Effect of Public Reporting on Intensive Care Unit Discharge Destination and Outcomes

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Abstract

Rationale: Public reporting of hospital performance is designed to improve healthcare outcomes by promoting quality improvement and informing consumer choice, but these programs may carry unintended consequences.

Objective: To determine whether publicly reporting in-hospital mortality rates for intensive care unit (ICU) patients influenced discharge patterns or mortality.

Methods: We performed a retrospective cohort study taking advantage of a natural experiment in which California, but not other states, publicly reported hospital-specific severity-adjusted ICU mortality rates between 2007 and 2012. We used multivariable logistic regression adjusted for patient, hospital, and regional characteristics to compare mortality rates and discharge patterns between California and states without public reporting for Medicare fee-for-service ICU admissions from 2005 through 2009 using a difference-in-differences approach.

Measurements and Main Results: We assessed discharge patterns using post-acute care use and acute care hospital transfer rates and mortality using in-hospital and 30-day mortality rates. The study cohort included 936,063 patients admitted to 646 hospitals. Compared with control subjects, admission to a California ICU after the introduction of public reporting was associated with a reduced odds of post-acute care use in post-reform year 2 (ratio of odds ratios [ORs], 0.94; 95% confidence interval [CI], 0.91–0.96) and increased odds of transfer to another acute care hospital in both post-reform years (year 1: ratio of ORs, 1.08; 95% CI, 1.01–1.16; year 2: ratio of ORs, 1.43; 95% CI, 1.33–1.53). There were no significant differences in in-hospital or 30-day mortality.

Conclusions: Public reporting of ICU in-hospital mortality rates was associated with changes in discharge patterns but no change in risk-adjusted mortality.

Keywords: intensive care; mortality; health policy

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Critical illness affects more than 5 million Americans each year and is associated with high morbidity and mortality (1, 2). Because of this clinical burden, system-wide efforts to improve intensive care unit (ICU) outcomes have the potential to

significantly impact healthcare delivery outcomes. One such strategy is the public reporting of ICU outcome data. In theory, public reporting can improve outcomes by motivating providers to compete on quality and implement local quality-

improvement efforts (3). Additionally, public reporting may provide the necessary information for consumers to select high-quality providers (3). Public reporting appears effective in some healthcare settings, such as cardiac

surgery and obstetric care (4–7), but other studies show no benefit (8–11).

Public reporting may be particularly effective in the ICU for several reasons. First, ICU mortality is high, such that small relative risk reduction can lead to large absolute benefits. Additionally, evidence-based strategies to improve ICU outcomes are incompletely adopted, thereby providing targets for performance improvement in response to public reporting (12–18). On the other hand, public reporting may be less effective in the ICU compared with other settings because the emergent nature of critical illness does not typically allow patients to choose a particular hospital for their care. Public reporting in the ICU may also lead to specific unintended consequences related to gaming, including incentivizing premature hospital discharges to artificially improve in-hospital mortality measurements (3).

From 2007 to 2012, California publicly reported in-hospital ICU mortality rates via a website supported by the California HealthCare Foundation. The goal of our study was to examine the effects of public reporting in the ICU by assessing both the intended consequences (reduced mortality) and unintended consequences (changes in discharge patterns) of this initiative. Some of the results of this study have been previously reported in the form of an abstract (19).

Methods

Study Design, Setting, and Participants

We performed a retrospective cohort study using patient-level data from the Medicare Provider Analysis and Review (MedPAR) files, provided by the Centers for Medicare and Medicaid Services. MedPAR is an administrative dataset containing U.S. hospital discharge data for all fee-for-service Medicare beneficiaries (20). Unlike most other administrative datasets, MedPAR allows access to postdischarge outcomes in the Medicare Beneficiary Summary File, giving us the ability to calculate postdischarge mortality rates. We linked these data at the hospital level to Medicare's 2007 Healthcare Cost Reporting Information System to determine hospital characteristics, and the Dartmouth Atlas hospital to hospital referral region (HRR) crosswalk files to determine each hospital's HRR (21, 22).

