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Alice J. Chen, Michael R. Richards, Christopher M. Whaley, Xiaoxi Zhao

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THE EXTENT OF EXTERNALITIES FROM MEDICARE PAYMENT POLICY

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ABSTRACT

Medicare, accounts for roughly 20% of medical expenditures in the United States and is the dominant payer for many treatments. Consequently, Medicare payment policy may have diffuse consequences. Using a contemporary bundled payment reform (the "CJR" program) and a difference-in-differences research design, we estimate spillovers from Medicare payment reforms to non-Medicare populations. We find that altered treatment decisions for targeted joint replacement procedures are closely, though not perfectly, mirrored between traditional Medicare, Medicare Advantage, and the non-elderly commercially insured populations. Results for untargeted procedures performed by CJR affected providers also show suggestive evidence consistent with a spillover effect. Our collage of findings aligns with the "norms hypothesis" for provider decision-making; however, providers do not rigidly apply changes to treatment choices. Instead, key decision nodes appear to gain greater salience under Medicare's new incentive structure, which leads to revised treatment choices for different payer-procedure combinations. Ignoring the breadth of externalities from Medicare policies risks understating their social welfare impact.

JEL Classification: H44, I11, I13, I18

Keywords: bundled payment, episode-based payment, Comprehensive Care for Joint Replacement, value-based purchasing, Affordable Care Act

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Chen: University of Southern California, Los Angeles CA. Richards: Baylor University, Waco TX. Whaley: RAND Corporation, Santa Monica CA. Zhao: Boston University, Boston MA.

*Corresponding author contact info: Michael Richards, Department of Economics, Baylor University. One Bear Place, Waco TX 76798. Office: (254)710-4861. Email: Michael_Richards@baylor.edu

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I. Introduction

Externalities represent fundamental concepts of economic activity but quantifying them is often absent or incomplete when evaluating many policies. Moreover, ignoring policy externalities can be particularly consequential within mixed markets where public and private entities are purchasing identical goods and services from the same suppliers. Mixed market structures are home to many legislative and regulatory interventions and can be found across a variety of economic sectors. Depending on how firms behave within a mixed market, policymaking that applies to just one segment of the market (e.g., the public side) could affect the flows of goods and services to the other segment of the market (e.g., the private side). Any indirect changes to firm behavior may generate desirable as well as suboptimal outcomes from a social welfare perspective. This suggests that mixed markets are a potential source of important externalities, and the scope and magnitude of such externalities is likely to grow with the size—and hence influence—of the relevant public market as well as the extent to which firms overlap across markets.

A prominent example of a mixed market, with regular policy activity and considerable overlap among firms, is health care. Multiple public and private insurers often act as intermediaries for patients and establish their own bespoke incentive schemes for local health care providers (Relman and Reinhardt 1986; Hughes Tuohy, Flood, and Stabile 2004). More specifically, Medicare, the public insurance program for the elderly and disabled in the US, is responsible for more than one out of every five medical expenditure dollars[1] and also directly or indirectly covers nearly 60 million individuals.[2] The program's size and prominence suggests that Medicare policy

[1] These and related annual national spending statistics are provided and regularly updated by the Centers for Medicare & Medicaid Services and can be found here: https://www.cms.gov/Research-Statistics-Data-and-Systems/Statistics-Trends-and-Reports/NationalHealthExpendData/NHE-Fact-Sheet.

[2] Medicare enrollment counts for traditional Medicare and Medicare Advantage are available from the Kaiser Family Foundation. Note, there are roughly 40 million beneficiaries in traditional (fee-for-service) Medicare and another 20 million Medicare-eligible individuals that have opted for the privatized Medicare Advantage route. These statistics can be found here: https://www.kff.org/medicare/state-indicator/total-medicare-beneficiaries.

could wield significant influence over private actors within US health care markets—especially since nearly all hospitals and related health care providers will have a blend of Medicare and non-Medicare business.

Accompanying research demonstrates that commercial (i.e., private) health insurers partially anchor their respective provider service fees to Medicare's administered price schedule (White 2013; Clemens and Gottlieb 2017; Clemens, Gottlieb, and Molnár 2017; Trish et al. 2017; Cooper et al. 2019), and similarly, changes in Medicare reimbursement levels seem to alter providers' effort devoted to the non-Medicare, private market (Sloan, Morrisey, and Valvona 1988; Yip 1998; He and Mellor 2012; White 2014). Recent work also finds that Medicare regulations can lead to changes in provider decisions for non-Medicare patients (Geruso and Richards 2020; Richards, Seward, and Whaley 2020). Yet, the conduct and potentially broad impact of the Medicare program goes beyond price and rule setting. For instance, Medicare is often the source of payment model experimentation for health care providers—a role that has accelerated since the passing of the Affordable Care Act (ACA) in 2010 (Abrams et al. 2015).

For both Medicare and commercial (i.e., private) insurers, fee-for-service (FFS) payments for providers are the longstanding norm and are widely believed to contribute to inefficiencies and excessive spending via their piece-rate incentives—see Burns and Pauly (2018) for a recent commentary. Moving to alternative incentive structures to make providers more cost-conscious has proven difficult—likely due, in part, to the small influence of any individual insurer as well as the lack of contracting coordination across many insurers within the multi-payer landscape (Frandsen, Powell, and Rebitzer 2019). To overcome these inherent challenges, the traditional (FFS) Medicare program has at various times unilaterally introduced new approaches to provider reimbursements (e.g., prospective payment systems and payment innovation demonstration models). Any direct effects from these Medicare policy changes in terms of curbing beneficiary spending and/or improving health outcomes are of first order importance for the Medicare program and generally the focus of subsequent evaluations. But indirect effects (i.e., spillovers onto other payers and/or treatments) could be comparable in magnitude and consequently reveal more diffuse social welfare implications for a given payment reform. To date, there is considerable evidence related to the former (i.e. direct effects) but scarce evidence for the latter (i.e., indirect effects) when it comes to Medicare reimbursement policy.

In this paper, we focus our attention on Medicare's Comprehensive Care for Joint Replacement ("CJR") Model that was announced in mid-2015 and began in 2016. The CJR program is devoted to lower extremity joint replacements (LEJRs), i.e., hip and knee arthroplasty for beneficiaries enrolled in traditional (i.e., publicly administered) Medicare insurance. While the scope of the payment reform is narrow, LEJRs are the most common inpatient surgery for Medicare patients, with over 400,000 procedures and more than $7 billion in spending in 2014 alone. Moreover, the total cost to Medicare for an LEJR procedure and associated recovery care demonstrates wide variation—e.g., as much as double the average cost between the highest and lowest spending geographies, despite the public insurer setting its own national price schedule.[3] For these reasons, opportunities for greater care standardization to shrink the LEJR cost variance appear plentiful.

The CJR program (discussed in detail in Section II) relies on a version of bundled or "episode-based" reimbursement, where providers are held accountable for the costs of the entire episode of care (i.e., surgery and recovery periods). This contrasts to the FFS model where each increment of care would be reimbursed and providers face no downside risk from choosing more

[3] These background details as well as program details are provided by the Centers for Medicare & Medicaid Services (CMS) and are available here: https://innovation.cms.gov/innovation-models/cjr.

expensive treatment options. Importantly, provider participation in the CJR program was not voluntary. Instead, a geographic randomization procedure sharply defined implementation areas, and all providers within designated areas were required to participate in the payment model. This procedure removes selection effects from non-random participation—an issue that has challenged evaluations and scale-ups for many other contemporary payment reforms.

With these programmatic features, we leverage data on the universe of LEJR procedures in Florida from the beginning of 2013 through the end of 2018 and a difference-in-differences (DD) and event time estimation framework to identify the spillover effects of a targeted Medicare policy onto other payers and services. First, we demonstrate that first-order CJR policy effects for Medicare LEJR cases in Florida align with the existing literature. Second, we determine if the observed changes in treatment decisions extend to Medicare Advantage and non-Medicare commercially insured patients—consistent with a cross-payer spillover for the same procedure. We then go further to exploit the richness and completeness of our data to execute identical analyses on non-LEJR procedures performed by LEJR surgeons. Doing so allows us to assess if any policy-induced adjustments to provider clinical decision-making extend beyond the policy's remit. Importantly, our policy context and analytic environment offer a unique opportunity to test the presence and reach of the "norms hypothesis" for physician behavior. This model generally states that tangible and/or cognitive costs limit the amount of treatment customization providers will engage in for otherwise similar patients (Newhouse and Marquis 1978; Glied and Graff Zivin 2002; Frank and Zeckhauser 2007; Landon 2017). Well-identified empirical evidence for the presence or absence of such provider behavior is currently lacking but also needed to fully design and interpret welfare assessments of provider payment policies.

At this time, we are aware of three very recent studies of the effect of the CJR program on LEJR cases for Medicare Advantage enrollees—Meyers et al. (2019); Wilcock et al. (2020); Einav et al. (2020a)—which we discuss in detail within Section II. Our paper is distinct from these existing works in several respects. First, we have a complete universe of encounters for all payers within our health care markets of interest (i.e., not just Medicare FFS and a subset of Medicare Advantage). Second, we benefit from a longer time series (2013-2018) and deliberate estimation approach that transparently demonstrates the market dynamics as well as persistency in providers' policy responses over time. Finally, and as previously noted, we broaden our analyses to non-LEJR procedures to understand if, and to what degree, targeted payment policies influence provider treatment choices that are not subject to the incentive scheme. This collection of empirical attributes from our study is unique to the CJR literature and more comprehensively quantifies the reach of Medicare payment policy.

