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ABSTRACT

The Hospital Readmission Reduction Program (HRRP) and the Hospital Value Based Purchasing Program (HVBP), two components of the Affordable Care Act's cost containment measures, introduced potentially sizeable penalties to underperforming hospitals across a variety of metrics. To the extent that penalized hospitals subsequently changed their processes of care, such changes may translate into higher payments from commercial insurance patients. In this paper, we estimate the effects of these pay-for-performance programs on private hospital payments using data on commercial insurance payments from a large, multi-payer database. We find that nearly 70% of the costs of the HRRP and HVBP penalties are borne by private insurance patients in the form of higher private insurance payments to hospitals. Specifically, we show that HRRP and HVBP led to increases in private payments of 1.4%, or approximately $183,700 per hospital based on an average relative penalty of $271,000. We find very limited evidence that these effects are driven by quality improvements, changes in treatment intensity, or changes in service mix.

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1 Introduction

Public pay-for-performance (P4P) programs tie public payments to a predetermined set of measures, which allow policy makers to encourage or discourage certain outcomes. While the potential advantages of such programs are clear, P4P may also introduce unintended consequences depending on the design of the program, the relevance of the outcomes, and the precision with which relevant outcomes can be measured. The United States health care system has historically operated in the absence of any large scale public P4P programs; however, this changed with the introduction of the Hospital Readmission Reduction Program (HRRP) and the Hospital Value Based Purchasing Program (HVBP), both of which were introduced in 2012 as part of the cost containment provisions of the Patient Protection and Affordable Care Act (ACA). The HRRP and HVBP were designed to penalize hospitals with lower-than-expected quality, and an active literature has emerged that attempts to measure the effects of these programs on hospital quality outcomes (Ryan *et al.*, 2015; Mellor *et al.*, 2016; Gupta, 2016; Ryan *et al.*, 2017; Gupta *et al.*, 2018; Wilcock *et al.*, 2018). While the empirical results remain mixed, implicit in this important literature is the assumption that hospitals pursued some costly investments in an attempt to improve their performance. Any such costly investments may then have the unintended consequence of increasing a hospital's negotiated payments with private insurers, particularly in the highly concentrated U.S. hospital market.[1]

In this paper, we use a compelling data set on actual payments from private insurance firms to hospitals to quantify the effects of public P4P programs on hospital payments from private insurers. Our data, maintained by the Health Care Cost Institute (HCCI), contain all hospital inpatient claims to three national commercial insurers.[2] These unique data include payments for every claim, which capture the negotiated payments between hospitals and insurers and which may differ substantially from charge-based estimates of payments often used in the literature (Dafny, 2009; Dranove *et al.*, 2017). Our data cover approximately 28% of individuals under the age of 65 who have employer-sponsored insurance (ESI). When merged with several other datasets on hospital and county characteristics, our final analytic data constitute a balanced panel of 50% of all inpatient prospective payment hospitals in the U.S. between 2010 and 2015.

Under the HRRP and HVBP programs, hospitals were penalized (or potentially rewarded

[1] Throughout, rather than use the term "price," we refer to the financial transfer for a given procedure as the "payment" from a private insurance firm to a hospital. A payment is distinctly different than a hospital "charge," which effectively represents a hospital's list price for a give procedure. Private insurance firms negotiate substantial discounts from charges.

[2] Cooper *et al.* (2017) also use HCCI data to examine broad trends in hospital pricing from 2007 through 2011.

under the HVBP) by up to 3% of the hospital's total Medicare revenues based on observed quality metrics.[3] Since penalty amounts vary from year to year, and because not all hospitals are penalized, the HRRP/HVBP generate both cross-sectional and temporal variation in P4P penalties. Exploiting this variation, our baseline empirical specification is a hospital fixed effects estimator in which we estimate the difference in average payments between those hospitals with a net penalty under the HRRP/HVBP relative to those not penalized, discussed in detail in Section 3.2. Our baseline results reveal an increase in average payments of 1.4% for penalized hospitals, equivalent to a $167 increase in the average private payer payment from 2013 through 2015. We also find evidence that penalty size matters with respect to payment changes, with a 2.4% increase in payments for the most heavily penalized hospitals relative to those hospitals receiving no penalty or a bonus. As a back-of-the-envelope calculation, our estimated increase of 1.4% equates to a total increase in private payments of $183,700 per hospital, based on an average relative reduction in Medicare payments of $271,000.[4]

At least four factors support a causal interpretation of our findings. First, a central feature of HRRP/HVBP is that penalized hospitals had little, if any, opportunity to adjust their penalties *ex ante*. This is because the HRRP/HVBP penalties were calculated using data from several years prior to the start of the programs. For example, penalties incurred in Fiscal Year (FY) 2013 were based on Medicare claims from July 2008 through June 2011. The set of quality metrics underlying the penalty formulas also changed over time, further limiting a hospital's ability to predict their penalty status in advance. For example, the set of conditions covered by the HRRP/HVBP expanded in FY 2015, but the new conditions were not announced until FY 2014, at which point the data underlying the new conditions were already collected.

Second, there is evidence that the formulas used to assign HRRP/HVBP penalties have not sufficiently identified marginally low- versus high-performing hospitals. For example, the HRRP penalizes hospitals for under-performance in any of the relevant conditions, even if hospitals significantly over-perform in other areas. As a result, nearly 80% of hospitals in our sample are ultimately penalized under the HRRP at some point in our panel. Recent studies also document substantial noise in HVBP penalties or rewards and suggest that a hospital's performance under the HVBP is largely due to chance (Friedson *et al.*, 2016; Wilcock *et al.*, 2018).

[3]Some private insurer contracts explicitly tie payments to Medicare reimbursement rates (Cooper *et al.*, 2017), but in our context, there is no change in the prospective payment but rather a downward adjustment of some percentage.

[4]The total relative reduction in Medicare payments incorporates bonus payments made to some hospitals, such that the relative reduction is larger than the average penalty amount.

Third, our data offer a compelling advantage relative to most other studies of hospital payments.[5] In particular, the correlation between actual payments and a charge-based proxy for payments from the Healthcare Cost Report Information System (HCRIS) is 0.435, suggesting that charge-based estimates of payments may contain significant measurement error. Since we observe actual payments made to hospitals from private insurers, we avoid this source of measurement error.

Fourth, from an econometric perspective, time-varying unobserved heterogeneity in payments that is correlated with HRRP/HVBP penalties would tend to produce differential trends and biased results. We consider several additional analyses to test for the potential presence of such differential trends and any subsequent effects on our estimates, including a series of event studies for each treatment group, alternative specifications testing for and allowing for differential trends by penalty status, and an instrumental variables strategy that exploits the timing of treatment as instruments. Ultimately, we fail to reject a test of differential trends by penalty status, and we demonstrate that allowing for ever-penalized differential trends does not change our conclusions. With a series of alternative specifications and robustness checks, we further show that our results are not driven by regional differences, the ACA Medicaid expansion, or patient severity mix. Collectively, we find strong empirical evidence that penalties incurred under the HRRP and HVBP led to increases in private insurance payments to hospitals.

In the remainder of the paper, we examine heterogeneity in and mechanisms behind the effects of the HRRP and HVBP. We find substantial variation in payment increases across different service lines, with increases in average hospital payments for circulatory system (1.9%) and nervous system (2.1%) claims, but with economically small and insignificant effects for respiratory system, musculoskeletal system, and labor and delivery claims.[6] In addition, we estimate larger effects among hospitals that are likely to be in a better relative bargaining position with insurers, as proxied by the hospital's share of private insurance patients (Wu, 2010). Finally, we find significant heterogeneity in the effects of HRRP/HVBP by a hospital's financial relationship with its physicians, with a 2.3% increase in mean payment for vertically integrated hospitals and physician groups.

To investigate the mechanisms behind these effects, we study investments that hospitals may make following the HRRP/HVBP for which private insurers would plausibly be willing to pay. With our data, we can directly test changes in hospital quality and changes

[5] Notable exceptions include Clemens & Gottlieb (2017), who study the market for physician services and find that private payments decreased following a reduction in Medicare payment rates, and Dranove *et al.* (2017), who find little evidence of changes in private payments to hospitals following the 2008 stock market collapse.

[6] We identify "admission categories" based on the major diagnostic category classifications.

in hospital services and costs. First, because the HRRP penalizes hospitals based on risk-adjusted 30-day readmission rates for Medicare patients, hospitals may have undertaken costly investments in improving these metrics which spilled over to readmission rates for the privately insured. Thus, we estimate our preferred model on over 3 million individual acute care claims, and we find a statistically insignificant 0.1 percentage point decline in the probability of readmission. We find similar null effects across several other outcomes, including: 1) a measure of profitable services offered by the hospital (Horwitz & Nichols, 2009), suggesting that hospitals did not simply internalize the loss from HRRP/HVBP penalties; 2) the hospital's average DRG weight, suggesting little change in patient selection; and 3) average hospital length of stay and costs per discharge, suggesting no change in treatment intensity. Therefore, across a range of outcomes intended to capture mechanisms related to quality improvements, treatment intensity, and patient selection, we find economically small and statistically insignificant effects of HRRP/HVBP penalties.

