infrequent, which makes identifying a coefficient on an indicator of whether two hospitals are in the same system using variation in system membership over time challenging.

[71] For example, when $\rho = 1$, all patients stay within their chosen nest. When $\rho = 0$, the nested logit simply collapses to standard logit.

6.2 Part One: Interoperability Levels

In previous sections, we assumed that internal, within-vendor interoperability (between hospitals with the same EHR vendor) was higher than external, across-vendor interoperability (between hospitals with different vendors), and we used a same-vendor indicator as a proxy for $\iota_{e_{ht},e_{h't},t}$. However, evidence from Section 4 showing that hospital pairs both using Epic have a larger increase in shared patient flows than hospital pairs both using any other major vendor suggests that interoperability may vary across vendors. To address and utilize this variation, we now develop a method to quantify vendor-year specific interoperability levels from hospital-level reports of interoperability in the AHA IT Survey.

Estimation Strategy For tractability, we assume that $\iota_{e_{ht},e_{h't},t} = \iota_{e_{ht},-e_{ht},t}$ if $e_{ht} \neq e_{h't}$. That is, we assume that each EHR vendor has two dimensions of interoperability: (1) internal, within-vendor, interoperability $\iota_{e_{ht},e_{ht},t}$ (i.e., the ease with which data is exchanged between providers using the same vendor; e.g., Epic to Epic), and (2) external, across-vendor, interoperability $\iota_{e_{ht},-e_{ht},t}$ (e.g., Epic to non-Epic).[72] We can then think of the reported interoperability ι_{ht} of hospital h in year t using EHR vendor e_{ht} as a simple function of $\iota_{e_{ht},e_{ht},t}$, $\iota_{e_{ht},-e_{ht},t}$, and the fraction $r(ht, e_{ht})$ of patients hospital h receives in year t that come from other hospitals using the same EHR vendor:

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

The logic behind this decomposition is as follows. If hospital h uses Epic in year t and $r(ht, e_{ht}) = 1$, i.e., h receives all of its patients from other Epic hospitals in that year, then h's reported interoperability level in year t is entirely indicative of how good Epic is at exchanging data with other Epic users in that year. In the opposite case, suppose h uses Epic in year t but now $r(ht, e_{ht}) = 0$, i.e., h receives all of its patients from non-Epic hospitals in that year; then h's reported interoperability in year t is entirely indicative of how good Epic is at exchanging data with non-Epic users in that year. Moreover, we would expect that the more often h sends its patients to Epic hospitals rather than non-Epic hospitals, the better ι_{ht} is as a predictor of Epic's within-vendor interoperability than Epic's across-vendor interoperability. We can thus recover vendor-specific interoperability levels $\left\{\iota_{e,e,t}, \iota_{e,-e,t}\right\}_e$ using just hospital-level reports of interoperability and data on shared patient flows. Function f is a monotonic transformation of this linear equation, which we define more precisely below.

[72] Our assumption of a single across-vendor interoperability level for each vendor is to facilitate estimation given limited data.

Since Figure 2a shows that integration is the most difficult standard of interoperability for hospitals to achieve, we let ι_{ht} be hospital h's reported ability to integrate data in year t.[73] Beginning in 2013, the AHA IT Survey asks hospitals whether they can integrate data received electronically from outside sources without the need for manual entry, and hospitals can respond to this question with one of three answers: "yes, routinely," "yes, but not routinely," or "no." To flexibly allow for all three of these ordinal outcomes, we set function f in Equation 6.3 to be an ordered probit. Further, to increase precision, we bin interoperability levels into 2-3 year periods (i.e., 2013-2015, 2016-2017, 2018-2019). We can also control for additional variables such as whether hospital t participates in a regional health information exchange organization, though results are robust to their exclusion. This specification ultimately yields estimates of within- and across-vendor interoperability separately by vendor and time period on a scale between 0 and 1.[74]

There is a potential endogeneity concern inherent in this setting: hospitals that frequently send patients to others with the same EHR vendor may invest more in within-vendor interoperability.[75] This endogeneity would upward bias the contribution of within-vendor interoperability to reported hospital-level interoperability. Since f is nonlinear, we address this endogeneity concern using a control function approach (Wooldridge, 2015). We instrument for $r(ht, e_{ht})$ using the share z_{ht} 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 $r(ht, e_{ht})$ would be if hospital h randomly allocated its patients across all other hospital beds in its market). More details on this estimation procedure are available in Appendix Section C.2.[76]

Estimation Results Figure 9 presents estimates of across-vendor (Panel A) and within-vendor (Panel B) interoperability separately by EHR vendor and time period, with dots representing point estimates and bars the 95% confidence intervals. Three main insights emerge from these results.[77] First, across-vendor interoperability is significantly and consistently lower than

[73]This is 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, Henry, Pylypchuk, and Mpa, 2016). For the remainder of the paper, we use the word "interoperability" exclusively to mean the ability to integrate.

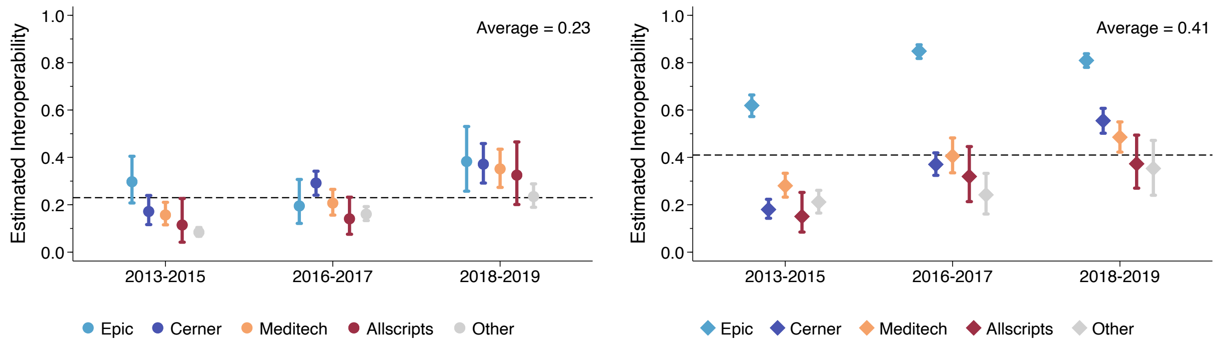
[74]Estimation does not directly yield interoperability estimates between 0 and 1, but we scale the estimates to put them in this range. Details of this estimation procedure are available in Appendix Section C.2.

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

[76]Broadly, this control function strategy involves two steps of estimation. First, we regress the endogenous variable $r(ht, e_{ht})$ on the instrument z_{ht} and save the residuals. Second, we estimate the ordered probit and include the residuals from the first step as an additional variable in this second specification. We then bootstrap over 1000 samples to produce valid confidence intervals.

[77]Similar patterns emerge when we use a simple ordered probit without the control function, when we impose linearity and estimate with 2SLS, and when we impose linearity and estimate with OLS.

Figure 9: Estimated Across- and Within-Vendor Interoperability Levels



(a) Across-Vendor

(b) Within-Vendor

Notes: These figures plot ordered probit estimates of across-vendor (Panel A) and within-vendor (Panel B) interoperability, separately by EHR vendor and time period, following the estimation strategy described by Equation 6.3 with more detail in Appendix Section C.2. Dots represent point estimates while bars show 95% confidence intervals, which are calculated using 1000 bootstrapped samples. The colors in both panels consistently represent vendors, e.g., blue is always Epic. Data is pooled for 2013-2015, 2016-2017, and 2018-2019, i.e., these estimates represent average interoperability over these year bins.

within-vendor interoperability for all EHR vendors and time periods.[78] This result supports our use of same-vendor status as a proxy for interoperability between hospital pairs 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.[79] 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. The wedge between the two dimensions has thus grown over time.[80]

[78] Appendix Figure D16 shows a clearer picture of statistical significance by plotting average levels of within-vendor and across-vendor interoperability across all years for each vendor on the same graph.

[79] See Appendix Figure B5. Note also that after Epic rolled out its Care Everywhere network, some of Epic's competitors (Cerner, McKesson, Allscripts, athenahealth, Greenway, and RelayHealth) formed the CommonWell Health Alliance to set common interoperability standards and facilitate data exchange across themselves. This multi-vendor network was established in 2013 but rolled out slowly to health centers. A breakdown by year shows that the share of Cerner hospitals reporting multi-vendor network participation rises from less than 30% in 2016 to more than 70% in 2019. This reflects the increase in CommonWell participation among Cerner hospitals over time and likely contributes to Cerner's increase in both across- and within-vendor interoperability over time.

[80] With these estimates, we can re-estimate our reduced form analysis 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

Table 8: Model Parameter Estimates

	Baseline	**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 Part Two: Hospital Demand

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).[81] We calculate transfer shares for each sending hospital as in Section 4 and define the outside option as transferring to a hospital outside the sending hospital's market (i.e., to hospital $h' \notin m$, where m is the HRR). We instrument for interoperability using the instrument described in Section 4 and estimate the model using Generalized Method of Moments (GMM).[82] The interoperability coefficient is identified from hospitals switching EHR vendors and from changes in interoperability levels over time.

Table 8 presents the model parameter estimates for our baseline specification in Column

interoperability on shared patient flows. See Appendix C.2 for more details.

[81]We can estimate the model on referrals, but defining the market is more challenging. Appendix Table D26 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.

[82]Recall that this instrument is only defined for hospital sender-receiver pairs in which both hospitals belong to multi-hospital systems spanning across multiple HRRs. To include all hospital pairs in our analysis, we create a second instrument equal to one if the first, primary, instrument is defined and zero otherwise. We then reset the first instrument to zero where undefined. Nested logit also requires an additional instrument for the endogenous within-nest share of each receiving hospital. We instrument for these shares using the number of receiving hospitals in each nest, as is common in the literature. The GMM moments are then $E[\nu_{hh't} Z_{hh't}] = 0$, where $Z_{hh't}$ are the instruments. We implement this estimation using the PyBLP software (Conlon and Gortmaker, 2020).

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 controls 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, Nevo, and Town, 2015; Raval and Rosenbaum, 2021) and a strong preference for going to other hospitals belonging to 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. 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.[83],[84] 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 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 that of quality and cost.[85]

7 Welfare Gains of Counterfactual Interoperability Improvements

7.1 Conceptual Framework

Using the estimated model parameters, we next simulate patient flows between providers under several counterfactual scenarios with modified interoperability levels. For each scenario, we simulate patient allocations across hospitals by drawing a vector of logit shocks for each patient and determining their hospital choice under the counterfactual conditions. We then

[83]The specification with hospital pair fixed effects cannot estimate a coefficient on distance (nor on 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 effects to get a sense of how much each component matters in this utility function. Doing so yields a coefficient of -0.00768 on distance.

[84]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.

[85]These estimated elasticities are robust to including receiver hospital-by-year fixed effects, which account for changes in receiver hospital characteristics (e.g., quality) over time. 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.

calculate the change in joint sending hospital and patient welfare compared to that under the data conditions. Finally, we decompose these welfare changes into two components: an allocative effect and a direct effect. The allocative effect reflects patients selecting different hospitals, while the direct effect captures the impact of improved interoperability on utility, such as reduced hassle costs and health benefits from enhanced information exchange. We express this decomposition as:

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

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 the patient in the counterfactual (i.e., the counterfactual interoperability level of the hospital the patient chooses in the counterfactual). Variables without superscripts refer to the corresponding inputs from the observed data. The allocative effect captures utility changes from patients choosing different hospitals, holding interoperability levels constant. Conversely, the direct effect measures utility changes from patients experiencing different interoperability levels, holding hospital choice constant.

7.2 Counterfactual Results

We now examine the welfare gains for patients and sending hospitals of a first-best counterfactual where all interoperability levels are maximized as well as three intermediate scenarios of increasing interoperability. Table 9 summarizes these gains, reporting changes in total welfare, decompositions into direct and allocative effects, and the shares of patients that are reallocated. We then explore the allocative effects in greater depth. Revisiting the first-best counterfactual, we find that patient reallocation and allocative welfare gains vary significantly by EHR vendors and market structure. Additionally, we demonstrate that patient reallocation affects not only patient outcomes but also receiver hospital revenue.

Welfare Gains The first row of Table 9 shows that completely eliminating the technological friction in this setting yields substantial welfare gains. 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

Table 9: 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
Perfect Within (Within = 1)	7.2%	19.1		20.6		-1.4	0.064
Epic Monopoly (All Epic)	12.9%	34.4		33.1		1.4	0.053
Minimum Standard (Min. = 0.51)	7.2%	19.1		18.5		0.6	0.032

Notes: Table displays the results of four counterfactual scenarios: (1) increase all interoperability levels to 1; (2) increase within-vendor interoperability levels to 1; (3) all hospitals switch to Epic and thus all interoperability levels are set to Epic's within-vendor level in each time period; (4) increase all interoperability levels to a minimum of 0.51, which is the patient-weighted average interoperability level in 2019. "%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 counterfactuals than in the data.

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.[86] On average, these patients are sent to hospitals that are slightly farther away but also lower cost and much higher quality.[87]

To understand how much of these first-best gains in welfare are driven by improved within-vendor interoperability levels relative to those across-vendor, the second row of Table 9 examines a counterfactual in which only within-vendor levels are maximized while across-vendor levels remain the same. This scenario achieves only 34% of the first-best welfare gains due to two factors. First, low across-vendor interoperability levels still persist, limiting the positive direct effects for patients who are sent to different-vendor facilities. Second, increasing within-

[86] This first-best scenario not only reallocates patients across hospitals inside the market but also diverts some patients from the outside option by weakly improving the choice set of hospitals in the market from increasing interoperability. Appendix Tables D27 and D28 show separate summaries of the changes in welfare for patients who choose hospitals in the market under the data conditions ("intensive patients") and for patients who choose hospitals outside of the market under the data conditions ("extensive patients"), respectively.

[87] These changes are shown in Table D29. Although our hospital demand model does not directly incorporate receiver hospital quality, Appendix Table D25 shows that our estimates of receiver hospital fixed effects are negatively correlated with both receiver hospital mortality and readmission rates. Patients are thus on average being reallocated to hospitals with higher fixed effects.

vendor interoperability levels without similarly increasing those across-vendor widens the gap between these two levels and thus increases the distortive incentive to send patients to other same-vendor hospitals. Consequently, 6.4% of patients are reallocated from different-vendor to same-vendor facilities, causing important allocative welfare losses.[88] Given the observed time trend of improving within-vendor but stagnating across-vendor interoperability levels, this scenario illustrates a possible future without any public policy intervention.

Perfect interoperability may be unrealistic due to current technological constraints. Even with universal usage of Epic EHR systems, interoperability levels would range from 0.62 to 0.85 depending on the time period. The third row of Table 9 simulates a scenario in which all hospitals switch to Epic systems, thus raising interoperability to the maximum levels observed in the data. This achieves 61% of the first-best welfare gains, surpassing the "Perfect Within" counterfactual due to both (1) implementing higher average interoperability levels, and (2) eliminating allocative distortions and thus increasing allocative efficiency.[89,90] While this Epic monopoly substantially improves welfare in our framework, it may have unintended consequences in the EHR market (e.g., higher prices) that we do not model. This trade-off between competition and interoperability-driven welfare gains presents an interesting direction for future research.

