Randomized trial shows healthcare payment reform has equal-sized spillover effects on patients not targeted by reform

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Changes in the way health insurers pay healthcare providers may not only directly affect the insurer’s patients but may also affect patients covered by other insurers. We provide evidence of such spillovers in the context of a nationwide Medicare bundled payment reform that was implemented in some areas of the country but not in others, via random assignment. We estimate that the payment reform—which targeted traditional Medicare patients—had effects of similar magnitude on the healthcare experience of nontargeted, privately insured Medicare Advantage patients. We discuss the implications of these findings for estimates of the impact of healthcare payment reform and more generally for the design of healthcare policy.

**Significance**

In the US healthcare system, most doctors and hospitals treat patients that are covered by multiple health insurers. This raises the possibility that a change in the way one health insurer pays for medical services could, by changing provider behavior, spill over onto patients covered by other insurers. In this article, we present evidence of such spillovers in the context of a large Medicare payment reform that was implemented in some areas of the country but not in others, via random assignment. We show that the payment reform had effects of similar magnitude on both the directly targeted patients and on patients covered by other insurers.

In particular, the program, known as Comprehensive Care for Joint Replacement (CJR), was designed and implemented in 2016 by the Centers for Medicare and Medicaid Services (CMS) as a nationwide randomized trial. Randomization took place at the metropolitan statistical area (MSA) level, with 67 MSAs randomly assigned to treatment and 104 MSAs randomly assigned to control. In the treatment group, Medicare makes a single bundled payment to hospitals for all services related to the episode of care, including the initial hospital stay and subsequent care by other medical providers during the recovery period. In the control group, Medicare continues to reimburse providers under the status quo fee-for-service system in which each provider receives a separate payment based on the amount of care delivered.

In the first 2 years of the program, about 400,000 patients and 1,500 hospitals were assigned to the treatment or control group. Prior work has examined the direct impact of this bundled payment reform on targeted TM patients and estimated a ∼3% decline in TM spending, driven predominantly by reduced discharges from the hospital to postacute care facilities, particularly skilled nursing facilities (1–5).

We take advantage of the random assignment to study the spillover impacts of this payment reform on patients who are covered by private insurers through the Medicare Advantage (MA) program, which provides private insurance coverage in lieu of TM to about one-third of Medicare-eligible enrollees. Our work builds on Wilcock et al. (6) and Meyers et al. (7), who were the first to document spillovers of this program onto MA. We enrich this analysis by examining heterogeneity across hospitals and by considering implications for healthcare payment reform.

We study the direct effect on TM patients and spillover effects on MA patients using data from CMS. These data provide comprehensive claims-level information for TM patients and encounter-level information for MA patients. Although not as detailed, the

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data on MA patients still allow us to quantify spillovers along several important margins.

Our results reveal spillover effects of the same sign and magnitude as the direct effects. For nontargeted MA patients, we estimate that the payment reform reduces discharges to postacute care facilities, such as skilled nursing facilities, by a statistically significant 3.3 percentage points, or about 12%. This average spillover effect is remarkably similar to the average direct effect on TM patients. We also estimate a strong, positive correlation between the direct effects and the spillover effects at the hospital level: Hospitals with larger direct effects on the discharge destination for TM patients also have larger spillover effects on the discharge destination for MA patients. The effects—both direct and indirect—tend to be larger for hospitals with a large number of directly affected patients and for higher-quality hospitals.

Our findings have implications for the optimal design of healthcare payment policies. When health insurers separately design payment policies, they may not take into account the spillovers to other patients. As a result, the separately designed payment policies may jointly result in suboptimal incentives for healthcare providers, even when the policies are individually optimal. In other words, this is a classic case of what economists call externalities.

Our evidence of substantial spillovers onto nontargeted patients also has potential implications for the existing empirical literature on the impact of healthcare policy. Because existing research tends to focus on directly targeted patients, the costs and benefits on nontargeted patients may not be counted. In addition, if nontargeted patients are used as a “control group” for targeted patients, estimates of the direct effects may be biased. In our context, if we used nontargeted MA patients as a control group, the estimated impacts on targeted TM patients would be substantially biased toward zero.

