Invited Commentary

The Medical Hospital Readmission Reduction Program
Does It Do Any Good?

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One of the largest experiments in health care payments in recent years is the Hospital Readmission Reduction Program (HRRP), a provision of the Affordable Care Act. Roughly 20% of Medicare admissions lead to a readmission within 30 days, and many readmissions are considered prima facie evidence of poor care. Historically, readmissions were a source of revenue for hospitals but a large cost for the Medicare program. The HRRP cut hospitals' Medicare inpatient payments as a penalty for high readmission rates on targeted conditions.

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Two findings in early studies[1,2] suggested that the HRRP dramatically lowered readmission rates. First, risk-adjusted readmission rates dropped markedly soon after the Affordable Care Act's passage; 1 study[1] found that for 3 targeted conditions (acute myocardial infarction, heart failure, and pneumonia), readmission rates declined from 21.5% in 2007 to 17.8% in 2015. Second, declines were largest for hospitals subject to the penalties and for those conditions for which the hospital stood to lose financially from readmissions.[1,2]

Over time, however, the evidence has become more mixed. In this issue of *JAMA Internal Medicine*, 2 new studies[3,4] suggest that at least some of the effect of the HRRP on readmissions is an illusion. Results by Tsugawa et al[3] suggest that early studies overestimated the declines in readmission rates. At issue is whether the reduction in readmissions was owing to contemporaneous but unrelated changes in the reported health of hospitalized patients. Earlier work by Ody et al[5] and Ibrahim et al[6] pointed out that Medicare allowed hospitals to list up to 24 comorbid conditions just after the HRRP was enacted, compared with 8 in the period before. As a result of this coding change, severity of patient conditions appeared to increase, and risk-adjusted readmissions appeared to fall. Ody et al[5] estimated that the true decline in readmissions was roughly 1.35 percentage points, or half as large as prior estimates suggested. However, the risk adjustment underlying this analysis relied on specific assumptions about how to select comorbidities after the coding change.

Tsugawa et al[3] investigate 3 methods of selecting comorbidities after the coding change: (1) select 24 comorbidities, (2) select 8 comorbidities at random, and (3) select the first 8 comorbidities. They plot the prevalence of 5 important comorbid conditions (chronic heart failure, chronic obstructive pulmonary disease, diabetes, hypertension, and renal failure) before and after the coding change. Using method 1, patients appear to become sicker after the coding change. Early studies on the HRRP used this method and therefore estimated that declines in readmissions were larger than they actually were. Using method 2, patients appear to become healthier. Method 3, the method used by Ody et al,[5] performs best because hospitals report "more important" comorbidities (ie, those more predictive of readmission risk) in earlier diagnosis spots. Tsugawa et al[3] provide clear evidence for how future researchers should address the coding change.

The second article in this issue of *JAMA Internal Medicine*[4] challenges the conclusion that hospitals that were initially penalized by the HRRP decreased readmissions by more than hospitals that were not.[2] Joshi et al[4] argue that a statistical artifact explains 74.34% to 86.45% of this greater responsiveness. Intuitively, it makes sense that penalized hospitals would work harder to lower readmissions; who would not drive slower after a speeding ticket? Joshi et al[4] consider a different hypothesis, that maybe the speeding ticket was handed out during an aberration day where the driver was going extra fast. Statistically, the issue is that hospitals were penalized based on past readmission rates, but that regression to the mean occurs in readmission rates. Joshi et al[4] performed several tests to evaluate how important this statistical phenomenon is. For example, they created a "placebo" treatment 3 years before the actual HRRP. Using earlier data, they assigned hospitals to placebo treatment and control groups and instituted a placebo HRRP policy. Hospitals with high readmission rates meaningfully responded to the placebo treatment. This, in combination with other tests, suggests that much of the "response" by penalized hospitals reflects regression to the mean. The results by Joshi et al[4] imply that, after accounting for mean reversion, penalized hospitals decreased readmissions by approximately 0.35 percentage points more than nonpenalized hospitals.

Joshi et al[4] are not the only ones to question whether readmissions actually declined by more for conditions and/or hospitals where the policy had more bite. Ody et al[5] argued that a statistical artifact explains effectively all the evidence that decreases in readmission rates were larger for penalized conditions than nonpenalized conditions and for general acute care hospitals (which could face penalties) than critical access hospitals (which could not face penalties). Specifically, penalized conditions and general acute care hospitals had higher baseline readmission rates than nonpenalized conditions and critical access hospitals. When Ody et al[5] accounted for differences in baseline rates, the reductions in readmissions where the policy might bite were no longer larger than where it could not. In total, these pieces of evidence undermine the assertion that decreases were larger where the program had more bite.

One remaining study by Gupta[7] also suggests that decreases in readmissions were larger where the program had more bite. This report is not subject to the critique of Joshi et al.[4] Gupta relied on an instrumental variables strategy that uses the fact that patient socioeconomic status affects readmis-

sion rates but was excluded from the risk-adjustment models. Thus, hospitals serving patients of lower socioeconomic status were more likely to face penalties, and these hospitals also lowered risk-adjusted readmissions by more. His estimates suggested that the HRRP lowered risk-adjusted readmission rates by approximately 1 percentage point.

The magnitudes in these reports by Gupta,[7] Joshi et al[4], and Ody et al[5] are closer to each other than to the estimates in earlier reports but are meaningfully different from each other (statistically and for policy purposes). Taken together, the best evidence seems to suggest that the HRRP likely had a small, favorable effect on readmission rates. Although smaller than the first set of findings, these changes are not trivial. Avoiding readmission for 1 in 100 Medicare admissions (as in Gupta[7]) or 1 in 300 admissions (as in Joshi et al[4]) is financially and clinically beneficial.

A remaining question is whether hospitals lowered readmissions in desired ways, such as providing better care, or in undesired ways, such as turning away patients seeking readmission. The program's effect on mortality serves as a crude but straightforward summary measure of whether its effects on patient health are salutary or detrimental. Although many studies have reported on the topic, again, the most robust statistical analysis is that of Gupta.[7] He finds the program lowered mortality. Thus, desirable steps that hospitals are taking to lower readmissions seem on balance to be more than counteracting any undesirable steps that they are taking.

Many of the gains from the HRRP are likely to be longer term and more difficult to quantify. Hospitals are experimenting with a range of processes to reduce readmissions; the program's long-term success depends on identifying and spreading best practices. Although the program's structure is national, it is agnostic on how hospitals should reduce readmissions; appropriate best practices likely vary with each hospital's population. Unfortunately, hospitals that successfully reduced readmissions may not see evidence of this in data that do not address the measurement issues identified in these studies.

ARTICLE INFORMATION

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