To examine the effect of public reporting of ICU outcomes we took advantage of a natural experiment in which California, but not other states, publicly reported hospital-specific severity-adjusted in-hospital ICU mortality rates between 2007 and 2012 (23). Organized by the California HealthCare Foundation, this program used hospital-supplied clinical data from voluntarily participating hospitals to calculate hospital-specific risk-adjusted mortality rates on adult ICU patients using Mortality Probability Model risk-adjustment methodology, publishing these results on www.calhospitalcompare.org (24). This website publicly reported performance measures on more than 240 California hospitals, representing 85% of California's acute care hospital admissions (23). We examined data from the 2 years before and 2 years after the introduction of public reporting on March 7, 2007. We excluded admissions during the first quarter (Q1) of 2007, because the effect of public reporting is unlikely to be immediate, and hospitals may have initiated changes in practice during the months preceding the introduction of public reporting.

We selected Arizona, Nevada, and Texas as control states. We selected Arizona and Nevada because they are contiguous with the state of California and have similar demographic characteristics. We selected Texas because it is of similar size and demographic characteristics to California. All patients in MedPAR admitted to an ICU in California or a control state were initially eligible for inclusion. We defined ICU admission using revenue codes as previously defined (25). We excluded patients less than

65 years of age, because these patients are typically enrolled in Medicare due to disability or end-stage renal disease and may systematically differ from the elderly population as a whole. We also excluded admissions after the first to allow for the assumption of independence among observations. In addition, we excluded patients with missing data required for the analysis.

Study Variables and Definitions

The primary exposure was ICU admission after the introduction of public reporting in California (Q2 of 2007 or later). We categorized the date of each admission as pre-reform year 2 (Q1–Q4 2005), pre-reform year 1 (Q1–Q4 2006), post-reform year 1 (Q2 2007–Q1 2008), or post-reform year 2 (Q2 2008–Q1 2009). We categorized each hospital as a California hospital or a control hospital, depending on whether the hospital in which the patient was admitted was located in California or a control state (Arizona, Nevada, and Texas).

We assessed the effects of public reporting along two domains: mortality and discharge patterns. We assessed mortality using in-hospital mortality rates as well as 30-day mortality rates, to account for postdischarge deaths that might have occurred in another institution. We assessed discharge patterns by determining post-acute care use and acute care hospital transfer rates, defined using the MedPAR discharge location field. We considered skilled nursing facilities and long-term acute care hospitals to be post-acute care facilities in this analysis.

We included key patient, hospital, and regional characteristics, identified *a priori*,

Figure 1. Patient sample.

as potential confounders. Patient characteristics included age, sex, race (white, black, other), admission source (emergency department, acute care transfer, direct admission, skilled nursing facility), primary diagnosis (categorized into groups using the Agency for Healthcare Research and Quality Clinical Classifications Software [26]), and comorbidities (defined in the manner of Elixhauser and colleagues [27]). Hospital characteristics included hospital bed size (<100, 100–250, >250), ICU bed number (<10, 10–25, >25), teaching status defined using resident-to-bed ratio (nonteaching 0, small teaching >0 to <0.25, large teaching ≥ 0.25), ownership (nonprofit, for profit, government), and urban/rural status according to metropolitan statistical area (MSA) size (<100,000 or non-MSA, 100,000 to 1 million, >1 million). Regional characteristics, determined at the HRR level, included population, size, percent of the population age 65 or older, percent of the population identified as black, and percent of the population identified as Hispanic (from the U.S. Census); the number of hospitals and number of long-term acute care beds (0, 1–200, >200) (from Healthcare Cost Reporting Information System); and percent of Medicare beneficiaries enrolled in a Medicare Advantage Plan (from the Beneficiary Summary File).

Statistical Analysis

We summarized hospital and patient characteristics using standard descriptive statistics. We compared California and control hospital characteristics using Chi-square tests.

To examine the trends in ICU mortality and discharge patterns, we fit a series of multivariable logistic regression models with a fixed effect for year, fixed effect for state, and interaction terms between year and state. Models also included a hospital-specific random effect to account for patient-level clustering within hospitals (28). We adjusted models for the patient, hospital, and regional characteristics described above. We used indirect standardization to estimate the adjusted mean for each outcome in California and control hospitals during each year of the study, and then we graphed these estimates along with their 95% confidence intervals.