Overall, our findings reveal that provider behavior towards Medicare Advantage and commercially insured LEJR patients is meaningfully altered following the payment reform for traditional FFS Medicare patients. Specifically, we observe pronounced changes in post-acute care treatment decisions across all three payer populations. Use of inpatient rehabilitation facilities (IRFs), which in total cost the Medicare program alone approximately $8 billion per year (MedPAC 2018), declines following the LEJR hospital stay for Medicare patients once the CJR program is announced (i.e., pre-implementation) and grows in magnitude over time. Medicare Advantage LEJR patients likewise demonstrate a sharp and persistent drop in IRF use once the CJR program is implemented. The lower reliance on IRF post-acute care services is also sustained over the three years of post-bundled payment implementation for both payers, with estimated payment reform effect sizes reflecting a 20–40% decrease in IRF use for traditional Medicare

LEJR cases and a 50–60% decrease among Medicare Advantage cases when compared to their pre-CJR levels. The non-elderly, commercially insured LEJR patients do not show an immediate spillover effect, but instead, begin to receive less IRF care after the CJR program has been in place for one year. The relative policy effect size is roughly 25-38% compared to the pre-CJR IRF utilization rate for this patient population. Interestingly, while the use of home health post-acute services only suggestively climbs for traditional Medicare LEJR patients, both Medicare Advantage and commercially insured LEJR patients show marked increases in home health service utilization that peak by 2018 and reveal 29% and 16% relative changes for the Medicare Advantage and commercially insured populations, respectively. For the non-elderly commercially insured group, specifically, much of the substitution toward home health is largely at the expense of discharging these patients with no post-acute care services following hospitalization (i.e., the policy spillover is increasing treatment intensity for this group). When examining comparable non-LEJR procedures, we additionally find some evidence of less IRF use and more home health services among those exposed to the CJR program—suggestive of a secondary spillover effect for treatment decisions belonging to untargeted procedures.

Our documented and policy-induced changes in provider behavior reveal statistically significant and economically meaningful externalities from the Medicare CJR program that have not been captured in previous studies. The effects we document are more comprehensive, consistent, and larger than effects reported in the existing literature, perhaps due to our all-payer universe of patient encounters for each hospital, as well as our study setting that includes some of the highest Medicare spending areas in the country—i.e., where legislated payment reforms may have their greatest bite. The strongest and most important effects localize to post-acute care services, as opposed to within-hospital management (e.g., length of stay), and our estimates offer empirical support for the long-debated "norms hypothesis" of provider behavior. That said, providers do not seem to rigidly apply their treatment approaches to all procedure-payer combinations, and their post-reform changes in behavior evolve with time. Our evidence is therefore more consistent with specific decision nodes in the treatment pathway (e.g., post-surgical discharge planning) gaining new and greater salience with the introduction of new Medicare financial incentives. Moreover, the externality effect dynamics we observe may be indicative of learning about the cost-quality tradeoffs from altering their treatment approaches for different patients, which consequently amplifies shifts in clinical decision-making over time.

II. Background

Improvements to provider payment structures to achieve better patient outcomes and better align providers' incentives with those of patients, payers, and society have long been sought. While the approaches and nomenclature have evolved over time (e.g., pay-for-performance, value-based payment, etc.), the rates of adoption and ultimate impacts have been underwhelming (e.g., see Rosenthal and Frank 2006; Mullen, Frank, and Rosenthal 2010; Conrad 2016; Markovitz and Ryan 2017; Burns and Pauly 2018).

Medicare has recently used its large presence and financial leverage to overcome some of the inertia belonging to provider-payer contracting. As part of the Alternative Payment Models (APMs) launched by the ACA, LEJR procedures (namely knee and hip arthroplasty) became focal targets of two separate bundled (episode-based) payment reforms: the voluntary BPCI program beginning in 2013 and the mandatory CJR program beginning in 2016 (Siddiqi et al. 2017).[4] Exhaustive descriptions of the CJR program can be found among existing studies as well as

[4] Even prior to Medicare's payment model experiments, researchers had advocated for a bundled approach for joint replacements as an opportunity to reduce healthcare costs (Sood et al., 2011).

accompanying CMS resources (e.g., see footnote 3). We, in turn, only highlight key features of the payment reform that motivate our empirical aims and inform our identification strategy.

As previously remarked, the CJR program relies on a version of bundled service reimbursement, meaning that providers are incentivized to manage the full costs of the care episode, which spans the hospital stay and 90 days post-hospitalization. Medicare did not implement a posted price approach for LEJR bundled payments. Each hospital was ex ante given a target spending amount derived from its own historical Medicare LEJR costs and historical Medicare LEJR costs from its region. FFS payments would prevail during the course of treatment and recovery, and then an end-of-year reconciliation process would determine if the hospitals' average total episode spending met or deviated from the benchmark amount. Higher spending would trigger financial penalties and lower spending would trigger financial rewards from the public payer.[5] Additionally, provider participation was not voluntary, which is in stark contrast to other ACA-driven incentive structure experiments emanating from Medicare—including other episode-based payment programs, such as the Bundled Payments for Care Improvement (BPCI) initiative. Outside of the CJR program, we are aware of only one other contemporary and large-scale bundled payment model with mandatory participation.[6] And finally, CJR implementation was not universal. As opposed to a blanket rollout, CMS administrators leveraged a randomization approach to select clusters of counties (i.e., Metropolitan Statistical Areas or MSAs) across the US that would be exclusively incorporated into the CJR's five-year demonstration model. By design, this programmatic element creates precisely demarcated treatment and control classifications for

[5] For the first year of the program, providers only faced upside risk, which they knew upfront. As planned at the CJR program outset, downside risk was introduced beginning in 2017.
[6] In 2013, the state of Arkansas introduced an analogously structured initiative that focused on perinatal care and applied to all payers within the state (see Carroll et al. 2018).

health care providers based on their location and the pre-implementation geographic randomization.

Despite the enthusiasm for this Medicare policy development, several studies pre-emptively raised concerns over CJR implementation (Ellimoottil 2016, 2017; Ibrahim, Kim, and McConnell 2016), and since its inception, others have highlighted an uneven financial impact across providers—particularly with respect to safety net and other lower resourced hospitals (Kim et al. 2019; Navathe et al. 2018a; Thirukumaran et al. 2019a, 2019b). In terms of care delivery, existing evaluations of the CJR program find modest declines in spending overall (e.g., around 3%), with changes most pronounced in the use of post-acute care institutional stays, such as skilled nursing facilities (SNFs) and IRFs (Finkelstein et al. 2018; Barnett et al. 2019; Haas et al. 2019). Relatedly, a convenience sample survey of affected orthopedic surgeons commented that post-acute care services and spending became a top priority under the CJR regime (Sood et al. 2019). These findings align with what was previously documented for the voluntary bundled payment initiatives tied to LEJRs—though some BPCI participating hospitals also reported shorter length of stay and lower joint implant costs (Dummit et al. 2016; Iorio et al. 2016; Navathe et al. 2017). Due to post-reconciliation transfers (i.e., CJR provider “bonuses”), however, the estimates for the net savings to the Medicare program range from zero to small (Finkelstein et al. 2018; Barnett et al. 2019; Haas et al. 2019; Einav et al. 2020b). But as previously noted, we have more limited evidence as to if, and how, the introduction of the CJR program may have influenced provider behavior outside of Medicare LEJR cases, which could generate private gains for other market participants (i.e., Medicare Advantage and commercial insurers).

Among the three existing studies of non-Medicare LEJR patients, each focuses on a subset of Medicare Advantage patients found within the CMS MedPAR data. These data have the benefit of being national in scope but are also inherently incomplete records of the case mix, payer mix, and treatment behavior for a given hospital.[7] Using these data, Wilcock et al. (2020) asses the first year of the CJR program (2016) and find a small but symmetrical decline in the use of institutional post-acute care facilities between traditional Medicare and Medicare Advantage patients. They are unable to detect a statistically significant change among specific types of post-acute care facilities, however. Meyers et al. (2019) extend the analyses of Wilcock et al. (2020) by adding an additional year of data and incorporating more detailed post-acute care databases. The authors likewise see modest reductions in institutional post-acute care across the two payers; though, they also remark that there is no effect on home health receipt for Medicare Advantage patients and that the Medicare Advantage payer group typically demonstrates small effects overall. Similarily, Einav et al. (2020a) find a nearly equivalent decline (10-12%) in post-acute care facility utilization across both payers but also no change in home health post-acute services for Medicare Advantage patients. Importantly, when the authors repeat their spillover analyses within a national claims database from three large insurers,[8] the Medicare Advantage results become equivocal—perhaps due to power issues and/or an incomplete census of Medicare Advantage patients belonging to a given hospital. Taken together, the current and small literature on this aspect of the CJR program lacks consensus on Medicare Advantage market effects. It also offers no empirical evidence for the non-elderly commercial market or for the non-LEJR cases performed within CJR-affected

[7] For instance, because managed care organizations do not submit all claims to CMS, the Research Data Assistance Center (ResDAC) typically recommends removing these data from analyses, unless studying hospice care or a subset of added facility payments. Additional details regarding the limitations of analyses within the MedPAR database are described here: https://www.resdac.org/articles/differences-between-inpatient-and-medpar-files. Additionally, Meyers et al. (2019) formally assessed the post-acute discharge information from MedPAR and deemed it inaccurate, which motivated their inclusion of alternative data sources.
[8] The authors use Health Care Cost Institute (HCCI) data, which include Aetna, Humana, and UnitedHealth Medicare Advantage enrollees from across the country.

hospitals. Thus, the extent of Medicare bundled payment externalities remains an open empirical question.

We also note a modest but growing literature that has explored the reverse direction for spillover influence among payers (i.e., other insurers affecting Medicare). A few studies have taken interest in how the growth in the Medicare Advantage program affects the care and spending for traditional Medicare patients (Baicker, Chernew, and Robbins 2013; Baicker and Robbins 2015; Callison 2016), and other recent research has examined Medicaid (Bond and White 2013; Joynt et al. 2013, 2015; McInerney, Mellor, and Sabik 2017; Glied and Hong 2018; Carey, Miller, and Wherry 2020) as well as non-Medicare commercial insurance (He, McInerney, and Mellor 2015; Richards and Tello-Trillo 2019) demand and contracting externalities on the Medicare FFS population. Though indirectly related to our context, the findings from this complementary work help underscore the frequency and importance of cross-payer influences within multi-payer US health care markets.