Public policies could potentially achieve some of these welfare gains without hampering market competition by directly providing options for higher interoperability. One such policy that is currently under development is the Trusted Exchange Framework and Common Agreement (TEFCA), which aims to facilitate the exchange of patient records across different EHR systems through a voluntary nationwide health information exchange network. The final row of Table 9 simulates the possible welfare effects of TEFCA by raising low interoperability levels to a minimum quality standard of 0.51 (the average level observed in the final data period), while leaving higher existing levels unchanged to reflect TEFCA's voluntary nature. This policy counterfactual achieves 34% of the first-best welfare gains, though the exact percentage depends on the level of the minimum quality interoperability standard that we implement.[91]

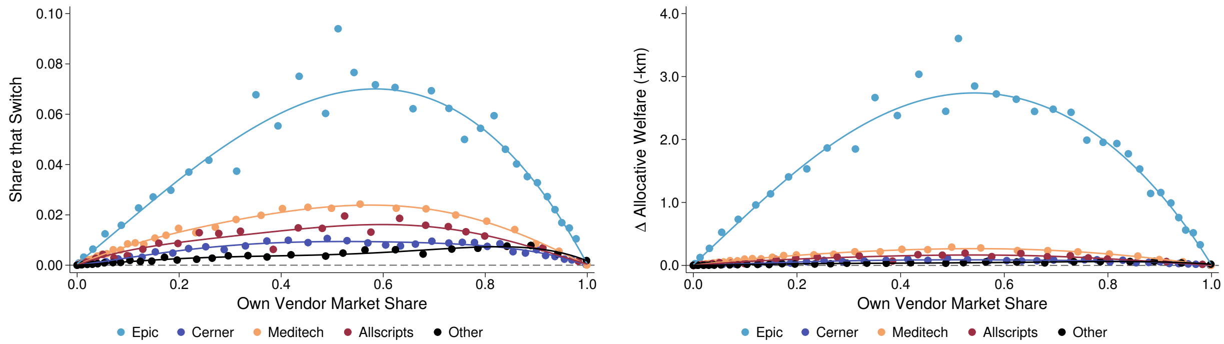
[88] For patients switching from different-vendor to same-vendor hospitals, allocative losses offset 64% of their direct gains from higher within-vendor interoperability.

[89] Average interoperability levels under the "Epic Monopoly" counterfactual are higher than those under the "Perfect Within" counterfactual because the former increases all levels to between 0.62-0.85 while the latter increases interoperability only for hospital pairs that share the same vendor.

[90] While both the first-best full interoperability counterfactual and the Epic monopoly counterfactual eliminate the allocative distortion, the first-best yields larger allocative welfare gains and a higher share of patients that switch. This is due to higher interoperability levels in the first best, which makes the choice set of hospitals inside the market even more attractive for patients who initially choose the outside option and thus results in more of these patients diverting from the outside option.

[91] Higher minimum interoperability levels will result in larger welfare gains. We choose to implement a minimum standard of 0.51 because that is the average level of interoperability that patients experience in the final

Figure 10: Full Interoperability Counterfactual: Role of Market Concentration, Vendors



(a) Patient Switches

(b) Δ Allocative Welfare for All Patients

Notes: Figures show heterogeneity for the first-best full interoperability counterfactual by plotting the share of patient switches (Panel A) and the change in allocative welfare for all patients (Panel B) by vendor market concentration and sending market vendor. The x-axis is the market share of the vendor used by the sending hospital. Dots are binscatters, while lines are fifth-order polynomial fit lines; both are estimated separately by vendor of the sending hospital. Patient sample is limited to those who choose hospitals in the market under the data conditions ("intensive patients"). Adding patients who choose hospitals outside of the market under the data conditions ("extensive patients") simply causes the curves to vertically shift up at every point due to extensive patients entering each market.

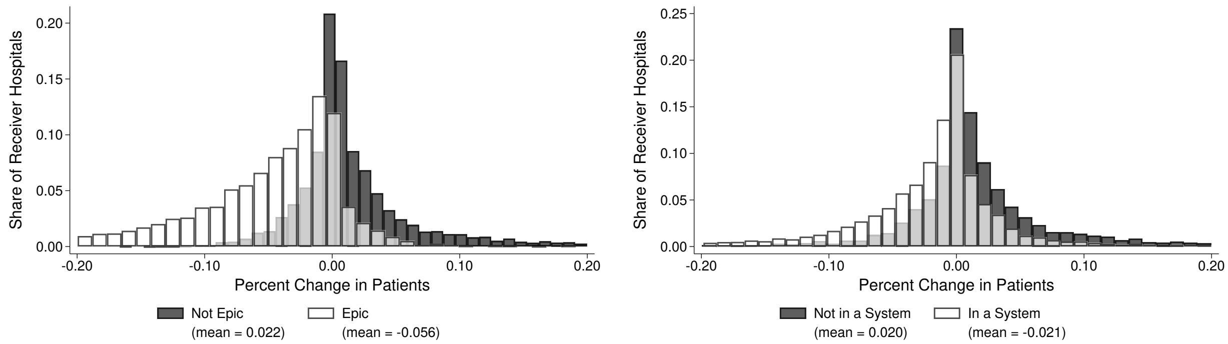
Role of Market Structure in Reallocation The extent of allocative distortions from technological frictions depends on vendor market concentration and which vendor each sending hospital uses. Revisiting the first-best full interoperability counterfactual, Figure 10 shows that patient reallocation (Panel A) and allocative efficiency gains (Panel B) are largest (1) in fragmented markets with more variation in EHR vendors across hospitals and (2) when the sending hospital uses an Epic EHR system.[92] This second result is due to Epic's largest wedge between within- and across-vendor interoperability in the data, which creates the strongest incentive for Epic hospitals to send patients to other Epic facilities. Eliminating this wedge would yield substantial allocative welfare gains for patients that start at Epic hospitals. Appendix Figure D18 further reveals increasing allocative distortions over time, driven by widening gaps between within- and across-vendor interoperability and growing market concentration.

Effects on Hospital Revenue Patient reallocation across hospitals affects not only patient outcomes but also revenue for receiving hospitals. Under the first-best counterfactual where interoperability frictions are eliminated, we find patients are shifted towards smaller, non-

year of our sample period (2019).

[92]In Figure 10, the colors represent the sending hospital's EHR vendor (e.g., Epic is blue). The x-axis is then the local market share of that vendor. Middle values of the x-axis represent the highest market fragmentation from the perspective of the sending hospital.

Figure 11: Full Interoperability Counterfactual: Effects on Receiving Hospitals



(a) Whether Have Epic

(b) Whether in a System

Notes: Figures plot the distribution of percent change in patients received under the first-best full interoperability counterfactual compared to the data for receiver hospitals that either have an Epic EHR system or not (Panel A) and that either belong to a larger health system operating in their local market or not (Panel B). Patient sample is limited to those who choose hospitals in the market under the data conditions ("intensive patients"). Adding patients who choose hospitals outside of the market under the data conditions ("extensive patients") simply causes the distributions to shift to the right due to extensive patients entering each market.

Epic, non-AMC, higher-quality, and independent hospitals. Figure 11 illustrates these patterns, showing percent changes in the number of patients that hospitals receive separately by whether they use Epic (Panel A) and by whether they are affiliated with larger health systems operating in their markets (Panel B). Distributions for Epic-using and system-affiliated hospitals are centered below zero, while those for non-Epic and independent hospitals are centered above zero. Thus, eliminating interoperability frictions not only improves patient outcomes by facilitating transfers to higher-quality hospitals but also has the important distributional consequence of increasing revenue for smaller and independent hospitals that do not currently use Epic EHR systems.[93]

8 Conclusion

In an increasingly interconnected world, the ability (or inability) of different systems to work together can significantly shape market dynamics and consumer welfare. This paper demonstrates that technological frictions in markets where consumers move between products

[93]The current interoperability landscape favors larger hospitals that belong to multi-hospital systems because they are more likely to use Epic (Appendix Table B1). We see this directly in our reduced form results that hospitals receive more referrals after switching to Epic (Appendix Figure D5). Eliminating interoperability frictions would remove this advantage and cause patient reallocation towards non-Epic hospitals, which are often smaller and independent.

can have both direct and allocative effects on consumer welfare. Focusing on the setting of Electronic Health Record (EHR) system interoperability, we find evidence for both of these channels. Using an event study design, we find that when two hospitals switch to using the same EHR vendor, charges and hospital readmission rates for their shared transferred and referred patients decrease. Simultaneously, these hospitals now share more inpatient transfers and referrals. This change in patient flows further affects patient outcomes: when a hospital switches to an EHR vendor used by closer, cheaper, or higher-quality local hospitals, its transfer and referral patients travel shorter distances, incur fewer charges, and experience better health outcomes, respectively.

To quantify the welfare gain from reducing interoperability frictions, we develop and estimate a demand model of how patients and providers trade-off interoperability with other receiving 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 patients to different hospitals and increase joint hospital-patient welfare by 21%, the equivalent of a 57-kilometer reduction in travel distance per patient. This improvement stems from both maximizing the positive direct effects of interoperability and eliminating allocative distortions. However, improving only within-vendor interoperability without similarly improving that across-vendor achieves only 34% of the first-best welfare gains due in part to allocative welfare losses.

These findings open avenues for future research, particularly in examining healthcare provider incentives to adopt interoperable EHR systems and EHR vendor incentives to create them. While our study holds hospital EHR vendor choices fixed, endogenizing this decision could allow a comprehensive evaluation of the role of market competition and the impact of government interventions in this critical healthcare technology sector.

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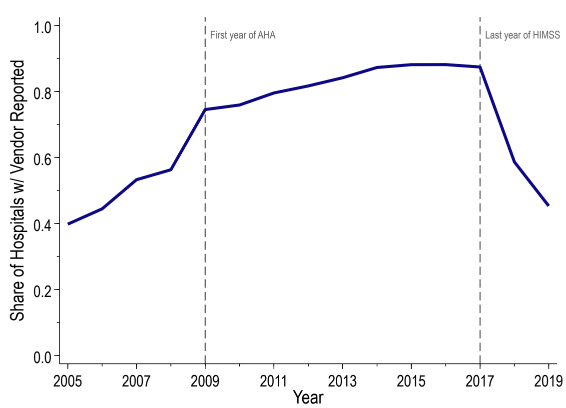
A Data Construction

A.1 Hospital EHR Vendors

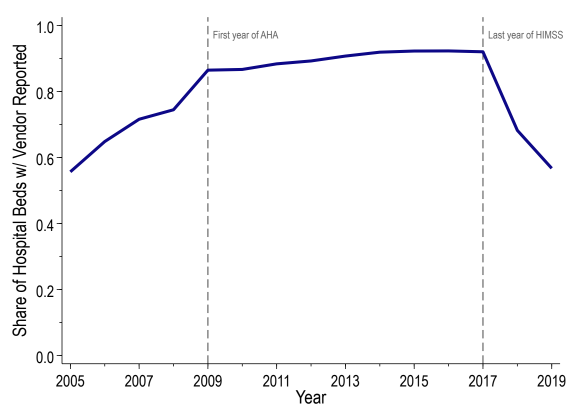
We combine data from the American Hospital Association (AHA) IT Survey and the Health Information and Management Systems Survey (HIMSS) to identify the primary EHR vendor used by hospitals in the US from 2005 to 2019. Concordance between the EHR vendor information contained in the AHA IT Survey and that contained in the HIMSS is quite high, with 87% of hospitals that respond to both surveys in any given year reporting to use the same EHR vendor in both data sources. In cases where the surveys contain different responses, we take the AHA IT survey response as the truth.

Appendix Figure A1 shows the shares of hospitals (Panel A) and hospital beds (Panel B) for which we have data on their annual EHR vendor using the above methodology. The dotted lines show the first year in which the AHA IT records EHR vendors (2009) and the last year in which HIMSS data is available (2017). The figures show that the share of hospitals and beds with vendor reported rises quickly from just under 60% to 90% over the period from 2005 to 2017. Following the end of the HIMSS in 2017, however, this share drops steeply.

Figure A1: Share of Hospitals with EHR Vendor Reported



(a) Hospitals



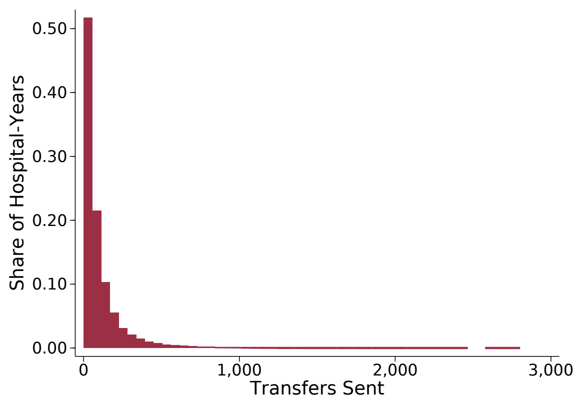
(b) Beds

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).

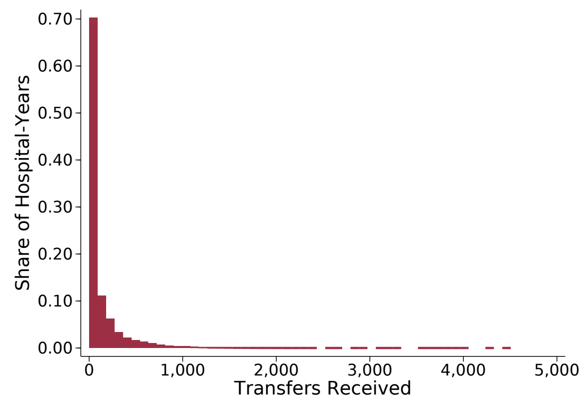
A.2 Hospital Medicare Transfers

We define a cross-hospital inpatient transfer as when a patient is discharged from one hospital and admitted to another on the same day or the following day. Table 1a contains summary statistics for transfers at the patient level, comparing the sample of transfer patients to the full sample of inpatient hospital visits. Appendix Figure A2 shows the distribution of transfers sent by each hospital in any given year.

Figure A2: Distribution of Number of Transfers Sent & Received



(a) Sent



(b) Received

Notes: This figure plots the distribution of transfers sent by each hospital in any given year (2005-2017).

A.3 Hospital Medicare Referrals

A.3.1 Overview

We define a *hospital referral* as a carrier claim line in which the service took place at a hospital and a referring physician is listed. We count each line in the carrier claim that meets this definition as a separate referral.

We 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. This implicitly assumes that the place of service that led to the referral (i.e., the "sender") is also a hospital. More simply, cross-hospital referrals are defined as when a patient is referred from one provider to a second provider who works at a different hospital than the first. Patients who are referred between two providers who both work at the same hospital do not meet this definition.

The Medicare Carrier claims list the NPI of the performing physician and the NPI of the referring physician, but they do not always list the Medicare Certification Number (MCRNUM) of the place of service (i.e., the hospital) nor do they ever list the MCRNUM of the place of referral. We thus have to take several steps to identify both the sending and receiving hospitals' MCRNUMs.

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

Place of referral is never defined in the Carrier claims, and we believe it would be quite difficult to attempt to track down the exact prior claim in the inpatient/outpatient/carrier files that led to each individual referral. Thus, we assume the place of referral is a hospital. We then define that hospital as the primary hospital affiliation of the referring physician, which we observe in the Medicare data.