One possible explanation for our estimated payment increases, which is consistent with anecdotal evidence from physicians, is that the HRRP/HVBP penalties encouraged hospitals to change their processes of care (e.g., introducing more checklists and more oversight). For example, Tanguturi *et al.* (2016) describes these introduction of case managers to identify high risk patients and the use of a discharge checklist designed to reduce preventable readmissions following a hospitalization for percutaneous coronary intervention. Such investments would not necessarily be captured in our cost data but would intuitively be more salient for hospitals that are financially integrated with their physicians, which is consistent with our estimates of larger effects among vertically integrated hospitals. Another explanation may be that private insurers are willing to pay for investments that improved outcomes for *Medicare* patients (even without an improvement in private insurance patients) if such investments improved overall hospital reputation and thus increased willingness-to-pay for private insurance patients. Finally, we acknowledge that hospitals may have pursued some costly investments in response to P4P penalties, but that the effects of P4P penalties on such costs may not be precisely estimated in our analysis to the well-documented measurement error in the HCRIS.

Ultimately, our analysis offers two central contributions to the literature. First, we extend the literature on P4P in health care. Much of this existing literature studies other areas of care delivery, such as skilled nursing facilities and home health agencies, and studies of P4P in the hospital setting focus almost exclusively on quality outcomes. To our knowledge, we are the first to examine the effects of P4P on private insurance payments to hospitals. The potential unintended consequence of an increase in hospital prices due to P4P programs is an important issue as we further refine existing P4P programs and expand P4P into other areas.

Second, and more generally, our analysis introduces another important factor in our understanding of variation in health care pricing. As clearly documented in Cooper *et al.* (2017), hospital market power explains a large amount of geographic variation in hospital prices. A large literature also considers the role of public payments on hospital prices, often examined in the context of hospital cost-shifting (Dranove, 1988; Cutler *et al.*, 2000; Frakt, 2011). Cost-shifting may play some role in our estimates given that we do not identify a clear change in costs or quality to explain our estimated payment increases; however, given the stated goals of the HRRP/HVBP and anecdotal evidence regarding hospital responses to these programs, we do not interpret the HRRP/HVBP penalties are a pure reduction in public payments. We instead take as given that hospitals responded in some costly way to the P4P penalties. In this context, our results show that changes in public policy also meaningfully contribute to variation in health care prices, even after adjusting for market power, hospital fixed effects, and other observable hospital and market characteristics.

2 Policy Background: The HRRP and HVBP

The adoption of the Medicare prospective payment system (PPS) in 1983, in which Medicare payments changed from pure fee-for-service to a capitated amount for each inpatient stay depending on diagnosis, generated incentives for hospitals to cut "excessive" procedures. PPS also created incentives for hospitals to discharge patients quickly. By 2011, Medicare paid \$24 billion per year for 1.8 million hospital *readmissions* – admissions to any hospital within 30-days of discharge for the same condition. While some readmissions are unavoidable, the HRRP was a cost containment in the ACA designed to levy penalties on hospitals with "excessive" readmissions.

Starting in FY 2013 (October 2012-September 2013), the HRRP penalized hospitals for which 30-day readmissions for acute myocardial infarction (AMI), heart failure (HF), and pneumonia (PN) exceeded risk-adjusted thresholds constructed as a function of national averages. Recall that this assessment was based on data collected from July 2008 through June 2011. In this first year of the program, hospitals faced a maximum cut in Medicare payments of 1% across all DRGs. In FY 2015, the maximum penalty increased to 3%, total penalties rose to \$420m (Rau, 2015), and applicable conditions were expanded to include chronic obstructive pulmonary disease (COPD) and total hip and knee replacements. The Congressional Budget Office (2010) estimates that HRRP would reduce hospital payments from Medicare by \$113 billion through 2019. There is also strong evidence suggesting that hospitals were aware of the potential impact of the HRRP. For example, a national survey of hospital leaders found that nearly two-thirds of respondents reported that the HRRP had a substantial impact on their hospital's efforts to reduce readmissions compared to prior readmission policies (Joynt *et al.*, 2017).

By contrast, the HVBP program is rooted in a standard principal-agent model in which the principal (CMS in this case) contracts with agents (hospitals) to provide quality care to Medicare enrollees. The HVBP program scores hospitals based on their achievement (comparison to other hospitals) as well as their improvement (comparison to their own previous performance). Similar to the HRRP, the HVBP bases changes in payments on past quality, with data collected over the same lagged time period as in the HRRP. However, unlike the HRRP, the HVBP program is funded by reducing all hospitals' base operating Medicare severity diagnosis-related group (MS-DRG) payments and creating rebate incentives depending on defined quality metrics. The percentage reduction increased annually up to 2%. The program defines several quality domains and converts measures of quality within each domain to points, which are aggregated and mapped to a total point score. The total point score determines the magnitude of the payment change, which may be positive or negative depending on if a hospital generates a rebate large enough to offset the reduction.

Since the goal of both the HRRP and HVBP is to improve hospital quality, a recent literature examines the effects of the HRRP/HVBP on hospital readmission rates and other quality metrics. The existing literature in this area remains mixed. Gupta *et al.* (2018) find that the HRRP was associated with a 1.6 percentage-point reduction in 30-day Medicare readmissions for heart failure but a 1.4 percentage-point *increase* in 30-day mortality. Gupta (2016), however, finds evidence of a reduction in Medicare hospital mortality rates (a decrease of about 3%, significant at the 10% level) from the HRRP, which may account for as much as 60% of the reduction in readmissions. Mellor *et al.* (2016) similarly find that the HRRP led to a decline in Medicare AMI 30-day readmission rates; however, new evidence from Ibrahim *et al.* (2017) suggests that observed decreases in readmissions may have been driven by hospitals coding patients more severely and not by "real" quality improvements. Consistent with this result, Wilcock *et al.* (2018) find that the majority of HRRP penalties are a reflection of poor risk adjustment in the penalty calculation and not of true, underlying hospital quality.

Regarding the HVBP, the literature generally finds little or no effect on hospital quality (Ryan *et al.*, 2015; Doran *et al.*, 2017; Norton *et al.*, 2017; Ryan *et al.*, 2017). Examining data from 2015 to 2016, Norton *et al.* (2017) did find some hospital response to the HVBP, but this response was in specific areas with the greatest marginal revenue rather than those areas with larger quality benefits. Conversely, based on quality data from 2005 through 2014, the Government Accountability Office (2015) found no effect of HVBP on quality. This study also interviewed a handful of hospital officials and concluded "the HVBP program generally reinforced ongoing quality improvement efforts, but did not lead to major changes in focus." Friedson *et al.* (2016) offer an explanation for these findings, where the authors find that the HVBP does not sufficiently discriminate between hospitals, and whether hospitals are penalized or rewarded by the HVBP program is largely a matter of chance rather than a reflection of true underlying quality.

3 Empirical Analysis

3.1 Data

Our primary data come from three large health insurance firms and account for roughly 28% of all individuals under the age of 65 with employer sponsored health insurance over the period of 2010 through 2015. To these data, we merge information on HRRP and HVBP penalties/rewards and other cost information from the Healthcare Cost Report Information System (HCRIS); hospital-level characteristics such as bed count, for-profit status, and system membership from the American Hospital Association (AHA) annual surveys; data on a hospital's payer mix (i.e., the number and share of Medicare, Medicaid, or private insurance patients) also from HCRIS; and county-level demographic characteristics from the American Community Survey (ACS). We restrict our sample to community hospitals in urban areas and in the contiguous United States, with at least 30 staffed beds, at least 25 admissions in a given year in the HCCI data, and observed HRRP/HVBP from HCRIS. Our final sample consists of 1,386 hospitals and 8,316 hospital/year observations.[7]

Because hospital payments are often bundled across services, we follow Gowrisankaran *et al.* (2015), who use similar payment data from Northern Virginia, and aggregate payments to the hospital level by dividing the total payment for each claim by the appropriate DRG weight and regressing this amount on individual (claimant) characteristics and hospital fixed effects. Using the estimated regression results, we predict the risk-adjusted mean hospital payment for a given year, which reflects the mean bargained payment. Table 1 presents mean payments across hospitals over time. While average risk-adjusted payments received by hospitals increase roughly 5% annually between 2010 to 2015, shares of public (Medicare & Medicaid) and private patients remain relatively stable over time. Importantly, while shares remain stable, within-hospital patient mix may vary considerably over time as a function of

[7] We also consider alternative samples in which we allow for missing net penalty values from HCRIS or where we arbitrarily set missing HRRP/HVBP values to 0 (e.g., under the assumption that missing values indicate that the hospital was excluded for the program in that year). Results from these samples are similar to those presented in the text and available upon request.

public payments, which is why we treat payer-specific discharges as a separate dependent variable. The last column of Table 1 shows the fraction of hospitals subject to a net Medicare payment reduction. Note that the CMS fiscal year runs from October through the following September. Because of discrepancies between the fiscal year of the hospital and that of CMS, 32% of hospitals faced a penalty in their 2012 FY. By FY 2015, 79% of hospitals faced some payment reduction. Beginning FY 2013, the average penalty amount among hospitals ever penalized was $204,711, which increased from $171,279 in 2013 to $272,438 in FY 2015. With non-penalized hospitals receiving an average bonus of just over $66,000, the average relative payment reduction among penalized hospitals was around $271,000.