To statistically test the effect of public reporting on ICU mortality and discharge

patterns, we compared the trends of California hospitals with the baseline trends of control hospitals using a difference-in-differences approach. Under this approach, the interaction terms in the models measured if the effect of public reporting was different for California and control hospitals, with a ratio of odds ratios to describe the magnitude of its effect. For our models, we could treat the two pre-reform years either as independent or as a single time period to estimate the pre-reform risk. To make this determination, we performed a test of controls. We assumed mortality and discharge patterns were unaffected by the impending reform before its actual implementation. We fit interaction terms between state and each time period before the introduction of public reporting. If the test of controls was significant, we assumed the divergence in mortality or discharge pattern trends was unrelated to the reform and used only pre-reform year 1 as the referent group. If not, we grouped all observations before the introduction of public reporting into a single referent group.

To assess the robustness of our findings we performed a sensitivity analysis in which we varied our control hospitals. We used propensity score matching to match regions in California to other regions in the continental United States. We then compared hospitals in the California regions

to hospitals in the matched regions using the same difference-in-differences approach described above, except that the models were not adjusted for regional characteristics. Complete details regarding the methods for propensity score matching are provided in the online data supplement.

This study used deidentified data and therefore was considered exempt from human subjects review by the University of Pittsburgh Institutional Review Board. We performed all statistical analyses using Stata 12.0 (StataCorp, College Station, TX). All tests were two tailed, and we considered a P value ≤ 0.05 to be significant.

Results

The study cohort included 936,063 patients admitted to 646 hospitals (Figure 1). There were fewer small (<100 beds) hospitals, nonteaching hospitals, for-profit hospitals, and rural (MSA size < 100,000 or non-MSA) hospitals in California compared with control states (Table 1). In addition, the percentage of white and black patients was lower in California compared with control states (Table 2). Over the 4 years of the study, unadjusted ICU in-hospital mortality ranged from 10 to 15%, and unadjusted ICU 30-day mortality ranged from 15 to 20%.

Table 1. Hospital characteristics

Hospital Characteristic	California (N = 281)	Control (N = 365)	P Value
Hospital bed size			0.003
<100 beds	96 (34)	172 (47)	
100–250 beds	127 (45)	125 (34)	
>250 beds	58 (21)	68 (19)	
ICU bed number			0.001
<10 beds	74 (26)	86 (24)	
10–25 beds	122 (43)	117 (32)	
>25 beds	85 (30)	162 (44)	
Teaching status*			0.044
Nonteaching	204 (73)	293 (80)	
Small teaching	51 (18)	53 (15)	
Large teaching	26 (9)	19 (5)	
Ownership			0.004
Nonprofit	150 (53)	147 (40)	
For profit	79 (28)	138 (38)	
Government	52 (19)	80 (22)	
MSA size			<0.001
<100,000 or non-MSA	11 (4)	47 (13)	
100,000 to 1 million	75 (27)	90 (25)	
>1 million	195 (69)	228 (62)	

Definition of abbreviations: ICU = intensive care unit; MSA = metropolitan statistical area size.

Results are listed as frequency (%). Not all percentages add to 100 because of rounding.

*Teaching status categorized by resident-to-bed ratio (nonteaching 0, small teaching >0 to <0.25, large teaching ≥ 0.25).