Prior research on potential spillovers from health policies is not limited to the randomized, bundled payment reform we study. There is indirect evidence of spillovers from quasi-experimental research. For instance, Finkelstein (8) finds that the introduction of Medicare for the elderly increased the number of hospital stays for Medicare beneficiaries, even when the policies are individually optimal. In other words, this is a classic case of what economists call externalities.

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spending in 2016, representing about 5% of TM admissions and 5% of its inpatient spending. Under CJR, an episode begins with a hospital stay in a qualifying diagnosis-related group (DRG) and ends 90 days after hospital discharge. Hospitals are paid a fixed target price and are responsible for medical spending over the entire episode (except for spending that is deemed as obviously unrelated). There are measures to limit hospital risk exposure and to make sure that hospitals meet minimum quality thresholds. By contrast, under the status quo fee-for-service regime, hospitals are paid a fixed amount for the hospital stay, while the surgical procedure and postdischarge care are reimbursed separately.

We study the impact of this bundled payment reform on the healthcare of non-TM patients. There are multiple insurers in the US healthcare system. On the public side, there is both TM and Medicaid, the health insurance program for low-income individuals. On the private side, a large number of private insurers provide MA coverage to Medicare enrollees and “commercial insurance” to the nonelderly through employer-sponsored insurance (ESI) or the plans available on the health insurance exchanges set up under the Affordable Care Act. TM is large enough that changes in its payment structure may potentially spill over onto the care of other patients; for example, in 2016, TM accounted for 28% of overall hospital admissions and 40% of LEJR admissions. We focus on potential spillovers of the TM bundled payment program to privately insured MA patients. As mentioned, under MA, private insurers receive capitated payments from Medicare for providing Medicare beneficiaries with health insurance that roughly mimics commercial health insurance for the under-65 population. The idea is that because the private insurer is exposed to enrollees’ healthcare costs, it will have incentives to reduce care utilization relative to fee-for-service TM. Prior work has consistently demonstrated that MA is associated with lower healthcare utilization for patients relative to similar patients in TM (31–34). Again, as with bundled payments, the concern with MA is that because private insurers’ reimbursement does not vary with how much care their enrollees use, they may cut back on necessary care or cherry-pick patients with lower relative costs (35).

Data. The main data we use are Medicare data from the CMS. We also use private insurer data from the Health Care Cost Institute (HCCI). We analyze claims and enrollment data from four years: 2013, 2014, 2016, and 2017. We study outcomes in 2016 and 2017, which are the first 2 years of the program. We use data from 2013 and 2014, which was before the program started, in order to construct control variables. We omit data from 2015, since assignment of MSAs to treatment and control was announced in July 2015 (36), potentially causing contaminating anticipatory effects. SI Appendix, Appendix A provides details on the data and sample definition.

We use three main files from CMS: the Master Beneficiary Summary File (MBSF), the Inpatient file (IP), and the Medicare Provider Analysis and Review file (MedPAR). The MBSF provides basic demographics on all Medicare enrollees, including age, race, sex, Medicaid enrollment, and whether the individual receives Medicare through TM or MA. To measure outcomes for TM patients, we use the IP file. These data contain claims-level data on hospital inpatient services, including information on the provider, diagnoses, admission and discharge dates, and discharge destinations for each inpatient admission. These data are based on standard, widely used Medicare claims data submitted by providers for reimbursement by CMS. To measure outcomes for MA patients, we use the MedPAR file. For MA patients, these data contain information on inpatient hospital admissions. MedPAR files have been used in several previous studies of the MA population (6, 7, 37, 38). They have the advantage of covering the universe of MA patients but also have two potential limitations. First, because these claims are not submitted for reimbursement purposes, there exists some uncertainty about the reliability of these “information-only” claims (39). Second, they do not contain any information on postdischarge healthcare utilization for MA patients, or information on healthcare spending for MA patients.

To address these limitations, we also obtained private insurance data from HCCI. The HCCI data are provided by three large insurers: UnitedHealthcare, Aetna, and Humana. HCCI pools these data—masking insurer identities—and makes them available for research. The three insurers cover approximately half of MA enrollees and about a quarter of enrollees with ESI (40, 41). HCCI data have been previously used to analyze both MA claims and claims for ESI (34, 41–47). The data on claims paid to healthcare providers appear comprehensive and high-quality, with certain known exclusions (specifically, enrollees in high capitated plans and special needs plans) (34). The enrollment file includes monthly enrollment indicators and limited demographic information, including age in 10-year bins, gender, whether the enrollee is also receiving Medicaid, and indicators that allow us to identify whether an enrollee is in a MA or a commercial plan.