Since our baseline empirical specification exploits within-hospital variation, we split our sample by whether a hospital ever faced a payment reduction under the HRRP and HVBP during our sample period. Table 2 presents summary statistics of our main dependent variable and selected independent variables by ever-penalized status. Payments to never-penalized hospitals are marginally higher than those to penalized hospitals over the 2010–2015 period. Non-profit hospitals (public and private) constituted a much larger share of never-penalized hospitals, suggesting that non-profit hospitals may be of higher quality, at least in terms of the HRRP and HVBP. However, case mix is significantly more severe in the ever-penalized hospitals, which suggests that CMS risk-adjustment in the HRRP and HVBP may not perfectly adjust penalty thresholds (consistent with Wilcock *et al.* (2018)). Ever-penalized hospitals tend to be in more competitive markets, have lower Medicare share, and come from more heavily populated counties. Evidence from Table 2 therefore suggests that controlling for hospital fixed effects is important in models of hospital payments because of persistent differences between ever-penalized and never-penalized hospitals.

The log of the annual, within-hospital mean of private insurance payments constitutes our primary dependent variable of interest. For brevity, we refer to this variable simply as the log mean payment. For comparison with the literature, we also follow Dafny (2009) in estimating hospital payments using the average net revenue for non-Medicare inpatient discharges. Since Medicaid revenues are not provided in HCRIS, the measure is a weighted average of net revenue per discharge for commercially insured and Medicaid patients where the weights equal the share of inpatient discharges belonging to each payer. This same measure has been used in recent studies examining hospital pricing behavior, including Schmitt (2018) and Lewis & Pflum (2015). To eliminate outliers, we trim the lower and upper tails at the 5th and 95th percentile of the resulting payment distribution, and we normalize this estimated payment based on the hospital's observed case mix index (CMI) from the inpatient prospective payment system (IPPS) final rule files. To differentiate this measure of payments from our observed payments from the HCCI data, we refer to this measure as the log mean net charge.

Finally, since a natural way to reduce exposure to HRRP/HVBP penalties is to avoid treating Medicare patients, we include measures of payor mix as an additional set of outcomes. These measures include the log number of Medicare discharges, the log number of Medicaid discharges, and the log number of other discharges (non-Medicare and non-Medicaid). We also considered the Medicare, Medicaid, and other insurer shares (rather than log counts). Those results are excluded for brevity but qualitatively similar to the analysis of log counts.

3.2 Regression Analysis

Our preferred empirical specification isolates within-hospital variation in private payments over time by whether a hospital faced a net penalty from the HRRP and HVBP. This analysis therefore focuses on the extensive margin of penalties. Equation 1 presents our main empirical model:

$$y_{ht} = \alpha_h + x_{ht}^{'}\beta + \delta 1[Penalty_{ht}] + \sum_{j=2011}^{2015} \theta_t 1[t = j] + \epsilon_{ht}, \qquad (1)$$

where outcome y_{ht} at hospital h in fiscal year t is a function of a hospital specific intercept, α_h; a vector of time-varying hospital and market-level exogenous characteristics, x_{ht}; an indicator for a net penalty under the combination of HRRP/HVBP policies; controls for year effects, θ_t; and an i.i.d. error term ϵ_{ht}. Because the penalty indicator is zero for all hospitals in 2010 and 2011, and because we include hospital fixed effects, Equation 1 represents an unscaled difference-in-differences estimator, which constitutes a weighted average of four difference-in-differences estimates corresponding to the four years in which a hospital may have first been penalized. Our parameter of interest, δ, captures the extent to which hospitals penalized under the HRRP/HVBP receive differential private payments relative to hospitals with no penalty (which includes hospitals that received a bonus).

Table 3 presents estimated effects of HRRP/HVBP penalties on the log of mean payments, the log of mean net charges, and payer-specific (log) discharges. The first column of Table 3 demonstrates that hospitals that faced payment reductions increased payments by 1.4% over the period of 2012-2015. This represents a roughly \$167 increase in payments among penalized hospitals, on average.[8] Column 2 presents estimates from a similar model

[8] This interpretation is based on the average private insurance payment of \$12,100 among penalized hospitals after FY 2012. Assuming this average payment reflects a 1.4% increase in the average payment in the absence of the penalty, we calculate the effect in dollar terms as $\$12,100 - \frac{\$12,000}{1+0.014}$.

in which we replace negotiated payments with the log of mean net charges as discussed previously (Dafny, 2009; Lewis & Pflum, 2015; Schmitt, 2018; Dranove *et al.*, 2017). Results in column 2 suggest a smaller and statistically insignificant change in log mean net charges for penalized hospitals, which we argue demonstrates the importance of using actual payment data. Columns 3 and 4 of Table 3 show movement *away* from Medicaid and Medicare patients for penalized hospitals, with discharges decreasing by 4.5% and 2.7%, respectively.

While Table 3 demonstrates higher payments on the extensive margin, we investigate the intensive margin effect of penalties on payments by breaking the distribution of penalty size into quartiles and replacing the indicator for net penalty in Equation 1 with indicators for each of the four penalty quartiles, where the omitted category represents those hospitals which either saw no penalty or a net bonus in Medicare reimbursements. Results are presented in Table 4. Consistent with our results in Table 3, we find that average payments are significantly higher in penalized hospitals relative to those receiving no change or a small bonus. We find no effect on payments for hospitals in the first (smallest) quartile of penalties, defined as a per Medicare discharge penalty of between \$0.01 and \$12.59; however, we find a 2.4% increase in mean payments for hospitals in the highest quartile of penalties (between \$57.10 and \$291.60 per Medicare discharge). Results in Table 4 therefore suggest that private payment increases are larger as the HRRP/HVBP penalty increases. Furthermore, we find monotonically more negative effects of a penalty on Medicaid and Medicare discharges in the size of the penalty.

3.3 Sensitivity and Robustness

For a causal interpretation of δ, the underlying assumption in Equation 1 is that there are no time-varying unobserved characteristics that differentially affect payments in penalized hospitals relative to non-penalized hospitals. While we cannot directly test this assumption empirically, we can examine the presence of pre-trends as suggestive evidence for or against the assumption of parallel counterfactual trends. Note that in our data, we have four different treatment groups defined by hospitals first penalized in 2012, 2013, 2014, or 2015, respectively. There is also substantial persistence in treatment, such that most hospitals penalized in year t are also penalized in years $t+1$, $t+2$, etc. Therefore, we test for evidence of differential trends in two ways. First, we present an event study for each treatment group, in which we interact the treatment dummy with year dummies and estimate separate treatment coefficients in each year (relative to the year prior to the penalty). Second, we plot the mean residual from a regression of average payments, analogous to Equation 1 but where we exclude the net penalty variable. Based on the regression results, we then form the mean residual separately for penalized versus non-penalized hospitals in each year and add to this the average observed payment among penalized/non-penalized hospitals by year.

Results are presented in Figures 1 and 2, which demonstrate important differences in trends across treatment groups. While our pre-period data are limited in the early treatment groups, we fail to reject the null hypothesis of parallel pre-trends among hospitals penalized in 2012 or 2013 (Figure 1). This is not the case for hospitals treated in 2014 or 2015 (Figure 2), where we do find evidence of differential pre-trends. Allowing for differential trends as reflected in the mean residual payment graphs, we see clear graphical evidence of parallel movement in average payments with a discontinuity at the treatment period in the 2012 and 2013 treatment groups. Figures 1 and 2 also demonstrate that payment results in Table 3 are likely driven by those hospitals initially penalized earlier in our sample.

To proceed, we consider three additional estimators. First, we re-estimate Equation 1 when setting $\alpha_h = \alpha$ in order to gauge the sensitivity of our results to the presence of unobserved and time-invariant hospital factors. Second, we include in Equation 1 a set of time dummies interacted with a dummy variable that equals one if the hospital is ever observed to be penalized. Differential trends conditional on penalty status and other controls would be suggestive of time-varying unobserved heterogeneity, which may bias our estimate of δ toward zero. Finally, we estimate a fixed-effects instrumental variables regression that scales the four difference-in-differences estimates by the probability a hospital is first penalized in each respective year. Based on graphical evidence in Figure 2 suggesting differential trends among hospitals penalized in 2014 or 2015, we also present estimates from these additional estimators for both the full sample and a sample that restricts the treatment group to only those hospitals first penalized in 2012 or 2013.