Table 2. Patient characteristics

Patient Characteristic	Pre-reform Year 2 (Q1 2005–Q4 2005)		Pre-reform Year 1 (Q1 2006–Q4 2006)		Post-reform Year 1 (Q2 2007–Q1 2008)		Post-reform Year 2 (Q2 2008–Q1 2009)	
	California (N = 91,663)	Control (N = 152,689)	California (N = 81,024)	Control (N = 129,681)	California (N = 94,725)	Control (N = 148,929)	California (N = 95,915)	Control (N = 141,437)
Age, mean $\pm$ SD, yr	77.8 $\pm$ 8.0	76.9 $\pm$ 7.7	77.9 $\pm$ 8.1	76.9 $\pm$ 7.8	78.0 $\pm$ 8.2	77.0 $\pm$ 7.9	78.0 $\pm$ 8.2	76.9 $\pm$ 7.9
Female	45,916 (50.1)	77,530 (50.8)	40,543 (50.0)	65,614 (50.6)	47,053 (49.7)	75,404 (50.6)	47,360 (49.4)	71,241 (50.4)
Race								
White	70,467 (76.9)	127,071 (83.2)	62,434 (77.1)	109,131 (84.2)	72,262 (76.3)	124,404 (83.5)	73,176 (76.3)	118,638 (83.9)
Black	5,405 (5.9)	11,826 (7.8)	4,808 (5.9)	9,798 (7.6)	5,966 (6.3)	11,447 (7.7)	6,181 (6.4)	10,686 (7.6)
Other	15,791 (17.2)	13,792 (9.0)	13,782 (17.0)	10,752 (8.3)	16,497 (17.4)	13,078 (8.8)	16,558 (17.3)	12,113 (8.6)
Admission source								
Emergency department	50,649 (55.3)	89,356 (58.5)	46,270 (57.1)	75,135 (57.9)	55,169 (58.2)	89,921 (60.4)	56,389 (58.8)	85,906 (60.7)
Outside hospital	2,511 (2.7)	7,974 (5.2)	2,356 (2.9)	6,985 (5.4)	3,753 (4.0)	8,126 (5.5)	4,393 (4.6)	8,674 (6.1)
Skilled nursing facility	2,314 (2.5)	1,774 (1.2)	1,788 (2.2)	1,296 (1.0)	2,222 (2.4)	1,984 (1.3)	2,259 (2.4)	2,649 (1.9)
Direct	36,189 (39.5)	53,585 (35.1)	30,610 (37.8)	46,265 (35.7)	33,581 (35.5)	48,898 (32.8)	32,874 (34.3)	44,208 (31.3)
Primary diagnosis								
Cardiac	28,899 (31.5)	51,493 (33.7)	24,136 (29.8)	42,020 (32.4)	25,967 (27.4)	44,166 (29.7)	26,420 (27.6)	40,694 (28.8)
Gastrointestinal	7,465 (8.1)	13,019 (8.5)	6,652 (8.2)	11,353 (8.8)	7,522 (7.9)	12,418 (8.3)	7,169 (7.5)	11,946 (8.5)
Neurologic	10,029 (10.9)	16,537 (10.8)	9,491 (11.7)	15,050 (11.6)	10,462 (11.0)	16,570 (11.1)	11,870 (12.4)	17,354 (12.3)
Oncologic	6,412 (7.0)	9,211 (6.0)	5,613 (6.9)	8,036 (6.2)	6,251 (6.6)	8,441 (5.7)	6,613 (6.9)	8,857 (6.3)
Respiratory	12,638 (13.8)	19,913 (13.0)	10,273 (12.7)	15,697 (12.1)	12,279 (13.0)	19,415 (13.0)	11,025 (11.5)	16,512 (11.7)
Trauma	2,961 (3.2)	5,293 (3.5)	3,045 (3.8)	4,822 (3.7)	3,702 (3.9)	5,448 (3.7)	3,648 (3.8)	5,451 (3.9)
Other	23,259 (25.4)	37,223 (24.4)	21,814 (26.9)	32,703 (25.2)	28,542 (30.1)	42,471 (28.5)	29,170 (30.4)	40,624 (28.7)
≥ 3 Comorbidities	31,309 (34.2)	56,075 (36.7)	29,181 (36.0)	49,442 (38.1)	36,419 (38.5)	61,653 (41.4)	33,909 (35.4)	53,870 (38.1)
Mechanically ventilated	16,286 (17.8)	22,469 (14.7)	14,586 (18.0)	18,563 (14.3)	18,127 (19.1)	22,436 (15.1)	17,892 (18.7)	21,679 (15.3)
Discharge location								
Home	51,219 (55.9)	91,501 (59.9)	44,594 (55.0)	77,308 (59.6)	49,807 (52.6)	85,163 (57.2)	50,444 (52.6)	80,279 (56.8)
Acute care hospital	3,660 (4.0)	5,043 (3.3)	3,046 (3.8)	3,821 (2.9)	3,651 (3.9)	4,232 (2.8)	4,397 (4.6)	3,690 (2.6)
Post-acute care facility	22,659 (24.7)	35,805 (23.5)	20,390 (25.2)	31,284 (24.1)	25,504 (26.9)	38,476 (25.8)	25,141 (26.2)	36,913 (26.1)
Hospice	1,012 (1.1)	4,466 (2.9)	1,082 (1.3)	4,252 (3.3)	1,590 (1.7)	5,734 (3.9)	1,864 (1.9)	6,120 (4.3)
Expired	13,113 (14.3)	15,874 (10.4)	11,912 (14.7)	13,016 (10.0)	14,173 (15.0)	15,324 (10.3)	14,069 (14.7)	14,435 (10.2)
Unadjusted 30-d mortality	16,507 (18.0)	23,813 (15.6)	15,026 (18.6)	19,996 (15.4)	18,321 (19.3)	24,205 (16.3)	18,450 (19.2)	23,165 (16.4)

Definition of abbreviations: Q1 = quarter 1; Q2 = quarter 2; Q4 = quarter 4.