Experimental Design and Baseline Sample. CJR was initially designed by CMS as a 5-year, mandatory participation, randomized trial. Year 1 was defined as 1 April to 31 December 2016, and years 2 through 5 were defined as the calendar years 2017 through 2020. CMS randomized eligible MSAs into treatment (bundled payments) or control (status quo fee-for-service) by strata based on MSA population and historical LEJR spending. CMS announced assignment to treatment in the July 2015 Federal Register (36). Fig. 1 shows the final assignment. After exclusions, the program covered 67 treatment MSAs and 104 control MSAs. Within the 171 MSAs assigned to treatment or control, a small number of hospital types and episode types were further excluded from eligibility (1).

We analyze the first and second years of the CJR program, which include episodes that begin between 1 April 2016 and 15 September 2017, virtually guaranteeing that they end by 31 December 2017 (the end of the second year of the program). An episode is defined as an acute-care hospital stay that results in a discharge in one of the two included DRGs (469 and 470) and ends 90 days after discharge from the acute-care hospital. We
analyze the final set of 171 MSAs included in the CJR program. Following Finkelstein et al. (1), we impose hospital and patient eligibility criteria on LEJR admissions on TM, MA, and ESI patients to restrict our sample to LEJR episodes that would have otherwise been eligible for CJR. The criteria for MSA eligibility were determined by CMS and described in detail in prior work [see Finkelstein et al. (1) appendix, section 1]; SI Appendix, Appendix A provides more detail.

Following Finkelstein et al. (1), our primary outcome of interest is the share of CJR-eligible episodes discharged to institutional postacute care. Institutional postacute care consists of three types of care—skilled nursing facilities, inpatient rehabilitation facilities, and long-term care hospitals—of which skilled nursing facilities are by far the most common. We also analyze share discharged home with home health care services and the share discharged home without home health. Secondary outcomes—which are not available for MA patients in the CMS data—include the number of days in skilled nursing facilities during the episode, spending in skilled nursing facilities during the episode, and total episode spending. Finally, we examine the impact of bundled payment on patient volume. SI Appendix, Appendix B provides details on the construction of specific variables we analyze.

Econometric Framework. We study the impact of CJR during the first 2 years of the program. We focus on the first 2 years of the program because starting in year 3 the program was unexpectedly modified to be voluntary for some treatment MSAs, with hospitals endogenously selecting out of the program (4).

Since participation in the program was mandatory during the years we study it, and was randomly assigned at the MSA level, estimation of its impact is straightforward. Essentially, we compare average outcomes in MSAs randomly assigned to the bundled payment regime to average outcomes in MSAs randomly assigned to remain under the status quo fee-for-service payment program. This was conducted within strata, with differences in the probability of treatment assignment, we include strata fixed effects [δs] to isolate the experimental variation. In all tables, we report heterogeneity-robust SEs.

Results
Summary Statistics. Table 1 reports summary statistics for CJR-eligible patients in control MSAs in 2016 and 2017. Part A reports demographics, and part B reports healthcare utilization and spending.

We show CMS data on CJR-eligible patients in TM (column 1) and in MA (column 2). We observe about 340,000 CJR-eligible episodes in control MSAs; about 65% are in TM, with the remaining 35% in MA. TM and MA patient demographics are similar: The average age is 72, over 60% are female, about 10% are eligible for Medicaid, and the vast majority of admissions are without a major complication or comorbidity.

Discharge locations for TM and MA patients are also strikingly similar: About 30% are discharged to institutional postacute care, a little over one-third are discharged home with home health care services, and about one-third are discharged home without home health care. The similarity in discharge destinations is somewhat surprising, given that prior work has shown that MA patients are much less likely to be discharged to institutional postacute care relative to similar TM patients, for the broader population of hospital patients (34).

