Management Science

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To cite this article:
Aishwarrya Deore, Martin Holzhacker, Ranjani Krishnan (2024) Direct and Spillover Effects of Quality Disclosure Regulation: Evidence from California Hospitals. Management Science 70(4):2477-2496. https://doi.org/10.1287/mnsc.2023.4817

https://pubsonline.informs.org/journal/mnsc

MANAGEMENT SCIENCE
Vol. 70, No. 4, April 2024, pp. 2477–2496
ISSN 0025-1909 (print), ISSN 1526-5501 (online)

Direct and Spillover Effects of Quality Disclosure Regulation: Evidence from California Hospitals

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Received: July 28, 2021
Revised: May 8, 2022; August 25, 2022
Accepted: October 27, 2022
Published Online in Articles in Advance: May 31, 2023

https://doi.org/10.1287/mnsc.2023.4817

Abstract. Quality disclosures regarding medical outcomes such as patient mortality are common healthcare policy instruments. Mixed evidence exists on whether quality disclosures improve outcomes for disclosed ailments. Moreover, disclosure policies can generate spillovers and impact ailments not targeted by the policy. We examine the effects of quality disclosure regulation on mortality improvements in disclosed and nondisclosed ailments. We use patient records for California hospitals for 1995–2014 and construct three groups of ailments: those that were the target of disclosure regulation, complementary ailments that were not the target of disclosure regulation but are medically related to the disclosed ailments, and medically unrelated ailments. Using a difference-in-differences design, we find that quality disclosure regulation is associated with declines in mortality risk of disclosed ailments ranging from 11.5% to 23%. Quality disclosure regulation is also associated with improvements for complementary ailments that are not the target of the disclosure policy. Such positive spillover effects yield an estimated 15.2% decline in mortality risk. Consistent with demand-side pressures driving improvements, market shares become sensitive to disclosed quality measures after disclosure regulation. Our findings of direct and spillover effects have implications for disclosure regulation.

History: Accepted by David Simchi-Levi, accounting.
Supplemental Material: The data files and online appendix are available at https://doi.org/10.1287/mnsc.2023.4817.

Keywords: quality disclosure • spillover • patient safety • healthcare

1. Introduction

Mandatory disclosures of quality performance are pervasive, especially in the healthcare sector. Mandatory quality disclosures result from efforts by a regulatory or certification agency to systematically measure and publicly report quality to consumers and other market participants (Dranove and Jin 2010). These disclosures are costly to design and implement, and their value depends on the extent to which they promote real improvements. Two questions of importance to regulators, consumers, and firms are as follows. First, do quality disclosures lead to real improvements in the quality of targeted outcomes? Second, does the disclosure policy generate spillovers by impacting outcomes beyond those targeted by the policy? Extant evidence on the effects of disclosure regulation on health quality outcomes is mixed (Evans et al. 1997, Dranove et al. 2003, Hannan et al. 2003, Werner et al. 2009, Li et al. 2010, Chen and Meinecke 2012, Lu 2012). Identifying disclosure effects is challenging because of confounding factors such as innovations in technology, patient risk, and patient selection. Spillover effects are also difficult to detect because of medical differences in the disclosed vs. nondisclosed ailments, which makes it difficult to pinpoint the reasons for improvements, or lack thereof, in the nondisclosed ailments.

We study the direct and spillover effects of quality disclosure regulation in the California hospital industry. The changes to the regulatory structure of the California hospital industry over the last two decades allow us to test whether the disclosure policy has spillover effects. California hospitals were subject to disclosure regulation that targeted six ailments for mandatory mortality reporting. To identify the direct and spillover effects resulting from the disclosure regulation, we extract the principal diagnosis from patient records (i.e., the diagnosis primarily responsible for the admission of the patient during the focal hospitalization) to classify patients into three ailment groups. The first group, labeled *Disclosed Ailments*, consists of patients with ailments *directly* targeted by the disclosure regulation (stroke, heart attack, heart failure, gastro hemorrhage, hip fracture, pneumonia).[1] The second group consists of patients with ailments not directly targeted by the disclosure regulation but who *indirectly* benefit from medical

complementarities with *Disclosed Ailments*. We label this group *Specialty Ailments*, which includes patients treated for an ailment of the same specialty (cardiovascular, digestive, injuries, and respiratory) as one of the disclosed ailments.[2] The third group consists of patients with ailments that are neither the direct target of disclosure regulation nor share medical complementarities with the disclosed ailment (labeled *Other Ailments*).

We gather patient records from the Patient Discharge Data (PDD) database of the California Office of Health and Statewide Planning and Development (OSHPD) for 1995–2014 to create patient cohorts and construct the variables required for our analysis.[3] To measure the quality outcome, we construct mortality rates per hospital, year, and patient group.[4] We first examine whether disclosure influences quality of outcomes for *Disclosed Ailments*. We use a difference-in-differences analysis to compare postdisclosure changes in mortality for the *Disclosed Ailments* relative to (a) *Other Ailments* and (b) *Specialty Ailments*. We include ailments, hospital, and time fixed effects, cohort-level controls for patients' severity of illness, hospital-level controls for size, patient mix, profitability, and market competition. We find that quality disclosure regulation improves mortality for *Disclosed Ailments* relative to *Other Ailments* and *Specialty Ailments,* consistent with a direct impact of disclosure regulation. The economic magnitudes of these effects are meaningful, as we find a decline in mortality of 23% (using *Other Ailments* as a benchmark) and 11.5% (using *Specialty Ailments* as a benchmark).

We next examine whether disclosure regulation has spillover effects on ailments not targeted by disclosure regulation. We use a difference-in-differences design and compare the changes in mortality after the disclosure regulation for *Specialty Ailments* versus *Other Ailments*. We find evidence of positive quality spillover effects that are economically meaningful and associated with a decrease in mortality risk by 15.24%.

Spillovers stem from treatment complementarities within specialties due to shared knowledge, labor, and infrastructure (Panzar and Willig 1977, Hill and Hoskisson 1987). We test this through a more granular analysis within each specialty of disclosed ailments. We delve deeper into *Specialty Ailments* to explore a patient's secondary diagnoses or *comorbidities*, that is, ailments that were not the reason for the admission but coexist. Patients with comorbidities (e.g., cardiomyopathy) that are part of the same specialty as the disclosed ailment (e.g., cardiovascular) will have a larger scope for spillovers than patients with comorbidities from unrelated specialties (e.g., diabetes, which belongs to the endocrinology specialty). We find greater improvements in the former group following disclosure regulation. We unpack two contextual features of the environment that accentuate or attenuate such spillovers: (1) patient volume for the regulated ailment and (2) emerging routines.

In additional analyses, we find that a hospital's market share for a disclosed ailment is sensitive to its quality after the introduction of mandatory disclosure regulation, but not before its introduction. These results support the notion that demand-side pressures may be one of the potential driving forces behind the improvements we observe in our study. We also find that hospitals located in markets with higher competition have greater quality improvements.

We offer three extensions to the literature on quality disclosures. First, although quality disclosures are regarded as important policy instruments, calibrating the impacts are difficult (Dranove and Jin 2010). By matching the disclosed ailments with similar yet nondisclosed ailments using patient-level data, we obtain a more rigorous test of whether quality disclosures lead to improvements. Our model minimizes the risk that results are driven by underlying patient factors such as severity of illness, ailment characteristics such as technology improvements, or market characteristics such as demographic shifts. Our process of matching treatment and benchmark groups allows for a greater degree of commonality, reduces unobserved heterogeneity between the groups, and improves the rigor of the empirical specifications. Thus, we respond to the Agency for Healthcare Research and Quality (AHRQ) report (2012) that calls for studies that investigate mortality changes following public reporting using appropriate comparison groups, which has been a topic of importance for regulators. Relative to the literature that studies specific *procedures* (e.g., coronary artery bypass graft (CABG)), we examine specific *ailments* (e.g., stroke).[5] Procedures such as CABG (for treatment of coronary heart disease) have alternatives (e.g., angioplasty). When one of the procedures is targeted for disclosure regulation, hospitals can strategically allocate patients to alternative procedures and manage the quality of regulated procedures. This is not feasible with ailments, which are diagnosed based on the symptoms present at time of admission. Furthermore, our study examines a broader set of regulated ailments over a longer period, thus providing a more comprehensive understanding.

Second, we examine whether quality disclosures improve quality outcomes of ailments not targeted by the disclosure policy. Such spillovers can occur within firms, from firms to peers, from firms to capital markets, and across industries. We focus on intrafirm spillovers, which are relevant in many settings where personnel perform complementary tasks and increased effort in one task has positive spillovers on other tasks. A fundamental debate in the health quality disclosure literature is whether public quality disclosures are welfare-enhancing or welfare-reducing (Dranove et al. 2003). Our study shows that disclosure policies that focus on a limited set of services can achieve broader overall benefits by exploiting positive intrafirm spillovers.

Finally, we explore factors through which the effects of quality disclosure regulation spill over to ailments that were not targeted for disclosure. Assessment of the effects of public quality disclosures not only requires robust methodology and study of both direct and indirect spillover effects but also analysis of how such effects on nondisclosed dimensions occur.