To calculate these affiliations, we do the following steps separately for each year of data. First, we limit the 100% sample of inpatient claims and the 100% sample of outpatient claims to only those claims that take place at a hospital.[94] For all claims, we retain the medicare number of the place of service (MCRNUM) as well as the provider numbers (NPIs) of the physicians involved with the claim. Second, for each unique site and provider (MCRNUM-NPI) pair, we count the number of claims on which that provider is listed as either the attending, operating, rendering, or other physician on the claim. We separately count for the inpatient claims and the outpatient claims, and then add the two to form a total count of claims for each site-provider pair. Third, for each unique provider (NPI), we rank the sites (MCRNUMs) by the total number of claims that the provider worked on that took place at that facility. Finally, we define the top (or primary) affiliation for each unique provider (NPI) as the site (MCRNUM) with which the provider has the largest number of total claims. Approximately 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 their claims take place at their primary hospital affiliation.[95]

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

To define the hospital from which patients are referred, we do the following steps separately for each year of data:

(i) For the 20% sample of the Carrier claims, we first attempt to define the place of ser-

[94] 100% of the inpatient claims and 77% of the outpatient claims take place at a hospital.
[95] For providers with more than one hospital affiliation, this average is 76%.

vice using information from matching inpatient and outpatient claims with the Carrier claims:[96]

- Limit the 20% sample inpatient and 20% sample outpatient claims to only those that take place at a hospital. Reshape the data to 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 is inpatient or outpatient at a single facility. The result is a dataset that says, on date d, patient p received inpatient or outpatient care at hospital h (and only hospital h).
- Limit the 20% sample Carrier claims to only those that take place at a hospital. Only keep claim lines that span a single day.
- Merge the inpatient/outpatient patient-day data onto the Carrier files.

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

A.3.4 Cross-Hospital Referral Summary Statistics

Table 1b shows summary statistics of these cross-hospital referrals at the patient level, comparing this sample of referrals to the full sample of referrals that take place in a hospital setting.

A.4 Use of DocGraph

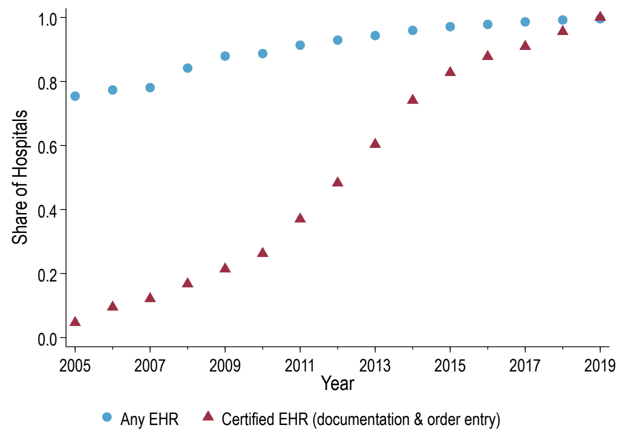
For our decomposition of interoperability levels across EHR vendors and time, we require a comprehensive picture of patient sharing relationships across healthcare providers. We get this information from DocGraph Hop Teaming from 2013 to 2019. The DocGraph data is derived from the CMS Medicare Claims and contains information on both the number of patients shared between any two providers for each year as well as the directionality of these flows. More information about the DocGraph data is available here: https://careset.com/docgraph-hop-teaming-dataset/.

[96]Background: Inpatient and outpatient claims contain information on the place of service. What's the relationship between Carrier claims and inpatient/outpatient claims? Carrier claims are claims from the provider, whereas outpatient and inpatient claims are claims from the facility at which service was received. For example, if a patient has surgery at an outpatient center, the physician's bill will be in the carrier claims while the bill for use of the center's equipment will be in the outpatient claims—same event, two different files. Thus, for each Carrier claim where service takes place a hospital, it may be possible to match that claim to the inpatient/outpatient files using patient ID and date of service.

B Additional Background Information

B.1 Hospital EHR Adoption

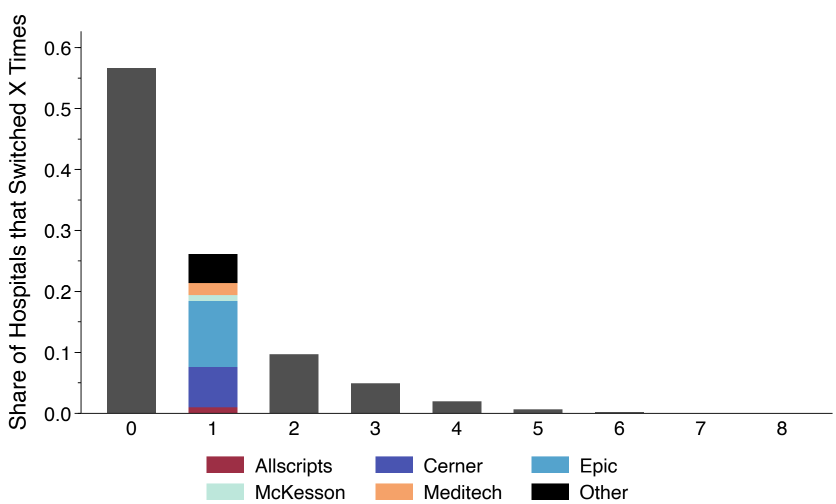
Figure B1: EHR Adoption



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

B.2 Hospital EHR Switches

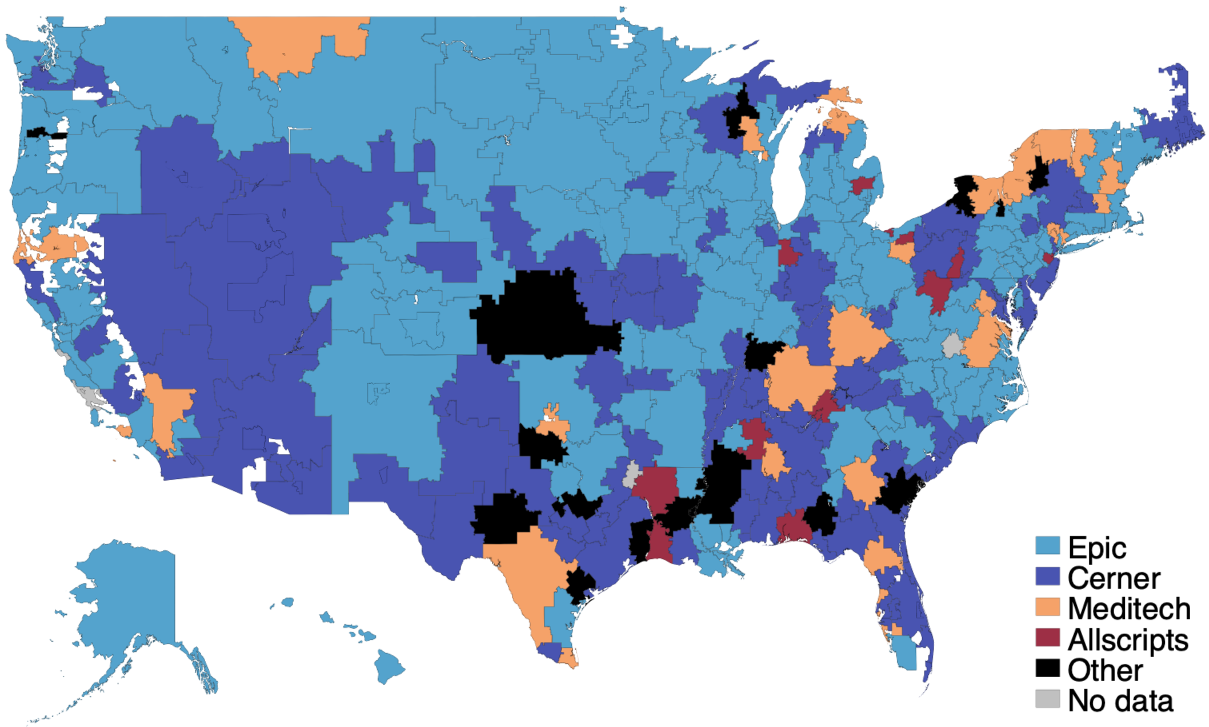
Figure B2: EHR Switches per Hospital



Notes: This 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 shows in color which vendor the hospital switched to.

B.3 Modal Hospital EHR Vendor by Market

Figure B3: Modal EHR Vendor by Market (2019)



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

B.4 Characterizing Hospitals that use Epic

Table B1 regresses an indicator for whether each hospital-year observation has an Epic EHR system on a number of time-varying hospital characteristics including: whether the hospital belongs to a larger health system, whether the hospital is an academic medical center, whether the hospital is a non-profit or a for-profit organization (with public organizations as the omitted category), and the number of hospital beds. We find that hospitals belonging to systems, academic medical centers, non-profit hospitals, and larger hospitals are more likely to use Epic as their primary EHR system vendor.

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 the results of a regression of 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.5 Why Hospitals Switch EHR Vendors

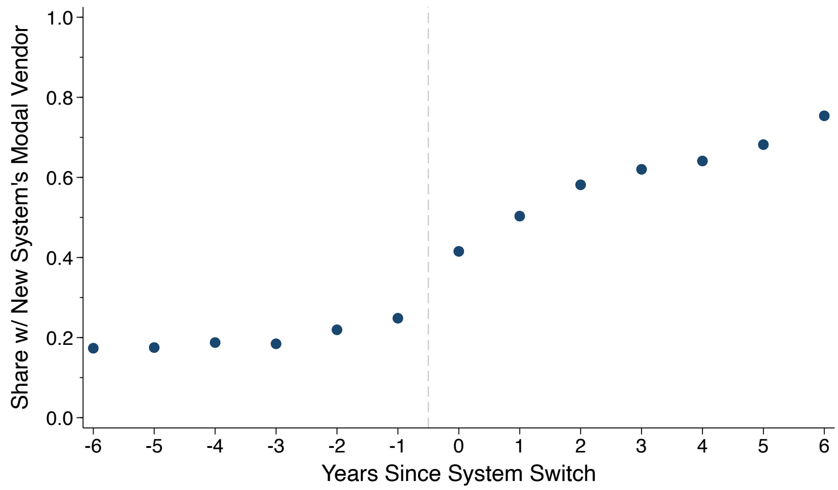
As discussed in the main text, hospitals switch vendors for many reasons including costs, system functionalities, vendor relations (e.g., customer service), and interoperability. For interoperability, we see three pieces of evidence that hospitals prefer to switch to EHR vendors that are dominant in both their local markets and their hospital systems:

(i) 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.

(ii) 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 is due to an overlap between modal in system and modal in HRR (i.e., 35% switch to the modal in both).

(iii) Among hospitals that switch hospital systems once during our sample period (approximately 23% of hospitals), 25% have 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 ultimately caps at 75% at 6 or more years after switching systems. This time pattern is shown in Appendix Figure B4.

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



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

B.6 Descriptive Evidence Supporting the Interoperability Decomposition

Panel A of Figure B5 plots the average frequency at which hospitals of different EHR vendors use different electronic methods to send patients' records, including (i) interface connection between EHR system (e.g., HL7 interface), (ii) direct access, (iii) regional health information exchange, (iv) single EHR vendor networks (e.g., Epic's Care Everywhere), and (v) multi-EHR vendor networks (e.g., Commonwealth Health Alliance).[97] Epic hospitals report the highest use of all advanced methods. In particular, the frequent use of Epic's single network, Care Everywhere, explains why hospitals using this vendor's EHR systems have such high within-vendor interoperability. Cerner hospitals have decent use of all methods and are the only ones besides Epic hospitals to make use of a multiple-vendor network.[98] In contrast,

[97]Between 2016 and 2019, the AHA IT surveys asked hospitals to report how frequently they used different methods to send or receive patients' data. Hospitals can answer either "often", "sometimes", "rarely", or "never". We translate 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. The responses regarding sending versus receiving show similar patterns.

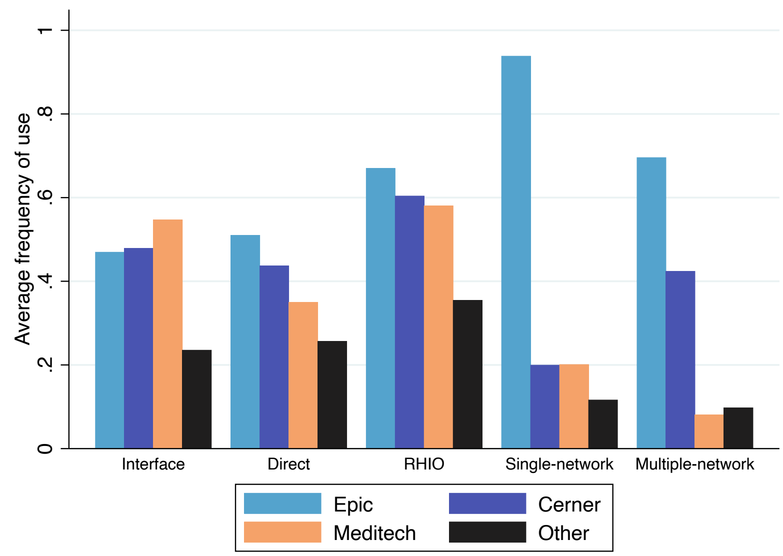
[98]After Epic rolled out its Care Everywhere network, some of Epic's competitors (Cerner, McKesson, Allscripts, athenahealth, Greenway, and RelayHealth) formed the CommonWell Health Alliance to set common interoperability standards and facilitate data exchange across themselves. This multi-vendor network was established in

Meditech, which is built on HL7 standards, relies more on interface connection between EHR systems of the same standard.

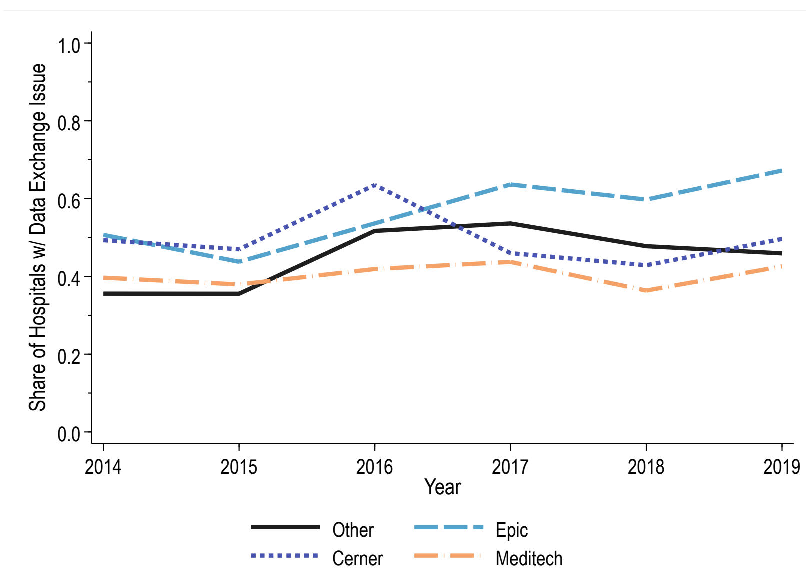
Panel B of Figure B5 shows the share of hospitals that report difficulties exchanging data with other medical facilities that use different EHR vendors over time and across vendors from 2014 to 2019. This share is similar across the major EHR vendors and, if anything, appears to increase over time. This is consistent with our estimates of across-vendor interoperability remaining lower than within-vendor across all vendors and all years.

2013 but only rolled out slowly to health centers. A breakdown by year shows that the share of Cerner hospitals reporting multi-vendor network participation rises from less than 30% in 2016 to more than 70% in 2019. This reflects the increase in CommonWell participation among Cerner hospitals over time and likely contributes to Cerner's increase in both across and within vendor interoperability over time.

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 share of hospitals that report using 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. 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 that is commonly 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 to this question with one of three answers: "yes, routinely," "yes, but not routinely," or "no." 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 that answer "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 code ι_{ht} as "no." For ease of notation, hereafter we drop the dependence of our interoperability estimation on time.

C.2 Interoperability Decomposition

Our objective is to construct estimates of vendor-specific within- and across-interoperability from hospitals' reported interoperability $\{\iota_h\}_h$ and the shares $r(h, e_h)$ of patients sent to same-vendor hospitals. We proceed in two main steps: (1) estimate an ordered probit model of hospitals' reported interoperability ι_h, and (2) convert estimates of this model into estimates of vendors' within-interoperability $\iota_{e,e}$ and across-interoperability $\iota_{e,-e}$.