As a point of comparison, column 3 shows summary statistics for the ~35,000 CJR-eligible episodes covered by MA in the HCCI data. MA patient demographics for the three insurers providing MA in the HCCI data are generally similar to the demographics for the total MA patient sample (column 2); the exception is the share eligible for Medicaid is substantially lower in the HCCI data. The share discharged to different locations is also very similar for the MA patients covered by the HCCI insurers and the total MA patient sample. This suggests that the HCCI data are reasonably representative of the overall MA population.

The HCCI data in column 3 also allow us to examine length of stay and spending postdischarge and compare it to TM patients (column 1). In both populations, the vast majority of patients discharged to institutional postacute care are sent to skilled nursing facilities. Despite similar rates of discharge to skilled nursing facilities (25% of patients in both TM and MA), MA patients have shorter length of stay (5.0 days for MA patients vs. 7.3 days for TM patients) and they have substantially lower average

\[ y_{ij}(2016−2017) = \beta_0 + \beta_1 BP_{ij} + \beta_2 y_{ij}(2014) + \beta_3 y_{ij}(2013) + \delta_{ij} + \epsilon_i \]  

where \( y_{ij}(2016−2017) \) is the average per-episode outcome in MSA \( j \) during the first 2 years of the program, \( BP_{ij} \) is an indicator for the MSA being randomly assigned to bundled payments, and \( \beta_i \) is the average treatment effect of bundled payment. As in Finkelstein et al. (1), we include lagged outcomes from years 2014 and 2013 to improve statistical power. Because the randomization was conducted within strata, with differences in the probability of treatment assignment, we include strata fixed effects \([\delta_{ij}]\) to isolate the experimental variation. In all tables, we report heterogeneity-robust SEs.
spending ($2,081) for MA patients vs. $3,164 for TM patients) at skilled nursing facilities.

Finally, we use the HCCI data to examine the treatment of CJR-eligible LEJR patients in the commercial (under 65) privately insured population covered with ESI by one of the HCCI insurers. A comparison of columns 3 and 4 indicates that ESI patients are naturally much younger (ESI patients in our sample are under 65 by definition, while 90% of MA patients are 65 and over). Interestingly, despite being younger, average episode spending on ESI patients is about twice as large than on MA patients ($40,000 compared to $20,000), presumably reflecting much higher rates paid by insurers relative to Medicare (41).

ESI patients are also associated with a very different distribution over postdischarge destinations relative to MA or TM patients. In particular, institutional postacute care use is rare among ESI patients; only 2% are discharged to institutional postacute care compared to about 30% of patients in TM and in MA. The low usage of institutional postacute care among ESI...

### Table 1. Summary statistics (control MSAs 2016–2017)

<table>
<thead>
<tr>
<th></th>
<th>CMS data</th>
<th>HCCI data</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>TM (1)</td>
<td>MA (2)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>ESI (4)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>MA (3)</td>
</tr>
<tr>
<td>A: Demographics and diagnosis characteristics</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Average age</td>
<td>72.3 (0.83)</td>
<td>72 (1.61)</td>
</tr>
<tr>
<td>Share 65 and older</td>
<td>0.89 (0.03)</td>
<td>0.87 (0.07)</td>
</tr>
<tr>
<td>Share female</td>
<td>0.64 (0.03)</td>
<td>0.64 (0.04)</td>
</tr>
<tr>
<td>Share eligible for Medicaid</td>
<td>0.1 (0.05)</td>
<td>0.12 (0.08)</td>
</tr>
<tr>
<td>Share discharged to skilled nursing facilities</td>
<td></td>
<td></td>
</tr>
<tr>
<td>No. of days in skilled nursing facilities</td>
<td>7.3 (2.4)</td>
<td>5.0 (2.1)</td>
</tr>
<tr>
<td>Episode spending in skilled nursing facilities</td>
<td>3,164 (1,190)</td>
<td>2,081 (938)</td>
</tr>
<tr>
<td>Total episode spending</td>
<td>22,662 (3,544)</td>
<td>20,765 (2,999)</td>
</tr>
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</table>

Table reports mean and SD for the summary statistics of CJR-eligible LEJR patients at the MSA level for the control MSAs. All outcomes are measured during the episode. All measures are based on the 2016–2017 Medicare and HCCI claims data. Episodes admitted between 1 April 2016 and 15 September 2017 are included. Number of MSAs count the number of MSAs with any CJR-eligible LEJR episode, among the final set of 104 control MSAs.