2. Related Literature

2.1. Direct Effects of Mandatory Quality Disclosure Regulation

Mandatory quality disclosures are a popular policy tool in healthcare settings to redress adverse effects of information asymmetry between consumers and healthcare providers (Akerlof 1970, Dranove and Satterthwaite 1992). Quality disclosures can prompt quality improvements due to several factors such as demand push, the intrinsic utility of physicians to provide high-quality services, and providers' concerns for patient outcomes.[6] Quality performance is an integral part of healthcare providers' professional ethos (Lindenauer et al. 2007) and professional norms (Hyman 2001). Regulated quality disclosures provide feedback to physicians and hospitals about their own and peer performance, which enables learning and provides additional motivation. For example, Evans et al. (1997) find that poorly performing Pennsylvania hospitals use information from mandated disclosures of hospital mortality performance for benchmarking and quality improvements. Hannan et al. (2003) find that four of the five regions with CABG surgery mortality disclosure programs had significantly lower mortalities relative to the rest of the country after the disclosure regulation.

Some evidence points to adverse effects of certain types of disclosure regulation. Early disclosure regulation inadequately adjusted for patient risk. As a result, hospitals and physicians who treated the sickest patients ended up with the worst reported performance. This encouraged cream skimming behaviors whereby providers refused to treat sicker patients (Dranove et al. 2003, Cutler et al. 2004, Werner and Asch 2005), leading to under-provision of care for sicker patients (Werner and Asch 2005). In New York and Pennsylvania, after the introduction of report cards for CABG, almost two thirds of cardiac surgeons expressed reluctance to provide surgery for high-risk patients. Regulators responded to the criticism by risk-adjusting disclosed quality measures that accounted for patient characteristics and comorbidities.

A 2012 review by the Agency for Healthcare Research and Quality (AHRQ) examines whether public reporting results in improvements in healthcare quality (Totten et al. 2012).[7] The review concludes that (pg. ES-8) "most of the studies found a decrease in mortality, although these results are not uniformly consistent and many questions about the appropriateness of the comparisons (both groups and risk-adjustment methods) are an ongoing subject of debate." The study also examined whether public reporting leads to changes in healthcare delivery structures or processes, which can impact quality. A conclusion was that (ES-8) "providers, both individual clinicians and organizations, responded to public reports by making positive changes in their behavior."

As illustrated previously, a concern expressed by prior literature is the difficulty in empirically determining the effects of healthcare disclosures on quality outcomes. A challenge is to distinguish the effects of the disclosure regulation from general improvements over time in technology or advancements in medical science. We propose a remedy by developing several treatment and benchmark groups that enable isolating the effects of disclosure regulation from other changes that can drive quality improvements. We next summarize the history of disclosure regulation that provides the setting for the empirical analyses.

2.2. History of Quality Disclosure Regulation in Healthcare

A watershed moment in the U.S. debate on hospital quality was the publication of the article "To Err is Human" by the Institute of Medicine in 1999, which showed that about 98,000 patients died each year in hospitals from preventable medical errors. An increased interest in and demand for quality information ensued. During the early 2000 s, New York and Maryland adopted report cards for surgeons that disclosed mortality rates for cardiology-related ailments. Evidence is equivocal on the quality effects of report cards (Rainwater et al. 1998, Marshall et al. 2000). In May 2000, the AHRQ used hospital discharge data from the Healthcare Cost and Utilization Project (HCUP) to study quality indicators that would be suitable output measures. This study titled "Refinement of Quality Indicators" (Davies et al. 2010) reviewed more than 2,600 journal articles to develop a preliminary indicator set of 200 potential ailments. From this list, the study recommended that quality disclosures should include mortality indicators for inpatient ailments for which mortality varies substantially across institutions, and for which empirical evidence suggests that high mortality may be associated with deficiencies in the quality of care. These ailments included stroke, heart attack, heart failure, gastro hemorrhage, hip fracture, and pneumonia.

Following the AHRQ recommendations, there was pressure for a federal quality reporting initiative. Mandatory quality regulation was passed in December 2003 under the Medicare Prescription Drug, Improvement, and Modernization Act, with implementation responsibility assigned to the Centers for Medicare & Medicaid Services (CMS). CMS designed a website *Hospital Compare* (https://www.medicare.gov/care-compare/) that provided disclosures on a variety of indicators for

selected ailments, including heart attack, heart failure, and pneumonia. Studies examining the effects of disclosure regulation on mortality report equivocal results. For example, Werner and Bradlow (2010) find postregulatory improvements in mortality for heart attacks but not for pneumonia or heart failure. The study lacked a benchmark group and notes (Werner and Bradlow 2010, 1319) "we cannot conclude that public reporting caused the improvement in processes or outcomes." Ryan et al. (2012) find no reductions in mortality beyond existing trends for heart attack and pneumonia, and modest reduction in mortality for heart failure, relative to an unmatched benchmark group of nonreported diagnoses. Joynt et al. (2016) examine mortality rates for three disclosed ailments relative to a benchmark group of unrelated high-volume ailments and do not find conclusive evidence of a postregulatory decline in mortality. Their analysis does not control for the changing profile of illness severity of patients in various groups.

In addition to the required *Hospital Compare* disclosures, the State of California launched additional disclosures of mortality for six selected ailments, including heart attack, heart failure, gastro hemorrhage, hip fracture, stroke, and pneumonia, through its own website (see Online Appendix EC.1. for an overview of ailments selected for disclosure and their specialties). Disclosure was mandatory, and hospitals could lose their license for failure to comply.[8]

2.3. Spillover Effects of Quality Disclosures

Although disclosure regulation could spur improvements in quality for the ailments targeted by disclosure, its potential effect on nontargeted ailments is unclear. Hospitals could undertake quality improvement efforts broadly, with benefits to other ailments through positive spillovers. Spillovers do not occur automatic: Hospitals could choose to narrowly focus on the targeted ailments instead. Even negative spillovers could occur if providers allocate resources away from nontargeted ailments (Lu 2012). To identify spillovers, we use multitask theory as a framework for our empirical design (Holmstrom and Milgrom 1991). If two ailments are complementary, then increasing inputs to improving the quality of one ailment will positively spill over to improvements in quality of the other complementary ailment but not yield any improvements in the quality of a third unrelated ailment. We posit that disclosure regulation provides the impetus for motivating an increase in inputs to improving quality of a disclosed ailment, which in turn improves quality outcome in the specialty that includes the disclosed ailment. Conversely, when two services do not exhibit complementarities, there would be no spillovers. Hence, disclosure regulation has no effect on other, unaffected ailments.[9]

Spillovers from complementarities are likely for patients who have ailments included the same specialty as the disclosed ailment.[10] For example, one of the ailments targeted by disclosure regulation is *hip fracture*, which belongs to the *Injuries* specialty. Another ailment belonging to this specialty is *femur fracture*, which is the fracture of the thigh bone. A femur fracture is very similar to a hip fracture. In fact, a hip fracture refers to a fracture in the upper quarter of the femur bone. If the fracture occurs more (less) than 2.5 inches from the neck of the femur and a lower bony prominence, it is a femur (hip) fracture. The clinical pathways followed for a hip fracture and a femur fracture are almost identical and display high degrees of complementarity in treatment. If hospitals and physicians gain knowledge, increase effort, deploy more resources, or restructure treatment processes to improve the quality of hip fractures after disclosure regulation, positive spillovers will occur to the quality of femur fractures and other ailments in the *injury* specialty.

3. Data and Methodology

3.1. Data

The analyses use OSHPD's PDD for 1995–2014. The PDD data contain patient level information on the population of patients in the state of California. For each year, the database contains about 3–4 million patient observations, which amounts to about 60 million observations across 19 years. Patient diagnoses are recorded using an ICD code.[11] Each patient-level observation includes the hospital ID, patient diagnosis, comorbidities procedures, in-hospital mortality, length of stay, and type of insurer. We focus on mortality rate, which is the most widely used objective measure of treatment quality and has been the focus of various state and national level disclosure regulation (Ryan et al. 2012, Silber et al. 2016). We measure in-patient mortality by group in a given hospital as

$$MR_{ijt} = \frac{Deaths_{ijt}}{Admits_{ijt}}, \tag{1}$$

where MR_{ijt} reflects the mortality rate in hospital i for group j in year t.

3.2. Difference-in-Differences Empirical Design

We compare changes in mortality rates of treatment groups relative to various benchmarked groups using the following difference-in-differences specification:

$$MR_{ijt} = \alpha_i + \rho_j + \eta_t + \beta Treatment \times Post + \delta \boldsymbol{X} + \varepsilon_{ijt}, \tag{2}$$

where MR_{ijt} reflects the mortality rate in hospital i, group j, and year t; α_i, ρ_j, and η_t are hospital fixed effects, ailments fixed effects, and year fixed effects, respectively.[12] Treatment is a binary variable that takes the value of one for a treatment group and zero otherwise. Post is a binary variable that takes the value of one if the

observation year belongs to the postdisclosure regulation time-period, that is, 2004–2014, and zero otherwise; β, the coefficient on *Treatment* × *Post*, reflects the improvements in mortality rates of a treatment group relative to a benchmark group following disclosure regulation. $\boldsymbol{X}$ is a vector of control variables that include patient characteristics such as patient risk and insurance, and hospital-related control variables including beds, profitability, and competition obtained from OSHPD's hospital annual financial disclosure reports.[13] Standard errors are clustered by hospital.[14] Appendix B contains variable descriptions.