The ordered probit model is as follows:

$$\iota_h = \begin{cases} 0 & \text{if } \text{score}_h \leq c_1 \\ 0.5 & \text{if } c_1 < \text{score}_h \leq c_2 \\ 1 & \text{if } \text{score}_h > c_2 \end{cases} \tag{C.1}$$

where

$$\text{score}_h = \left(\sum_e \mathbf{1}_{\{e_h = e\}} \times \left(\alpha_e^0 + \alpha_e^1 r(h, e_h) \right) \right) + \beta \text{RHIO}_{h,m} + \epsilon_h \tag{C.2}$$

and $\epsilon_h \sim$ N(0,1). Here, the numerical values of 0, 0.5, and 1 are recoded for "no", "yes, but not routinely," and "yes, routinely," respectively. The variable $\text{RHIO}_{h,m}$ is a control for the share of hospital beds in h's local market m that participate in a regional health information organization (RHIO) if h participates in a RHIO, and zero otherwise.

To address potential endogeneity of $r(h, e_h)$, we instrument for $r(h, e_h)$ using the share $z(h, e_h)$ of other hospital beds in hospital h's local market whose hospitals use the same EHR vendor as h (i.e., what $r(h, e_h)$ would be if hospital h randomly allocated its patients across all other hospital beds in its market) using a two-step control function approach (Wooldridge, 2015). First, we run the following regression:

$$r(h, e_h) = \left(\sum_e \mathbf{1}_{\{e_h = e\}} \times \left(\tilde{\alpha}_e^0 + \tilde{\alpha}_e^1 z(h, e_h) \right) \right) + \tilde{\beta}\text{RHIO}_{h,m} + \nu_h \tag{C.3}$$

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

Next, we convert the ordered probit estimates into numerical estimates of within- and across-interoperability $\{\iota_{e,e}, \iota_{e,-e}\}_e$. Our procedure (1) follows the intuition that the higher $r(h, e_h)$ is, the more reflective is hospital reported interoperability of the hospital vendor's within-interoperability than across-interoperability and (2) ensures that estimated interoperability levels are in the range of $[0, 1]$. Specifically:

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

and

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

That is, $\hat{\iota}_{e,e}$ is the expected value of ι_h if $r(h, e_h) = 1$, and $\hat{\iota}_{e,-e}$ is the counterpart for $r(h, e_h) = 0$.

D Additional Empirical Results

D.1 Additional Evidence of Direct Effect on Patient Outcomes

D.1.1 Robustness of Direct Effect of Gaining Same Vendor

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	75314	75314	75314	75314	75314
	Charges	Charges	Charges	Charges	Charges	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 have a specification with patient diagnostic code categories and thus the columns numbers are different. 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

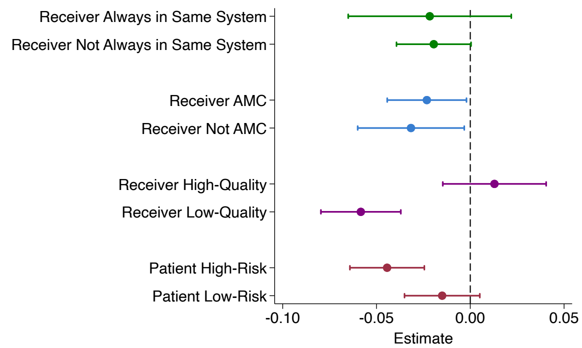
(b) Referrals

	(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

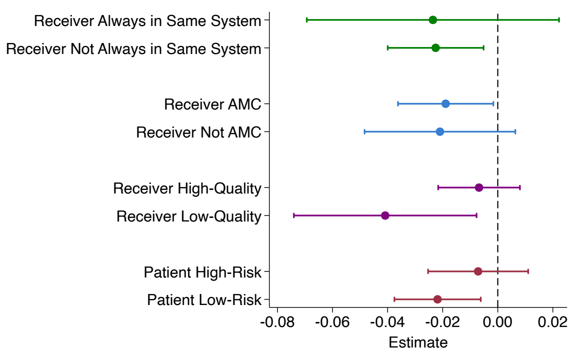
Notes: Each column in each panel in each table is a separate difference-in-differences regression of the effect of the patient's sending and receiving hospitals gaining the same EHR vendor on patient outcomes as described in Equation 3.1. Panel A shows transfer patient outcomes, while Panel B shows referral 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 limiting 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.

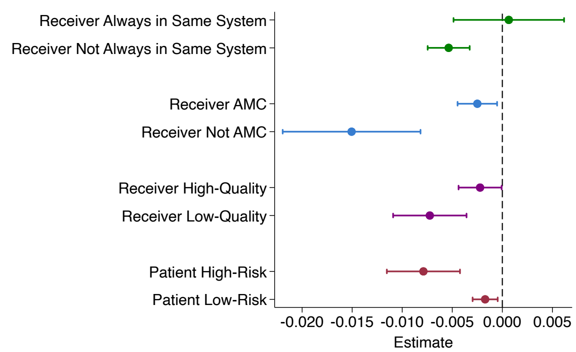
D.1.2 Additional Heterogeneity of Direct Effect of Gaining Same Vendor

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



(a) Transfer # Images





(b) Referral # Tests

(c) Referral 60 Day Readmit

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 total charges (in 2019$) incurred at the receiver hospital by transfer 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 baseline risk-adjusted mortality rate for transfer patients. "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 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.

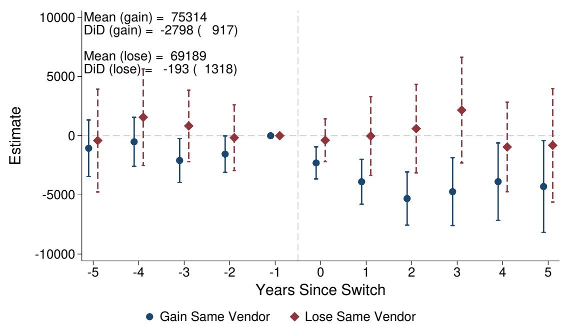
D.1.3 Direct Effect of Losing Same Vendor

In the main text, we show that when a hospital sender-receiver pair gains the same EHR vendor, charges and readmission rates for transfer and referral patients who are shared between the two hospital decrease. Now, we ask whether the opposite is also true, i.e., when a hospital sender-receiver pair *loses* the same EHR vendor, do charges and readmission rates for transfer and referral patients *increase*?

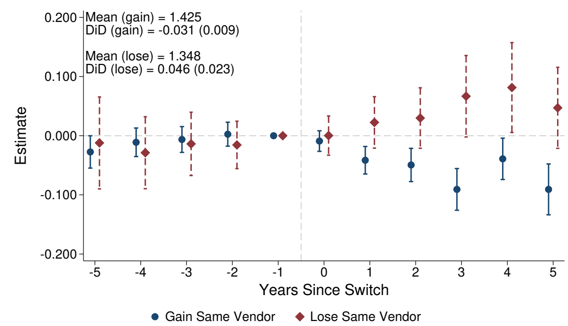
We test this question using the same event study specification as Equation 3.1 but modified such that the main independent variable of interest is an indicator for whether the hospital pair has *different* EHR vendors rather than the same. Further, we limit the sample of hospital pairs to those that either (1) always have the same vendor over the sample period (i.e., "always treated", 7.5% of hospital pair observations) or (2) start with the same vendor and switch to different vendors only once over the sample period (6.3% of hospital pair observations).

Figure D2 plots these event study estimates for our four main outcomes of interest for transfer patients, while Figure D3 does the same for referral patients. Overall, there is a positive relationship between two hospitals losing the same EHR vendor and the number of images and tests that their shared transfer patients undergo. This is suggestive of duplicate healthcare procedures increasing when two hospitals are no longer as interoperable as they were before. However, unlike when a hospital pair gains the same EHR vendor, we see no effect of losing the same EHR vendor on shared transfer patient charges. We also see no significant effects for any referral patient outcomes.

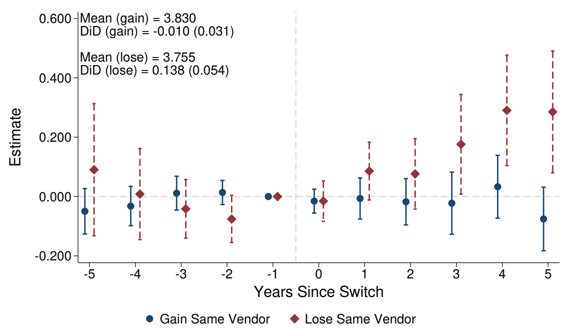
Figure D2: Event Studies of Direct Effect on Losing Same Vendor on Shared Transfer Outcomes



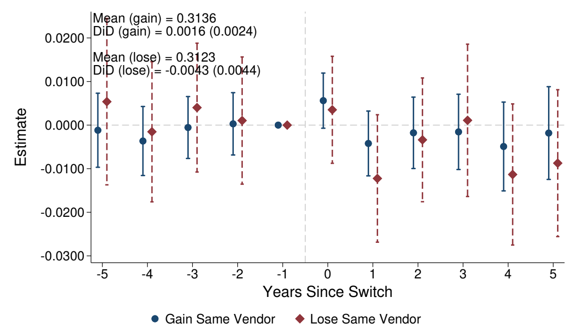
(a) Transfers, Charges



(b) Transfers, # Images



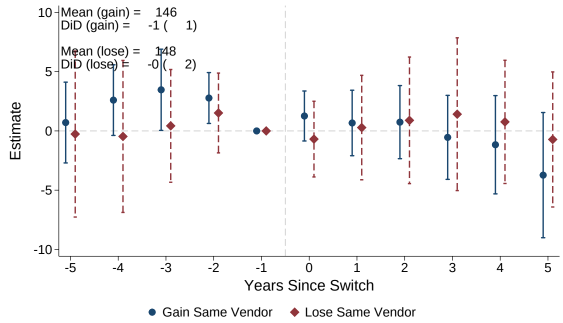
(c) Transfers, # Tests



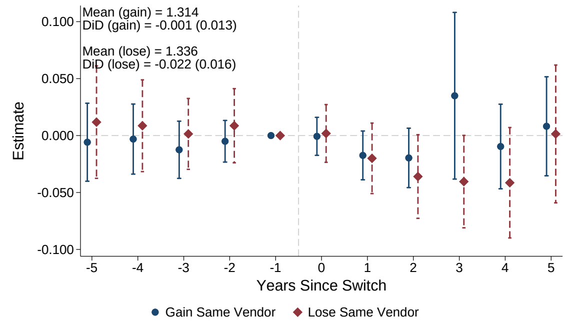
(d) Transfers, 30-Day Readmit

Notes: This 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 X. 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.

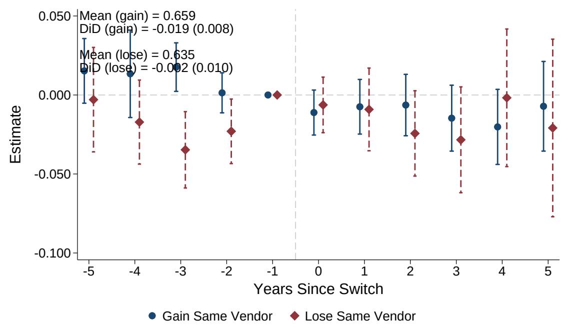
Figure D3: Event Studies of Direct Effect on Losing Same Vendor on Shared Referral Outcomes



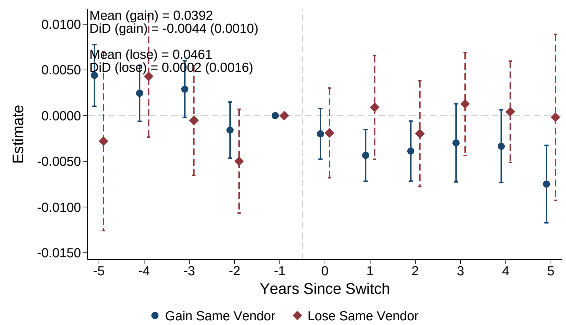
(a) Referrals, Charges



(b) Referrals, # Images



(c) Referrals, # Tests



(d) Referrals, 60-Day Readmit

Notes: This 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 X. 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 effect of interoperability on patient outcomes are due to hospitals switching EHR vendors rather than hospitals switching to the same EHR vendor, we conduct a placebo tests 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 only the control hospital pairs in our main specification (i.e., pairs that never share the same vendor) where both the sending and receiving hospital switch EHR vendors a maximum of one time over our sample period. We also discard pairs where the sender and receiver switch vendors in different years in order to more simply identify a single event for each pair. We then run the following difference-in-differences specification around whether either the sender or the receiver has yet switched vendors (as opposed to an indicator for whether both hospitals in the pair yet share the same vendor):

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

Here, $\mathbf{1}_{\left(\text{Post-Switch}_{(hh')t}\right)}$ is an indicator for whether either of the two hospitals has yet switched vendors (but not to the same vendor as the other) by year t. Estimates from this specification are shown in Table D3.

Table D3: 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., the 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.

D.2 Additional Evidence of Effect on Patient Flows

D.2.1 Additional Summary Statistics

Table D4: 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 D5: 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.

D.2.2 Details of Effect of Switches on Total Patient Flows

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

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

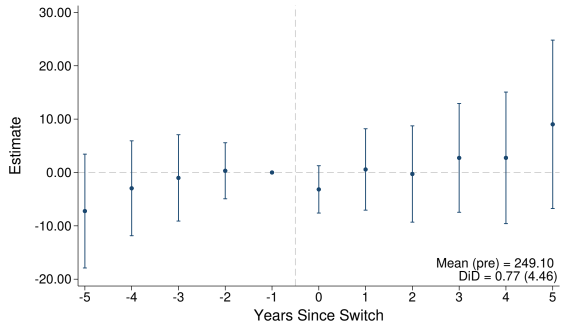
We cluster standard errors at the hospital level and our baseline specification weights observations by the hospital's baseline number of beds. We also estimate an event study version of the above equation:

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

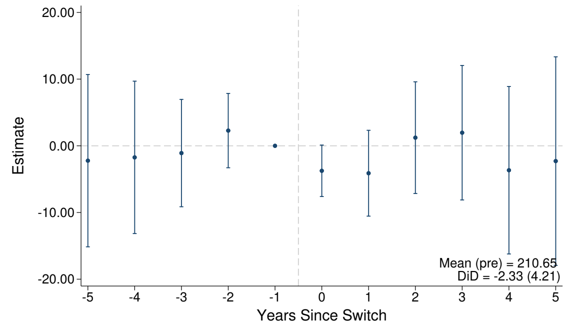
where r is the relative year since the hospital switched vendors (which occurs in year s_h) and we exclude $r = -1$. All other parameters are the same as in Equation D.2. To estimate these equations cleanly, we limit our analysis to the set of hospitals that either (1) never switch EHR vendors over the sample period ("never treated"), or (2) switch EHR vendors over the sample period only once.

Appendix Figure D4 plots event study estimates of the effect of switching EHR vendors on the number of transfers sent and those received by the switching hospital as well as the number of referrals sent and those received by the switching hospital. Table 4 display static differences-in-differences estimates as described in Equation D.2, while Appendix Tables D6 and D7 subjects the main specification's results to a series of robustness checks. Overall, switching EHR vendors does not appear to affect the total number of transfers a hospital either sends or receives nor the total number of referrals a hospital send but does increase the total number of referrals received (e.g., by 8.8% of the mean.