Results are listed as frequency (%) unless otherwise specified. Not all percentages add to 100 because of rounding.

We illustrate trends in adjusted mortality and discharge patterns in Figure 2. Adjusted in-hospital mortality rates in both California and control states decreased over the 4 years of the study (California: 15.3% in pre-reform year 2 to 13.9% in post-reform year 2; control states: 11.7–10.0%), as did 30-day mortality rates (California: 18.6–17.6%; control states: 18.3–17.0%).

In the test of controls, the interaction term between state and pre-reform year 1 was not significant for post-acute care use rates ($P = 0.28$). Therefore, for this analysis we grouped all observations in pre-reform years 1 and 2 into a single referent group. The interaction term between state and pre-reform year 1 was significant for acute care hospital transfer rates ($P = 0.01$), in-hospital mortality ($P = 0.001$), and 30-day mortality ($P = 0.02$), indicating that California hospitals had a different trend in these outcomes than control hospitals before 2007. We assumed these differences

were unrelated to the introduction of public reporting and used pre-reform year 1 as the baseline to evaluate the effect of the reform.

The association between public reporting and ICU mortality and discharge patterns are shown in Table 3. There was no significant difference in adjusted in-hospital or 30-day mortality rates after the introduction of public reporting in California compared with control states. Compared with admission to a control state ICU, admission to a California ICU in post-reform year 2 was associated with a significant reduction in the odds of post-acute care use. Compared with admission to a control state ICU, admission to a California ICU in both post-reform years was associated with a significant increase in the odds of transfer to another acute care hospital.

We found similar results in our sensitivity analysis, except that the reduction in the odds of post-acute care use was significant in both post-reform years.

Additionally, we found a significant increase in the odds of 30-day mortality in post-reform year 1 but no significant difference in post-reform year 2. Complete results for the sensitivity analysis are provided in the online supplement.

Discussion

In this study, we found that public reporting of hospitals' in-hospital mortality rates for ICU patients was associated with changes in discharge patterns but no change in mortality rates. Specifically, public reporting was associated with a reduction in post-acute care use and an increase in acute care hospital transfers, whereas in-hospital and 30-day mortality remained unchanged. With small exceptions, these findings were robust to different assumptions about the ideal control group, which we varied using

Figure 2. Adjusted mortality and discharge patterns in California compared with control hospitals by year. (A) In-hospital mortality. (B) 30-day mortality. (C) Post-acute care use rates. (D) Acute care hospital transfer rates. Error bars represent the 95% confidence intervals.

similar states to California (in the primary analysis) or hospitals in regions similar to California regions (in the sensitivity analysis).

Surprisingly, we found that public reporting of ICU in-hospital mortality rates was associated with decreased rather than increased post-acute care use. This finding is in contrast to the Cleveland Health Quality Choice program, where public reporting

of in-hospital mortality rates in the 1990s was associated with increased post-acute care use (10). At the same time, we found an increase in acute care hospital transfers. There are several possible explanations for these findings. First, because in-hospital mortality rates do not account for death after transfer (unlike 30-d mortality rates), transferring patients to another hospital may be an alternative way to game

the system by discharging patients “sicker and quicker” (29, 30). The fact that we observed an increase in acute care hospital transfers but not post-acute care facility transfers may reflect the relative availability of acute care versus post-acute care beds in California. Alternatively, providers may have transferred more patients to the best hospitals (according to the publicly reported data) in an attempt to concentrate

Table 3. Adjusted odds of each of the outcome measures after public reporting in California compared to control states

Outcome Measure	State × Post-reform Year 1		State × Post-reform Year 2	
	Ratio of ORs (95% CI)	P Value	Ratio of ORs (95% CI)	P Value
Mortality				
In-hospital mortality	1.00 (0.96–1.04)	0.97	0.99 (0.95–1.03)	0.72
30-d mortality	1.00 (0.96–1.03)	0.84	0.99 (0.96–1.02)	0.55
Discharge patterns				
Post-acute care use	0.98 (0.96–1.01)	0.15	0.94 (0.91–0.96)	<0.001
Transfer to another acute care hospital	1.08 (1.01–1.16)	0.03	1.43 (1.33–1.53)	<0.001

Definition of abbreviations: CI = confidence interval; OR = odds ratio.

care in the highest-quality hospitals, akin to *de facto* regionalization (31). Unfortunately, we did not have access to each hospital's individual rankings, so we could not directly test this hypothesis. Yet, even if this was the case, it did not appear to translate into improved overall outcomes.