The top panel of Table 5 presents results for the full sample while the bottom panel presents results for the sample restricted to never penalized hospitals and those penalized initially in 2012 or 2013. The first row in Table 5 shows that estimates for log mean payments and log net charges without hospital fixed effects are negative, large, and significant. Relative to our initial results, these findings suggest that: 1) persistent and unobserved hospital-level heterogeneity is an important driver of outcomes in our setting; and 2) hospital fixed effects may in fact go a long way toward controlling for mean differences between charges and payments. Many studies of hospital pricing proxy for payments with hospital charges and argue that hospital fixed effects control for mean differences between charges and payments (Cutler *et al.*, 2000). Our results offer some assurance that findings of a significant effect using charge-based estimates of prices may indeed be reflective of a true price increase; however, we also emphasize the importance of payment data with respect to the precision and measurement of private insurance payments, noting the lack of statistical significance in our model of log mean net charges presented in Table 3. Findings of an insignificant effect using charge-based proxies for private payments may therefore be driven by incorrect inference (e.g., due to measurement error) or due to a true underlying null effect.

The second row in Table 5 repeats our initial results in Table 3, and the third row presents results when allowing for differential trends by whether a hospital is ever penalized. Here, the estimate for log mean payments decreases from 1.4% to 1.0% (or from 1.8% to 1.2% in the 2012/2013 treatment group) but remains economically meaningful and statistically significant in the full sample. We also present the p-value of a joint test of the null that the time trend dummies interacted with ever-penalized are jointly zero. For our log mean payment outcome, we fail to reject the null of common trends between the ever-penalized and never-penalized hospitals, with a p-value of 0.497 in the full sample and 0.903 for the 2012/2013 treatment group.

The final row of Table 5 presents results from the fixed effects instrumental variables estimator. Specifically, we estimate the following with two-stage least squares (2SLS):

$$y_{ht} = \alpha_h + x_{ht}^{'}\beta + \delta 1[\hat{Penalty}_{ht}] + \sum_{j=2011}^{2015} \theta_t 1[t=j] + \epsilon_{ht} \tag{2}$$

$$Penalty_{ht} = \alpha_h + x_{ht}^{'}\beta + \sum_{j=2011}^{2015} \theta_t 1[t=j] + \sum_{j=2011}^{2015} \lambda_t 1[t=j]E_h + \tau_{ht}, \tag{3}$$

where Equation 2 is the main equation that estimates the effect of penalty status predicted from the first-stage in Equation 3. Here, interactions between year dummies and E_h, a time-invariant dummy for ever-penalized status, are highly predictive instruments for penalty status, which remove the temporal component of hospital penalties. The λ parameters then measure the adjusted proportion of ever-penalized hospitals that were penalized in year t.[9] Estimates in the final row of Table 5 find a larger effect of penalties on payments, with a 3.7% and 2.9% increase in mean payments in the final and restricted samples, respectively. Furthermore, an over-identification test is available. Because no hospitals were penalized in 2011 and because there is only one endogenous variable in Equation 2, we can include the interaction $E_h 1[t = 2011]$ in the main equation. Table 5 presents the p-value on the t-test that the parameter on $E_h 1[t = 2011]$ is zero in the main equation. For both samples, we fail to reject the null hypothesis, which suggests that trends in payments are similar between those hospitals that will eventually be penalized versus those that will not.

The results in Tables 3-5, coupled with the graphical evidence in Figures 1 and 2 and

[9]Kaestner *et al.* (2014) and Carton *et al.* (2016) use a similar estimator as an alternative to two-way fixed effects estimators.

the institutional details of the HRRP/HVBP, offer compelling evidence of a causal effect of HRRP/HVBP penalties on private insurance payments to hospitals; however, we remain concerned about several institutional confounding factors that may bias this result. Thus, we consider different specifications for Equation 1 intended to assess the sensitivity of our estimates to such potential confounders. All results are presented in Table 6.

First, we are concerned that unobserved differences across markets (e.g., with regard to insurer market power) may influence our estimates. We therefore include a set of county-level fixed effects, with results summarized in panel 1 of Table 6. Here, we continue to find positive and significant effects on private insurance payments, as well as significant reductions in the log number of Medicare discharges. These results suggest that local area variation in provider or insurer markets is not driving our results.

Second, we remain concerned that other changes in the hospital-insurer relationship may drive our estimated increase in payments, particularly with respect to the implementation of the ACA. We therefore consider an alternative specification in which we include an indicator for whether the hospital was in a Medicaid expansion state as of 2014. These results are presented in panel 2 of Table 6 and are largely unchanged from our initial estimates. We also note that this concern is partially alleviated in the bottom panel of Table 5 where we restrict the treatment group only to those hospitals treated before 2014.

Third, since the HRRP and HVBP are intended to reward and/or punish hospitals based in-part on quality of care, hospitals may respond to HRRP/HVBP penalties by improving along a broad set of quality metrics. These metrics need not directly align with quality underlying the HRRP/HVBP. Indeed, an optimal hospital response may be to focus on patient satisfaction or other non-clinical measures to potentially offset the financial effects of the penalties. One requirement for such a response to exist is that the HRRP/HVBP penalties reveal new quality information to the market. The distribution of readmission rates across hospitals before the HRRP/HVBP suggest this is not the case, as penalized hospitals already displayed higher readmission rates relative to other hospitals in the years prior to 2012 (see Figure 3). Another requirement for this type of response is that patients are responsive to any hospital improvements. Studies from Dranove & Sfekas (2008) and others suggest that this is unlikely given the relatively small estimated effects of quality reporting on hospital choice. We nonetheless examine this issue with an alternative specification in which we control for a hospital's quality as measured by patients' overall hospital rating from the Hospital Consumer Assessment of Healthcare Providers and Systems (HCAHPS). Panel 3 of Table 6 reports results from this model, with estimates almost identical to those in Table 3.

Fourth, it may be that other changes introduced through the ACA (e.g., expansion of insurance on the individual market) changed the "typical" patient being admitted to the hospital. In panel 4 of Table 6, we demonstrate that our results are again unchanged when conditioning on the hospital's average case mix.

3.4 Heterogeneous Effects

If HRRP/HVBP penalties induce some hospital behaviors that ultimately affect private insurance payments, then there are several dimensions by which we would expect effect sizes to vary. Perhaps the most natural source of variation is across service lines, particularly since only selected conditions are included as part of the HRRP/HVBP penalty calculations. Therefore, we examine heterogeneity across service lines by estimating effects of net penalty status on the log of mean payments within selected acute care admission service categories. Estimates for δ are presented in Table 7 for several specific categories, where we find the largest increases in payments for nervous and circulatory admissions.[10]

Because hospitals cannot unilaterally translate costly investments into higher payments from commercial insurers, an important source of heterogeneity is the relative bargaining power of hospitals as payments are the result of bilateral negotiation with insurers, and a hospital's ability to negotiate higher payments will depend on the hospital's bargaining position (Dor *et al.*, 2004; Gowrisankaran *et al.*, 2015; Lewis & Pflum, 2015; Ho & Lee, 2017). To investigate, we attempt to proxy for a hospital's bargaining position by constructing the quartile of the hospital's share of public patients relative to total patients, and we interact our penalty variable with indicators for each quartile.[11] This analysis is similar to that of Wu (2010), who intuits that a hospital with a large share of private payers represents a more important client for the insurance market.[12] Results are presented in Table 8 and suggest that our initial estimate is driven by hospitals with the smallest share of public patients. Indeed, the first column of Table 8 demonstrates that payments increased by 3.9% for hospitals with the smallest share of public patients. This increase was nullified for hospitals in the third and fourth quartile of public patient shares.

[10]For each admission category, we restrict our sample to hospitals with at least 25 admissions in that category in each year of our sample.

[11]We also tested for differential effects of the penalty among hospitals operating as a monopoly, duopoly, or triopoly. Here, we find a relatively large and positive effect of the interaction between a monopoly indicator and the penalty indicator, with a point estimate of 0.013; however, the effect is statistically insignificant with a p-value of 0.23. We estimate smaller and statistically insignificant effects on other interaction terms between penalty status and duopoly or triopoly indicators. This pattern of results persists for different measures of the hospital market. For brevity, the full results from these specifications are excluded from the paper but are available upon request.

[12]Applying this intuition to a study of hospital cost-shifting following the Balanced Budget Act of 1997, Wu (2010) finds that hospitals with larger shares of private patients were more able to pass Medicare payment reductions on to private payers.

Another proxy for bargaining position is whether a hospital is aligned with its network of physicians. Lewis & Pflum (2015), for example, find that hospitals that are affiliated with a physician group are able to negotiate a larger share of surplus. Vertical integration with physicians may therefore put some hospitals in a more favorable bargaining position, and thus facilitate a larger increase in private payments. To investigate, we estimate our preferred empirical model on data from only those hospitals that already owned a physician group or physician practice *prior* to 2012.[13] We also estimate our model on hospitals never observed to be vertically integrated. As shown in Table 9, among those hospitals already vertically integrated, the effect of a net penalty on payments is 2.3% and significant. Meanwhile, penalties are associated with a small and statistically insignificant effect on payments among those hospitals never observed to be vertically integrated.[14]

4 Mechanisms for Payment Increases

The results in Section 3 provide strong empirical evidence that penalized hospitals were able to increase private insurance payments. The effect size varies along several dimensions, but on average, we estimate an increase in private insurance payments to hospitals by 1.4%. It is unclear, however, exactly how a hospital could translate a penalty into higher private insurance payments. In this section, we therefore consider different mechanisms that may have facilitated such an increase.