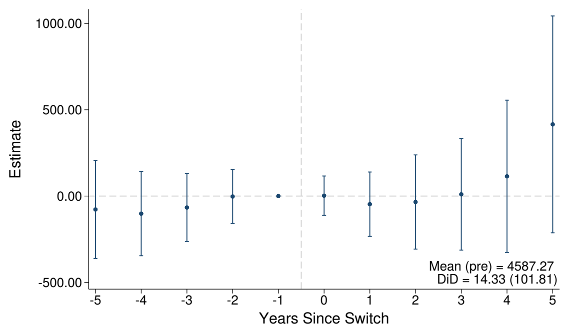
Figure D4: Hospital Vendor Switches on Total Patient Flows



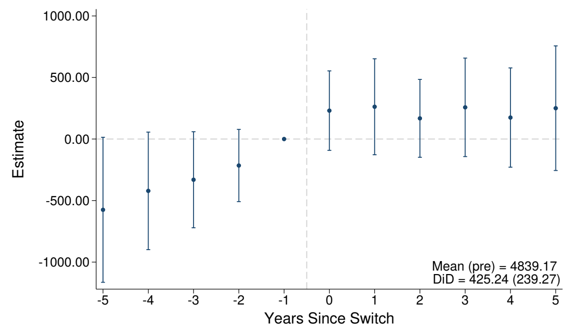
(a) # of Transfers Sent



(b) # of Transfers Received



(c) # of Referrals Sent



(d) # of Referrals Received

Notes: This figure plots event study estimates of the effect of switching EHR vendors on number of transfers and referrals both sent from and received by the switching hospital. Observations are weighted by bed size in 2005. Bars show 95% confidence intervals.

Table D6: 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 runs Callaway and Sant'Anna (2021) on the event study sample. * p<0.10, ** p<0.05, *** p<0.01.

Table D7: Hospital Vendor Switches on Total Patient Flows - Robustness for Referrals

(a) Referrals Sent

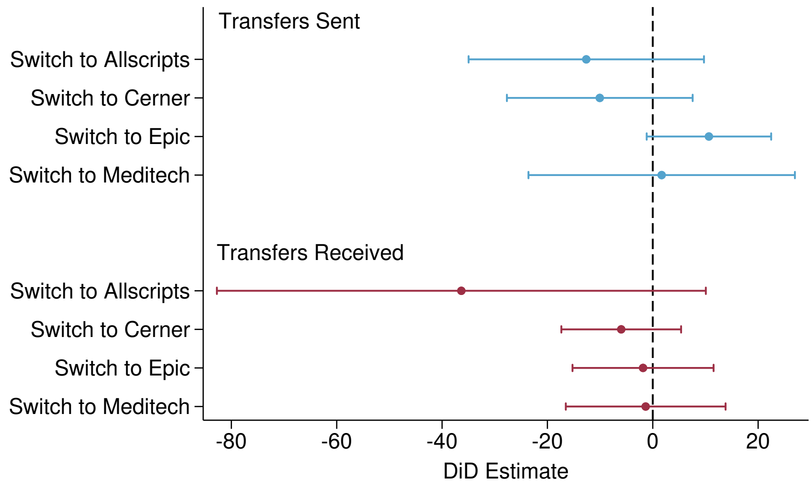
	# of Referrals Sent				
	(1)	(2)	(3)	(4)	(5)
Post-EHR Switch	14.33	26.22	-65.17	-65.17	132.7
	(101.8)	(102.2)	(91.50)	(123.9)	(185.3)
Observations	29805	29805	28319	28319	
Mean of Outcome	4587.27	4587.27	4587.27	4587.27	4587.27
Sample	Event	Event	Event	Event	C&S
Hospital Covariates		X	X	X	
Market x Year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

(b) Referrals Received

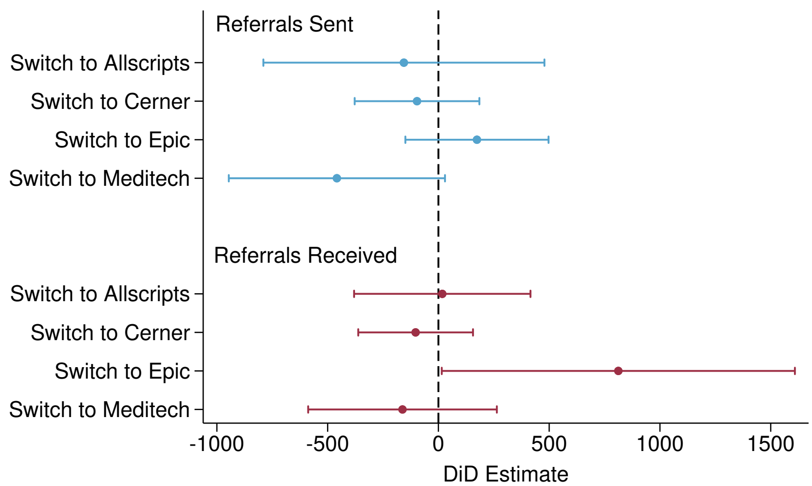
	# of Referrals Received				
	(1)	(2)	(3)	(4)	(5)
Post-EHR Switch	425.2*	417.5*	387.5*	387.5	268.9*
	(239.3)	(237.6)	(211.7)	(260.5)	(147.2)
Observations	29805	29805	28319	28319	
Mean of Outcome	4839.17	4839.17	4839.17	4839.17	4839.17
Sample	Event	Event	Event	Event	C&S
Hospital Covariates		X	X	X	
Market 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) on the event study sample. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Figure D5: Total Flows Heterogeneity by Vendor



(a) Transfers



(b) Referrals

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

D.2.3 Robustness of Effect of Gaining Same Vendor on Patient Flows

Table D8: Gaining Same Vendor - Robustness

(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. 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 for all columns (except Column 8) and are reported in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table D9: Gaining Same Vendor - Poisson Pseudo Maximum Likelihood (PPML)

(a) Transfers

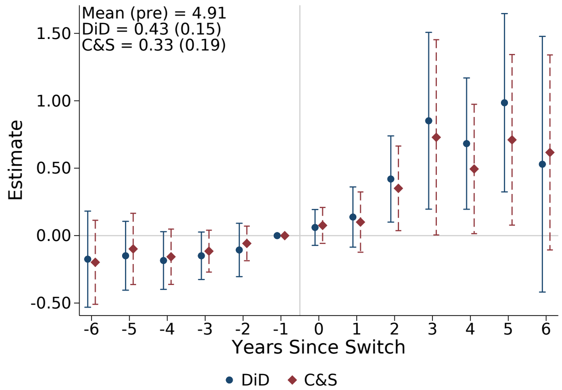
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: # of Transfers			
Same EHR Vendor	0.120***	0.113***	0.158***	0.120***	0.113***	0.158***
	(0.0321)	(0.0264)	(0.0220)	(0.0321)	(0.0264)	(0.0220)
Observations	390458	390399	342308	390458	390399	342308
Mean of Outcome	4.91	4.91	4.91	4.91	4.91	4.91
			Panel B: Fraction 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

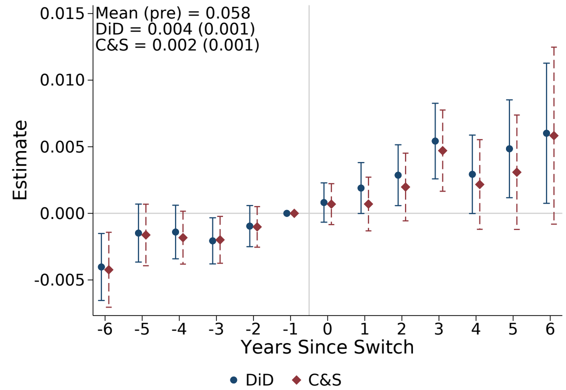
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: # of Referrals			
Same EHR Vendor	0.137***	0.143***	0.132***	0.137***	0.143***	0.132***
	(0.0209)	(0.0167)	(0.0126)	(0.0209)	(0.0167)	(0.0126)
Observations	1374119	1349101	1337024	1374119	1349101	1337024
Mean of Outcome	50.16	50.16	50.16	50.16	50.16	50.16
			Panel 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 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.

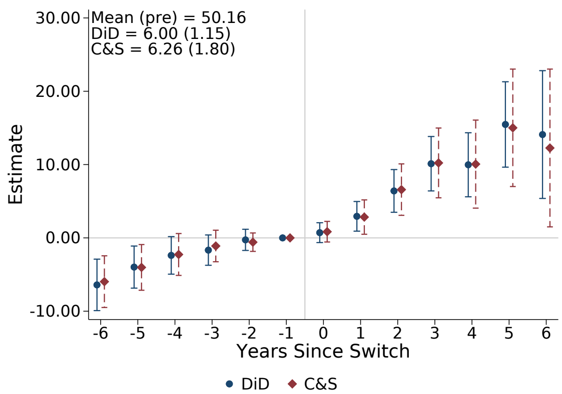
Figure D6: Event Studies of Gaining Same Vendor - Callaway and Sant'Anna (2021)



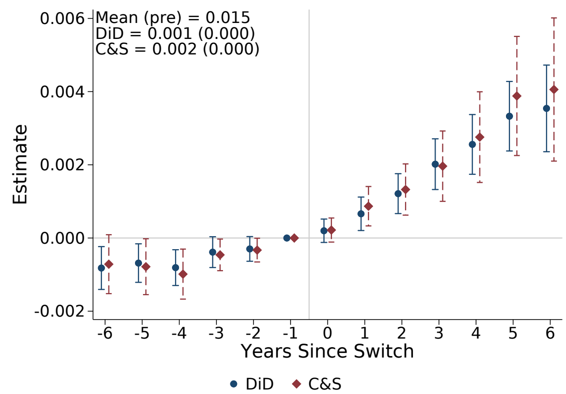
(a) # of Transfers



(b) Fraction of Transfers



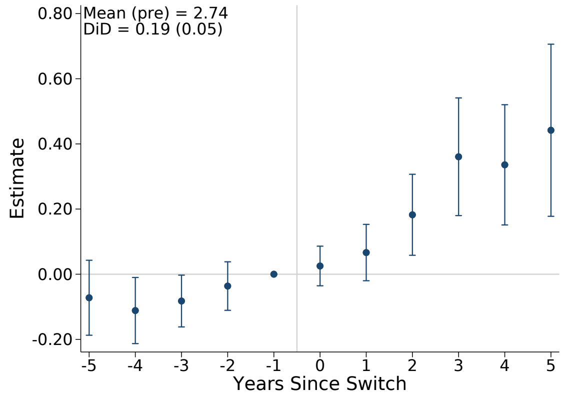
(c) # of Referrals



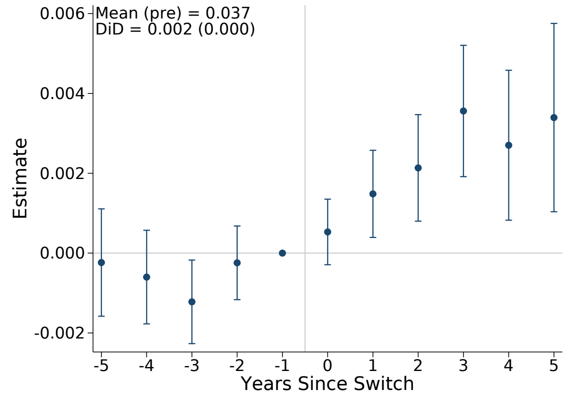
(d) Fraction of Referrals

Notes: This 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 4.2 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"). The mean of each dependent variable among treated observations in the pre-treatment period is given.

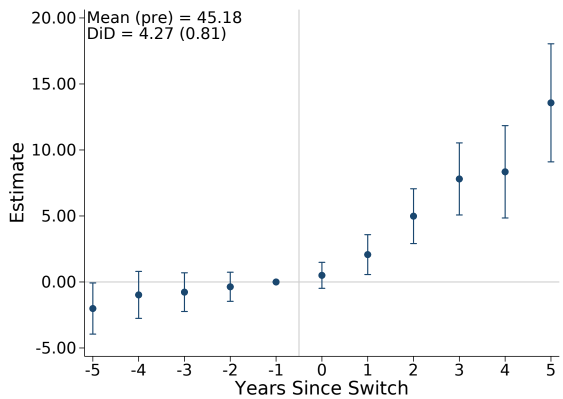
Figure D7: Event Studies of Gaining Same Vendor - Unweighted



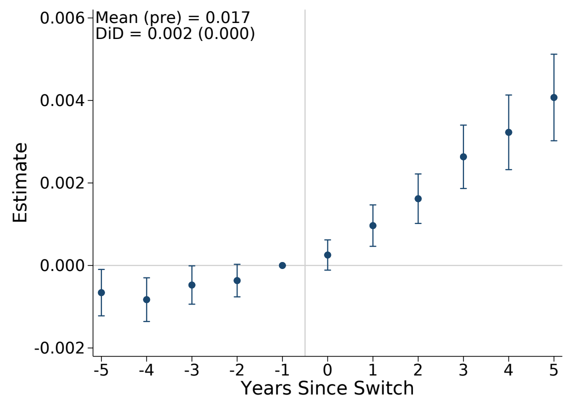
(a) # of Transfers



(b) Fraction of Transfers



(c) # of Referrals



(d) Fraction of Referrals

Notes: This 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 4.2 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.

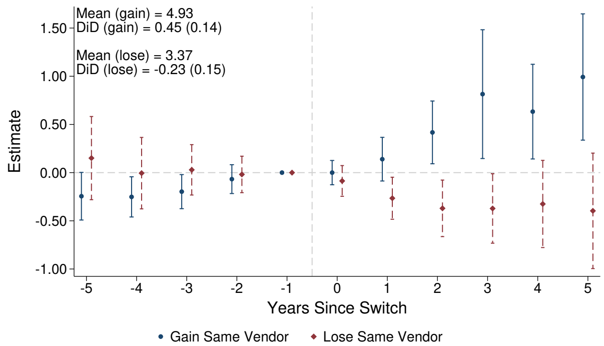
D.2.4 Effect of Losing Same Vendor on Patient Flows

In the main text, we show that patient flows between a hospital sender-receiver pair increase when the pair gains the same EHR vendor; now, we ask whether the opposite is also true, i.e., do patient flows decrease when the pair loses the same vendor? Asymmetry in treatment effects are possible. For example, after switching EHR vendors a hospital's extensive margin of patient flows could change, e.g., that hospital could start increasing the total number of patients it sends externally and send all new flows to now-same-vendor hospitals without changing flows with previous-same-vendor hospitals. We would then see a positive treatment effect of switching to the same vendor but not a negative treatment effect of switching away from the same vendor.

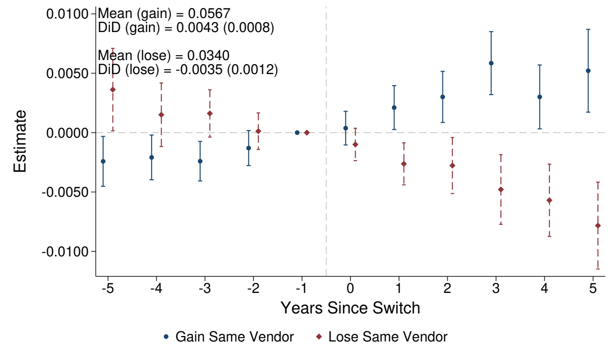
We test this question using the same event study specification as Equation 4.2 but modified such that the main independent variable of interest is an indicator for whether the hospital pair has *different* EHR vendors rather than the same. Further, we limit the sample of hospital pairs to those that either (1) always have the same vendor over the sample period (i.e., "always treated", 7.5% of hospital pair observations) or (2) start with the same vendor and switch to different vendors only once over the sample period (6.3% of hospital pair observations).