*Age is only available in 10-year bins in HCCI data: 45 to 54, 55 to 64, and so on.
†Dual eligibility indicator is not available in ESI data.
§Institutional postacute care includes skilled nursing facilities, long-term-care hospitals, and inpatient rehabilitation facilities.
|                  |          |          |
|                  |          |          |

### Table 2. Impact of CJR on TM and MA patients

<table>
<thead>
<tr>
<th>Outcome</th>
<th>Control mean</th>
<th>Treatment effect</th>
<th>95% CI</th>
<th>P value</th>
<th>Control mean</th>
<th>Treatment effect</th>
<th>95% CI</th>
<th>P value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Share discharged to</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Institutional postacute care</td>
<td>0.313</td>
<td>-0.034</td>
<td>[-0.051, -0.017]</td>
<td>0.0001</td>
<td>0.283</td>
<td>-0.033</td>
<td>[-0.050, -0.015]</td>
<td>0.0003</td>
</tr>
<tr>
<td>Home (with home health care)</td>
<td>0.339</td>
<td>0.004</td>
<td>[-0.031, 0.039]</td>
<td>0.81</td>
<td>0.365</td>
<td>0.004</td>
<td>[-0.033, 0.040]</td>
<td>0.843</td>
</tr>
<tr>
<td>Home (without home health care)</td>
<td>0.329</td>
<td>0.042</td>
<td>[0.007, 0.077]</td>
<td>0.02</td>
<td>0.336</td>
<td>0.042</td>
<td>[0.004, 0.080]</td>
<td>0.030</td>
</tr>
<tr>
<td>Other destinations</td>
<td>0.020</td>
<td>-0.004</td>
<td>[-0.008, -0.0001]</td>
<td>0.05</td>
<td>0.016</td>
<td>-0.003</td>
<td>[-0.007, 0.001]</td>
<td>0.184</td>
</tr>
<tr>
<td>No. of CJR episodes</td>
<td>2,133</td>
<td>-10</td>
<td>[-155, 135]</td>
<td>0.89</td>
<td>1,163</td>
<td>-5</td>
<td>[-125, 116]</td>
<td>0.937</td>
</tr>
<tr>
<td>No. of treatment/</td>
<td>67/104/171</td>
<td></td>
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<tr>
<td>control/all MSAs</td>
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Table reports results from estimating Eq. 1 using the CMS data. Specifically, it reports MSA-level estimates from a regression of the outcome on an indicator for CJR, controlling for strata fixed effects, and two lags of the outcome variable. CI and P value are based on heteroskedasticity robust SEs. In CMS TM and CMS MA columns, the outcomes are measured using all LEJR admissions between 1 April 2016 and 15 September 2017 that would have qualified for CJR, among TM and MA enrollees, respectively. See notes to Table 1 for variable definitions.
patients limits the scope and magnitude of any potential spillover to ESI patients, since decreased rates of discharge to institutional postacute care is the main channel by which the payment reform affects the treatment of TM patients (1–3, 5). We therefore do not explore spillovers to the ESI population.

**Average Spillover Effects.** Table 2 shows the estimated impact of the payment reform on discharge location for CJR-eligible patients. The left-hand side shows the direct effect on targeted TM patients; this has already been extensively studied using these same data (1–3, 5). Consistent with these prior studies, we find that the payment reform decreases the probability a TM patient is discharged to institutional postacute care. Specifically, we estimate a 3.4 percentage point (95% confidential interval −5.1 to −1.7 percentage points) decline in the probability a TM patient is discharged to institutional postacute care, or about 10% relative to the control mean. As in prior work, there is no effect on the probability of being discharged home with home health care and an 4.2 percentage point increase in discharges home (without home health), which is of similar magnitude to the decline in discharges to institutional postacute care. Also consistent with prior work, there is no evidence of an impact of CJR on the volume of CJR-eligible episodes.