4.1 Changes in Hospital Quality

Since the HRRP/HVBP are designed to improve hospital quality, it may be that hospital quality improvements ultimately led to our estimated payment increases. As discussed in Section 2, most of the existing studies of the HRRP/HVBP on quality tend to focus on the Medicare population, but to explain increases in private insurance payments, we need to consider the effects of the HRRP and HVBP on quality among private insurance payments. We are not aware of any evidence in the literature suggesting that quality in the private insurance market improved due to the HRRP or HVBP programs. Indeed, in a study of

[13] The AHA surveys provide information at the hospital-level on whether a hospital currently has an "integrated salary model." This measure unfortunately does not capture *how many* physicians are employed by a hospital, but instead only captures if there is any integrated model reported between the hospital and any of its physicians.

[14] We also considered whether the penalty itself led to more integrated salary models by treating the binary integration measure as an additional outcome. Here, we estimate a very small and insignificant negative effect of being penalized on the probability of reporting an integrated salary model, suggesting that penalized hospitals were not integrating with physicians due to the penalty. These results are limited by the nature of our vertical integration data and are therefore excluded from the paper but available upon request.

private insurance patients in Florida and California, Demiralp *et al.* (2017) find no evidence that the HRRP reduced the readmission rate among the non-Medicare population. To test this in our data, we directly investigate whether penalized hospitals improved quality (as measured by readmissions) in the commercial insurance market.[15] We estimate the effect of hospital penalty status on the probability of readmission using a linear probability model with data at the individual admission level. Following the Agency for Healthcare Research and Quality definition, we classify a readmission to be any admission to any inpatient prospective payment hospital within 30 days of a discharge.[16]

Our linear probability model includes all controls from our main specification plus patient controls such as age range, gender, length of stay, DRG weight, insurance product type (HMO, PPO, POS, EPO), and DRG fixed effects. As summarized in column 1 of Table 10, the results demonstrate that, even with a sample of over 3 million observations, we find an economically and statistically insignificant effect of penalty status on the probability of readmission.[17] To the extent that penalized hospitals are investing in quality to lower Medicare readmissions among the indicated areas, we find no evidence that such quality improvements are changing readmissions on average for the commercially insured population.

4.2 Changes in Services or Treatment Intensity

Since our outcome is calculated as an average payment per patient, our results could simply reflect increases in the intensity of treatment rather than an increase in the payment received for an otherwise identical service. Using our data on private payments, we therefore consider the extent to which hospitals respond to public penalties by changing treatment patterns or reallocating resources towards more profitable services. We first estimate the effects of Medicare payment reductions on charges among the commercial insurance population. This analysis uses within-hospital variation in charges as a general proxy for changes in intensity of treatment, with results presented in column 2 of Table 10. Here, we find no economically or statistically significant increase in charges among penalized hospitals.

We also follow Horwitz & Nichols (2009) in constructing a set of indicators for "profitable" (e.g., angioplasty or neonatal intensive care) versus "unprofitable" (e.g., alcohol dependency

[15]Our data do not have a reliable measure of mortality. We therefore focus the analysis on readmissions. We also note that our data include inpatient stays in which the patient may have died in the hospital or soon after; however, given the age composition of the commercial sample, death is likely to be less frequent than in the Medicare population.

[16]See 2017 AHRQ Statistical Brief #230 for additional details on the readmission calculations. Our sample excludes newborns and transfers, and we limit the analysis to all patients with 12 months of private insurance coverage in a calendar year.

[17]We also estimated the model using the lagged net penalty, where we again find an economically and statistically insignificant effect of penalty status on the probability of readmission.

services or hospice care) hospital services.[18] We then constructed a "profitable services index" calculated as the ratio of profitable services to all profitable and unprofitable services identified by Horwitz & Nichols (2009). For example, if the hospital offered 2 profitable services and 2 unprofitable services, then the ratio for this hospital would be 50%. Treating this profitable services index as an additional outcome and repeating our analysis from Section 3, column 3 of Table 10 demonstrates that we find small and insignificant effects of being penalized. These insignificant effects also persist across all robustness checks presented in Table 6. A similar pattern emerges in Table 10 when we consider average DRG weights and average length of stay (among our commercial insurance population) as separate outcomes, with insignificant effects of HRRP/HVBP penalties on these outcomes in all specifications considered.

Finally, it may be that penalized hospitals incurred some costly investments, perhaps with the aim of improving quality of care. While our data are limited in these areas, we also estimated the effect of hospital penalty status on the log of cost per discharge (hospital-wide).[19] Here, we again find no significant or economically meaningful effects of being penalized on hospitals' average costs per discharge.

5 Conclusion

This paper uses novel payment data from a large, multi-payer database to investigate how hospital payments from private insurers change under a large scale pay-for-performance program. We use variation in pay-for-performance incentives generated by two cost-containment policies within the ACA — the hospital readmissions reduction program and the hospital value based purchasing program — to estimate the effect of pay-for-performance penalties on average hospital payments. Our initial analysis estimates a 1.4% increase in average private insurance payments to hospitals that were penalized under the HRRP/HVBP programs. Subsequent analysis finds that this estimate is robust to a variety of alternative specifications, including differential trends among penalized and non-penalized hospitals. We also find little empirical evidence that HRRP/HVBP penalties induced hospitals to increase quality in the commercial insurance population, increase intensity of treatment, adjust service offerings toward more profitable areas, or otherwise increase overall costs per discharge.

Our results therefore suggest that hospitals were able to negotiate higher private insurance payments without any clear quality improvements among private insurance patients. While

[18] A full list of relatively profitable and relatively unprofitable services is provided in Table 2 of Horwitz & Nichols (2009). Following their analysis, we identify whether a hospital offers these services based on responses from the AHA annual surveys.

[19] We calculate costs per discharge based on data available in HCRIS.

we do not find evidence of a change in average costs per discharge (based on hospital cost reports), we also acknowledge that granular data on hospital cost structures is notoriously difficult to obtain. Indeed, the stated goal of the HRRP/HVBP was to improve hospital quality, and a large body of anecdotal evidence suggests that hospitals actively attempted to improve their performance under these programs. We suspect that such efforts are potentially valuable to private insurers and should therefore translate into higher private insurance payments; however, to the extent that these efforts involve non-monetary investments such as changes to processes in care delivery or specific administrative oversight, we likely cannot measure these investments using existing data from hospital cost reports or claims data. That said, we examine heterogeneities in the effects of HRRP/HVBP penalties and find that effects are largest among hospitals that also appear to be in a better bargaining position with commercial insurers, which is consistent with the bargaining mechanism by which hospitals might translate investments into higher private insurance payments.

Another theory that is potentially consistent with these results is that of hospital cost-shifting. This is plausible in our setting if we assume that the HRRP/HVBP did not induce any costly investments among penalized hospitals. Since the HRRP/HVBP were specifically designed as quality improvement programs, we acknowledge the likelihood that penalized hospitals would incur some additional costs (not necessarily direct monetary costs) as they attempt to improve their performance. Given these details, as well as the limited theoretical basis for cost-shifting to occur in practice, we do not claim definitive evidence of cost-shifting based on our results. Instead, we interpret our findings as an unintended consequence of the HRRP/HVBP, in which penalized hospitals were able to pass on any additional investments to private insurers in the form of higher payments.

Collectively, our analysis offers three central findings: 1) private insurance payments increased among hospitals penalized by the HRRP and HVBP; 2) effects were largest among hospitals with larger penalties and among hospitals better positioned in a bilateral negotiation with insurers; and 3) the payment increases do not appear to be explained by changes in hospital services or quality of care. To quantify this effect, note that our estimated 1.4% increase in payments implies an increase of $167 per inpatient stay based on an average private insurance payment of approximately $12,100 among penalized hospitals. As a back-of-the-envelope calculation, if one assumes that this payment increase applies to around 1,100 inpatient stays per year, then we estimate a total increase in private insurance payments of up to $183,700 per hospital per year.[20] To put this in context, penalized hospitals saw an

[20] Our price data are based on just over 550 inpatient stays per year per hospital and reflect nearly 30% of all commercial insurance claims. Extrapolating to 1,100 assumes that some but not all commercial insurers captured in our data would have experienced the same price increase as estimated in our analysis.

average penalty of around $205,000, while non-penalized hospitals received an average bonus of just over $66,000. This yields a differential payment between penalized and non-penalized hospitals of approximately $271,000. An estimated increase of $183,700 in private insurance payments therefore suggests that 68% of the cost of HRRP/HVBP penalties is passed on to private insurers in the form of higher payments.