III. Data

Our encounter-level data encompass the universe of inpatient discharge records from the state of Florida, which we obtained from the Florida Agency for Health Care Administration (AHCA). The detailed discharge records include a rich set of variables, such as diagnosis and procedure codes, type of insurance, patient demographic information, the specific facility and geographic location where the procedure was performed, ancillary care services provided during the inpatient stay, and post-hospitalization discharge disposition. We use the administrative data over a relatively long time series, starting in 2013 and ending in 2018. Unlike many other data resources, the data also capture all payers (including self-pay) in Florida markets over our full study period, rather than a specific payer or a subset of payers in the market.[9] Florida is also home to the second largest Medicare population in the country and some of the highest Medicare spending areas,[10] and it is somewhat over-represented in the CJR demonstration model since the randomization process placed meaningful weight on areas' historical Medicare spending levels. Notably, Florida accounted for 21% of the pre-policy LEJR cases among CJR treated MSAs nationwide.[11]

IV. Empirical Strategy

A. TREATMENT DECISIONS FOR LEJR CASES

Our primary analyses focus on the specific procedures (i.e., LEJR cases) targeted by the CJR bundled payment program. We consequently restrict to inpatient stays with Medicare Severity Diagnosis Related Group (MS-DRG) classifications 469 and 470 for hip and knee replacement hospitalizations—consistent with the program parameters and other studies in this literature. We then further restrict to three main payer groups: 1) Medicare FFS 2) Medicare Advantage and 3) non-Medicare commercial insurance. These three payer sources account for 93% of all LEJR cases observed in Florida in a typical year.[12]

Classifying a given provider (i.e., hospital) as part of the treatment or control group for our subsequent difference-in-differences (DD) analyses is accomplished by applying the sharp geographic boundaries set by CMS for the CJR payment model demonstration experiment. Using

[9] For example, existing studies of the Medicare CJR program largely use claims data that contain data from the Medicare fee-for-service population. Other commonly used sources of data include data from three private insurers offered through the Health Care Cost Institute, or data from employer-sponsored plans provided by IBM Watson (formerly Truven).
[10] State-level statistics on aggregate Medicare populations can be found here: https://www.kff.org/medicare/state-indicator/total-medicare-beneficiaries. Regional variation in spending per beneficiary can be found here: https://www.dartmouthatlas.org/interactive-apps/medicare-reimbursements/.
[11] Meyers et al. (2019) report 269,723 Medicare LEJR procedures from 2013 to 2016Q1 in treated MSAs nationwide. Over that same period, Florida had 57,424 Medicare LEJR procedures in its treated MSAs.
[12] The remaining payers include self-pay, workers compensation, Veterans Administration, and other government payers.

the list of included counties distributed by CMS, we map Florida counties belonging to the CJR program in Figure 1. The darker shaded counties are those mandating CJR participation for all hospitals present in those areas. Hospitals operating in the lighter shaded counties in Figure 1 are explicitly excluded from bundled payment model and therefore maintain the traditional FFS incentive structure for their Medicare LEJR patients. In this way, we ultimately capture the effects of the Medicare CJR program on both Medicare and non-Medicare patients and how they evolve over time by leveraging the quasi-random assignment of Florida hospitals to the bundled payment model.

We operationalize our DD estimation by using two complementary specifications. We first estimate the standard two-way fixed effects model for each LEJR patient (i) in hospital (h) at time (t):

$$Y_{iht} = \delta\left(\mathbf{1}\left(CJR_h\right) \times \mathbf{1}\left(Post_t\right)\right) + \tau_t + \lambda_h + \varepsilon_{iht} \tag{1}$$

The DD model in Equation (1) includes full vectors of half-year fixed effects (τ) as well as hospital fixed effects (λ) and a binary indicator for CJR inclusion status (i.e., the CJR variable) at the hospital level based on the original participant structure of the CJR bundled payment model. The Post variable is equal to one for all time points after the announcement of payment reform demonstration project (i.e., 2015H2), meaning we allow for anticipatory behavior in providers' responses. The δ parameter consequently reveals a summary DD estimate that averages over any immediate and longer term direct and indirect effects of the Medicare policy.

Our focal outcomes of interest (Y_{iht}) include inpatient length of stay (LOS) and post-hospitalization discharge decisions (sent directly to home with no further care, a SNF discharge, an IRF discharge, or a discharge with home health post-acute care services). LOS is a continuous measure while the discharge status outcomes are all binary. Of note, 99% of all FFS Medicare LEJR cases in the pre-CJR period (2013-2015H1) have one of the four discharge outcomes that belong to our DD estimation. We cluster standard errors at the hospital level, and we estimate Equation (1) separately for each specific payer group of interest to determine the presence and extent of first-order policy effects on traditional Medicare patients and spillover effects on Medicare Advantage and commercially insured patients.[13]

For our second modeling approach, we intentionally adapt our specification to an event study setup at the half-year level (2013-2018).[14] We do so for two key reasons. First, we wish to leverage our granular and relatively long time series to examine any differential behavior between treated and control hospitals prior to as well as following the introduction of the CJR program. The former informs the validity of the DD research design in our study setting and hence the credibility of our resulting inferences. The latter allows us to observe any dynamic effects from the policy intervention, which could result from provider adjustment frictions and/or provider learning over time. Second, in practice, there are three distinct "post" periods that are analytically relevant in the context of the CJR program. The demonstration model structure, including hospital participants, was announced in mid-2015; however, the official start date was April 2016. Additionally, the program underwent a dramatic change at the end of 2017—leading to one half of the national treatment areas being switched into a voluntary participation model (based on their

[13] We have also explored clustering the standard errors at the geographic treatment (i.e., MSA) level. With only 29 clusters available in Florida, we needed to use random inference approaches to account for small numbers of geographic clusters and small numbers of treated geographic units (8 in total in our context). This alternative standard error approach actually yielded tighter estimates, meaning that our standard errors from hospital level clustering are more conservative than MSA clustering or no clustering at all within our analytic context.
[14] The discharge records are quarterly; however, we implement the estimation at the half-year level to smooth out some strong seasonality in procedure volumes for these elective surgeries. Event studies at the quarterly level reveal qualitatively the same patterns of findings and inferences.

status as having historically lower LEJR per episode spending). Also, among the remaining mandatory participation areas, opt-out provisions were introduced for qualifying "low-volume" or "rural" hospitals.[15] Approximately three-fourths of eligible hospitals nationwide withdrew from the CJR program when allowed to do so in 2018. Both Kim et al. (2018) and Einav et al. (2020b) unsurprisingly document strategic staying for hospitals electing to do so once the CJR program shifts to voluntary participation among the affected geographic areas.

On a practical level, these changes have little bearing on participation in the CJR program among Florida-specific hospitals. Only a single CJR-included county in Florida (affecting two hospitals in total) was granted the voluntary participation option beginning in 2018. The number of Florida CJR participating hospitals subsequently declines by only four with this mid-demonstration project administrative disruption—87 hospitals at the start of the third implementation year (2018) compared to 91 hospitals during the original program structure (2016-2017). In this way, Florida health care markets effectively represent the counterfactual scenario where the CJR bundled payment demonstration continues as originally intended—i.e., without the 2017 administrative intervention by new Department of Health and Human Services (DHHS) and CMS leadership. However, 2018 is also unique in that Medicare made a long-awaited ruling that removed LEJRs from its inpatient-only (IPO) list—allowing outpatient surgical delivery for these procedures for the first time (e.g., see Richards, Seward, and Whaley 2020). For these reasons, we want to adopt a model that allows us to observe whether hospital responses between 2015 to 2017 differ from 2018, should these two separate Medicare LEJR policies (CJR and IPO) inadvertently interact in the latter year.[16]

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[15] See the Federal Register, 82 (158) August 17th 2017, for full descriptions of these specific programmatic changes.
[16] For example, mandated participation in the CJR program could blunt the incentive to shift clinically appropriate cases to the outpatient setting insofar as those patients would be healthier, on average, and therefore help hold down

Our flexible event study approach consequently estimates CJR-affected hospitals' differential behavior for our outcomes of interest during the three years prior to the CJR program announcement, during the announcement, and for three years of post-implementation. The accompanying specification for each LEJR patient (i) in hospital (h) at time (t) is a slight adaptation of Equation (1):

$$Y_{iht} = \sum_{\substack{j=2013H1 \\ j\neq 2015H1}}^{2018H2} \theta_j \left(\mathbf{1}\left(\tau_t = j\right)\right) + \sum_{\substack{k=2013H1 \\ k\neq 2015H1}}^{2018H2} \delta_k \left(\mathbf{1}\left(CJR_h\right) \times \mathbf{1}\left(\tau_t = k\right)\right) + \lambda_h + \varepsilon_{iht} \tag{2}$$

The modified DD model in Equation (2) includes full vectors of half-year fixed effects (τ) as well as hospital fixed effects (λ) and a binary indicator for CJR inclusion status (i.e., the CJR variable) at the hospital level based on the original participant structure of the CJR bundled payment model, just as before. The omitted time point is the 6-month period immediately preceding the CJR program's announcement by CMS (i.e., when τ_t = 2015H1). The set of δ_k parameters capture our focal DD estimates for our treated hospitals (i.e., those that are randomized to the CJR program in 2015 so that CJR = 1) over our full study period. Of note, we also estimate Equation (2) with a set of patient demographics (age, sex, race, and number of comorbid conditions) on the left-hand side. The patient demographic outcomes are examined to test for evidence of post-policy strategic screening of LEJR patients (i.e., "cream skimming") by hospitals to improve their likelihood of meeting or beating their respective Medicare spending targets.

average episode spending on the inpatient side, which raises the probability of receiving a reconciliation bonus payment at the end of 2018.