Figure D8 plots these event study estimates for our four main outcomes of interest, while Table D10 shows static difference-in-differences estimates from additional specifications that test for robustness. Overall, there is a negative relationship between two hospitals losing the same EHR vendor and their shared patient flows that is statistically significant for all outcomes except number of transfers. When two hospitals lose the same vendor, they experience an 10% decrease from the mean in the fraction of transfers shared and an 18% decrease from the mean in the fraction of referrals shared. For fraction of transfers, the magnitude of the effect of losing the same vendor is slightly larger that of gaining the same vendor (8%). For fraction of referrals, however, the magnitude of the effect of losing is nearly double that of gaining (9%). These differences may be partially driven by the nature of our regression specifications. As we will show in Section 5, an average hospital that switches EHR vendors selects a new vendor that is more widely adopted by other hospitals—particularly larger hospitals—in their local market than their old vendor. Since our empirical strategy is at the hospital sender-receiver pair level, any positive effect of gaining the same vendor is distributed more thinly across a greater number of hospital pairs than the equivalent negative effect of losing the same vendor. More simply, if a hospital reallocates 10% of its patients from Cerner hospitals to Epic hospitals after switching from Cerner to Epic, and there are more Epic hospitals in its local market than Cerner hospitals, the average pair-level effect will be smaller in magnitude for the gaining Epic hospitals than the losing Cerner hospitals.

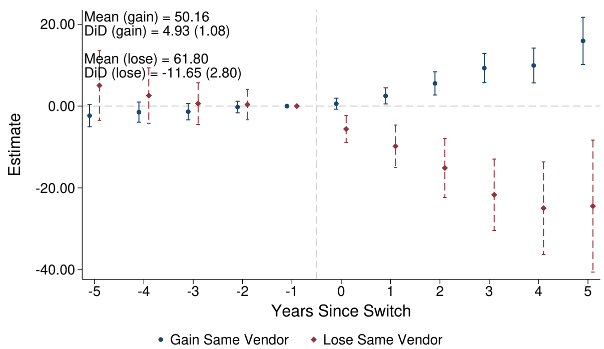
Figure D8: Event Studies of Losing Same Vendor on Shared Patient Flows



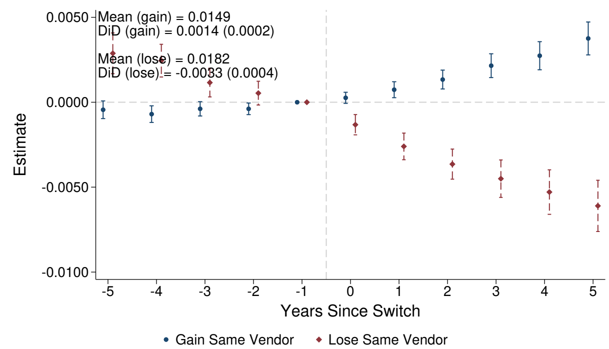
(a) # of Transfers



(b) Fraction of Transfers



(c) # of Referrals



(d) Fraction of Referrals

Notes: This 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 4.2 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 mean of each dependent variable among treated observations in the pre-treatment period is given.

Table D10: Losing Same Vendor - Robustness

(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. 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 estimates of the effect of interoperability on patient flows are due to hospitals switching EHR vendors rather than hospitals switching to the same EHR vendor, we conduct a placebo tests 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 only the control hospital pairs in our main specification (i.e., pairs that never share the same vendor) where both the sending and receiving hospital switch EHR vendors a maximum of one time over our sample period. We also discard pairs where the sender and receiver switch vendors in different years in order to more simply identify a single event for each pair. We then run the following difference-in-differences specification at the hospital-sender, hospital-receiver, year level around whether either the sender or the receiver has yet switched vendors (as opposed to an indicator for whether both hospitals in the pair yet share the same vendor):

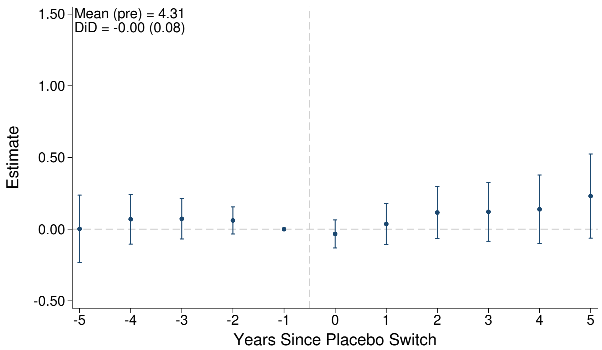
$$Y_{(hh')t} = \beta * \mathbf{1}_{\left(\text{Post-Switch}_{(hh')t}\right)} + \alpha_{(hh')} + \gamma_t + \delta X_{(hh')t,mt} + \epsilon_{(hh')t}. \tag{D.4}$$

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

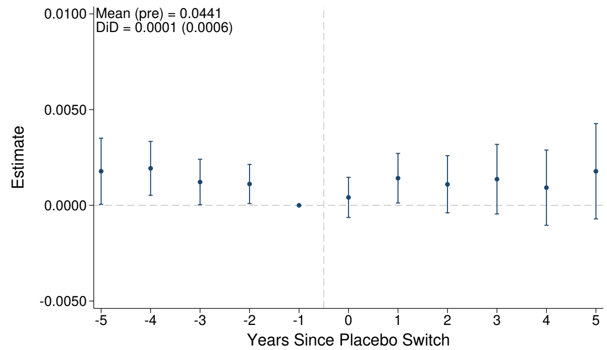
$$Y_{(hh')t} = \left(\sum_{r=-6}^{6} \beta_r \mathbf{1}_{\{t=switch_{(hh')}+r\}}\right) + \alpha_{(hh')} + \gamma_t + \delta X_{(hh')t,mt} + \epsilon_{(hh')t}, \tag{D.5}$$

where r is the relative year since either the sender or the receiver switched vendors (which occurs in year $switch_{(hh')}$). Estimates from these specifications are shown in Table 5 and Appendix Figure D9, respectively.

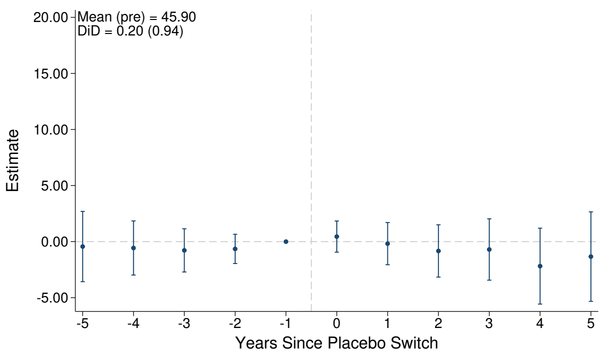
Figure D9: Placebo Test of Hospital Vendor Switches on Shared Patient Flows



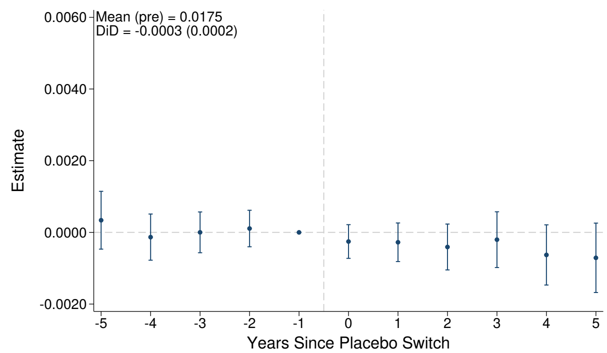
(a) # of Transfers



(b) Fraction of Transfers



(c) # of Referrals



(d) Fraction of Referrals

Notes: These figures 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 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 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. * p<0.10, ** p<0.05, *** p<0.01.

D.2.6 Heterogeneity by Which Hospital in the Pair Switches Vendors

A primary concern with our main empirical results is the possibility of endogeneity biasing the event study estimates. In particular, we may be concerned about one hospital explicitly switching to the same vendor as another because it wants to send or receive more patient flows with that other hospital. To mitigate this concern, we look more closely at the sending and receiving results. While the decision to send patients is almost entirely within a hospital's control, the decision to receive patients is not, instead relying more heavily on sending hospitals' decisions. Thus, the margin of receiving patients is less likely to be biased by the type of endogeneity described above than that of sending patients.

Appendix Tables D11 tests whether treatment effects exist along the receiving margin as well as the sending margin. The table displays estimates of Equation 4.1 for the full sample of treated observations in which only one hospital in the pair switches (Columns 1 and 4), the sample of treated observations that gain the same EHR vendor due to the sending hospital switching vendors (Columns 2 and 5), and the sample of treated observations that gain the same EHR vendor due to the receiving hospital switching vendors (Columns 3 and 6).[99] The outcome of all columns is patient flows from the sending hospital to the receiving hospital. Columns 2 and 5 show that when the sending hospital switches EHR vendors to have the same vendor as the receiving hospital, transfers from sender to receiver increase. In addition, Columns 3 and 6 show that when the receiving hospital is the one who switches, transfers from sender to receiver still increase, though typically by a smaller magnitude. The receiver switching is less endogenous than the sender switching since the sender is not the one making the EHR vendor choice. That we still see significant positive effects on the sender's patient flow decisions suggests that the type of endogeneity described above is not entirely driving our main empirical estimates (Columns 1 and 3).

[99] Of the approximately 167,000 treated observations (i.e., the hospital sender-receiver pair observations that start with different vendors and switch to the same vendor only once over the sample period), 46% become same-vendors due to the sending hospital switching vendors, while 45% become same-vendors due to the receiving hospital switching vendors, and the final 9% become same-vendors due to both hospitals switching vendors. Estimates in Columns 1 and 4 of Table D11 are slightly different than those in Columns 3 of Table D8 because we discard the 9% of treated observations that become same-vendors due to both hospitals switching vendors.

Table D11: 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. 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 Details of 2SLS Specification

To construct our instruments, we do the following steps. For each focal hospital h_A, we:

(i) Identify all hospitals that are in 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 the market shares of each major EHR vendor e among $h \in H_A$ with respect to baseline hospital beds b. Call this the "System Pressure" for h_A to have that vendor e:

$$\text{SP}(h_A, e) = \frac{\sum_{h \in \text{H}_A} b_h \times \mathbf{1}_{(e_h = e)}}{\sum_{h \in H_A} b_h}. \tag{D.6}$$

Appendix Figure D10 shows a visualization of this calculation. We do this calculation for each focal hospital in each year of our sample period. We then 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 $e \in$ {Epic, Cerner, Meditech, Allscripts}. That is, each instrument is the interaction of the sender hospital h's System Pressure to have a certain EHR vendor e in year t and the receiver hospital h''s System Pressure to have that same EHR vendor e in year t. Table D12 shows summary statistics of these instruments and compares the sample for which the instruments are defined to the full sample of hospital sender-receiver pair-year observations.

Our first stage estimation equation is as follows:

$$\begin{aligned}
\mathbf{1}_{(e_{ht} = e_{h't})} = {} & \beta_1 \times \text{SP}(ht, e = \text{Epic}) \times \text{SP}(h't, e = \text{Epic}) \\
& + \beta_2 \times \text{SP}(ht, e = \text{Cerner}) \times \text{SP}(h't, e = \text{Cerner}) \\
& + \beta_3 \times \text{SP}(ht, e = \text{Meditech}) \times \text{SP}(h't, e = \text{Meditech}) \\
& + \beta_4 \times \text{SP}(ht, e = \text{Allscripts}) \times \text{SP}(h't, e = \text{Allscripts}) \\
& + \alpha_{(hh')} + \gamma_t + \delta X_{(hh')t,mt} + \epsilon_{(hh')t}
\end{aligned} \tag{D.7}$$

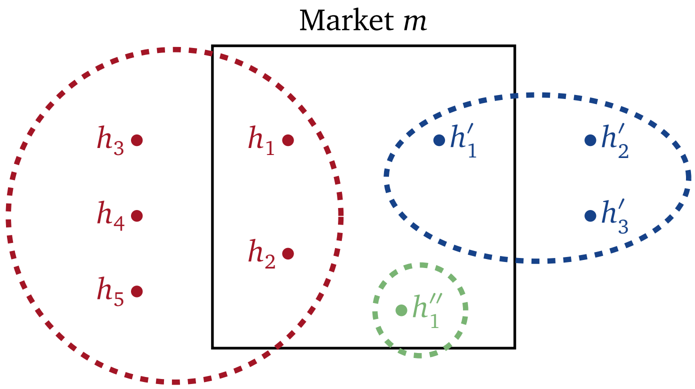
where $\mathbf{1}_{(e_{ht} = e_{h't})}$ is an indicator for whether the two hospitals actually have the same EHR vendor in year t (as in Equation 4.1). This specification is at the hospital-sender, hospital-receiver, year level. Then, our reduced form estimation equation is as follows:

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

where $Y_{(hh')t}$ denotes patient flows from hospital h to hospital h' in year t (as in Equation 4.1).

The validity of this instrumental variable strategy relies on two main assumptions: relevance and exclusion. The first stage estimates in Column 1 of Table 6 clearly 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 of the first stage estimates tell us that, for example, a 10 percentage point increase in the interacted System Pressure for the hospital pair to have Cerner (either due to an increase in System Pressure for Cerner for the sender, the receiver, or perhaps both) leads to a 5.5 percentage point increase in the likelihood of that pair having the same vendor. For our exclusion restriction, we assume that the four instruments only affect patient flows through their effect on whether the hospital pair has the same EHR vendor. We believe that this is plausible given the separation between the instruments and the markets in which the hospitals are located. Our primary endogeneity concern is that a focal hospital may switch its EHR vendor in order to increase patient flows with specific other hospitals in its market. Our instruments are constructed based on information on other same-system hospitals outside of the focal hospital's market. We do not believe that this is likely correlated with any local market conditions for the focal hospital. One potential limitation of this instrumental variable strategy is that, while it eliminates market-specific desires to share patients with certain hospitals, it will not necessary eliminate a hospital's generic desire to share patients with, for example, Epic hospitals.

Figure D10: Illustration of System Pressure Instrument Calculation



Notes: 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 Referrals

	Same EHR Vendor	Fraction of Referrals		
	(1)	(2)	(3)	(4)
	FS	RF	2SLS	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.7. Column 2 presents reduced form results of the effect of the four instruments on fraction of referrals as given by Equation D.8. 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 4.1. * p<0.10, ** p<0.05, *** p<0.01.

Table D14: Instrument for Same Vendor - # of Transfers

	Same EHR Vendor	# of Transfers		
	(1)	(2)	(3)	(4)
	FS	RF	2SLS	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.7. Column 2 presents reduced form results of the effect of the four instruments on number of transfers as given by Equation D.8. 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 4.1. * p<0.10, ** p<0.05, *** p<0.01.

Table D15: 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.7. Column 2 presents reduced form results of the effect of the four instruments on number of referrals as given by Equation D.8. 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 4.1. * p<0.10, ** p<0.05, *** p<0.01.