We stress that these results should not be interpreted to suggest that pay-for-performance in health care is inherently bad. Instead, we interpret our results as highlighting the importance of how the pay-for-performance program is designed. In the case of the HRRP, hospitals need only be below average in one area in order to incur some percent penalty levied on all Medicare payments. Most hospitals are not better than average in every dimension, and indeed, as the number of conditions in the HRRP has grown, so too has the percentage of hospitals penalized in a given year. In practice, the HRRP is a relatively blunt instrument that penalizes most hospitals in a given year. Subsequently, HRRP penalties may serve as a poor quality signal. The HVBP may similarly suffer from some basic design problems. For example, in tracking a hospital's performance across 20-plus metrics, it becomes difficult to discern a true quality signal from each hospital. When applied to a highly concentrated private industry, our results suggest that such pay-for-performance programs may have important unintended consequences.

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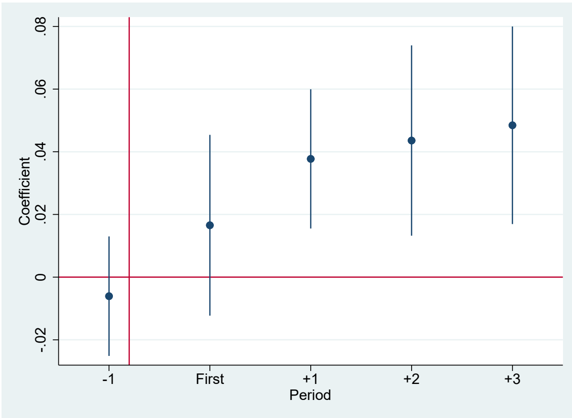
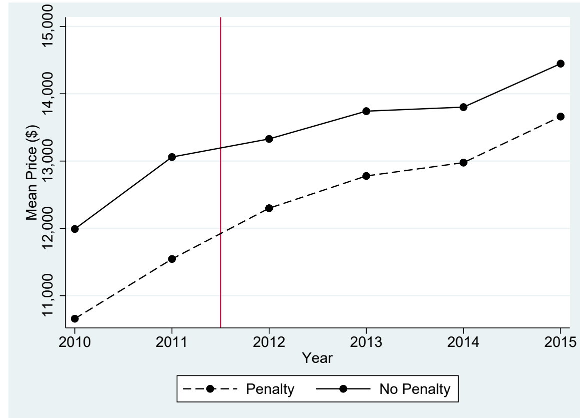
Figures

Figure 1. Evidence of Parallel or Differential Trends, 2012 and 2013

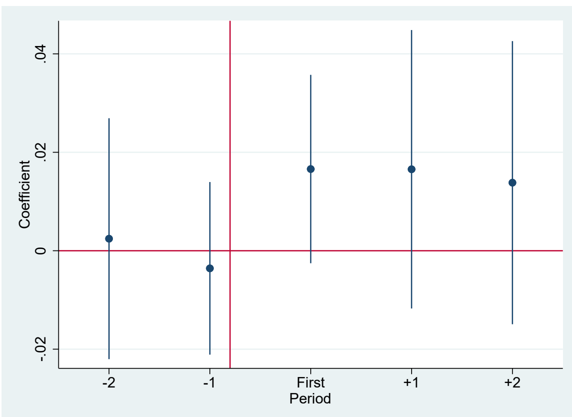
(a) Event Study: 2012 Treatment



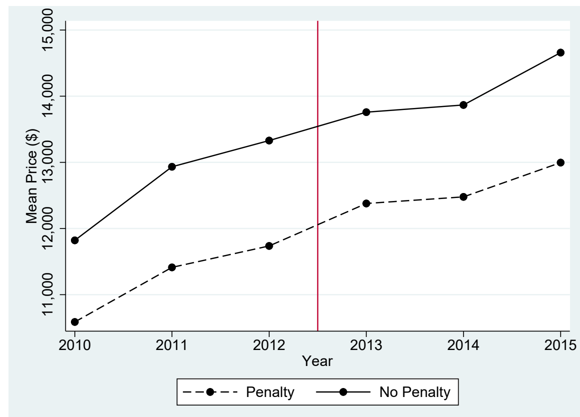
(b) Mean Residual Payments: 2012 Treatment