A causal interpretation of the estimated post-CJR coefficients from Equation (2)—i.e., δ_{2015H2} through δ_{2018H2}—relies on the standard difference-in-differences assumptions. Moreover, we believe our approach is valid for three reasons. First, while we cannot explicitly test the absence of contemporaneous shocks, CJR inclusion areas were randomly assigned, which makes the potential for contemporaneous shocks specific to just these geographies unlikely. Second, our event study approach allows us to formally examine pre-implementation trends in our outcomes of interest for several years. Any divergent behavior among providers within CJR counties would be likely to occur in the periods prior to implementation as well. Finally, as noted above, to test if changes in treatment approaches reflect post-policy LEJR patient selection, we estimate Equation (2) with patient characteristics as outcomes. We do not find that implementation of the CJR program leads to changes in observable patient characteristics among LEJR cases, which suggests our main findings are attributable to changes in provider behavior and not differences in patient composition (fully discussed in Section V).

B. TREATMENT DECISIONS FOR NON-LEJR CASES BY LEJR PHYSICIANS

Our secondary DD analyses merely apply the estimation approaches developed in Section IVA to inpatient orthopedic surgery cases that are beyond the scope of the CJR program. Specifically, we examine treatment decisions for non-LEJR cases performed by policy-affected surgeons and hospitals. To identify the relevant cases for each of our three key payer groups, we focus on orthopedic surgeons who performed both LEJR Medicare cases in our primary analytic sample and any non-LEJR cases also associated with the musculoskeletal systems and connective tissues diagnoses.[17] The mapping is accomplished via the included 10-digit National Provider Identification (NPI) number for the primary surgeon attached to each inpatient discharge record. The resulting set of cases represents non-LEJR procedures that are most similar diagnostically to LEJR cases and most commonly inpatient procedures performed by LEJR surgeons.[18] We do not use all non-LEJR cases belonging to these surgeons since the greater heterogeneity in the types of procedures belonging to the mix of non-LEJR cases could mask spillover effects for cases more similar to LEJR cases in terms of case complexity, patient severity, and clinical decision-making. For instance, Ryan (2018) remarks that CJR payment reform effects may not extend to other surgery types where post-acute care use is low at baseline, even in the presence of direct incentives (e.g., a bundled payment model) for these other surgery types. We provide additional details on our sample selection in Appendix A.

We then adapt Equation (1) and Equation (2) from Section IVA to include a vector of DRG fixed effects (η):

$$Y_{iht} = \delta\left(\mathbf{1}\left(CJR_h\right)\times\mathbf{1}\left(Post_t\right)\right)+\tau_t+\lambda_h+\eta_d+\varepsilon_{iht} \tag{3}$$

$$Y_{idht} = \sum_{\substack{j=2013H1\\ j\neq 2015H1}}^{2018H2}\theta_j\left(\mathbf{1}\left(\tau_j=j\right)\right)+\sum_{\substack{k=2013H1\\ k\neq 2015H1}}^{2018H2}\delta_k\left(\mathbf{1}\left(CJR_h\right)\times\mathbf{1}\left(\tau_t=k\right)\right)+\lambda_h+\eta_d+\varepsilon_{idht} \tag{4}$$

[17] All diagnoses codes belong to 25 mutually exclusive groups, known as Major Diagnostic Categories (MDC). The two LEJR diagnosis codes in our primary analysis belong to MDC-8: disease and disorders of musculoskeletal system and connective tissues. Our non-LEJR cases focus on the remaining 97 diagnosis codes within MDC-8 that are performed by LEJR surgeons. In total, MDC-8 cases comprise 36% of all non-LEJR cases performed by LEJR surgeons.
[18] Nine of the top ten most performed non-LEJR procedures belong to MDC-8.

All other features of the prior specifications are preserved, including the requirements for valid inferences from Equation (3) and Equation (4).

V. Results

A. POLICY EFFECTS ON TARGETED LEJR CASES

We first examine the relevant and aggregated case volumes by payer type across treated and control Florida hospitals over our full study period (Figure 2). Notwithstanding seasonality-driven variation, all three patient groups demonstrate smooth upward trends in case volumes for both treatment and control hospitals. Figure 2 also offers prima facie evidence that hospitals mandated to participate in the CJR program did not respond by shrinking their set of CJR-eligible cases. The later dips in inpatient LEJR cases seen across payers is consistent with the findings from Richards, Seward, and Whaley (2020), which documents providers shifting a subset of hip and knee replacements to the outpatient surgical setting following the Medicare IPO ruling change. However, Figure 2 also demonstrates that such behavior in 2018 is common across CJR treatment status in Florida—indicating that these policies do not obviously interact. Importantly, Figure 2 establishes traditional Medicare as the dominant payer for LEJR cases facing hospitals. At any given time point during our study period, Medicare accounts for roughly 40-50% of all LEJR cases belonging to these three prominent payer groups. Furthermore, the shares of LEJR cases belonging to Medicare Advantage and commercially insured markets would be allocated over many rival insurers and plans, rather than concentrated among one insurer as is the case for traditional Medicare.

Within Table 1, we present the summary statistics for LEJR cases in the pre-policy announcement period (i.e., before the second half of 2015) along two dimensions: payer and CJR status. The patient demographics closely parallel each other across treatment-control statuses within a payer type in the top portion of Table 1. The one exception is the number of comorbidities for Medicare FFS patients where hospitals in CJR-included areas list nearly one more additional diagnosis, on average, than control group hospitals (i.e., 8.55 relative to 7.59). Moving to the bottom portion of Table 1, we see that LOS is also broadly similar for each of the three treatment-control comparisons and is approximately three to three and a half days, on average, for LEJR procedures. There is greater, though not always consistent, variation in discharge-related treatment decisions between CJR-included and CJR-excluded hospitals for a given payer type. For example, across all three payers, CJR hospitals have a higher rate of IRF use and lower rate of home health use prior to the CJR program's announcement. Given the stark cost differences between these two post-acute care modalities, this greater tendency to utilize more the more expensive (i.e., IRF) option may be partly responsible for the high historical Medicare spending found within these specific geographies. Relatedly, while CJR hospitals in Florida send more than 50% of their traditional Medicare and Medicare Advantage patients to IRFs or SNFs at baseline, the comparable national average for CJR hospitals from Meyers et al. (2019) is only 43% and 40%, respectively. This finding again highlights Florida hospitals' strong tendency to choose higher cost treatment options when addressing an identical medical problem and further indicates the potential threat of financial penalties facing Florida hospitals assigned to the bundled payment model for all Medicare LEJR cases. The departures in pre-period discharge treatment decisions among Florida hospitals in Table 1 also underscore the value of estimating the flexible DD event study specification. Namely, we want to ensure that these are fixed differences between CJR and non-CJR hospitals during 2013-2015, as opposed to pre-existing divergent trends in provider behavior, which would invalidate the DD research design.

Table 2 displays the summary DD estimates for the average effect of the CJR program during the announcement and 3-year post-implementation periods. LOS shows no change with the introduction of Medicare's CJR bundled payment initiative for any of the three payer types. The estimates are small in magnitude (i.e., less than five one-hundredths of a day) and far from statistical significance at conventional levels. Likewise, we do not observe any indications of direct or indirect policy effects on the probability of being discharged to SNF care (column 2, Table 2). The provider behavior that does change across all three payer groups is the propensity to discharge LEJR patients to IRFs following their hospitalization (column 3, Table 2). CJR hospitals decrease their use of IRFs for post-acute care by 2-percentage points (22%) for traditional Medicare, by 3.6-percentage points (45%) for Medicare Advantage, and by 0.8-percentage points (20%) for the non-elderly, commercially insured.[19] The DD estimates are all statistically significant for the IRF outcome in Table 2—though only at the 10% level for the commercially insured patients (Panel C). These findings for FFS Medicare patients align with the existing literature. For example, Barnett et al. (2019) show a nearly 6% decline in post-acute care discharges during the first two years of the CJR program, and more specifically, Haas et al. (2019) demonstrate a five-times larger relative decline in IRF spend following CJR implementation relative to SNF spending. Our policy effect sizes are potentially larger, however, due to Florida's higher spending levels at baseline (and hence greater room for improvement) and the inclusion of longer implementation period (i.e., through 2018).

Importantly, the remaining columns of Table 2 offer some insight as to where patients are increasingly being discharged if not to an IRF. For traditional Medicare patients, there is an approximately 2-percentage point increase in the probability of being discharged to home health

[19] The relative (i.e., percent) changes discussed throughout Section VA are based on comparing the DD estimates to the corresponding pre-CJR rates for the CJR-included hospitals found in Table 1.

post-acute care services following their LEJR procedure. Although the estimate is imprecise, the magnitude of the home health increase is virtually identical to the observed IRF reduction. Among Medicare Advantage LEJR patients, there is a statistically significant increase of almost 7-percentage points (21%) in the likelihood of being sent out with home health, which more than offsets the decline in IRF utilization. The estimate for routine discharges to home (i.e., without post-acute care) in column 5 of Panel B suggests that the remaining substitution toward home health services is coming from this margin; though, the decline in routine discharges is not quite significant with a t-statistic of 1.52. This shift is more clearly present among the commercially insured patient population. These patients are 7.6-percentage points (12%) more likely to receive home health care and 6-percentage points (33%) less likely to receive a routine discharge to home.

We show the corresponding event study results for discharges to IRFs, home health, and routine home discharges in Figure 3.[20] These graphs add important context to the results and inferences from Table 2. There is no clear pre-announcement differential trending among CJR hospitals for any of the outcomes in Figure 3, meaning that the parallel trends assumption underlying the DD strategy appears to be satisfied. Following announcement of the CJR program during the latter half of 2015, we observe a sharp (2-percentage point) drop in IRF use for traditional Medicare beneficiaries (Panel B, Figure 3). The effect approximately doubles in the later years of the payment demonstration model—reflecting a 44% decline in IRF utilization compared to the pre-CJR level.[21] The Medicare Advantage population shows no response with

[20] The corresponding event study figures for length of stay and SNF discharge outcomes are shown in Appendix Figure B1. The coefficients oscillate around zero and are rarely statistically different from zero over the full study period.