The right-hand side shows the spillover effects of the payment reform on the discharge locations of CJR-eligible patients covered by MA. The spillover effects are strikingly similar to the direct effects. The payment reform for TM patients reduces the share of MA patients discharged to institutional postacute care by 3.3 percentage points (95% confidence interval −5.0 to −1.5 percentage points). This is the same sign and similar magnitude (in both levels and proportions) to the direct effect on TM patients; Wilcock et al. (6) and Meyers et al. (7) previously documented comparable findings. As a confirmatory check on our spillover estimates using the CMS data, we also estimated the spillover effects on MA patients in the HCCI data. We found that the results were broadly similar, but less precisely estimated. Further analysis suggested the imprecision reflected the smaller sample size, rather than differential spillovers for the subset of insurers represented in the HCCI data; SI Appendix, Appendix C describes these analyses in more detail.

**Heterogeneity across Hospitals.** We examine heterogeneity across hospitals in both direct and spillover effects. These analyses inform our discussion of underlying mechanisms. We focus this analysis on the primary outcome and key margin of response: the probability a patient is discharged to institutional postacute care.

To estimate hospital-specific treatment effects, we follow Einav et al. (4) and use the following regression:

$$
Y_{ih}(2016−2017) = \beta_0 + \sum_h \beta_{1h} BP_h + \beta_2 Y_{ih,2014} + \beta_3 y_{ih,2013} + \delta_{ih} + \epsilon_{ih},
$$

where $h$ denotes a hospital, $y_{ih}(2016−2017)$ denotes the share of patients in 2016 and 2017 that were discharged to institutional postacute care, $BP_h$ is an indicator for the hospital being randomly assigned to bundled payments, and $\beta_{1h}$ is the hospital-specific treatment effect. Similar to our baseline specification, we include (hospital-level) lagged outcomes as covariates to improve statistical power. As before, we include strata fixed effects because randomization was conducted within strata. In the regressions, we weight hospital-level observations by the number of CJR-eligible episodes to reduce noise from hospitals with very few episodes.

We estimate Eq. 2 separately on the TM patients and on the MA patients, producing hospital-specific estimates of the impact of the payment reform on the share of TM patients discharged to institutional postacute care and the share of MA patients discharged to institutional postacute care. The estimates vary quite a bit across hospitals, presumably reflecting both true underlying heterogeneity across hospitals in the treatment effect as well as nontrivial sampling variation, especially in the context of some of the smaller hospitals.

More importantly, we estimate a positive and large correlation of 0.60 (SE 0.026) between the TM and MA hospital-specific estimates (SI Appendix, Table S2). Given the noise in the TM and MA estimates, we would not expect the correlations to be close to one, so we view a correlation estimate of 0.60 as indicating a fairly strong positive relationship between the TM and MA treatment effects at the hospital level. This is not surprising. As we discuss in more detail in the next section, any of the mechanisms that might produce same-signed spillover effects would predict that hospital-level estimates of the direct effects and indirect effects should be positively correlated.

To interpret the $\beta_{1h}$ from Eq. 2 as heterogeneous causal effects of the payment reform, the (admittedly strong) identifying assumption is that, conditional on the covariates, there are no hospital-specific time trends, and thus any heterogeneity in the change in outcomes across hospitals reflects heterogeneous treatment effects. To probe the sensitivity to this assumption, we estimate an alternative specification where we include hospital-specific linear time trends as controls. These time trends are identified from the hospital-specific outcomes in 2013 and 2014. In this specification, the identifying assumption is that, conditional on covariates, there are no hospital-specific deviations from the trend line. As a result, the correlation between the estimates is reduced to 0.17 (SE 0.039) but still positive and statistically significant (SI Appendix, Table S2).

We next examine how hospital-specific direct and spillover effects vary by specific hospital characteristics. This analysis is necessarily more speculative than the result on the existence of spillovers, since the hospital characteristics we examine are not necessarily more speculative than the result on the existence of spillovers. Nonetheless, it provides interesting insights on the characteristics of hospitals associated with greater direct and spillover effects.

Table 3 reports the average hospital-specific treatment effects by hospital characteristics. Fig. 2 then visually illustrates how closely related the TM effect is to the MA effect by plotting each one of the effects reported in Table 3 for MA against its analogous estimate for TM. Perhaps unsurprisingly, given the previous results, hospital-level treatment effects and spillover effects often point in the same direction within the binary groups defined by hospital characteristics.