4. Analyses of Direct and Spillover Effects

4.1. Direct Effects of Disclosure Regulation on Healthcare Quality

We begin by examining the direct effect of disclosure regulation on healthcare quality. For this, we examine whether mortality rates of the six ailments targeted by disclosure regulation (*Disclosed Ailments*) improved postregulation, relative to three benchmark groups. The first benchmark group (labeled *Other Ailments*) consists of a set of ailments that share minimal complementarities with *Disclosed Ailments* and are unaffected by disclosure regulation. For each hospital-year, up to six observations belong to the treatment group, *Disclosed Ailments* (i.e., stroke, heart attack, heart failure, gastro hemorrhage, hip fracture, and pneumonia). One observation belongs to the benchmark group, *Other Ailments* (i.e., all unaffected specialties without an ailment subject to the mandatory reporting). Our ailments fixed effect structure control for differences across ailments.

Means and standard deviations of mortality rates are presented in column (1) (for *Disclosed Ailments*) and column (2) (for *Other Ailments*) of Panel A, Table 1.[15] The means and standard deviations of individual ailments are provided in Online Appendix EC.3. To measure direct effects of disclosure regulation, we estimate Equation (2) using *Disclosed Ailments* as the treatment and *Other Ailments* as the benchmark group.[16] If disclosure regulation encourages quality improvements for ailments targeted by the regulation, we should observe greater improvements in mortality for the *Disclosed Ailments* relative to *Other Ailments* in the postregulation period. That is, we should observe a negative coefficient on *Disclosed Ailments X Post* (β). Estimation results are presented in Table 1, Panel B, column (1). The coefficient of the treatment effect *Disclosed Ailments* × *Post* (β) is negative and significant, indicating that relative to *Other Ailments*, *Disclosed Ailments* experiences larger declines in mortality rates following disclosure regulation. Specifically, the mortality of *Disclosed Ailments* declined by 1.48 percentage points, a relative decrease in mortality of 23.02%.[17] These results indicate that following disclosure regulation, ailments targeted by regulation improved more than a benchmark group of nontargeted ailments.[18]

Our model relies on the assumption that the treatment and benchmark groups would have followed similar trends in mortality rates in the absence of disclosure regulation. We investigate the validity of this assumption by plotting mortality rates around the introduction of disclosure regulation in Figure 1(a). We find evidence of parallel trends between the treatment group (*Disclosed Ailments*) and the benchmark group (*Other Ailments*) in the period leading up to the disclosure regulation. Importantly, no discernable trends are visible in the mortality rates of *Disclosed Ailments* relative to *Other Ailments* in the years preceding the disclosure regulation. Further, the decline in mortality rates of *Disclosed Ailments* relative to *Other Ailments* aligns with the introduction of disclosure regulation, thereby bolstering confidence that the observed improvements can be attributed to disclosure regulation.[19]

To gain a deeper understanding, we use two additional benchmark groups to gauge the direct effect of disclosure regulation. First, to address the issue of differences in production functions between *Disclosed Ailments* and *Other Ailments*, we use ailments that belong to the same specialties that include the *Disclosed Ailments*. We label such a benchmark group as *Specialty Ailments*. For example, heart attack (which belongs to *Disclosed Ailments*) is in the cardiovascular specialty. The cardiovascular specialty also includes arrhythmia. Therefore, arrhythmia belongs to the benchmark group of *Specialty Ailments*. Up to four observations (cardiovascular, digestive, injury, and respiratory) per hospital-year belong to the benchmark group, *Specialty Ailments*. Using the disclosed ailments' own specialties as a benchmark group holds constant the effects common to the specialty in a particular hospital.[20] The complementarities in care with *Disclosed Ailments* exposes *Specialty Ailments* to spillover benefits from disclosure regulation. We examine treatment effects using *Specialty Ailments* as a benchmark group to obtain a conservative estimate of the direct effect of disclosure regulation. Means and standard deviations of mortality rates for *Disclosed Ailments* and *Specialty Ailments* are presented in columns (1) and (3) of Panel A of Table 1. Results of estimating Equation (2) using *Disclosed Ailments* as the treatment group and *Specialty Ailments* as the benchmark group are presented in Table 1, Panel B, column (2). The coefficient on *Disclosed Ailments* × *Post* (β) is negative and significant, indicating that *Disclosed Ailments* had greater quality improvements following disclosure regulation relative to *Specialty Ailments*. In terms of economic magnitude, patient mortality in *Disclosed Ailments* decreased by 0.74 percentage points, a relative decrease of 11.51%.[21] Ailments targeted by disclosure regulation improved more than a benchmark group of closely related ailments, which

Table 1. Direct Effects of Disclosure Regulation: Improvement in Disclosed Ailments

Panel A: Means (standard deviations) of mortality rates for treatment and benchmark groups

Regime	(1) *Disclosed Ailments*	(2) *Other Ailments*	(3) *Specialty Ailments*	(4) *Matched Specialty Ailments*
Predisclosure	0.0643	0.0408	0.0479	0.0550
	(0.0929)	(0.0537)	(0.0766)	(0.1037)
Postdisclosure	0.0435	0.0390	0.0363	0.0394
	(0.0897)	(0.0477)	(0.0649)	(0.0866)

Panel B: Difference-in-differences estimate of direct effects of disclosure regulation

	(1) Treatment: *Disclosed Ailments* Benchmark: *Other Ailments*	(2) Treatment: *Disclosed Ailments* Benchmark: *Specialty Ailments*	(3) Treatment: *Disclosed Ailments* Benchmark: *Matched Specialty Ailments*
Disclosed Ailments × *Post*	−0.0148***	−0.0074***	−0.0038***
	(0.0014)	(0.0010)	(0.0010)
Mean Elixhauser Index	0.0027***	0.0030***	0.0032***
	(0.0003)	(0.0003)	(0.0003)
Mean Procedures	0.0009	0.0017**	0.0030***
	(0.0007)	(0.0008)	(0.0009)
Medicare %	0.0178**	0.0080***	0.0108***
	(0.0075)	(0.0029)	(0.0035)
Medicaid %	0.0383**	0.0226**	0.0398***
	(0.0174)	(0.0095)	(0.0102)
Beds	0.0000	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)
Margin	−0.0001	−0.0001	−0.0001
	(0.0001)	(0.0001)	(0.0001)
HHI	0.0205*	0.0137	0.0157
	(0.0122)	(0.0098)	(0.0098)
Observations	48,823	70,447	76,399
R^2	0.4235	0.4512	0.3719
Sample period	1995–2014	1995–2014	1995–2014
Hospital, ailments, year fixed effects	Yes	Yes	Yes
Clustered standard errors	Hospital level	Hospital level	Hospital level

Notes. Panel A shows risk adjusted means and standard deviations of treatment and benchmark groups mortalities. Panel B reports the estimates of the difference-in-differences specifications (Equation 2) for various treatment and benchmark groups. Hospital-level clustered standard errors in parentheses.

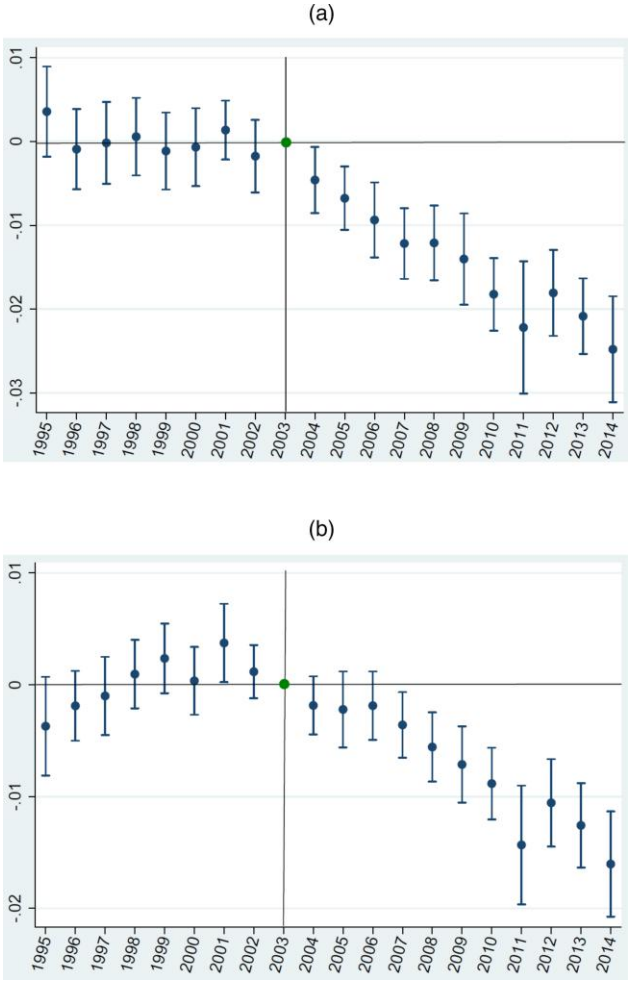
***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

provides corroborating evidence of direct effects of disclosure.