[21] Haas et al. (2019) unadjusted trends for treatment versus control hospital LEJR average spending per case shows suggestive—though somewhat noisy—indications of the spend differential narrowing in the months leading up to the official start of the CJR program. Likewise, Barnett et al. (2019) demonstrate some suggestive anticipatory behavior in terms of converging average post-acute care institutional spending across treatment and control hospitals as well. And Meyers et al. (2019) observe evidence of announcement effects for traditional Medicare and Medicare Advantage patients in their study.

CJR announcement but then an immediate reduction in IRF utilization once the FFS bundled payment incentives are rolled out in 2016. The post-period point estimates range from –3.5 to –5.0 percentage points and demonstrate a fairly stable and sustained effect over the three years of the CJR program. Providers do not shift their IRF use for the non-elderly, commercially insured LEJR patients until the second year of the Medicare bundled payment initiative. At that time, as much as a 1.5-percentage point decrease emerges, which for the younger LEJR patient population, translates to a 38% reduction compared to the pre-policy behavior. Importantly, across all three payer groups in Figure 3, IRF use remains depressed through the end of 2018, which is consistent with the CJR-inclusion restrictions continuing to bind for the overwhelming majority of Florida hospitals after the 2017 administrative changes by DHHS and CMS.

The event study results for home health post-acute care services for traditional Medicare patients (Figure 3, Panel B) show a post-CJR upward movement, but the estimates are noisy. On the other hand, Medicare Advantage and commercially insured LEJR patients show marked and growing policy effects for this outcome in Figure 4. By the end of our study period (i.e., three years out), the likelihood of being discharge with home health services increases by approximately 10-percentage points for each of these patient groups, representing a 29% and 16% relative change for Medicare Advantage and commercially insured, respectively. Finally, there is no obvious change in the likelihood of being sent home with no further recovery services for traditional Medicare patients (Figure 4, Panel C), but there is a suggestive decline over time for Medicare Advantage patients and a pronounced drop for the commercially insured. The latter group is 5 to 8 percentage points less likely to have a routine discharge home among CJR affected hospitals, which translates to an approximately 28 to 44% decrease over their baseline (pre-policy) rate in Table 1.

In Appendix Figure B2, we do not find any evidence for strategic patient selection in terms of observable patient characteristics (age, sex, and race) for any of the three payer groups. The number of reported other diagnoses does not obviously change for Medicare or commercially insured LEJR patients; though, there is an indication of 0.5 to 1.0 decrease in the number of reported additional diagnoses among the Medicare Advantage group following the policy change. This translates to only a 6% to 13% relative decline from a baseline mean of nearly 8 additional diagnoses reported per patient (Table 1). It therefore seems unlikely that this singular diagnosis reporting change limited to Medicare Advantage patients—which likely represents minimal changes in patient risk—can explain the pronounced spillover effects documented in Table 2 and Figure 3.

Taken together, our results on spillover effects onto other insurers indicate that both Medicare Advantage and commercial patients were affected by the traditional Medicare bundled payment incentive structure. Across all payers, providers favored discharges home with home-health care over more intensive and expensive care settings (i.e., IRFs). However, providers also reduced their discharges home with just routine care, particularly among the commercially insured—indicating that non-elderly commercially insured LEJR patients at CJR-affected hospitals actually receive greater post-acute care following Medicare's bundled payment initiative. Due to data constraints, however, we are unable to assess if the higher intensity of services for this patient group translates to net savings for their insurers, e.g., through faster recovery times or fewer follow-on procedures.[22]

B. POLICY EFFECTS ON UNTARGETED NON-LEJR CASES

[22] Specifically, there are no patient identifiers in the Florida AHCA discharge data that would allow longitudinal tracking of health care utilization at the patient level.

Next, we consider spillovers for provider behavior toward non-LEJR cases. Table 3 shows the summary statistics for our comparable non-LEJR cases among surgeons who also perform LEJR procedures. Across payer-specific control and treatment groups, patient demographics are again similar, with the exception of the commercially insured having a slightly younger population among CJR hospitals (47 versus 52 years of age, on average). The average LOS for non-LEJR procedures is slightly longer among CJR hospitals. However, discharge propensities to SNFs, IRFs, home-health care, and directly to home with no post-acute care all occur with almost equal frequency across treated and control populations. As before, we rely on the event study estimates to demonstrate whether the trends in these variables are similar, in spite of any small (and fixed) level differences in the pre-period. We also note the discrepancies in the discharge destination rates and patient characteristics between LEJR and orthopedic non-LEJR cases. Compared to Table 1, the summary statistics in Table 3 show that non-LEJR patients, particularly those insured with traditional Medicare, tend to have more additional diagnoses (i.e., comorbidities), are more likely to be discharged home without additional care, and are less likely to receive home health care, specifically.

The DD results from the two-way fixed effects estimations for these non-LEJR cases are displayed in Table 4. Among traditional Medicare patients (Panel A), there is a 2.4 percentage point (4%) reduction in discharges home without any post-acute support and a corresponding 2.4 percentage point (5%) increase in discharges to SNFs. Among Medicare Advantage patients (Panel B), the decrease in home discharges is smaller and is not statistically significant; however, there is evidence of a substantive spillover reduction in IRF post-acute care, which falls by 1.9 percentage points (32%) following the introduction of the LEJR bundled payment reforms. Substituting away from IRFs, CJR-affected providers increase their reliance on home health care by 2.0 percentage points (8.3%) and SNF care by 1.8 percentage points (4%) for non-LEJR Medicare Advantage cases. Finally, among the commercially insured (Panel C), there is no clear change in SNF or IRF care, but there is a decrease in routine discharges to home by 2.8 percentage points (10%) and a commensurate increase in discharges with home health post-acute care services by 3.0 percentage points (16%).

In Figure 4, we present similar event study regression results for non-LEJR cases. For each outcome and patient population, we do not observe strong indications of pre-CJR differential behavior between CJR and non-CJR hospitals. Similar to the DD regression results in Table 4, we observe post-announcement reductions in the probability of discharge to an IRF for traditional Medicare and Medicare Advantage patients (Panel A). Though, the decline in IRF use among traditional Medicare non-LEJR cases is more ephemeral, with a rebound toward the end of our study period. Conversely, the Medicare Advantage population experiences an approximately 2 percentage point decrease that is fairly stable over the three post-policy years. Consistent with the summary DD result in Table 4, we do not find a compelling change in IRF discharges for commercially insured patients in Panel A. For the two other primary outcomes that align with the LEJR results from Table 2 and Figure 3, we find suggestive increases in the use of home health services and declines in routine discharges to home without post-acute care services. Although, the event studies for the commercially insured populations in Panels B and C indicate that CJR hospitals differentially changed their behavior starting in the earlier part of 2015. In Panel D of Figure 4, the event study patterns of traditional Medicare and Medicare Advantage non-LEJR procedures offer additional suggestive evidence (consistent with Table 4) of increased SNF use among CJR-impacted hospitals after the CJR program is in place. Within Appendix Figure B3, we show no change in LOS for any of three payers' non-LEJR cases over the full study period.

These complementary results offer some evidence that the impact of the Medicare CJR program can extend to treatment decisions for non-LEJR cases. Specifically, providers appear to reduce their reliance on post-acute care IRFs, especially among the Medicare Advantage population, and to view home health services as a more attractive post-acute care option. That said, the magnitude of these changes for non-LEJR cases tends to be roughly half of the change in discharge location decisions observed for LEJR cases, which indicates that even if such spillovers are present, the impact is likely to be considerably weaker relative to procedures explicitly targeted by the payment reform. Unlike LEJR cases, the non-LEJR cases demonstrate a modest (4-5%) increase in SNF use among traditional Medicare and Medicare Advantage populations; though, the increase may result from underlying differences in care needs and post-acute care options among non-LEJR patients. Moreover, while the finding for this outcome contrasts with the LEJR findings from Table 2 and Figure 3 (i.e., no CJR effect on SNF receipt), it can still be consistent with the Medicare CJR bundled payment model creating the necessary impetus for affected providers to re-evaluate their post-acute care options and previous clinical decision-making tendencies.

VI. Discussion and Conclusions

Industry experts and policymakers regularly lament the state of US health care and advocate for more cost-conscious approaches to financing medical service delivery and population health outcomes. One path toward such ends is to use the frequently largest payer (i.e., Medicare) facing a given provider as a vehicle for driving non-FFS incentive structures. Despite a ballooning number of Medicare-based payment model demonstrations, externalities tied to a given model are rarely considered or quantified. We leverage an innovative Medicare payment reform and comprehensive data from the state of Florida to estimate the presence and reach of Medicare spillovers.

We find that the program leads to meaningful direct and indirect changes in provider behavior. Among the traditional Medicare population, discharges to high-cost inpatient rehab facilities decrease by 22%, but this understates the impact of the CJR program along this margin. IRF discharges also decline by as much as 44% for Medicare Advantage patients and as much as 38% for non-elderly commercially insured patients. For all three LEJR patient populations, the reduction in IRF discharges is offset by greater use of home health post-acute care services. IRF care is markedly more expensive than home health services, and importantly, there is no clear clinical evidence that joint replacement patients receiving IRF care, rather than home health care, fare better (Buhagiar et al. 2019). For these reasons, the observed post-acute care substitution behavior indicates a beneficial re-optimization of providers' LEJR treatment choices across payers following the introduction of stronger incentives from one prominent payer (i.e., Medicare) to economize on post-acute costs per episode. Additionally, we find suggestive evidence that the program creates spillovers for similar surgical procedures that are not covered by the CJR program, which has the potential to benefit patients and payers beyond the scope of a targeted payment reform intervention.