Table D16: Instrument for Same Vendor - Transfer Patient Charges

	Same EHR Vendor	Charges		
	(1)	(2)	(3)	(4)
	FS	RF	2SLS	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.7 (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.8 (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 D17: Instrument for Same Vendor - Referral Patient 60-Day Readmission Rates

	Same EHR Vendor	60-Day Readmit		
	(1)	(2)	(3)	(4)
	FS	RF	2SLS	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.7 (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.8 (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.2.8 Role of Measurement Error

The exact year in which a hospital switches EHR vendors is difficult to define. The AHA IT Survey—our primary data source—defines the switch year as the year in which the new EHR system is used by the majority of a hospital's patients, while HIMSS—our secondary data source—defines it as the year in which the new system is live and operational, likely for all patients. This section illustrates the potential for measurement error in which EHR vendor each hospital has each year to attenuate our results on patient flows by instrumenting for whether a hospital pair has the same vendor in one survey (e.g., HIMSS) with whether the hospital pair has the same vendor in the other survey (e.g., AHA IT). Results are shown in Table D18. These 2SLS estimates are on average twice the magnitude of the OLS estimates, suggesting that measurement error is significantly attenuating our estimates of the effect of interoperability on patient flows.

Table D19 next does the same analysis but limits the sample to the event study hospital pair sample and further makes 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 the measurement error stems from error in the exact year of an EHR vendor switch rather than error in general vendor identity that is evenly distributed across relative time.

Table D18: Role of Measurement Error in 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 D19: Role of Measurement Error in 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.2.9 Evidence of Spillovers in Reduced Form Analysis of Effect on 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 in this setting 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 in our setting because 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.[100]

Appendix Table D20 shows evidence that spillovers exist in this setting. The table displays difference-in-difference regression estimates at the hospital sender-receiver-year level where the outcomes are patient flows between the hospital pair and the two 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, where market shares are calculated using hospital beds, our market definition from Equation 4.1 (HRR for transfers; hospitals with baseline patient flows for referrals), and excluding the focal hospital. As in Equation 4.1, hospital sender-receiver pair and year fixed effects are included, with standard errors clustered at the sender-receiver level. Crucially, we limit the sample in these tables 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.

The results indicate that the market shares of both the sending and receiving hospitals' EHR vendors affect patient flows between the hospital pair. While the hospital pair never shares the same vendor, the decisions of other hospitals in their markets (which determine the market shares) affect their patient flows. For example, Column 1 of Panel B in Appendix Table D20 tells us 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

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

contrast, when the receiving hospital's vendor market share increases by 10 percentage points, the fraction of referrals that the sending hospital sends to the receiving hospital increases by 0.03 percentage points (2% of the mean).[101] 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 less than that of 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 treatment estimates.

While these results indicate the presence of spillovers, they unfortunately do not indicate the overall direction of their bias. Estimates for the effect of 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 and thus not all control units will be affected in the same direction. Abstracting away from market shares, a simple model can clearly illustrate how both positive and negative spillovers are possible, depending on our assumptions regarding the treatment effects on treated and control pairs. Imagine 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 the treated pair AB (since A and B gain the same vendor over time) as well as control pairs AD, BC, BD, and CD; pair AC is discarded because the two hospitals lose the same vendor over time. Assume that the number of patients sent from each hospital remains the same over time. Further, assume that A—after switching EHR vendors—reallocates some of its patients to B that had previously gone 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 and thus we would have 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 and thus we would have a positive spillover. And, alternatively, if both B and C react in the described manners but exactly equal in magnitude, then the total spillover to control pairs would be zero. The direction of the bias is non-obvious.

[101] Sender's vendor market share has a mean of 0.177 and a 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 D20: Evidence of Spillovers for Shared Patient Flows

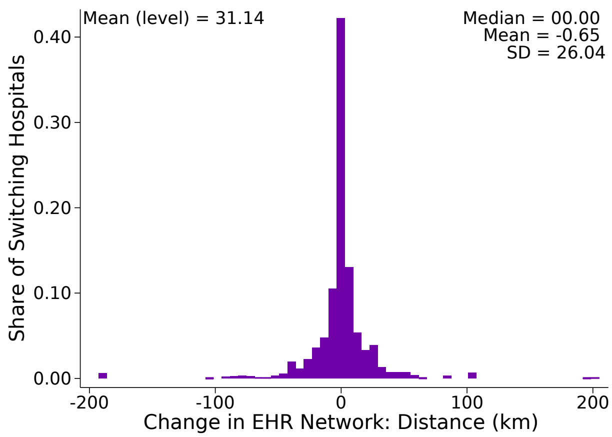
	(1)	(2)	(3)	(4)	(5)
Panel A: # of Referrals					
Sender's Vendor Market Share	2.592	3.147	4.614*	4.614	0.0949
	(2.652)	(2.572)	(2.678)	(3.981)	(4.905)
Receiver's Vendor Market Share	15.68***	15.20***	8.611***	8.611**	13.83***
	(2.216)	(2.097)	(1.966)	(3.436)	(4.103)
Observations	1131337	1131337	1109715	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.

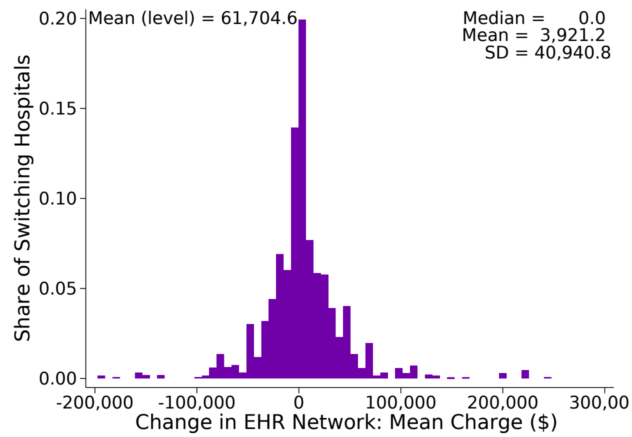
D.3 Additional Evidence of Allocative Effect on Patient Outcomes

D.3.1 Distributions of $\triangle\text{Network}_{Yh}$

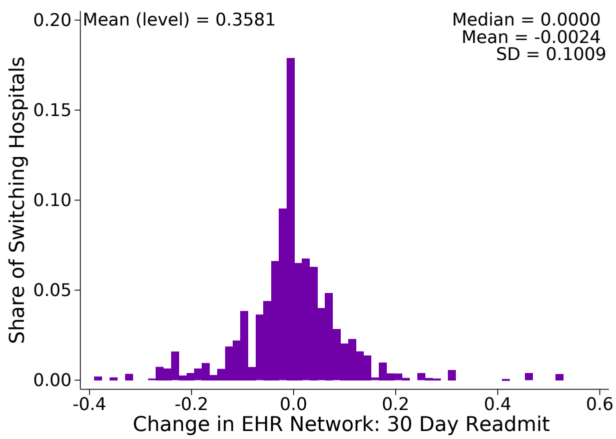
Figure D11: Changes in Local EHR Network Characteristics for Transfers after Vendor Switch



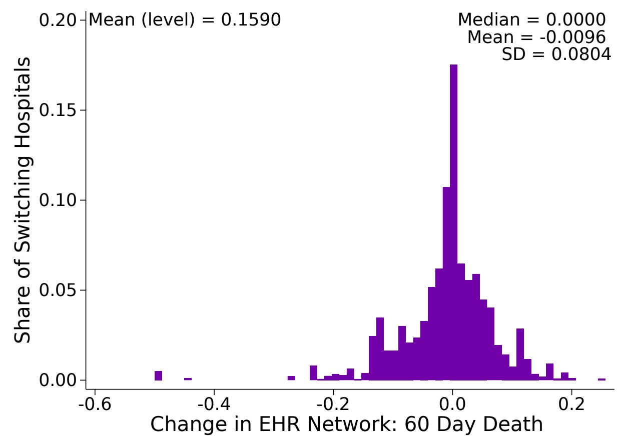
(a) Distance (km)



(b) Mean Charges



(c) Readmit 30 Days

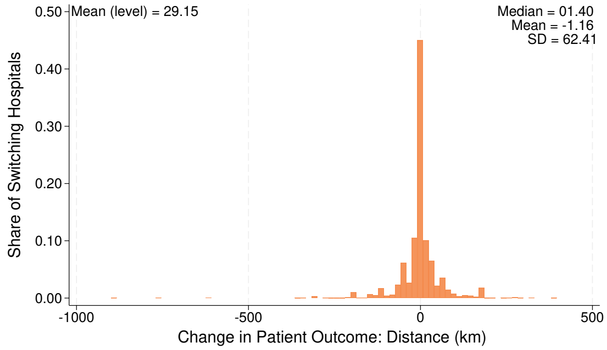


(d) Death 60 Days

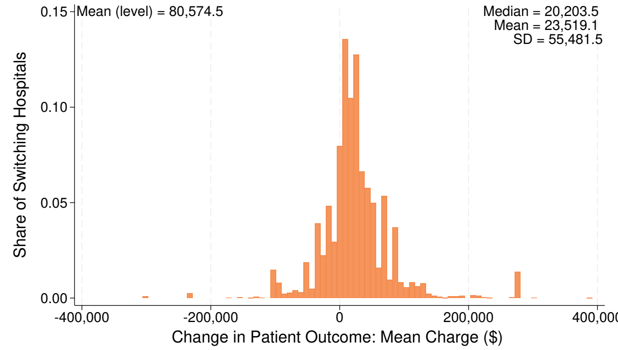
Notes: These figures plot distribution of the raw changes in baseline characteristics of a switching hospital's local EHR network from before versus after the hospital switches vendors. No controls; simply average of the outcome after minus average of the outcome before. Hospital observations are weighted by baseline bed size.

D.3.2 Distributions of $\triangle Y_h$

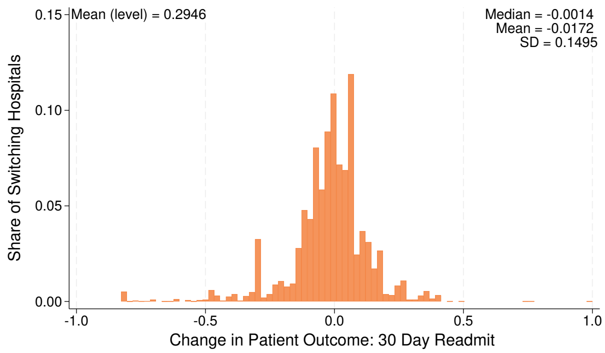
Figure D12: Changes in Same-Vendor Transfer Patient Outcomes after Vendor Switch



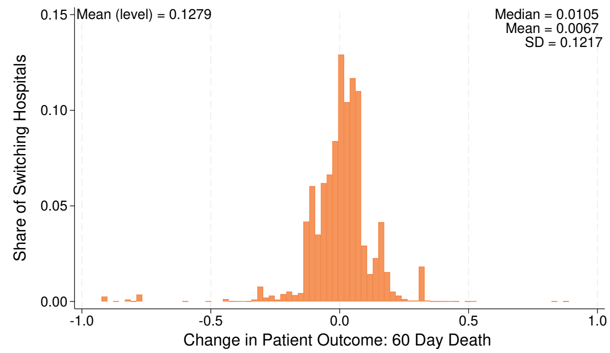
(a) Distance (km)



(b) Mean Charges



(c) Readmit 30 Days

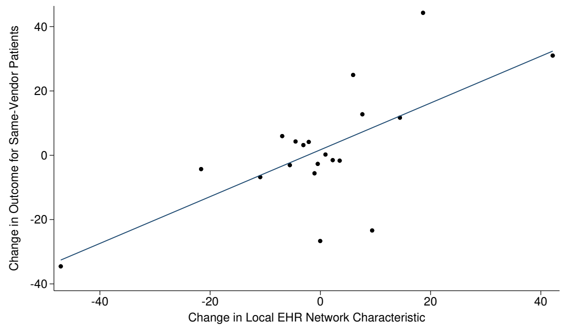


(d) Death 60 Days

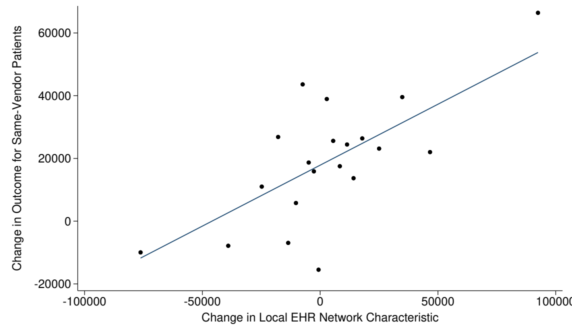
Notes: These figures plot distribution of the changes in experienced patient outcomes for same-vendor-sent patients sent from a switching hospital before versus after that hospital switches vendors. No controls; simply average of the outcome after minus average of the outcome before. Hospital observations are weighted by baseline bed size.

D.3.3 Relationship between $\triangle Y_h$ and $\triangle \text{Network}_{Yh}$

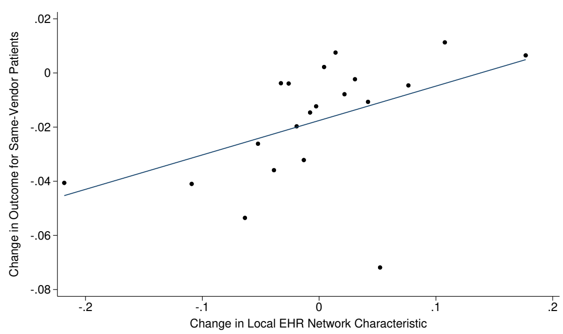
Figure D13: Changes in Patient Outcomes vs. Changes in Local EHR Network for Transfers



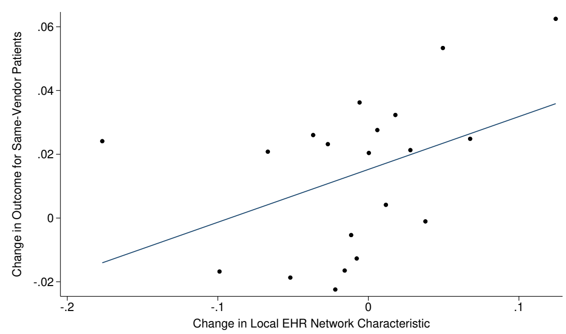
(a) Distance (km)



(b) Mean Charges



(c) Readmit 30 Days



(d) Death 60 Days

Notes: These figures plot the raw 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) and changes in baseline characteristics of a switching hospital's local EHR network from before versus after the hospital switches vendors (x-axis). Hospital observations are weighted by baseline bed size.