Although the choice of *Specialty Ailments* as a benchmark group allows us to keep production functions constant, predisclosure mortality rates differ among *Specialty Ailments* and *Disclosed Ailments* (see Table 1, Panel A, columns (1) and (3)). To address this, we use a specific subset of *Specialty Ailments* as a benchmark group. Ailments in this subset are matched to the corresponding disclosed ailment based on mortality rates in the year immediately preceding disclosure regulation (i.e., 2003).[22] We label this benchmark group *Matched Specialty Ailments*. For example, heart attack, which belongs to *Disclosed Ailments*, had a mortality rate of 9.40% in 2003. To construct a benchmark group for heart attack, we select a set of ailments from the cardiovascular specialty that have similar mortality rates. These ailments, which include chronic pulmonary heart disease, cardiomyopathy, late effects of cerebrovascular disease, and other diseases of pulmonary circulation, have a mean mortality rate of 9.0% in 2003.[23] Thus, this benchmark group is similar to the treatment group not only in terms of production functions, but also in terms of mortality rates, and hence more comparable in terms of scope for improvement. Since we identify benchmarks for *Disclosed Ailments* in each of the four specialties (matched cardiovascular, matched digestive, matched injury, and matched respiratory), *Matched Specialty Ailments* contains up to four observations per hospital-year.

Means and standard deviations of mortality rates for *Disclosed Ailments* and *Matched Specialty Ailments* are presented in columns (1) and (4) of Panel A of Table 1. To measure direct effects of disclosure regulation, we estimate Equation (2) using *Disclosed Ailments* as the treatment group and *Matched Specialty Ailments* as the benchmark group; results are presented in Table 1, Panel B, column (3). The coefficient on *Disclosed Ailments* × *Post* (β) is negative and significant, indicating that relative to *Matched Specialty Ailments*, *Disclosed Ailments* had greater quality improvements following disclosure

Figure 1. (Color online) Parallel Trends



Notes. (a) Direct effects of disclosure regulation: Mortality rates of *Disclosed Ailments* relative to *Other Ailments* over time. (b) Spillover effects of disclosure regulation: Mortality rates of *Specialty Ailments* relative to *Other Ailments* over time. This figure provides mortality improvements after disclosure regulation. We augment Equation (2) by replacing *Treatment* × *Post* with separate indicator variables for each year. The indicator for the year prior to the disclosure regulation is omitted in the estimation to serve as a benchmark period and therefore has a coefficient of zero. The dots plot the magnitude estimates for each year, relative to the benchmark period for *Direct Effect* (a) and *Spillover Effect* (b). The figure plots the coefficient estimates with 90% confidence intervals.

regulation. In terms of magnitude, the mortality of *Disclosed Ailments* improved by 0.38 percentage points over the benchmark group, a relative decline of 5.91%.[24] These results indicate that following disclosure regulation, ailments targeted by regulation improved to a greater extent than a benchmark group of closely related ailments that had a similar scope for improvement.

Taken together, our analyses in Table 1 find direct effects of quality disclosure regulation that are robust to various benchmark group choices. We next examine the spillover effect of disclosure regulation on healthcare quality.

4.2. Spillover Effects of Disclosure Regulation on Healthcare Quality

In this section, we investigate whether mortality rates of ailments that share complementarities with the ailments targeted by disclosure regulation (*Specialty Ailments*) improved postregulation. First, we use as a benchmark group *Other Ailments*, a group that is unaffected by disclosure regulation. For each hospital-year, our data has up to four observations that belong to the treatment group, *Specialty Ailments* (i.e., ailments in cardiovascular, digestive, injury, and respiratory specialties), and one observation that belongs to the benchmark group, *Other Ailments* (that contains all ailments unaffected by regulation). This construction allows us to control for differences across the four specialties as well as differences between the two groups.[25]

Means and standard deviations of mortality rates for these groups are presented in columns (1) and (2) of Panel A of Table 2. To measure spillover effects of disclosure regulation, we estimate Equation (2) using *Specialty Ailments* as the treatment group and *Other Ailments* as the benchmark group. If disclosure regulation triggers quality improvements that benefit the specialty as a whole and not just the targeted ailments, we should observe greater postregulation improvements in mortality rates of the *Specialty Ailments* relative to *Other Ailments*. Results in Table 2, Panel B, column (1), show that the coefficient on *Specialty Ailments* × *Post* (β) is negative and significant indicating that relative to *Other Ailments*, *Specialty Ailments* experienced greater declines in mortality rates following disclosure regulation. Mortality of *Specialty Ailments* declined by 0.77 percentage points, a decrease by 15.87%.[26] Thus, following disclosure regulation, ailments that are included in the same specialty as ailments targeted by regulation improved more than a benchmark group of unaffected ailments indicating spillover benefits.

Our model relies on the assumption that the treatment and benchmark groups would have followed similar trends in mortality rates in the absence of disclosure regulation. We investigate the validity of this assumption by plotting mortality rates around the introduction of disclosure regulation in Figure 1(b). We find evidence of parallel trends between the treatment group (*Specialty Ailments*) and the benchmark group (*Other Ailments*). Importantly, no discernable variations are visible in the mortality rates of *Specialty Ailments* relative to *Other Ailments* in the several years preceding the disclosure regulation. In contrast, a downward trend can be observed in the mortality rates of *Specialty Ailments* relative to *Other Ailments* in the years following disclosure regulation.

Although the previous analysis shows the spillover effects of disclosure regulation, the mortality rates in the predisclosure period reported in Table 2, Panel A, are slightly different for the treatment and benchmark groups. Although we control for differences in risk across groups using the Elixhauser index in all our

Table 2. Spillover Effects of Disclosure Regulation: Improvements in Specialty Ailments

Panel A: Means (standard deviations) of mortality rates for treatment and benchmark groups

Regime	(1) *Specialty Ailments*	(2) *Other Ailments*	(3) *Matched Other Ailments*
Predisclosure	0.0479	0.0408	0.0460
	(0.0766)	(0.0537)	(0.1033)
Postdisclosure	0.0363	0.0390	0.0390
	(0.0649)	(0.0477)	(0.0881)

Panel B: Difference-in-differences estimate of spillover effects of disclosure regulation

	(1) Treatment: *Specialty Ailments* Benchmark: *Other Ailments*	(2) Treatment: *Specialty Ailments* Benchmark: *Matched Other Ailments*
Specialty Ailments × *Post*	−0.0077***	−0.0032**
	(0.0011)	−0.0013
Mean Elixhauser Index	0.0040***	0.0046***
	(0.0005)	(0.0003)
Mean Procedures	0.0043***	0.0030**
	(0.0013)	(0.0011)
Medicare %	0.0046**	0.0081***
	(0.0019)	(0.0028)
Medicaid %	0.0243***	0.0248***
	(0.0079)	(0.0066)
Beds	−0.0000	0.0000
	(0.0000)	(0.0000)
Margin	−0.0001**	−0.0001***
	(0.0000)	(0.0000)
HHI	0.0054	0.0103
	(0.0075)	(0.0083)
Observations	36,324	54,669
R^2	0.6059	0.3959
Sample period	1995–2014	1995–2014
Hospital, ailments, year fixed effects	Yes	Yes
Clustered standard errors	Hospital level	Hospital level

Notes. Panel A shows risk adjusted means and standard deviations of treatment and benchmark groups mortalities. Panel B reports the estimates of the difference-in-differences specifications (Equation 2) for various treatment and benchmark groups. Hospital-level clustered standard errors in parentheses.

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

analyses, differences in the scope for improvement in the benchmark group relative to the treatment group may be only partly reflected. To address this issue, for each specialty in *Specialty Ailments*, we identify a specific subset of *Other Ailments* with similar mortality rates in the year immediately preceding disclosure regulation (i.e., 2003) as the respective benchmark group. For example, the cardiovascular specialty had a mortality rate of 4.65% in 2003. Based on their predisclosure mortality rates, we select the following ailments from *Other Ailments*: bacterial meningitis, malignant neoplasm of bone and articular cartilage, and myoneural disorders. These had a mean mortality rate of 4.75% in 2003.[27] This benchmark group that we label *Matched Other Ailments* is similar to *Specialty Ailments* in terms of mortality rates and hence has a similar scope for improvement.

Means and standard deviations of mortality rates for *Specialty Ailments* and *Matched Other Ailments* are presented in columns (1) and (3) of Table 2, Panel A. To measure spillover effects of disclosure regulation, we estimate Equation (2) using *Specialty Ailments* as the treatment group and *Matched Other Ailments* as the benchmark group. The results are reported in Table 2, Panel B, column (2). The coefficient for the treatment effect *Specialty Ailments* × *Post* (β) is negative and significant, indicating that relative to *Matched Other Ailments*, *Specialty Ailments* experienced larger declines in mortality following disclosure regulation. In terms of magnitude, this decline in mortality corresponds to 0.32 percentage points or a relative decrease of 6.68%.[28] Our analyses in Table 2 indicate robust spillover effects of disclosure regulation. Following disclosure regulation, ailments that shared complementarities with ailments targeted by regulation showed greater improvements in quality relative to various benchmark groups.