We further note that the provider behavior changes we empirically document are more consistent and substantively larger than what has been found among existing CJR studies—including those examining the LEJR Medicare Advantage market. As previously remarked, we have the analytic advantage of capturing more payers as well as the full universe of accompanying cases over a longer time period, which can help in identifying effects (especially those that evolve over time). However, another, and not mutually exclusive, potential explanation is that we focus on Florida, which contains some of the highest spending areas and providers in the country. For LEJR cases, specifically, comparing our data to the national data used in other work reveals that CJR-targeted Florida hospitals had a much stronger propensity to use high-cost post-acute care options at baseline when compared to their CJR peers from elsewhere around the country. Intuitively, we would expect more pronounced changes (i.e., greater policy bite) among high-cost providers once a new incentive structure has been introduced. This also likely means that we are capturing an upper bound of direct and indirect bundled payment policy effects, but this is still informative for health policy debates regarding interventions that seek to curb spending in very high-spending areas.

Our observed provider behavior changes concentrated among post-acute care decision-making is in concordance with the existing CJR literature and also providers' private financial interests within the CJR regime. Carroll et al. (2018) illustrate the importance of conflicting incentives facing a given provider under a bundled payment structure that relies on ex post reconciliation rewards or penalties—as the CJR program does. Namely, all care under the direct control of the provider is still paid FFS, so any billable effort reductions that help in achieving the benchmark total spend simultaneously reduce the provider's own payments. Conversely, all care delivered by other providers (but still part of the episode) can be restrained to make it more likely that overall spending reductions are accomplished (generating a financial reward) without lowering total FFS payments to the focal provider at risk. Our results are consistent with the Carroll et al. (2018) theoretical model and estimates for perinatal care in Arkansas. Specifically, the authors show an approximately 3% decline in total episode spending, which seems to reflect referrals to less expensive birth facilities by the providers directly incentivized by the bundled payment initiative. Our findings also support the argument made by Frandsen et al. (2019) in which the authors describe the importance of a given payer's relative share of the market for influencing provider contracting and behavior.[23] It also seems unlikely that pre-CJR LEJR providers experienced prohibitive fixed cost investments (e.g., specialized information technology needs) in order to improve discharge planning and hence participate in risk-bearing contracts for LEJR cases—as would be the case in the context of a payer coordination failure (see Frandsen et al. 2019).

Importantly, our estimates offer compelling empirical support for the "norms hypothesis" of provider behavior and complement the results from Barnett, Olenski, and Sacarny (2020). Our work is arguably most similar in spirit to Barnett et al. (2020) as well. The authors examine antipsychotic prescribing to commercially insured patients among physicians receiving a behavioral nudge about overprescribing for their Medicare patients. Crucially, the authors also assess changes in prescribing behavior for the intervention targeted drug as well as closely related pharmaceutical therapies. Despite finding nearly symmetrical reductions in prescribing for the targeted pharmaceutical therapy for Medicare beneficiaries and older adult commercially insured patients, Barnett et al. (2020) do not find changes in prescribing patterns beyond the focal drug tied to the Medicare information intervention. The authors also show that the cutbacks in prescribing for the focal drug appear indiscriminate (i.e., not reflective of underlying patient appropriateness). In comparison, we demonstrate more nuanced treatment decision-making, which even leads to greater post-acute care service provision for some patients. CJR-affected providers seem to devote more attention to post-hospitalization treatment choices and outcomes across payers and procedures but not rigidly impose the same changes to all clinical contexts. In these

[23] Recall, within our analytic data, traditional Medicare LEJR cases typically outnumber each of the other two prominent payers' (i.e., Medicare Advantage and non-Medicare commercial) cases by 50–100% in any given half-year.

ways, our findings juxtaposed to Barnett et al. (2020) offer a useful comparison for the spillover reach for two different styles of Medicare intervention (i.e., provider payment reforms versus provider information campaigns) that apply to two different types of physicians (i.e., surgeons versus primary care specialists). The evidence from each study indicates that Medicare policy can be the source of substantive externalities for other patients and payers.

One pre-implementation CJR study (Maniya et al. 2017) arrived at the conclusion that hospitals should by-and-large ignore the program and accept any resulting penalties. Clearly, many providers, especially in Florida markets, did not take such advice and instead used the CJR program as an impetus for rethinking treatment approaches for other (non-Medicare) patients. To date, no studies have documented adverse consequences from the CJR or BPCI programs, such as greater complication rates, altered case volumes, or patient cream-skimming (Dummit et al. 2016; Navathe et al. 2018b; Barnett et al. 2019; Meyers et al. 2019; Einav et al. 2020a), and previously, Sood et al. (2011) expressed a perhaps common view that other payers might eventually learn from the Medicare CJR experience. While still potentially true, our findings suggest that many of them received an immediate private benefit. Relatedly, existing studies of the CJR program (Finkelstein et al. 2018; Barnett et al. 2019; Haas et al. 2019; Einav et al. 2020b) remark that the net savings has been minimal; however, the more holistic view we take adds important qualifications. Our empirical evidence is consistent with uncontracted and uncompensated savings for Medicare Advantage insurers over a three-year period—meaning more efficient care delivery for the broader health care system during this time.[24] That said, bundled payments are not easy to construct or implement, especially for a wide variety of medical problems and clinical contexts (Burns and Pauly 2018). The iterative and circumscribed process to provider payment reforms inside and

[24] The spending implications for the commercially insured group are less clear due to data constraints.

outside of Medicare is therefore likely to continue; however, implementers and evaluators should pay closer attention to the range of externalities that may surface from a given intervention. Social welfare estimates and policymakers' ex post decisions could be distorted if the analytic view is too narrow, especially when the involved payer looms large within the relevant health care market.

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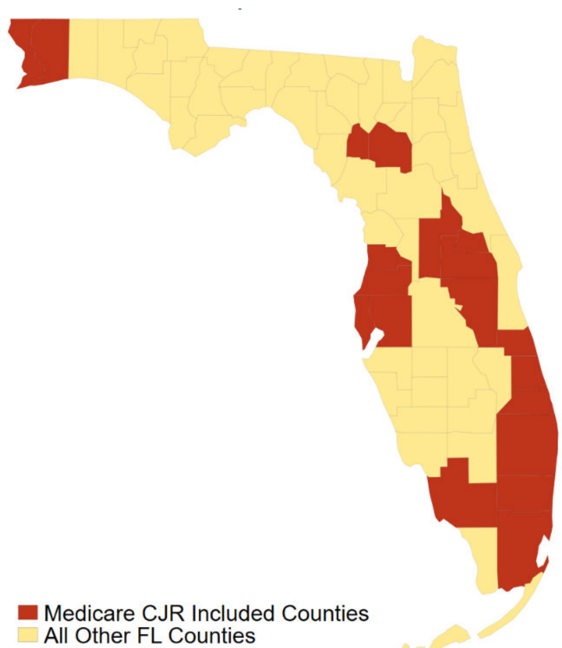
Tables and Figures

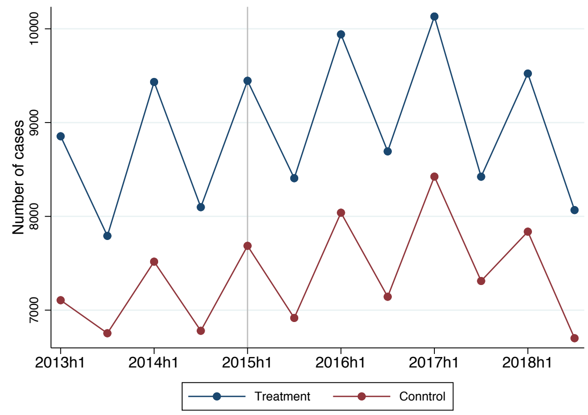
Figure 1: Treated versus Control Counties in Florida



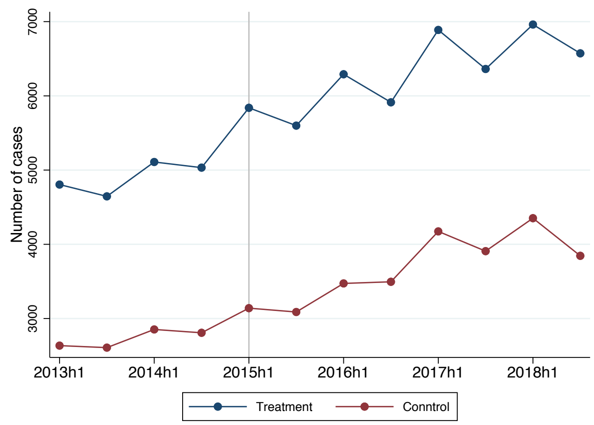
Notes: Counties that belonged to a treated MSA are in red.