D.3.4 Average Effect of Vendor Switch on Same-Vendor Patient Outcomes (Non-Interacted Version of Table 7)

Table D21: 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.3.5 Robustness of Predictive Validity of Changes in Local EHR Network Characteristics

Table D22: Predictive Validity of Changes in Local EHR Network Characteristics - Robustness

(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.3.6 Patient Balance Across Deciles of $\triangle\text{Network}_{Yh}$

Table D23: 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

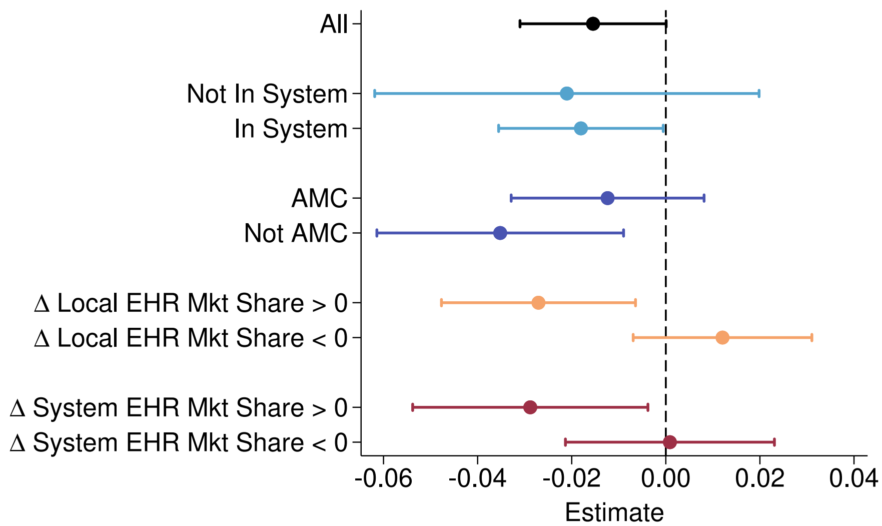
(b) Transfers, 60-Day Death

	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

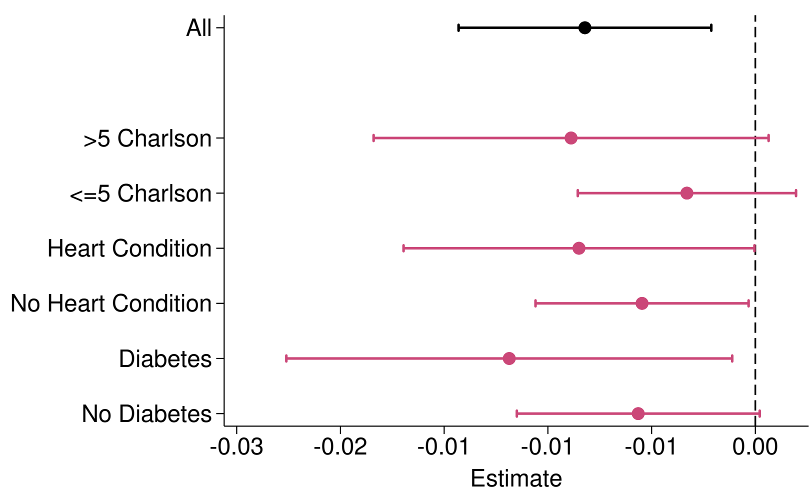
Notes: Each column is a separate patient-level regression of patient characteristics on whether the transfer patient is in the top decile (compared to being in the bottom decile) of change in local EHR vendor network for 30-day hospital readmission rates for transfer patients (Panel A) or 60-day death 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.3.7 Additional Predictive Validity Heterogeneity

Figure D14: Additional Heterogeneity for Predictive Validity



(a) 30 Day Readmit for Transfers



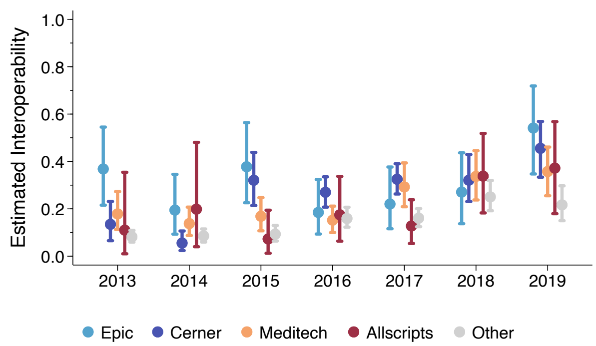
(b) 60 Day Death for Referrals

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 transfer (Panel A) or referral (Panel B) 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 30-day readmission rates for received transfer patients (Panel A) or lower baseline 60-day mortality rates for received referral patients (Panel B). The estimates show the effect of switching on 30-day readmission rates for same-vendor transfer patients (Panel A) and 60-day mortality rates for same-vendor referral patients (Panel B), grouped by hospital, network, or patient characteristics. "In System" indicates the sending hospital belongs to a hospital system. "AMC" denotes an Academic Medical Center. "Δ Local EHR Mkt Share > 0" signifies a switch to vendors with greater local market presence, while "Δ System EHR Mkt Share > 0" indicates a switch to vendors more prevalent within the hospital's system.

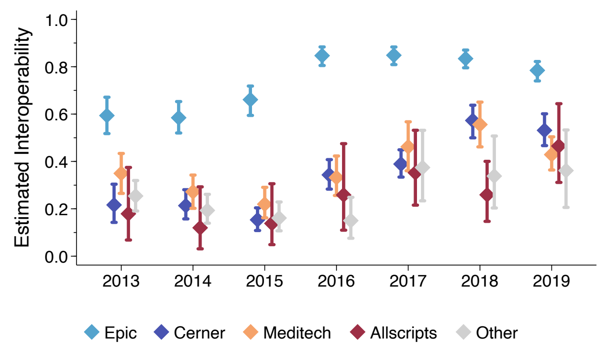
D.4 Additional Details of Model of Patient Flows

D.4.1 Additional Visualizations of Interoperability Estimates

Figure D15: Estimated Across- and Within-Vendor Interoperability Levels - Each Year



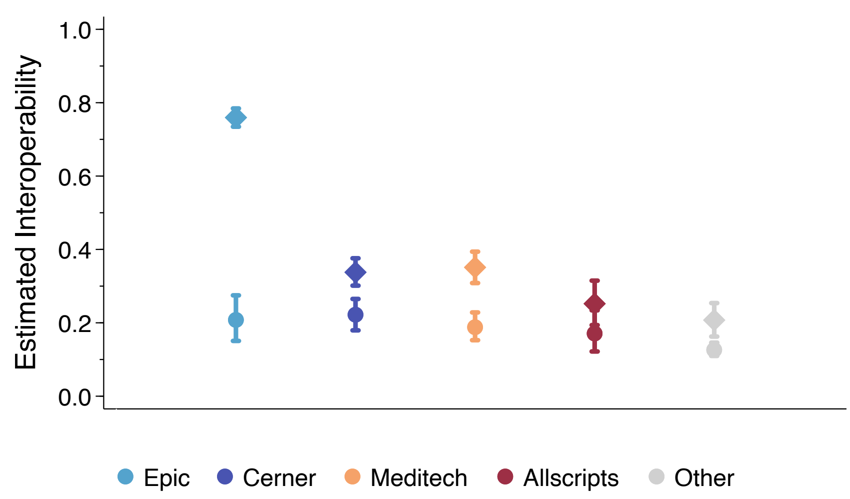
(a) Across-Vendor



(b) Within-Vendor

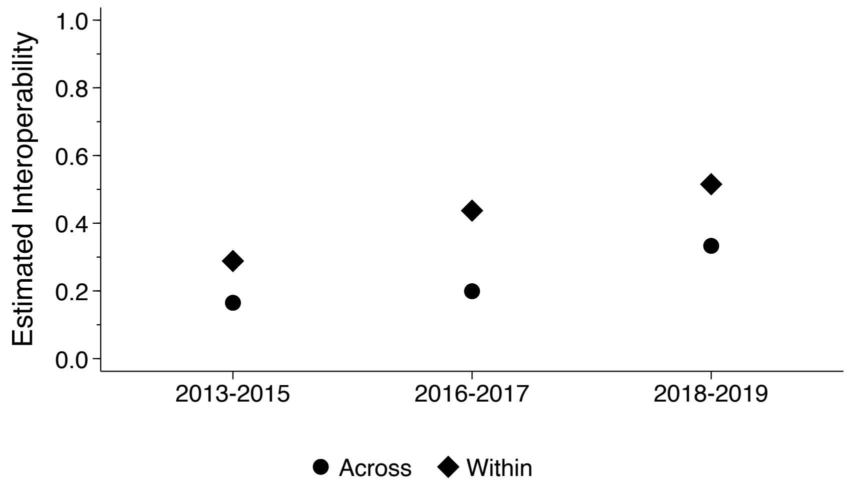
Notes: These figures plot ordered probit estimates of across-vendor (Panel A) and within-vendor (Panel B) interoperability, separately by EHR vendor and year, following the estimation strategy described by Equation 6.3 with more detail in Appendix Section C.2. Dots represent point estimates while bars show 95% confidence intervals, which are calculated using 1000 bootstrapped samples. The colors in both panels consistently represent vendors, e.g., blue is always Epic.

Figure D16: Estimated Across- and Within-Vendor Interoperability Levels - All Years



Notes: These figures plot ordered probit estimates of across-vendor (dots) and within-vendor (diamonds) interoperability, separately by EHR vendor, following the estimation strategy described by Equation 6.3 with more detail in Appendix Section C.2. All years are pooled. Dots and diamonds represent point estimates while bars show 95% confidence intervals, which are calculated using 1000 bootstrapped samples.

Figure D17: Estimated Across- and Within-Vendor Interoperability Levels - Average by Year



Notes: These figures plot ordered probit estimates of across-vendor (dots) and within-vendor (diamonds) interoperability, separately by time period, following the estimation strategy described by Equation 6.3 with more detail in Appendix Section C.2. Data is pooled for 2013-2015, 2016-2017, and 2018-2019, i.e., these estimates represent average interoperability over these year bins. All vendors are pooled. No confidence intervals are plotted.

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

Same-vendor status serves as a proxy for interoperability. Using this proxy is beneficial for estimating event studies centered around a binary independent variable and for extending our analysis period back to years before 2013, which is the first year in which we can measure interoperability. However, employing this proxy imposes the simplifying assumption that the interoperability advantage of sharing the same vendor is constant across all EHR vendors—a notion that Figure 6 suggests is inaccurate, likely due to variations in within-vendor interoperability by vendor. To incorporate variation in interoperability across vendors and over time, Table D24 compares static difference-in-differences estimates (as described in Equation 4.1 but limited to the 2013-2019 time-frame) across three distinct independent variables—the proxy indicator for same-vendor status, a time-invariant measure of interoperability between the two vendors used by the hospitals in the pair, and a time-varying measure of interoperability—across two different hospital pair samples: the event study sample utilized in all prior analyses within this section and the full sample. For nearly all outcomes and samples, employing our estimates of interoperability levels yields more precise estimates compared to utilizing the same-vendor proxy. Incorporating variation in interoperability over time further enhances precision. On average, estimates are twice as large when leveraging interoperability levels rather than the proxy, which is consistent with interoperability improving by approximately 0.5 (on a scale of 0 to 1) when moving from across-vendor to within-vendor. Overall, we interpret these findings as additional evidence that our proxy results reflect the impact of interoperability on patient flows.

Table D24: Interoperability on Shared Transfers, Referrals: Same-Vendor Proxy vs. Actual 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 4.1. 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 Decomposing the Receiver Fixed Effects in the Hospital Demand Model

Table D25: Decomposing the Receiver 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 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

Table D26: 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 parameter estimates 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 (2005). All other receiving hospitals are in the outside option. Standard errors are in parentheses.

D.5 Additional Exhibits for Counterfactuals

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

Table D27: 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
Perfect Within (Within = 1)	6.9%	24.5		27.3		-2.8	0.059
Epic Monopoly (All Epic)	12.5%	44.2		43.8		0.4	0.02
Minimum Standard (Min. = 0.51)	7.0%	24.8		24.5		0.3	0.012

Notes: Table displays the results of four counterfactual scenarios: (1) increase all interoperability levels to 1; (2) increase within-vendor interoperability levels to 1; (3) all hospitals switch to Epic and thus all interoperability levels are set to Epic's within-vendor level in each time period; (4) increase all interoperability levels to a minimum of 0.51, which is the patient-weighted average interoperability level in 2019. "%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 counterfactuals than in the data. In contrast to Table 9, this table limits the sample of patients to those who choose hospitals in the market under the data conditions ("intensive patients").

Table D28: 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
Perfect Within (Within = 1)	∞ %	2.7		0.0		2.7	0.079
Epic Monopoly (All Epic)	∞ %	4.3		0.0		4.3	0.157
Minimum Standard (Min. = 0.51)	∞ %	1.5		0.0		1.5	0.091

Notes: Table displays the results of four counterfactual scenarios: (1) increase all interoperability levels to 1; (2) increase within-vendor interoperability levels to 1; (3) all hospitals switch to Epic and thus all interoperability levels are set to Epic's within-vendor level in each time period; (4) increase all interoperability levels to a minimum of 0.51, which is the patient-weighted average interoperability level in 2019. "%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 counterfactuals than in the data. In contrast to Table 9, 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

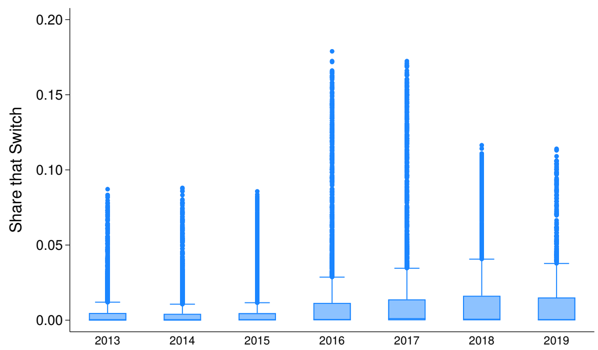
Table D29: Change in Hospital Characteristics for Patients who Switch

	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

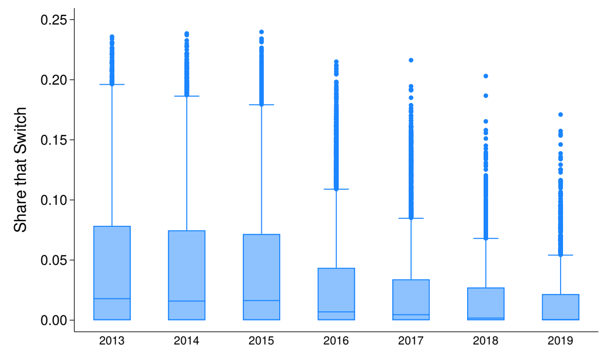
Notes: Table displays average changes in receiver hospital characteristics for patients who choose different receiver hospitals in the first-best full interoperability counterfactual than in the data (i.e., patient switchers).

D.5.3 Temporal Trends in Counterfactual Welfare Gains

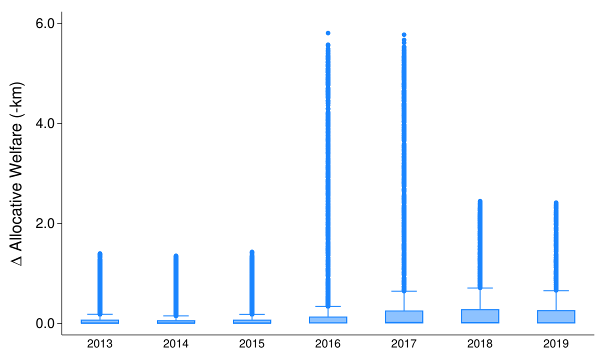
Figure D18: Counterfactual Switches and Δ Allocative Welfare by Year



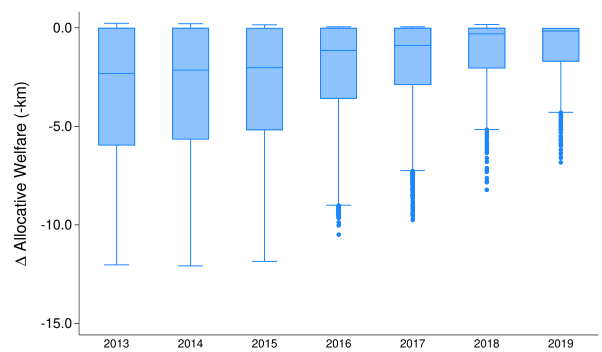
(a) Switches, First-Best CF



(b) Switches, Perfect Within CF



(c) Δ Allocative Welfare, First-Best CF



(d) Δ Allocative Welfare, Perfect Within CF

Results of two counterfactual scenarios: (1) increase all interoperability levels to 1 ("First-Best"); (2) increase within-vendor interoperability levels to 1 ("Perfect Within"). Panels A and B show the share of patients that choose different hospitals in the counterfactuals than in the data (y-axis) by year (x-axis), separately by vendor of the sending hospital. Panels C and D show the same but for a difference outcome: "Δ Allocative Welfare." Box plots are shown. Patient sample is limited to patients that choose receiver hospitals inside of the market (i.e., do not choose the outside option) under data conditions.