4.3. Complementarities as a Source of Spillovers: Within-Specialty Analysis

To establish complementarities as a source of spillover, we conduct a within-specialty analysis that focuses only

on patients admitted for care in a specialty that includes one of the disclosed ailments. Thus, this analysis is restricted to all patients included in the *Specialty Ailments* group that have a specialty ailment as the principal diagnosis. Within this group of patients, we identify varying levels of complementarity using information on patients' comorbidities. The subset of patients that is particularly likely to indirectly benefit from improvements spurred by disclosure regulation consists of those admitted with least one other comorbidity that is also a specialty ailment. We label such a group *Specialty with Specialty Comorb*. For example, a patient admitted with arrhythmia as a principal diagnosis and cardiomyopathy as a comorbidity—both of which belong to the cardiovascular specialty—is classified as *Specialty with Specialty Comorb*. The benchmark group consists of patients also admitted with a specialty ailment as the principal diagnosis, but with comorbidities that are not included in the specialty of the principal diagnosis. We label such a group *Specialty with Other Comorb*. For example, a patient admitted with arrhythmia as a principal diagnosis and diabetes as a comorbidity shares a similar production function as the *Disclosed Ailments* for their principal diagnosis. Although *Specialty with Specialty Comorb* would benefit from spillovers in *both* the primary diagnosis and the comorbidities, *Specialty with Other Comorb* would benefit from spillovers in *only* the primary diagnosis and not the comorbidities. Thus, *Specialty with Specialty Comorb* shares a higher degree of complementarity with the disclosed ailment and hence has a greater scope for improvement relative to the benchmark group, *Specialty with Other Comorb*. An advantage of this analysis is that it allows us to retain a similar production function, given that patients in both the treatment and control group are admitted with a principal diagnosis from the same specialty.

Panel A of Table 3 contains means and standard deviations of the previous treatment and benchmark groups. To compare spillover effects of disclosure regulation for the two groups, we estimate Equation (2) using *Specialty with Specialty Comorb* as a treatment group and *Specialty with Other Comorb* as the benchmark group. Results are presented in Table 3, Panel B, column (1). The coefficient on *Specialty with Specialty Comorb* × *Post* (β) is negative and significant, indicating that relative to *Specialty with Other Comorb*, *Specialty with Specialty Comorb* experiences larger declines in mortality following disclosure regulation. In terms of economic magnitude, the mortality of *Specialty with Specialty Comorb* declined by 0.44 percentage points more than *Specialty with Other Comorb*, a relative decrease in mortality of 7.90%.[29]

The treatment and benchmark groups in the analysis above have different levels of predisclosure mortality rates. Although the Elixhauser comorbidity index controls for patient risk, the issue of differential scopes for improvement remains. Finding a matched benchmark group based on predisclosure mortality rates (as in previous analyses) is infeasible because ailment mortality rates are reported based on patients' primary diagnosis, not their comorbidities. To mitigate concerns that the treatment group we define has a higher potential for improvements due to its higher predisclosure level of mortality, we augment Equation (2) with transformations of the dependent variables that account for differences in predisclosure mortality rates and measure *relative* improvements. We use the percentage change in mortality rate, logged mortality rate, and year-on-year log-changes in mortality rate in the analyses reported in columns (2)–(4) of Panel B, Table 3. These estimates continue to show evidence of greater improvements in *Specialty with Specialty Comorb* relative to *Specialty with Other Comorb*. Even after holding production functions constant, patient groups that share larger complementarities in care with disclosed ailments experience more pronounced improvements following quality disclosure regulation.

4.3.1. Specialty Characteristics and Magnitude of Within-Specialty Spillovers. We next use additional tests to examine whether potential for spillovers differs across hospitals based on characteristics of the task environment in each specialty. Such organizational enablers of spillovers include volume of disclosed ailments relative to specialty ailments and emergency care routines.

4.3.2. Volume of the Disclosed Services. Economic theory predicts that intrafirm spillovers occur when the additional attention to the disclosed ailment has productivity benefits for complementary ailments (Teece 1980, Panzar and Willig 1981). Higher volume of operations provides organizations with the ability to learn (Argote 2012, Freeman et al. 2021) and deploy specialized personnel (Cockburn and Henderson 1996). These knowledge and resource repositories can spill over and benefit complementary ailments. Spillovers occur from tangible relatedness in technology and specialized services and intangible synergies between focal and complementary tasks (Schilling et al. 2003, Clark and Huckman 2012). When more of the focal service is performed, quality spillovers to complementary ailments increase. Thus, we expect quality spillovers to be more pronounced when the relevant specialty in a hospital has a higher volume of disclosed ailments.[30]

We construct a variable that measures the volume of *Disclosed Ailments* admits relative to *Specialty Ailments* admits in the year immediately preceding disclosure regulation (labeled as *DisclosedShare*). We then rank hospital-specialties into quartiles based on *DisclosedShare* and re-estimate Equation (2) as in our within-specialty spillover analysis reported in Table 3, column (1), but we allow for heterogenous effects across different cross-sectional partitions of *DisclosedShare*. Results (column (1)

Table 3. Complementarities as a Source of Spillovers: Within-Specialty Analysis

Panel A: Means (standard deviations) of mortality rates for treatment and benchmark groups

Regime	(1) *Specialty with Specialty Comorb*	(2) *Specialty with Other Comorb*
Predisclosure	0.0557	0.0371
	(0.0817)	(0.0746)
Postdisclosure	0.0431	0.0300
	(0.0738)	(0.0671)

Panel B: Difference-in-differences estimate of spillover effects of disclosure regulation -within-specialty analysis

	Treatment: *Specialty with Specialty Comorb*			
	Benchmark: *Specialty with Other Comorb*			
	(1)	(2)	(3)	(4)
Dependent variable	*MR*	Δ% *MR*	*ln(MR)*	Δ*ln(MR)*
Specialty with Specialty Comorb × *Post*	−0.0044***	−0.0180**	−0.0034***	−0.0033***
	(0.0012)	(0.0079)	(0.0009)	(0.0007)
Mean Elixhauser Index	0.0039***	0.0162***	0.0033***	0.0021***
	(0.0004)	(0.0013)	(0.0003)	(0.0003)
	(0.0014)	(0.0048)	(0.0011)	(0.0008)
Observations	57,088	52,984	57,088	52,984
R^2	0.4771	0.0139	0.4920	0.0159
Sample period	1995–2014	1995–2014	1995–2014	1995–2014
All control variables	Yes	Yes	Yes	Yes
Hospital, ailments, year fixed effects	Yes	Yes	Yes	Yes
Clustered standard errors	Hospital level	Hospital level	Hospital level	Hospital level

Notes. Panel A shows risk adjusted means and standard deviations of treatment and benchmark groups mortalities. Panel B shows the results of estimating Equation (2) for treatment and benchmark groups within-specialty using different specifications of the dependent variable. Hospital-level clustered standard errors in parentheses.

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

of Table EC.8 in the online appendix) indicate that within-specialty spillovers are of greater magnitude in hospital specialties that have a high *DisclosedShare* relative to those that have low *DisclosedShare*.

4.3.3. Emergency Care Routines. A second characteristic of the task environment enabling intrafirm and particularly intraspecialty spillovers are emerging routines, which refer to "generative systems that produce repetitive, recognizable patterns of interdependent action carried out by multiple participants" (Feldman and Pentland 2003, p. 103). Routines provide structural and procedural mechanisms that permit building, sustaining, and exploiting knowledge over time and across activities (Zahra and George 2002). The codification and retention of knowledge that occurs with routines can benefit the focal task and spill over to complementary tasks (Zollo and Winter 2002, Freeman et al. 2021). Routines are important in hospitals because promptly guiding a patient into appropriate care pathways is critical, given the direct relationship between time-to-treatment and survival in many ailments such as heart attacks (Veinot et al. 2012). Routines have a flexible component that arises from improvisation as the need for change arises (Feldman and Pentland 2003). Disclosure regulation is likely to increase the impetus for new routines to improve the quality of care for *Disclosed Ailments*. These new routines get embedded within departments and specialties, and spill over to complementary services. We predict that the care for groups of patients within specialties that share more complementarities with the care for *Disclosed Ailments* will experience greater improvements and larger spillovers.

A sizable proportion of *Disclosed Ailments* are emergency admits (~84%). We posit that the care for *Specialty Ailments*, especially those with *Specialty with Specialty Comorb*, benefits from the new routines that arise in emergency care in response to targeted disclosure regulation. However, the benefits of such process improvements in emergency care do not easily transfer to patients outside of emergency care because of differences in processes, personnel, and protocols (Freeman et al. 2021). Hence, we predict that the potential for spillovers in *Specialty with Specialty Comorb* relative to *Specialty with Other Comorb* will be greatest when the specialty has a high share of emergency admits. We construct a variable called *SpecialtyEmergency* that measures the proportion of emergency admits in *Specialty Ailments* for a given hospital and specialty in the year preceding regulation. We rank hospital-specialties into quartiles based on *SpecialtyEmergency*. We re-estimate Equation (2) as in Table 3, column (1), but allow for heterogenous effects

across different cross-sectional partitions of *SpecialtyEmergency*. Results (column (2) of Table EC.8 in the online appendix) indicate that spillovers are of greater magnitude in hospital specialties that have high *SpecialtyEmergency* relative to those that have low *SpecialtyEmergency*.