In particular, both the direct and spillover effects of bundled payment reform are higher for hospitals with an above-median volume of TM CJR-eligible episodes compared to hospitals with a below-median number of TM CJR-eligible episodes. Indeed, the hospitals with above-median volume of TM patients seem to account for the entirety of the direct effect as well as the entirety of the spillover effect. Concentration of the direct effect and the

It can be either because the patients who would have been sent to institutional postacute care are being sent home without home health care or because there is a cascading effect where the patients who would have been sent to institutional postacute care are being sent home with home health, and patients who would have been sent home with home health are now being sent home without home health supports. A cascading effect seems (to us) more likely, but we cannot differentiate between these two channels.

For TM patients, we estimate that bundled payment reduces the share of patients discharged to institutional postacute care by 0.03 percentage points on average for hospitals with above-median TM CJR volume, compared to a 0.05 percentage point increase for hospitals with below-median volume; these estimates of statistically distinguishable ($P < 0.001$). For MA patients, the impact of bundled payments is, on average, a −0.019 percentage point decline for hospitals with above-median TM CJR volume, compared to a 0.019 percentage point increase for hospitals with below-median volume; again, these estimates are statistically distinguishable ($P < 0.001$).
Table 3. Heterogeneity in impact by hospital characteristics

<table>
<thead>
<tr>
<th>Hospital characteristics</th>
<th>TM</th>
<th>MA</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>Above-median TM CJR volume</td>
<td>−0.030</td>
<td>0.053</td>
</tr>
<tr>
<td></td>
<td>(0.093)</td>
<td>(0.160)</td>
</tr>
<tr>
<td>Above-median TM CJR share</td>
<td>−0.021</td>
<td>−0.027</td>
</tr>
<tr>
<td></td>
<td>(0.100)</td>
<td>(0.111)</td>
</tr>
<tr>
<td>Nonprofit</td>
<td>−0.021</td>
<td>−0.025</td>
</tr>
<tr>
<td></td>
<td>(0.101)</td>
<td>(0.108)</td>
</tr>
<tr>
<td>Teaching</td>
<td>−0.044</td>
<td>−0.017</td>
</tr>
<tr>
<td></td>
<td>(0.103)</td>
<td>(0.103)</td>
</tr>
<tr>
<td>Above-median no. of beds</td>
<td>−0.017</td>
<td>−0.035</td>
</tr>
<tr>
<td></td>
<td>(0.101)</td>
<td>(0.109)</td>
</tr>
<tr>
<td>Above-median quality score</td>
<td>−0.039</td>
<td>0.010</td>
</tr>
<tr>
<td></td>
<td>(0.094)</td>
<td>(0.114)</td>
</tr>
<tr>
<td>Geographical regions</td>
<td></td>
<td></td>
</tr>
<tr>
<td>South</td>
<td>−0.037</td>
<td>−0.014</td>
</tr>
<tr>
<td></td>
<td>(0.106)</td>
<td>(0.101)</td>
</tr>
<tr>
<td>Northeast</td>
<td>−0.017</td>
<td>−0.024</td>
</tr>
<tr>
<td></td>
<td>(0.124)</td>
<td>(0.099)</td>
</tr>
<tr>
<td>West</td>
<td>−0.017</td>
<td>−0.024</td>
</tr>
<tr>
<td></td>
<td>(0.089)</td>
<td>(0.107)</td>
</tr>
<tr>
<td>Midwest</td>
<td>−0.010</td>
<td>−0.027</td>
</tr>
<tr>
<td></td>
<td>(0.095)</td>
<td>(0.106)</td>
</tr>
</tbody>
</table>

The outcome is share discharged to institutional postacute care in all panels. Table is based on the hospital-specific treatment effects from estimating Eq. 2 using the CMS data. It reports these hospital-specific treatment effects by hospital characteristics for TM and MA, respectively. For each hospital characteristic in each row, the table reports the mean and SD of the hospital-specific treatment effects, separately for hospitals that satisfy the given characteristic (Yes) and hospitals that do not satisfy the given characteristic (No). The P value from a two-samples t test is reported. All regressions and summary statistics are weighted by the number of CJR (TM or MA) episodes in hospital.