Although in-hospital and 30-day mortality rates improved over the 4 years of the study, the rate of improvement over time was not significantly different in California compared with states without public reporting. This finding is similar to prior work demonstrating that improvement in mortality after the initiation of public reporting was related to temporal trends rather than a result of public reporting (8). Additionally, it demonstrates the essential role of controls in testing the impact of public reporting and other system-wide healthcare delivery interventions.

There are several possible reasons why public reporting was not associated with improvement in mortality rates. First, public reporting alone may not provide enough incentive for hospitals to initiate quality-improvement efforts, compared with more intensive efforts such as pay-for-performance (32). Second, hospitals may lack the tools to improve ICU quality or may have initiated quality-improvement efforts that did not translate into improved mortality (33–35). Third, public reporting may not have driven patients to higher-quality hospitals, either because patients and clinicians did not know the public reports existed, did not trust that the reports were accurate, or did not know how to appropriately interpret and use the reports, or because patients were unable to choose where to receive their care due to the emergent nature of critical illness (3, 36, 37). Fourth, the program was voluntary, such that hospitals with the worst outcomes may have simply opted out of participation, removing the incentive to improve quality.

Our study has several limitations. First, this was an observational study using administrative data. As such, it is subject to unmeasured confounding and misclassification from coding errors. With this method, a variable would need to have a different rate of change in California compared with control states to bias our study. Second, the use of administrative data precluded our ability to use the same risk-adjustment model used in California's public reporting initiative, necessitating use of a less robust administrative claims model. However, these risks are diminished by our use of a difference-in-differences approach that included control states, which would not be possible using only California data. Third, patients admitted just before and after the introduction of public reporting were excluded from this study because the effect of public reporting is unlikely to be immediate, and hospitals may have initiated changes in practice during the months preceding the introduction of public reporting. The choice of the time period in which to exclude admissions may have affected our results. Fourth, participation in the public reporting initiative in California was voluntary. As such, the outcomes for some of the ICU admissions in California were not subject to public reporting, and the inclusion of these admissions in our study could have affected our results. Unfortunately, we did not have access to a list of which hospitals did and did not participate in this initiative to test how their inclusion affected the results. Fifth, our study was limited to adults 65 years of age or older due to use of the MedPAR dataset. Thus, our findings might not generalize to a younger population. Nonetheless, Americans 65 years of age or older account for the majority of critical care days in the United States (38). However, use of this dataset allowed comparison of California to control states. This approach is a strength of our study in

that it minimizes potential bias from temporal trends, differences between states that are stable over time, or other changes that affect all states similarly.

Although the difference-in-differences approach is a strength of our study, it is also a limitation. This method assumes that public reporting was the only significant difference between California and control states across the time period that could have influenced our outcomes of interest: critical care mortality and discharge practices. However, to our knowledge there were no other large public health initiatives during this time period that would have affected our results. In addition, the results of this method may vary depending on the control group selected. However, as shown, our results were robust to a sensitivity analysis in which we varied the control group.

Despite these limitations, our study provides early evidence that public reporting of in-hospital ICU mortality may not reduce mortality as intended. Conversely, public reporting may alter discharge disposition, a finding of unclear clinical significance. Future work should investigate the mechanism and consequences of our findings, including whether patients were appropriately transferred to the best hospitals, whether the increase in transfers improved mortality rates at the accepting hospitals, or whether the changes in discharge patterns affected overall healthcare costs to help guide future quality reporting initiatives. In the meantime, policy makers should exercise caution in implementing public reporting initiatives in the ICU and move toward program designs, such as the use of 30-day rather than in-hospital mortality, that minimize the potential for unintended consequences and maximize the chances for improvements in quality. ■

Author disclosures are available with the text of this article at www.atsjournals.org.

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