4.4. Admission to Specialty of Disclosed Ailments as a Source of Improvement

Our analyses in Sections 4.2 and 4.3 assume that spillovers occur within the specialty due to a common production function shared by ailments in the same specialty. To examine this further, we compare two groups of patients who have been *diagnosed* with an ailment from the same specialty as one of the disclosed ailments. However, the two groups differ based on whether patients were *admitted* to that specialty. Our treatment group is *Specialty with Other Comorb*, that is, patients whose principal diagnosis is in the specialty of a disclosed ailment (e.g., arrhythmia) and all comorbidities are outside the specialty. *Specialty with Other Comorb* consists only of patients who were admitted *within* the specialty of a disclosed ailment. Our benchmark group (labeled *Other with Specialty Comorb*) contains patients whose principal diagnosis is in a different specialty (e.g., neoplasm) and a comorbidity within a disclosed ailment's specialty (e.g., arrhythmia). Thus, the benchmark group is similar from a medical perspective but was admitted into a specialty *outside* the specialty of a disclosed ailment. If spillovers occur at the specialty level, we should observe a postregulation improvement for the group *Specialty with Other Comorb* relative to *Other with Specialty Comorb*.

Panel A of Table 4 shows means and standard deviations of such treatment and benchmark groups. We estimate Equation (2) comparing improvements in *Specialty with Other Comorb* and *Other with Specialty Comorb*. Results (Table 4, Panel B) show a negative coefficient on *Specialty with Other Comorb* × *Post* (β), indicating that relative to *Other with Specialty Comorb*, *Specialty with Other Comorb* experienced greater treatment quality improvements following disclosure regulation. In terms of economic magnitude, the reduction in mortality of patients *Specialty with Other Comorb* was 0.21 percentage points larger than for those *Other with Specialty Comorb*, a relative decrease of 5.66%.[31] This analysis provides corroborating evidence that complementarities exist within specialties, and spillovers are driven by such complementarities.

4.5. Strategic Gaming by Hospitals as Alternative Explanation

Improvements in the quality of treatment of disclosed ailments after disclosure regulation could potentially result if hospitals game the quality measure rather than undertake real improvements. First, for high-risk patients that are diagnosed with one of the six ailments targeted by disclosure regulation (e.g., pneumonia), hospitals could strategically classify one of the patient's comorbidities (e.g., arrhythmia) as a principal diagnosis. Such a patient's mortality outcome would not count toward the reported metric. Our analyses control for mortality risk using the Elixhauser comorbidity index, which assuages this concern. Furthermore, we observe improvements in not only *Disclosed Ailments*, but also in *Specialty Ailments*. If hospitals were recoding high-risk patients, we should observe little to no improvements in *Specialty Ailments*.

Second, hospitals could reroute high-risk patients to last-resort hospitals such as district or county hospitals that treat the most vulnerable patients (Gupta and Fonarow 2018). To address this concern, we examine improvements by hospital type. If hospitals were able to strategically reroute high-risk *Disclosed Ailments*, we should observe negative disclosure effects in last-resort hospitals such as government, county, and district hospitals. Results (Table EC.9 in the online appendix) confirm that this is not the case, which makes the patient shifting unlikely.

Finally, hospitals may divert resources away from the treatment of *Other Ailments* to improve outcomes for *Disclosed Ailments*. Our research design that uses control groups from the same specialty addresses this concern because resource allocations in hospitals occur at the specialty level. In addition, we also find that mortality rates for *Other Ailments* do not worsen in the postdisclosure period (Table 1, Panel A), which somewhat mitigates the alternative explanation of redirected resource allocations between specialties.

4.6. Why Do Hospitals Improve?

Our main analyses indicate that hospitals improved the quality of their *Disclosed Ailments* and *Specialty Ailments* (relative to *Other Ailments*) following disclosure regulation that targeted six ailments. In this section we attempt to understand *why* such improvements occur. If quality information guided the choice of consumers upon disclosure, demand patterns could shift. Prior research documents demand shifts in response to other forms of quality regulation (Dranove and Sfekas 2008, Romano et al. 2011, Wang et al. 2011). Such demand-side pressures can incentivize hospitals to improve the quality of their services.

To test for demand side pressures, we estimate whether hospital market shares are sensitive to the disclosed quality measure. We calculate the hospital market share for each disclosed ailment, defined as the proportion of hospital ailment-level admits relative to ailment-level admits for the county ($MktShare\_DisclosedAilments_t$). We regress this variable on the ailment's mortality rate in the prior year ($MortalityRate\_DisclosedAilment_{t-1}$). Results (Table EC.10 in the online appendix) show that in the preregulation period market shares were not sensitive to mortality rates, whereas in the postregulation period they were.

Table 4. Admission to Specialties as a Source of Spillover

Panel A: Means (standard deviations) of mortality rates for treatment and benchmark groups

Regime	(1) *Specialty with Other Comorb*	(2) *Other with Specialty Comorb*
Predisclosure	0.0371	0.0387
	(0.0746)	(0.0491)
Postdisclosure	0.0300	0.0360
	(0.0671)	(0.0500)

Panel B: Difference-in-differences estimate of spillover effects of disclosure regulation - admission to specialties as a source of spillover

	(1) Treatment: *Specialty with Other Comorb* Benchmark: *Other with Specialty Comorb*
Specialty with Other Comorb × *Post*	−0.0021*
	(0.0012)
Mean Elixhauser Index	0.0027***
	(0.0006)
Observations	35,939
R^2	0.4742
Sample period	1995–2014
All control variables	Yes
Hospital, ailments, year fixed effects	Yes
Clustered standard errors	Hospital level

Notes. Panel A shows risk adjusted means and standard deviations of treatment and benchmark groups mortalities. Panel B reports the estimates of the difference-in-differences specifications (Equation 2) for various treatment and benchmark groups. Hospital-level clustered standard errors in parentheses.

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

These findings indicate that consumers choose higher quality hospitals after disclosure regulation.

4.7. Who Improves?

In this section we explore variations in the extent of quality improvements between hospitals based on their predisclosure quality levels and level of market competition. Prior research has found some evidence that low-quality or poor-performing hospitals improve more after disclosure regulation (Evans et al. 1997, Cutler et al. 2004). We split hospitals into quartiles based on the average mortality rates of their disclosed ailments in the year prior to the regulatory change (labeled as *Mortality_Rate_2003*). We then re-estimate Equation (2) using *Disclosed Ailments* and *Other Ailments* as treatment and benchmark groups respectively but allow for heterogenous effects across cross-sectional partitions of *Mortality_Rate_2003*. We find that all hospitals show postregulation improvements in *Disclosed Ailments* relative to *Other Ailment*s. While hospitals in the highest quartile of *Mortality_Rate_2003* (i.e., lowest quality) have larger improvements relative to hospitals in the lowest quartile of *Mortality_Rate_2003*, the difference in improvements is not statistically significant (Table EC.11, Panel A, of the online appendix).

We next examine whether hospitals which operate in competitive (less concentrated) markets and face high demand-side pressures (Chou et al. 2014) improve at faster rates. We classify hospitals into quartiles based on the Hirschman Herfindahl index in 2003 (*HHI_2003*) to indicate the extent of market concentration in the predisclosure period. We then re-estimate Equation (2) using *Disclosed Ailments* and *Other Ailments* as treatment and benchmark groups respectively but allow for heterogenous effects across cross-sectional partitions of *HHI_2003*. Although all hospitals show postregulatory improvements in *Disclosed Ailments* relative to *Other Ailments*, hospitals in the lowest quartile of *HHI_2003* (i.e., high competition) show significantly greater improvements than hospitals in the highest quartile of *HHI_2003* (i.e., low competition; Table EC.11, Panel B, in the online appendix).

4.8. How Do Improvements Occur?

Essential to studying the effects of quality disclosure regulation is understanding how hospitals achieve desired quality outcomes. Although we cannot observe the internal workings of a hospital, we can explore whether hospitals may have deployed more resources to improve quality and/or more effectively used these resources in departments treating disclosed ailments. We classify cardiovascular services, respiratory therapy, pulmonary therapy, and gastroenterological services as departments belonging to specialties affected by disclosure regulation. We classify psychological intensive care, pediatric intensive care, labor and delivery, and neonatal intensive care as departments belonging to

specialties not affected by the quality disclosures.[32] We find that following disclosure regulation, paid hours and productive hours increased significantly in departments treating *Disclosed Ailments* but no changes occur in departments treating *Other Ailments* (Table EC.12 in the online appendix). This suggests that hospitals improved quality in response to regulation by using more resources and using such resources more efficiently.

5. Conclusions

The literature emphasizes the vital role of disclosures in mitigating information asymmetries between the firm and stakeholders but cautions that tradeoffs should be considered in designing disclosure regulation. In this paper, we examine the real effects of a focused quality disclosure regulation that targets selected ailments. Using the California healthcare industry as a context, we examine direct effects of quality disclosure regulation on disclosed services, as well as indirect effects on nondisclosed services from intrafirm spillovers. We find that quality disclosure regulation leads to improvements in quality of care for disclosed ailments and creates spillovers to complementary but nondisclosed ailments not directly targeted by disclosure regulation. We demonstrate that the magnitude of these spillover effects predictably varies with the degree of complementarity and characteristics of the task environment that enable such spillovers.