**Quality is measured by a composite quality score (CQS), which is the official quality measure that determines bonus payments in CJR. The measure ranges from 0 to 20 points, and hospitals must score at least 5 points to be eligible for bonus payments. Up to 10 points are given based on a hospital’s quality performance percentile on a complication measure for total hip arthroplasty and total knee arthroplasty; up to 8 points are given based on a standardized national patient experience survey; up to 2 points are given for submitting the patient reported outcomes and risk variable data. Finally, up to 1.8 points can be added to the final score for improvement in either of the first two measures relative to the previous performance year, as long as the final score does not exceed 20. See "https://innovation.cms.gov/Files/cjr-qualup.pdf" for details on this measure.**

Understanding the economic forces driving the spillovers we detected is a useful first step in thinking about how it may generalize to other settings.

A priori, spillovers (if they exist) may be of the same sign or opposite sign from the direct effects. Same-signed spillovers could arise if changes in healthcare delivery arise from a “high fixed cost, low marginal cost” underlying mechanism. For example, in our setting, if hospitals build a computer model that

![Fig. 2. Correlation between estimated effects for MA and TM patients.](image-url)
uses available data to guide discharge decisions for targeted TM patients, this guidance could be extended to nontargeted MA patients with little additional cost. Same-signed spillovers could also arise if providers are constrained to provide similar care to patients irrespective of insurance plan; these constraints could reflect operational constraints (e.g., providers do not know a patients insurance type) or provider preferences (e.g., doctors might consider it unethical to treat patients differentially).

Opposite-signed spillovers could arise if the cost of delivering a particular type of care is increasing in the number of patients treated, what economists call an upward sloping supply curve for care delivery. For example, in our setting, if the supply of beds at institutional postacute care facilities is an important constraint on discharges, a decrease in discharges of TM patients might be offset by an increase in discharges of MA patients. Similarly, if hospitals have a fixed number of personnel who can help patients transition to their homes, then an increase in the share of TM patients discharged home, which would occupy these resources, might be offset by fewer discharges home of MA patients. While the same-signed estimates we find do not allow us to completely rule out the existence of such effects, they indicate that such effects are not large enough to offset factors that push in the other direction.

Several pieces of evidence suggest that a fixed-cost model of changing healthcare provision is a key driver of spillovers in our setting. Such a model would predict the same-signed and similar-magnitude spillovers that we find. It would also predict our finding that both the direct and spillover effects are concentrated in hospitals with a larger number of directly affected patients. This type of fixed-cost explanation is also supported by qualitative survey evidence of hospital administrators on how hospitals in the treatment group responded to the payment reform (49); the administrators report reducing discharges to institutional postacute care by using risk stratification and by forming networks of preferred skilled nursing facilities to influence quality and costs, conditional on discharge. Of course, the evidence supporting a fixed-cost model does not rule out a role for constraints on treating similar patients with different insurance differently, but we are unable to examine this directly. More broadly, given the variety of different potential mechanisms that may be at play, spillovers may well differ across contexts.

Our findings have several general implications. First, they suggest that the large empirical literature on the impacts of public healthcare policies may miss an important set of costs and benefits of these policies, since the literature tends to focus almost exclusively on the direct impacts on those covered by the policies. This includes estimates of the impact of public health insurance coverage [see Finkelstein et al. (50) for a recent review] and the impact of public health insurance payment policies (22, 51–54). Second, the finding of substantial spillovers suggests that in many empirical settings, estimates of the direct effects of health insurance policies may be biased since control” patients not directly affected by the policy may be indirectly affected. Examples of the type of work subject to these concerns are surveyed in Finkelstein et al. (50).

Third, our results raise important questions about the optimal design of healthcare payment policies. When there are multiple insurance providers, spillovers imply that single insurers may have suboptimal incentives to change provider behavior. Prior theoretical work has explored how common agency problems—such as free-riding, coordination failures, or capacity constraints—can lead to inefficient payment policies (55–57). A key assumption underlying these models is that the health insurance payments for one group of patients may affect provider behavior for other patients. Our findings provide empirical support for this assumption.

Most broadly, our findings suggest that researchers and policymakers should take a broader perspective when considering the design of healthcare policy. Even if the policy objective is narrowly focused on TM patients, the fact the providers change their treatment of nontargeted patients suggests that these nontargeted patients need to be accounted for in analyses of a policy’s impact or optimal design. How exactly healthcare payment design should be affected depends on the economic model underlying the spillovers. Our findings highlight the need for further work on this topic.

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