(c) Event Study: 2013 Treatment

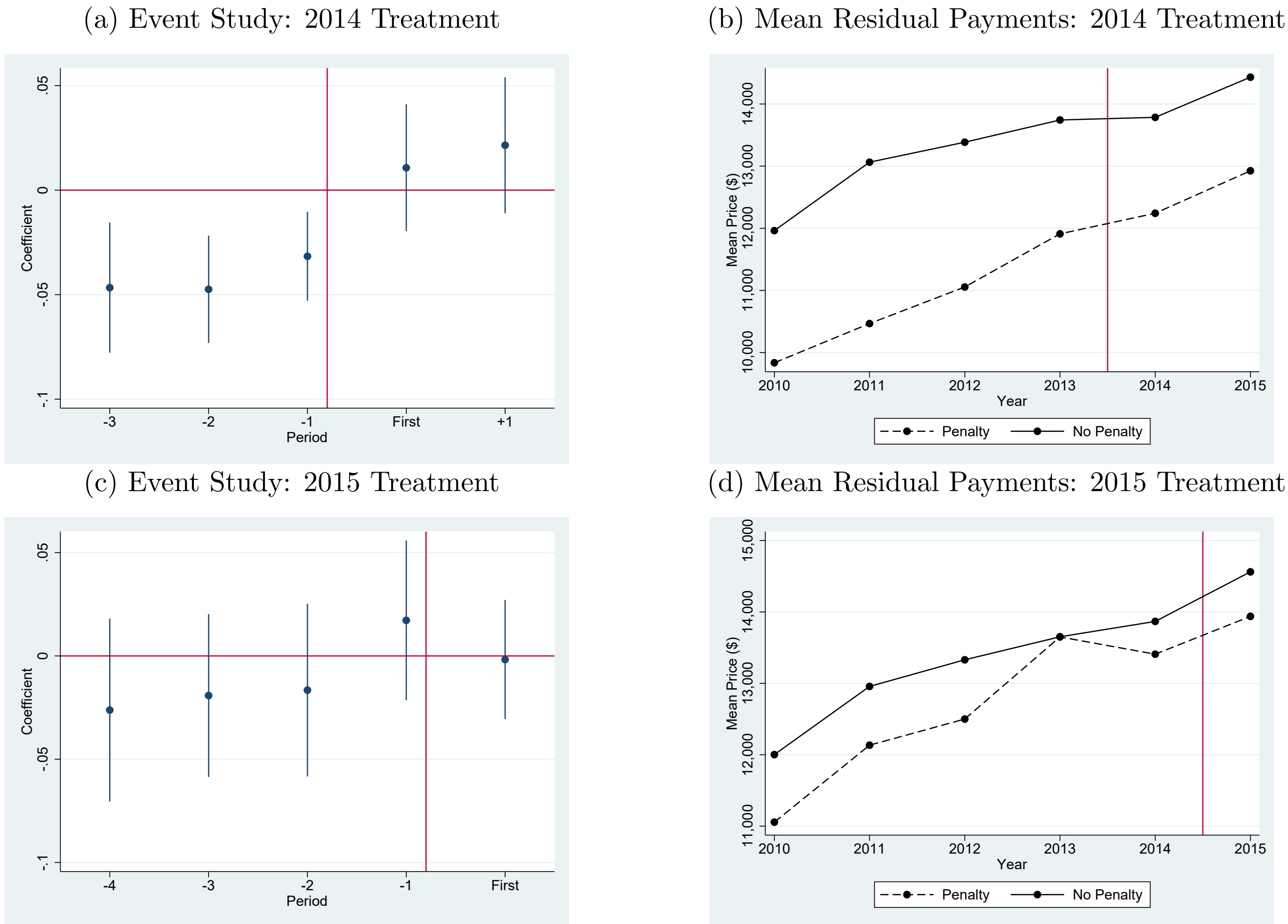


(d) Mean Residual Payments: 2013 Treatment



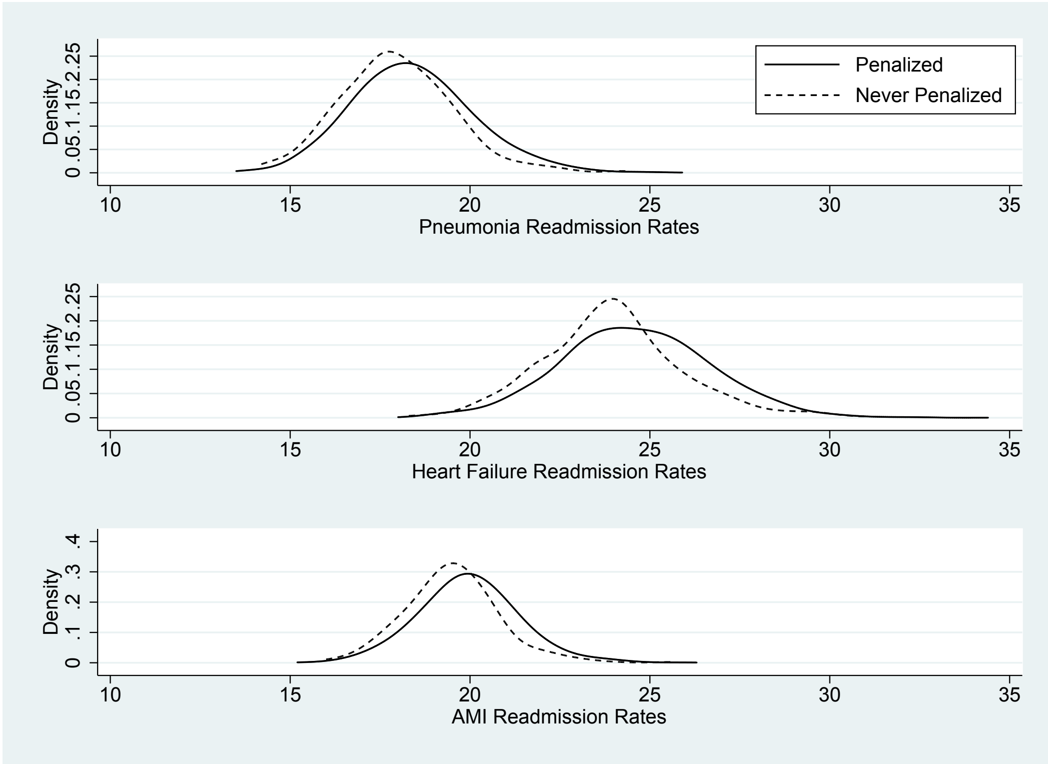
Event studies and mean residual payments as discussed in the main text. Each event study reflects estimated coefficients on the interaction between treatment and year dummies. The excluded year in all cases is the year before treatment. Mean residual prices reflect the mean residual from a regression of mean hospital payments similar to Equation 1 but excluding the net penalty variable. The residual payment in the figure is the mean residual by treatment group by year plus the mean observed payment by treatment group by year.

Figure 2. Evidence of Parallel or Differential Trends, 2014 and 2015



Event studies and mean residual payments as discussed in the main text. Each event study reflects estimated coefficients on the interaction between treatment and year dummies. The excluded year in all cases is the year before treatment. Mean residual prices reflect the mean residual from a regression of mean hospital payments similar to Equation 1 but excluding the net penalty variable. The residual payment in the figure is the mean residual by treatment group by year plus the mean observed payment by treatment group by year.

Figure 3. Pre-HRRP/HVBP Readmission Rates



Notes: Kernel density estimates for readmission rates prior to HRRP/HVBP among hospitals ultimately penalized versus those not penalized. Readmission rates reflect reported rates in 2010 and 2011, which are constructed from rates in 2006-2009 and 2007-2010, respectively.

Tables

Table 1. Characterization of Research Sample over Time

Fiscal Year	Sample Size	Payment $ Mean (St. Dev.)	Medicare Discharges	Medicaid Discharges	Other Discharges	Percent Penalized
2010	1,386	10,729.22 (4,936.50)	4,614.62	2,010.11	7,898.18	0.00
2011	1,386	11,602.74 (5,076.45)	4,618.93	1,960.05	7,892.21	0.00
2012	1,386	12,079.46 (5,477.37)	4,493.31	1,810.27	8,019.04	0.32
2013	1,386	12,668.44 (5,567.76)	4,396.32	1,783.81	7,996.10	0.74
2014	1,386	12,795.83 (5,444.21)	4,260.43	1,726.25	7,852.71	0.76
2015	1,386	13,397.63 (5,921.74)	4,311.41	1,578.86	8,261.74	0.79
Total	8,316	12,212.22 (5,481.55)	4,449.17	1,811.56	7,986.67	0.43

Notes: Balanced panel of hospitals over time between 2010 and 2015. Payment represents the mean dollar amount paid to a hospital in a year over all acute care admissions. Penalty is a binary variable for whether the combination of HRRP and HVBP resulted in a net payment reduction. Other discharges denotes all discharges other than Medicare and Medicaid.

Table 2. Hospital Characteristics by Penalties

Variable	Never Penalized	Ever Penalized	p-value
Log(Payment)	9.423	9.300	0.000
Log(Charge)	8.843	8.726	0.000
System Membership	0.768	0.784	0.352
Non-profit	0.790	0.692	0.000
Log(Case Mix Index)	0.437	0.447	0.090
Local Hospital			
Monopoly	0.133	0.113	0.110
Duopoly	0.282	0.156	0.000
Triopoly	0.139	0.108	0.012
Market Share			
Medicare	0.338	0.330	0.056
Medicaid	0.110	0.125	0.000
Medicare+Medicaid	0.447	0.455	0.086
Other	0.553	0.545	0.086
Total Pop. (1000s)	714	1,190	0.000
County Age Distribution			
[18, 34]	0.240	0.239	0.504
[35, 64]	0.393	0.393	0.947
>65	0.133	0.130	0.101
County Race Distribution			
White	0.795	0.734	0.000
Black	0.096	0.134	0.000
County Income Distribution			
< $50k	0.185	0.180	0.000
[$50k, 75k]	0.126	0.123	0.000
[$100k, 150k]	0.132	0.132	0.820
> $150k	0.095	0.101	0.007
County Education Distribution			
High School Only	0.270	0.270	0.925
Bachelor's Only	0.197	0.191	0.005

Notes: $n = 8,316$ Summary statistics are split by whether a hospital is ever observed to receive a net penalty in 2012-2015. Payment represents the mean dollar amount paid to a hospital in a year over all acute care admissions. County level characteristics are from the American Community Survey.

Table 3. Baseline Results

	Log Mean Payment	Log Mean Net Charge	Log Medicaid Discharges	Log Medicare Discharges	Log Other Discharges
Net Penalty	0.014***	0.008	-0.045**	-0.027***	-0.004
	(0.005)	(0.008)	(0.021)	(0.007)	(0.011)
Hospital Characteristics					
Monopoly	-0.008	0.004	-0.025	0.003	-0.012
	(0.012)	(0.011)	(0.055)	(0.025)	(0.029)
Duopoly	-0.005	0.010	0.036	0.030	0.013
	(0.010)	-(0.010)	(0.044)	(0.019)	(0.023)
Triopoly	0.000	0.003	-0.000	0.002	0.006
	(0.009)	(0.008)	(0.039)	(0.015)	(0.019)
Large Market	-0.041	0.001	-0.063	0.049**	0.179***
	(0.028)	(0.013)	(0.050)	(0.020)	(0.043)
Any Teaching	-0.018	-0.022	-0.047	-0.021	-0.013
	(0.012)	(0.014)	(0.039)	(0.016)	(0.022)
Major Teaching	0.003	-0.001	0.008	0.009	0.011
	(0.006)	(0.004)	(0.026)	(0.010)	(0.012)
System	0.019	-0.002	-0.091**	-0.066***	-0.083***
	(0.015)	(0.011)	(0.041)	(0.019)	(0.020)
Nonprofit	0.020	-0.009	0.073	0.036	0.016
	(0.026)	(0.016)	(0.058)	(0.028)	(0.032)
County Age Share					
[18,34]	-1.132*	-0.896*	2.902	-3.163***	-1.418
	(0.681)	(0.543)	(2.327)	(0.853)	(0.880)
[35,64]	-0.402	-1.182*	2.923	-3.428***	-0.044
	(0.910)	(0.656)	(2.781)	(1.171)	(1.295)
>64	-0.488	0.281	-1.440	0.361	-0.838
	(0.797)	(0.671)	(2.765)	(1.245)	(1.359)
County Share in Income Group					
50k-75k	-0.288	-0.034	1.518	-0.173	0.420
	(0.386)	(0.286)	(1.439)	(0.548)	(0.790)
75k-100k	-0.279	0.649*	0.281	-0.319	-0.286
	(0.479)	(0.352)	(1.736)	(0.623)	(0.791)
100k-150k	-0.736	0.290	-1.847	-0.017	0.072
	(0.457)	(0.313)	(1.533)	(0.625)	(0.776)
>150k	0.891**	-0.139	0.814	0.997*	-1.767***
	(0.402)	(0.314)	(1.375)	(0.511)	(0.671)

Notes: $n = 8,316$. All regressions include hospital and year fixed effects, and other hospital level controls include bed count, number of nurses, and number of other non-medical staff. Market power variables are constructed using the overall hospital service area. Large market is a binary variable for a hospital in the top half of the market size distribution. In cases in which independent variables are missing, we recode them and control for missing variable indicators to ensure a balanced panel. Standard errors are clustered at the hospital level. *** p-value<0.01, ** p-value<0.05, * p-value<0.1.