Our study is subject to limitations. First, our focus on the hospital industry provides internal validity that is difficult to achieve in a multi-industry study but can limit generalizability. Second, our analysis that uses a benchmark group in the same specialty alleviates concerns about unobservable factors associated with the regulation and provides a relatively clean design. However, we cannot rule out concurrent changes that we have not identified that align with our regulatory changes and differentially affect the three study groups. Finally, we are not able to address the overall welfare effects of quality disclosure regulation. We do not estimate the cost of investment in a disclosure policy or the private and public benefits of disclosure (Markopoulos and Hosanagar 2018), which are likely to vary by firm and the extent of consumer attentiveness to disclosures (Ghosh and Galbreth 2013).

Disclosures have value not only for market participants, but also for guiding the firm's strategy and decision making (Arya et al. 2017). Along these lines, we suggest that disclosure regulation that can only target a few services due to high complexity, cognitive burden, or reporting costs can still motivate broad improvements that extend beyond the disclosed services. Spillover effects must be considered by regulators when assessing the merits of a targeted disclosure policy. Achieving broad improvements is likely feasible in the presence of widespread complementarities among products or services. Thus, it is imperative for regulators to understand the nature of firms' production environment when designing disclosure regulation.

Acknowledgments

The authors thank Andrew Acito, Shannon Anderson, Ramji Balakrishnan, Jasmijn Bol, Clara Chen, Willie Choi, David Erkens, Harry Evans, Rachel Geoffroy, Satish Joshi, Lisa LaViers, Gans Narayanamoorthy, Hari Ramasubramaniam, Claire Senot, Jeffery Woolridge, and workshop participants at the Performance Management Research Symposium, the Midwest Accounting Research Conference, Georgetown University, Iowa State University, Michigan State University, Tulane University, and University of Michigan for helpful comments.

Appendix A. Related Literature

Authors, year	Setting	Focus of study	Sample doverage	Time period	Level of analysis	Quality dependent variables	Benchmark group	Findings	Our contribution
				Hospital settings					
Evans et al. (1997)	Hospitals	Approx. 37 high-volume Diagnosis Related Groups (DRG)	PA	1990–92	Hospital	Change in abnormal mortality	None (prepost comparison)	Quality improvements for poorly performing hospitals	Patient-level data; Within-hospital mortality rates; Medically matched benchmarks; Spillover effects
Chassin (2002)	Hospitals	Cardiac surgery (CABG)	5 hospitals in NY	1989–92	Surgeons	Postdisclosure mortality trend	None (case study)	Quality improvements for poorly performing hospitals (downward trend in mortality)	Controls for demand and risk factors; Ailments as opposed to procedures; Medically matched benchmarks; All hospitals in California; Spillover effects
Hannan et al. (2003)	Hospitals	Cardiac surgery (CABG)	Five regions versus all U.S.	1994–99	Hospital	Mortality odds ratio	Regions without quality disclosure	Better quality in regions with disclosure programs (odds ratio less than 1)	Ailments as opposed to procedures; Medically matched benchmarks; Spillover effects
Dranove et al. (2003)	Hospitals	Cardiac surgery (CABG)	NY, PA	1987–94	Hospital and patient	One-year post mortality	All other U.S. states	Quality deteriorates for sicker patients in disclosure states	Ailments as opposed to procedures; Risk adjusted mortality based on medical severity (as opposed to hospital expenditures); Within-hospital benchmark groups; Spillover effects.
Cutler et al. (2004)	Hospitals	Cardiac surgery (CABG)	NY	1991–99	Hospital	Mortality	None (prepost comparison)	Improvement in quality for poor-performing hospitals and low-severity patients	Ailments as opposed to procedures; Medically matched benchmarks; Within-hospital benchmark groups; Spillover effects
Carey et al. (2006)	Hospitals	Cardiac surgery (CABG)	CA	1998–2004	Patient	Mortality	None (prepost comparison)	Quality improvements for high-volume hospitals	Ailments as opposed to procedures; Medically matched benchmarks; Within-hospital benchmark groups; Spillover effects
Hollenbeak et al. (2008)	Hospitals	Acute myocardial infarction (AMI), heart failure, stroke, pneumonia	PA	2000–03	Patients	Mortality	PA patients versus. non-PA patients	Quality improvements for patients from public disclosure environments	Appropriate benchmarks for each ailment; Mandatory disclosure regimes instead of both voluntary and mandatory regimes; Spillover effects

Appendix A. (Continued)

Authors, year	Setting	Focus of study	Sample doverage	Time period	Level of analysis	Quality dependent variables	Benchmark group	Findings	Our contribution
Werner and Bradlow (2010)	Hospitals	AMI, heart failure, pneumonia	All U.S. states	2004–06	Hospital	Mortality	None (prepost comparison)	Quality improvements for AMI for all hospitals, and pneumonia in low performing hospitals	Larger set of ailments; Medically matched benchmarks; Spillover effects
Li et al. (2010)	Hospitals	Cardiac surgery (CABG)	CA	2003 and 2006	Patients	Mortality	None (2006 versus 2003 comparison)	Quality improvements after mandatory disclosure regulation	Ailments as opposed to procedures; Medically matched benchmarks; Spillover effects
Romano et al. (2011)	Hospitals	Cardiac surgery (CABG)	CA	2001, 2003, 2005	Hospitals	Mortality	None (prepost comparison)	No quality improvements after mandatory disclosure regulation	Ailments as opposed to procedures; Medically matched benchmarks; Spillover effects; Longer time period
Chen and Meinecke (2012)	Hospitals	Cardiac surgery (CABG)	PA versus. 10 other states	1988–92	Patient	Mortality	PA versus. control states	No improvement in quality after public disclosures	Ailments as opposed to procedures; Longer time period; Medically matched benchmarks; Spillover effects
Ryan et al. (2012)	Hospitals	Heart attack, heart failure, pneumonia	All U.S. Hospitals	2000–08	Patient	Mortality	Improvements relative to three nondisclosed diagnoses	Improvement in quality only for heart failure	Larger set of ailments; Medically matched benchmarks; Spillover effects
Joynt et al. (2012)	Hospitals	Percutaneous Coronary Intervention (PCI) for AMI patients	Reporting (NY, MA, PA) versus nonreporting	2002–10	Patient	Mortality	Mortality in reporting versus nonreporting states	No greater improvement in quality for public disclosure State	Ailments as opposed to procedures; Medically matched benchmarks; Within-hospital benchmark groups; Spillover effects.
Chou et al. (2014)	Hospitals	Cardiac surgery (CABG)	PA	1995–2004	Patient	Mortality	None (prepost comparison)	Greater quality improvements in competitive markets among more severely ill patients	Ailments as opposed to procedures; Medically matched benchmarks; Spillover effects
Joynt et al. (2016)	Hospitals	AMI, heart failure, pneumonia	All U.S. states	2005–12	Patient	Mortality	Mortality of reported versus 12 nonreported conditions	Some improvement in quality after public disclosures	Larger set of ailments; Medically matched benchmarks; Spillover effects
Shahian et al. (2019)	Hospitals	Cardiac surgery (CABG)	MA versus national	2003–14	Patient	Mortality	Mortality improvements in MA versus other states	Quality improved after disclosures	Ailments as opposed to procedures; Medically matched benchmarks; Within-hospital benchmark groups; Spillover effects.

Appendix A. (Continued)

Authors, year	Setting	Focus of study	Sample doverage	Time period	Level of analysis	Quality dependent variables	Benchmark group	Findings	Our contribution
				Other healthcare settings					
Werner et al. (2009)	SNF	Nursing home care	All	1999–2005	SNF	Three reported, nine unreported measures	None (prepost comparison)	Positive quality spillovers in unreported ailments for high-ranking facilities	Use of objective quality measures; Medically matched benchmarks; Difference-in-differences design; Longer time span of analysis
Grabowski and Town (2011)	SNF	Nursing home care	US	1996–2004	SNF	Five quality indicators	Pilot versus nonpilot states	Improvement in some quality indicators for nursing homes in competitive markets	Within state analysis; Medically matched benchmarks; Difference-in-differences design; Longer time span of analysis; Spillover effects
Lu (2012)	SNF	Nursing home care	CO, FL, MD, OH, RI, WA versus others	1999–2005	SNF- Year for 5 reported dimensions	Improvement in nursing care	Prepost comparison for pilot versus nonpilot states	Quality improves for the reported dimensions but deteriorates for the unreported ones	Objective construction of outcomes using one data set; Medically matched benchmarks; Difference-in-differences design; Longer time span of analysis
Clement et al. (2012)	SNF	Restraints, pressure sores	WI	2001–03	Inpatient	Restraints, pressure sores	None (prepost comparison)	Improvement in restraint use in low-quality nursing homes	Medically matched benchmarks; Difference-in-differences design; Longer time span of analysis; Spillover effects
Kolstad (2013)	Surgeons	Cardiac surgery (CABG)	PA	1995, 2000, 2003	Patient	Mortality	Improvement in mortality relative to expected	Greater quality improvements among poorly performing and high performing surgeons	Ailments as opposed to procedures; Hospitals as opposed to surgeons; Medically matched benchmarks; Spillover effects
Lamb et al. (2013)	Physician groups	Ambulatory clinics	WI	2004–09	Clinics	Ambulatory care measures	Improvements relative to first year of reporting	Quality improved after disclosures, with greater improvements for poor performers	Objective construction of outcomes using one data set; Medically matched benchmarks; Difference-in-differences design; Spillover effects