Figure 2: Average Number of Cases per County Over Time, by Payer

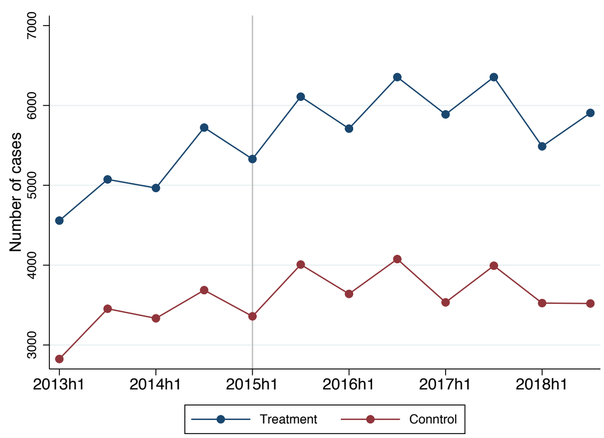
(A) Medicare FFS Cases



(B) Medicare Advantage Cases



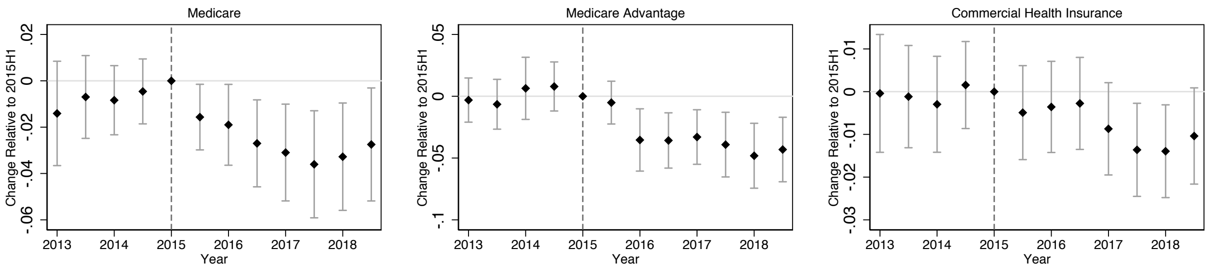
(C) Commercial Cases



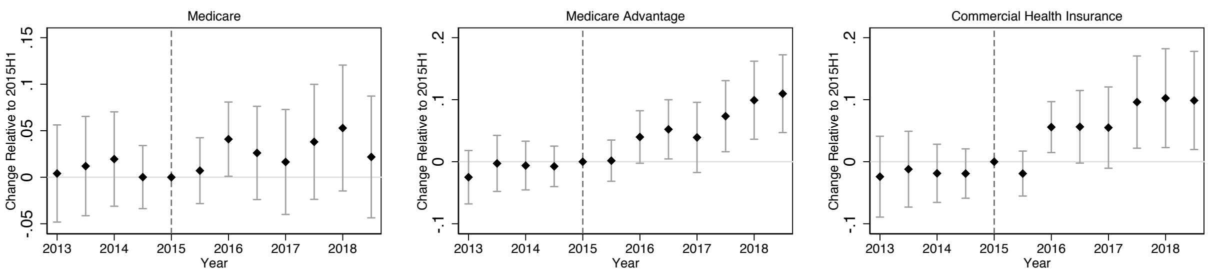
Notes: Each plot shows the average number of LEJR claims per county, as measured by Medicare-severity diagnosis related groups 469 and 470.

Figure 3: Effect of Bundled Payment on Discharge Location

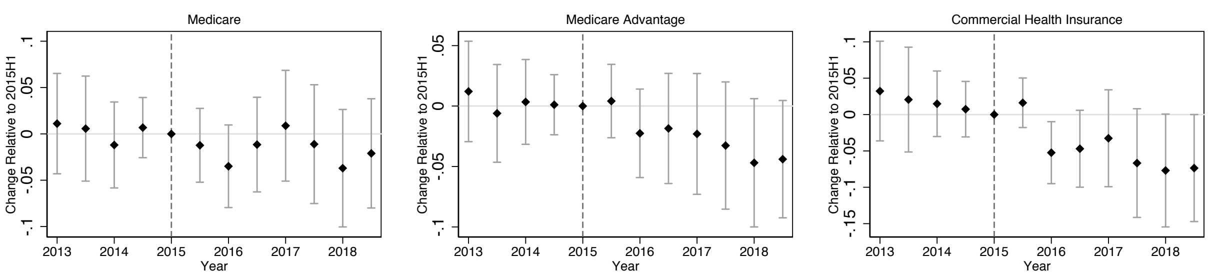
(A) Probability Discharged to an IRF



(B) Probability Discharged to Home Health



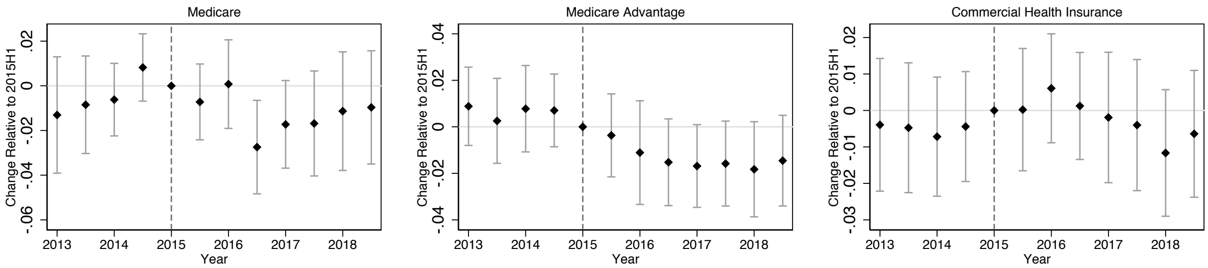
(C) Probability Discharged Home (Routine)



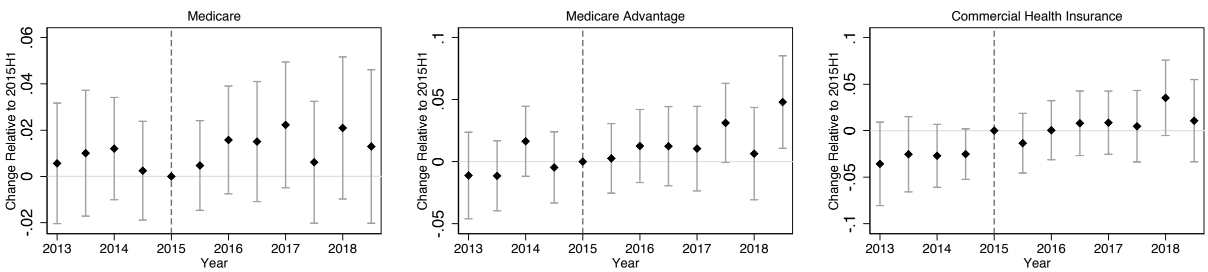
Notes: Each plot shows the event study coefficients and 95% confidence interval for outcomes pertinent to LEJR procedures. All changes are measured relative to 2015H1. Each regression additionally controls for patient demographics and hospital fixed effects. Standard errors are clustered by hospital. Dashed vertical line represents the six months immediately preceding the announcement of the CJR program and hospital assignment to the CJR program.

Figure 4: Spillover Effect on Discharge Location of Non-LEJR Procedures

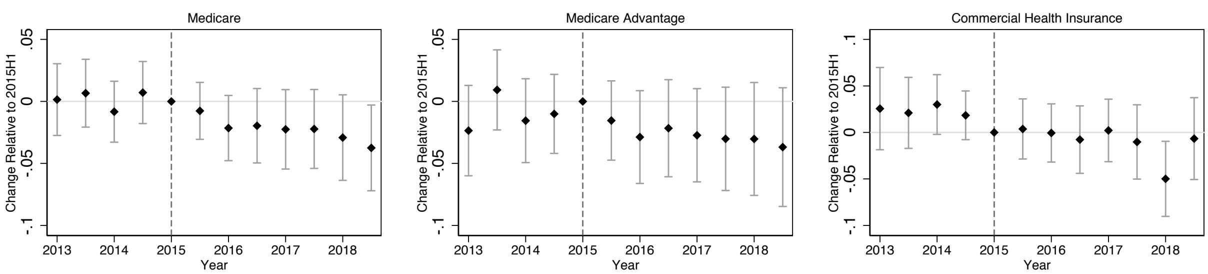
(A) Probability Discharged to an IRF



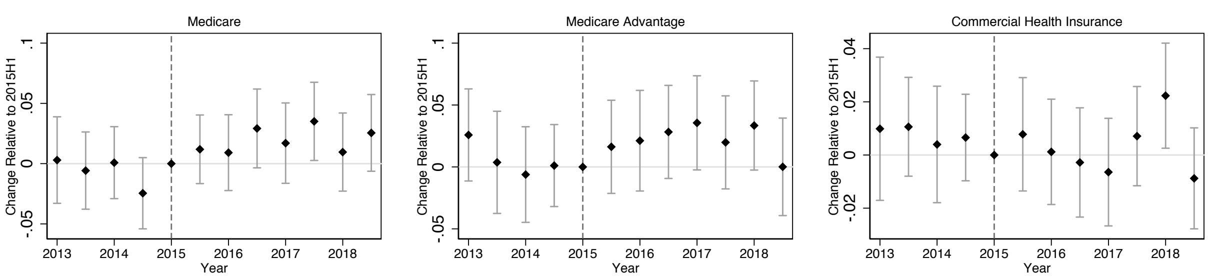
(B) Probability Discharged to Home Health



(C) Probability Discharged Home (Routine)



(D) Probability Discharged to a SNF



Notes: Each plot shows the event study coefficients and 95% confidence interval for outcomes pertinent to non-LEJR procedures. Sample includes physicians who perform both LEJR and related non-LEJR procedures (i.e., MDC-8). All changes are measured relative to 2015H1. Each regression additionally controls for patient demographics and hospital fixed effects. Standard errors are clustered by hospital. Dashed vertical line represents the six months immediately preceding the announcement of the CJR program and hospital assignment to the CJR program.

Table 1: Summary Statistics for LEJR Procedures in Pre-Treatment Period (2013Q1 – 2015Q2)

	Medicare		Medicare Advantage		Commercial	
	Control	Treatment	Control	Treatment	Control	Treatment
	(1)	(2)	(3)	(4)	(5)	(6)
Patient Demographics						
Age	73.95	74.50	72.33	72.78	58.69	58.21
	(8.66)	(8.9)	(8.49)	(8.85)	(7.49)	(7.99)
1(White)	0.93	0.91	0.89	0.82	0.88	0.85
	(0.26)	(0.28)	(0.31)	(0.39)	(0.32)	(0.36)
1(Female)	0.62	0.63	0.62	0.65	0.57	0.54
	(0.49)	(0.48)	(0.48)	(0.48)	(0.5)	(0.5)
Number of other diagnoses	7.59	8.55	7.13	7.80	5.41	5.94
	(4.87)	(5.17)	(4.54)	(4.82)	(3.81)	(4.01)
LEJR Outcomes						
LOS	3.47	3.64	3.44	3.69	2.82	2.90
	(2.02)	(2.35)	(1.97)	(2.37)	(1.24)	(1.59)
1(Discharged to SNF)	0.45	0.42	0.41	0.49	0.15	0.13
	(0.5)	(0.49)	(0.49)	(0.5)	(0.36)	(0.34)
1(Discharged to IRF)	0.06	0.09	0.03	0.08	0.02	0.04
	(0.24)	(0.29)	(0.17)	(0.27)	(0.15)	(0.19)
1(Discharged to Home Health)	0.42	0.40	0.48	0.34	0.68	0.64
	(0.49)	(0.49)	(0.5)	(0.47)	(0.47)	(0.48)
1(Discharged Home)	0.05	0.07	0.06	0.08	0.14	0.18
	(0.22)	(0.26)	(0.24)	(0.27)	(0.35)	(0.39)
N observations	35,843	43,623	14,044	25,432	16,659	25,650

Notes: Data from Florida Agency for Healthcare Administration Health Care inpatient LEJR claims from 2013Q1 – 2015Q2.