Table 4. Intensive Margin Results

Penalty Quartile	Mean Penalty Per Medicare Discharge [Range]	Log Mean Payment	Log Mean Net Charge	Log Medicaid Discharges	Log Medicare Discharges	Log Other Discharges
Reference Category = No Penalty or Bonus						
1	$6.00	0.004	0.01	-0.007	0.001	0.006
	[$0.01, $12.59]	(0.006)	(0.009)	(0.025)	(0.008)	(0.012)
2	$20.21	0.020***	0.007	-0.053**	-0.018**	0.005
	[$12.59, $29.08]	(0.006)	(0.009)	(0.024)	(0.008)	(0.013)
3	$41.77	0.014**	0.001	-0.061**	-0.035***	-0.006
	[$29.15, $57.06]	(0.006)	(0.011)	(0.027)	(0.009)	(0.013)
4	$94.25	0.024***	0.016	-0.085***	-0.085***	-0.036**
	[$57.10, $291.60]	(0.008)	(0.013)	(0.030)	(0.012)	(0.015)

Notes: $n = 8,316$. Results derived from breaking the size of the per Medicare discharge penalty into quartiles, with the omitted category as those hospitals receiving no penalty or a bonus. All regressions include hospital and year fixed effects, and other hospital level controls include bed count, number of nurses, and number of other non-medical staff. Market power variables are constructed using the overall hospital service area. In cases in which independent variables are missing, we recode them and control for missing variable indicators to ensure a balanced panel. Standard errors are clustered at the hospital level. *** p-value<0.01, ** p-value<0.05, * p-value<0.1.

Table 5. Alternative Specifications

	Log Mean Payment	Log Mean Net Charge	Log Medicaid Discharges	Log Medicare Discharges	Log Other Discharges
Full Sample: $n = 8,316$					
OLS	-0.061***	-0.049***	0.220***	0.094***	0.069***
	(0.015)	(0.018)	(0.045)	(0.026)	(0.022)
OLS + Hospital FE	0.014***	0.008	-0.045**	-0.027***	-0.004
	(0.005)	(0.008)	(0.021)	(0.007)	(0.011)
OLS + Hospital FE +	0.010*	0.019**	-0.038	-0.026***	-0.011
+ Ever Penalized Trends	(0.005)	(0.008)	(0.023)	(0.007)	(0.012)
Differential Trend p-value	[0.498]	[0.041]	[0.250]	[0.005]	[0.446]
OLS + Hospital FE + IV	0.037**	-0.048**	-0.087	-0.031	0.033
	(0.014)	(0.024)	(0.058)	(0.020)	(0.028)
OverID p-value	[0.899]	[0.511]	[0.875]	[0.990]	[0.924]
Restricted Sample: $n = 6,954$					
OLS	-0.073***	-0.077***	0.305***	0.150***	0.113***
	(0.021)	(0.025)	(0.062)	(0.036)	(0.030)
OLS + Hospital FE	0.018***	0.002	-0.029	-0.024***	0.001
	(0.007)	(0.011)	(0.029)	(0.009)	(0.012)
OLS + Hospital FE +	0.012	0.025**	0.001	-0.020**	-0.012
+ Ever Penalized Trends	(0.008)	(0.012)	(0.035)	(0.010)	(0.014)
Differential Trend p-value	[0.903]	[0.026]	[0.142]	[0.002]	[0.469]
OLS + Hospital FE + IV	0.029**	-0.049**	-0.095*	-0.032*	0.031
	(0.013)	(0.022)	(0.051)	(0.018)	(0.024)
OverID p-value	[0.941]	[0.414]	[0.949]	[0.877]	[0.717]

Notes: Each point estimate represents the estimated coefficient on a binary variable for whether or not a hospital received a net penalty in a given year. All regressions include year fixed effects and other hospital level controls include bed count, number of nurses, and number of other non-medical staff. Market power variables are constructed using the overall hospital service area. Large market is a binary variable for a hospital in the top half of the market size distribution. In cases in which independent variables are missing, we recode them and control for missing variable indicators to ensure a balanced panel. The restricted sample includes only hospitals that were never penalized and those first penalized prior to 2014. Differential trend p-value is from the F-test that all year dummy interactions with ever penalized are zero. OverID p-value is from the test that the interaction between a 2011 dummy and ever-penalized is zero in the second stage. Standard errors are clustered at the hospital level. *** p-value<0.01, ** p-value<0.05, * p-value<0.1.

Table 6. Robustness Checks

	Log Mean Payment	Log Mean Net Charge	Log Medicaid Discharges	Log Medicare Discharges	Log Other Discharges
1. Hospital, Year, and County Fixed Effects					
Net Penalty	0.015***	0.009	-0.048**	-0.027***	-0.003
	(0.005)	(0.008)	(0.022)	(0.007)	(0.011)
2. Controlling for Medicaid Expansion States					
Net Penalty	0.014***	0.008	-0.044**	-0.027***	-0.005
	(0.005)	(0.008)	(0.021)	(0.007)	(0.010)
3. Controlling for Overall HCAHPS Hospital Rating					
Net Penalty	0.014***	0.008	-0.045**	-0.026***	-0.003
	(0.005)	(0.008)	(0.021)	(0.007)	(0.010)
4. Controlling for Case Mix					
Net Penalty	0.014***	0.004	-0.044**	-0.026***	-0.005
	(0.005)	(0.008)	(0.021)	(0.007)	(0.011)

Notes: Further controls include those in our baseline specification for mean payments. The p-value in the first row of results is in reference to the null hypothesis that trends in the outcome of interest are the same between ever-penalized and never-penalized hospitals conditional on the model covariates. In cases in which independent variables are missing, we recode them and control for missing variable indicators to ensure a balanced panel. Standard errors are clustered at the hospital level. *** p-value<0.01, ** p-value<0.05, * p-value<0.1.

Table 7. Log Payments for Condition Specific Admissions

	Nervous System	Respiratory System	Circulatory System	Musculoskeletal System	Labor and Delivery	Neonatal
Net Penalty	0.021***	0.001	0.019**	0.004	-0.001	0.016
	(0.010)	(0.011)	(0.008)	(0.007)	(0.005)	(0.010)
n	1,410	1,758	2,754	3,060	5,226	3,204
Mean	13,762.86	12,015.13	13,071.17	12,981.58	11,308.56	8,911.19

Notes: All regressions include hospital and year fixed effects. The dependent variable is the log of average payments for each associated acute care admission. Further controls include those in our baseline specification for mean payments. In cases in which independent variables are missing, we recode them and control for missing variable indicators to ensure a balanced panel. Standard errors are clustered at the hospital level. We restrict the sample to include at least 25 admissions per hospital per year. *** p-value<0.01, ** p-value<0.05, * p-value<0.1.

Table 8. Triple Differences by Public Share

	Log Mean Payment	Log Mean Net Charge
Net Penalty	0.039***	0.043***
	(0.010)	(0.013)
* Public Share 2	-0.020*	-0.014
	(0.012)	(0.014)
* Public Share 3	-0.033**	-0.043***
	(0.013)	(0.015)
* Public Share 4	-0.044***	-0.070***
	(0.013)	(0.016)
Public Share 2	0.007	0.049***
	(0.010)	(0.013)
Public Share 3	0.016	0.087***
	(0.011)	(0.016)
Public Share 4	0.023*	0.157***
	(0.012)	(0.018)

Notes: All regressions include hospital and year fixed effects. Further controls include those in our baseline specification for mean payments. The share of a hospital's patients insured by the public sector is broken into quartiles and interacted with penalty variables. In cases in which independent variables are missing, we recode them and control for missing variable indicators to ensure a balanced panel. Standard errors are clustered at the hospital level. *** p-value<0.01, ** p-value<0.05, * p-value<0.1.

Table 9. Vertical Integration and Penalties

	Log Mean Payment	Log Mean Net Charge	Log Medicaid Discharges	Log Medicare Discharges	Log Other Discharges
Hospitals Integrated Vertically with Physician Groups Prior to 2012					
Net Penalty	0.023***	0.017***	-0.036	-0.026**	0.008
	(0.008)	(0.006)	(0.032)	(0.009)	(0.016)
Hospitals Never Observed to be Vertically Integrated with a Physician Group					
Net Penalty	0.008	0.021***	-0.063**	-0.024**	-0.005
	(0.007)	(0.012)	(0.031)	(0.010)	(0.015)

Notes: Empirical models are identical to those in Table 3. Standard errors are clustered at the hospital level. *** p-value<0.01, ** p-value<0.05, * p-value<0.1.

Table 10. Changes in Quality or Treatment Intensity

	Patient-Level Readmission	Log Charge	Profit Index	Average DRG Weight	Average LOS	Log Cost per Discharge
Net Penalty	-0.001	0.004	0.002	0.004	0.015	-0.001
	(0.001)	(0.004)	(0.001)	(0.004)	(0.012)	(0.001)
n	3,345,641	8,316	8,316	8,316	8,316	8,238

Notes: All regressions include hospital and year fixed effects, and other hospital level controls include bed count, number of nurses, and number of other non-medical staff. In cases in which independent variables are missing, we recode them and control for missing variable indicators to ensure a balanced panel. Standard errors are clustered at the hospital level. *** p-value<0.01, ** p-value<0.05, * p-value<0.1.