Appendix B. Variable Descriptions

Variable	Description
Dependent variable	
MR	Mortality rate computed as number of patient deaths divided by number of admits in hospital i for group j in a given year t.
Disclosure regulation indicator	
Post	One if year is any year following mandatory disclosure (2004–2014). Zero otherwise.
Groups	
Disclosed Ailments	One if ailment with which admitted is stroke, heart attack, heart failure, gastro hemorrhage, hip fracture, or pneumonia. Zero otherwise.
Specialty Ailments	One if ailment with which admitted is in the cardiovascular, digestive, injury, or respiratory specialties (excluding the above six disclosed ailments). Zero otherwise.
Other Ailments	One if ailment with which admitted is neither a *Disclosed Ailment* nor a *Specialty Ailment*. Zero otherwise.
Matched Specialty Ailments	Subset of *Specialty Ailments*. One if ailment with which admitted is in the cardiovascular, digestive, injury, or respiratory specialties and has a predisclosure mortality rate close to the corresponding *Disclosed Ailments*. Zero otherwise.
Matched Other Ailments	Subset of *Other Ailments*. One if ailment with which admitted is neither a *Disclosed Ailment* nor a *Specialty Ailment* and has a predisclosure mortality rate close to *Specialty Ailments*. Zero otherwise.
Specialty with Specialty Comorb	Subset of *Specialty Ailments*. One if ailment with which admitted as well as at least one comorbidity is in the cardiovascular, digestive, injury, or respiratory specialties. Zero otherwise.
Specialty with Other Comorb	Subset of *Specialty Ailments*. One if ailment with which admitted is in the cardiovascular, digestive, injury, or respiratory specialties and all other comorbidities are outside the specialty. Zero otherwise.
Other with Specialty Comorb	Subset of *Other Ailments*. One if ailment with which admitted is outside the disclosed specialties and has at least one comorbidity in the cardiovascular, digestive, injury, or respiratory specialties. Zero otherwise.
Control variables	
Mean Elixhauser Index	Mean complexity of care/riskiness of patients in a given group.
Mean Procedures	Mean number of procedures for patients in a given group.
Medicare %	% of Medicare admits in a given group.
Medicaid %	% of Medicaid admits in a given group.
Beds	Number of staffed beds in the hospital.
Margin	Net profit/Revenue of the hospital.
HHI	Herfindahl Hirschman Index (hospital based on patient days) based on county level market concentration.

Endnotes

[1] For ICD-9 diagnosis codes, see https://www.cms.gov/Medicare/Coding/ICD9ProviderDiagnosticCodes/codes.

[2] In healthcare, knowledge is concentrated within specialties and develops from education, certification, and experience in a medical subdomain. Thus, nondisclosed ailments within a specialty usually follow similar trends in treatment quality from scientific or technological advancements that benefit the entire specialty (Gaynor et al. 2015).

[3] OSHPD was subsequently renamed as California Department of Healthcare Access and Information (see https://hcai.ca.gov/).

[4] Mortality is an important outcome measure of healthcare quality that is reliable, objective, and reflects the entire continuum of care, rather than individual episodes (Lazar et al. 2013).

[5] See Appendix A for an overview of the healthcare quality disclosures literature and our contributions.

[6] For physicians' utility functions related to quality, see, for example, Feldstein (2012), Pauly (1980), Chalkley and Malcomson (1998), Fehr and Schmidt (2006), Harsanyi (1955), Kolstad (2013), McGuire (2000), and Segal and Sobel (2007). For physicians' concern for patient health outcomes, see, for example Allard et al. (2011), Chandra and Skinner (2012), Choné and Ma (2011), and Ellis and McGuire (1986).

[7] The study was conducted by the Oregon Evidence-based Practice Center (EPC) under contract to AHRQ and included 97 quantitative and 101 qualitative studies.

[8] California also implemented disclosure regulation for CABG outcomes. A few studies have examined whether disclosure regulation affects CABG quality of care (Li et al. 2010, Romano et al. 2011). Please see Appendix A.

[9] For a more detailed discussion of spillovers, see Online Appendix EC.2.

[10] Ailments within the same specialty follow similar patient care pathways (Freeman et al. 2021). Patient care pathways refer to a structured multidisciplinary plan of care that translates guidelines or evidence into local structures, details the steps in a course of treatment, designs timeframes and criteria-based progression, and standardizes care for a specific specialty (Kinsman et al. 2010).

[11] ICD-9 (International Statistical Classification of Diseases and Related Health Problems) codes classify medical conditions into a three-digit numeric code. Ailments are grouped by cause or etiology of ailment and the anatomical system involved, which is referred to as medical specialty. Details are in Online Appendix EC.1.

[12] Our fixed effects depend on the treatment and control groups for a given analysis. For example, when we compare *Disclosed Ailments* and *Other Ailments*, we use fixed effects for each of the following: stroke, heart attack, heart failure, gastro hemorrhage, hip fracture, pneumonia, and other ailments. When we compare *Specialty Ailments* and *Other Ailments*, we use fixed effects for each of the following: cardiovascular, digestive, injury, respiratory, and other ailments. Throughout our analyses, we refer to these as "ailments fixed effects."

[13] The Elixhauser index measures the severity of illness (Elixhauser et al. 1998) and is calculated at the patient-level using comorbidities that influence mortality risks such as liver disease and cancer. We aggregate the patient-level index to the hospital-specialty-year level for each cohort.

[14] Results (untabulated) are robust to clustering by ailments. We cluster standard errors at the hospital-level because for each hospital-year, we have only up to 11 unique ailments categories available, which is smaller than the recommended number in the econometrics literature (Hansen 2007).

[15] Risk-adjusted mean mortality rates are provided in Table 1, Panel A, and thereafter. Raw mortality means do not have a meaningful interpretation, because they are not adjusted for the risk profile of the patient. All the multivariate analyses include the Elixhauser index as a control for patient risk profile (Office of Statewide Health Planning and Development Healthcare Outcomes Center 2008).

[16] In this and any other analysis reported hereafter, we drop all other groups/ailments that are not specified as treatment or benchmark group.

[17] Relative percentage improvement in *Disclosed Ailments* is calculated as 0.0148/0.0643 = 23.02%.

[18] Prior studies estimate the direct effect of disclosure to range in size between 9% and 24% decline in mortality risk. Please see Table EC. 4 in the online appendix. Overall, the economic magnitude of our effect sizes is comparable with prior healthcare studies that examine other quality disclosure regulations.

[19] A potential threat to the validity in a difference-in-differences design stems from unobservable events that (1) align with the treatment both in terms of timing and subjects affected by the treatment, and (2) may influence the dependent variable of interest. Public discourse and interest in medical quality that may have picked up in the years immediately preceding the disclosure regulation could have prompted hospitals to invest in quality and encouraged regulators to pass disclosure regulation. To alleviate such concern, we run a falsification test that estimates leads of the treatment effect to pick up any effect of public discourse in the years leading up to the disclosure regulation. The test fails to find that quality improved for the targeted ailments after an increase in public interest in medical quality. The analysis provides reassurance that the quality improvements we observe in our analysis can be attributed to the introduction of quality disclosures. For a more extensive discussion of the endogeneity concerns pertaining to the passage of the quality disclosure regulation and the above-mentioned falsification test, see Online Appendix EC.5.

[20] Knowledge and infrastructure reside at the specialty level (e.g., cardiologists treat a patient with stroke as well as a patient with arrhythmia). Technologies adopted to treat disclosed ailments can also be used to treat ailments in the specialty. For example, a technique adopted to improve outcomes from heart failure is mechanical circulatory support (MCS) with ventricular assist devices (VADs). This technique also improves outcomes for other ailments in the same specialty such as cardiomyopathy and ventricular function defects (Dandel and Hetzer 2018).

[21] Relative percentage improvement in *Disclosed Ailments* is calculated as 0.0074/0.0643 = 11.51%.

[22] See Balakrishnan et al. (2014) and Gow et al. (2016) for examples of matching based on the dependent variable.

[23] Further details on ailments selected to construct *Matched Specialty Ailments* are in Online Appendix EC.6.

[24] Relative percentage improvement in *Disclosed Ailments* is calculated as 0.0038/0.0643 = 5.91%.

[25] For brevity, all four *Specialty Ailments* are combined to provide means. Means and standard deviations of individual specialties are provided in Online Appendix EC.3.

[26] Relative percentage improvement in *Specialty Ailments* is calculated as 0.0077/0.0479 = 15.87%.

[27] Details of groups of ailments used to construct *Matched Other Ailments* is provided in Online Appendix EC.7.

[28] Relative percentage improvement in *Specialty Ailments* is calculated as 0.0032/0.0479 = 6.68%.

[29] Relative percentage improvement in *Specialty with Specialty Comorb* is calculated as 0.0044/0.0557 = 7.90%.

[30] This benefit should accrue to a greater extent to *Specialty with Specialty Comorb* (relative to *Specialty with Other Comorb*) because both

their principal diagnosis and comorbidities lie within *Specialty Ailments*.

[31] Relative percentage improvement in *Specialty with Other Comorb* is calculated as 0.0021/0.0371 = 5.66%.

[32] Data are only available from 2003. Hence, we conduct resource allocation analysis only for the years 2003–2014. 2003 is considered the preregulation period, and 2004–2014 is considered the postregulation period. For robustness, we also restrict our analyses to years 2003–2005 and find similar results.

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