Table 2: Effect of Bundled Payment on LEJR Procedure Outcomes, by Payer

		Discharge Disposition for LEJR Procedures			
	LOS (days) (1)	SNF (2)	IRF (3)	Home Health (4)	Home (Routine) (5)
			A. Medicare FFS (N=195,021)		
1(CJR) x **1**(Post)	-0.0177	0.0120	-0.0202**	0.0221	-0.0189
	(0.0733)	(0.0185)	(0.00894)	(0.0236)	(0.0230)
			B. Medicare Advantage (N=110,397)		
1(CJR) x **1**(Post)	-0.0110	-0.00426	-0.0360***	0.0688***	-0.0292
	(0.0854)	(0.0198)	(0.0122)	(0.0232)	(0.0192)
			C. Commercial Health Insurance (N=110,418)		
1(CJR) x **1**(Post)	-0.0434	-0.0100	-0.00756*	0.0764**	-0.0606**
	(0.0632)	(0.0117)	(0.00392)	(0.0315)	(0.0295)

Notes: Data from 2013H1 to 2018H2. CJR equals one if the LEJR claim occurred in a hospital affected by the CJR program. Post equals one in 2015H2 and beyond. Each coefficient is a separate difference-in-difference regression, which includes hospital and half-year fixed effects. Standard errors are clustered at the hospital level.

Table 3: Summary Statistics for Non-LEJR Procedures In Pre-Treatment Period (2013Q1 – 2015Q2)

	Medicare		Medicare Advantage		Commercial	
	Control	Treatment	Control	Treatment	Control	Treatment
Patient Demographics						
Age	75.57	75.86	73.24	74.71	52.14	47.41
	(11.24)	(11.86)	(10.74)	(11.31)	(15.3)	(18.42)
1(White)	0.94	0.90	0.90	0.84	0.88	0.82
	(0.25)	(0.3)	(0.29)	(0.37)	(0.32)	(0.38)
1(Female)	0.65	0.65	0.64	0.66	0.53	0.50
	(0.48)	(0.48)	(0.48)	(0.47)	(0.5)	(0.5)
Number of other diagnoses	10.02	10.61	9.24	9.67	5.89	5.59
	(5.82)	(6.02)	(5.55)	(5.82)	(4.99)	(4.86)
Non-LEJR Outcomes						
LOS	4.46	4.94	4.51	4.98	3.49	3.75
	(3.8)	(4.31)	(3.98)	(4.13)	(3.71)	(4.06)
1(Discharged to SNF)	0.47	0.45	0.46	0.48	0.11	0.08
	(0.5)	(0.5)	(0.5)	(0.5)	(0.32)	(0.27)
1(Discharged to IRF)	0.11	0.12	0.04	0.06	0.05	0.05
	(0.31)	(0.33)	(0.2)	(0.25)	(0.22)	(0.22)
1(Discharged to Home Health)	0.23	0.23	0.28	0.24	0.29	0.28
	(0.42)	(0.42)	(0.45)	(0.43)	(0.45)	(0.45)
1(Discharged Home)	0.16	0.15	0.20	0.19	0.53	0.57
	(0.37)	(0.36)	(0.4)	(0.39)	(0.5)	(0.5)
N observations	30,037	43,020	10,634	23,386	12,418	25,821

Notes: Data are inpatient non-LEJR musculoskeletal and connective tissue claims from 2013Q1 – 2015Q2. Sample is restricted to surgeons who also perform LEJR procedures.

Table 4: Spillover Effect of LEJR Bundled Payment on Non-LEJR Procedures, by Payer

		Discharge Disposition for Non-LEJR Procedures			
	LOS (days)	SNF	IRF	Home Health	Home (Routine)
	(1)	(2)	(3)	(4)	(5)
			A. Medicare FFS (N=195,021)		
1(CJR) x **1**(Post)	0.0904	0.0244***	-0.00856	0.00814	-0.0241**
	(0.0802)	(0.00932)	(0.00931)	(0.00904)	(0.00965)
			B. Medicare Advantage (N=110,397)		
1(CJR) x **1**(Post)	0.123	0.0178*	-0.0191***	0.0199*	-0.0197
	(0.0902)	(0.0103)	(0.00682)	(0.0114)	(0.0125)
			C. Commercial Health Insurance (N=110,418)		
1(CJR) x **1**(Post)	0.0846	-0.00340	0.00188	0.0295**	-0.0280**
	(0.0880)	(0.00549)	(0.00421)	(0.0144)	(0.0134)

Notes: Data from 2013H1 to 2018H2, among physicians who perform both LEJR and related non-LEJR (i.e., MDC-8) procedures. CJR equals one if the non-LEJR claim occurred in a hospital affected by the CJR program. Post equals one in 2015H2 and beyond. Each coefficient is a separate difference-in-difference regression, which includes hospital, MS-DRG, and half-year fixed effects. Standard errors are clustered at the hospital level.

Supplemental Online Appendix

A. Details on Selection of Non-LEJR Cases

Our subset of non-LEJR cases includes all other Medicare-Severity Diagnosis Related Groups (MS-DRG) codes within the major diagnostic category (MDC) of musculoskeletal system and connective tissue diseases and disorders. Known as MDC-8, this group includes the two diagnosis codes—MS-DRGs 469 and 470—that were covered under the Medicare CJR bundled payment program.

In our sample of LEJR-surgeons who also performed non-LEJR procedures, all MDC groups are represented. However, as we show in Appendix Table A1, MDC-8 codes account for the majority of non-LEJR cases performed. Also of note, the discharge likelihoods of MDC-8 codes are most similar to LEJR codes.

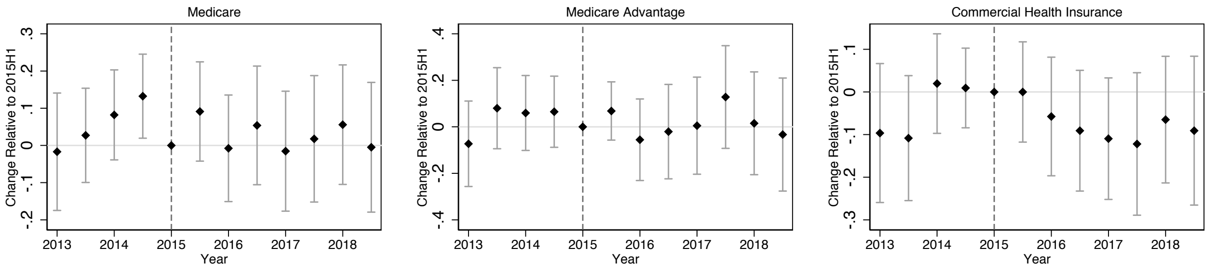
Appendix Table A1: Description of Non-LEJR Cases Performed by LEJR Surgeons

MDC	Description	Percent of Total Non-LEJR Cases (%)	Likelihood of Post-Acute Care
0	Ungroupable	3.66	0.53
1	Nervous System	3.95	0.42
2	Eye	0.04	0.31
3	Ear, Nose, Mouth, and Throat	0.31	0.3
4	Respiratory System	7.46	0.42
5	Circulatory System	13.29	0.38
6	Digestive System	8.98	0.29
7	Hepatobiliary System, Pancreas	2.65	0.21
8	**Musculoskeletal, Connective Tissue**	**36.08**	**0.71**
9	Skin, Subcutaneous Tissue, Breast	2.28	0.49
10	Endocrine, Nutritional, Metabolic	1.7	0.44
11	Kidney, Urinary Tract	4.05	0.43
12	Male Reproductive System	0.53	0.21
13	Female Reproductive System	0.35	0.14
14	Pregnancy, Childbirth, Puerperium	1.29	0.01
15	Newborn, Neonates	0.87	0
16	Blood, Immunological	2.45	0.27
17	Myeloproliferative	0.69	0.22
18	Infectious, Parasitic	5.6	0.52
19	Mental	0.3	0.16
20	Alcohol/Drug Use, Mental Disorders	0.29	0.18
21	Injuries, Poison	1.5	0.44
22	Burns	0.01	0.42
23	Health Status	0.78	0.6
24	Multiple Significant Trauma	0.72	0.71
25	HIV	0.17	0.32

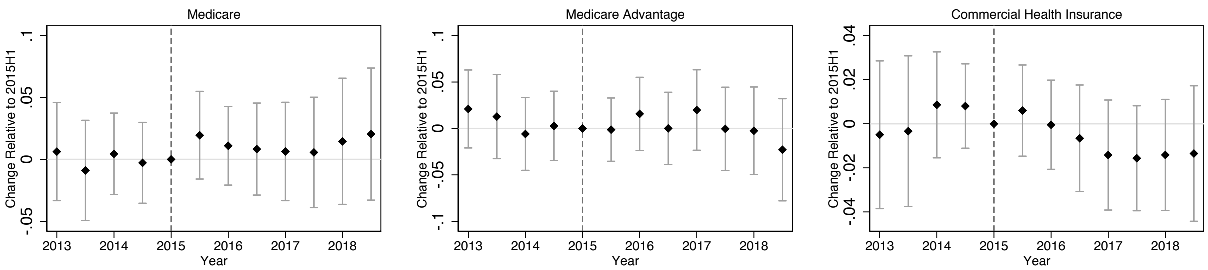
Notes: Data from 2013Q1 – 2015Q2 Florida inpatient claims data. We show the distribution of non-LEJR cases among surgeons who also perform LEJR cases.

Appendix Figure B1: LEJR Event Studies for Additional Outcomes of Interest

(a) Length of Stay



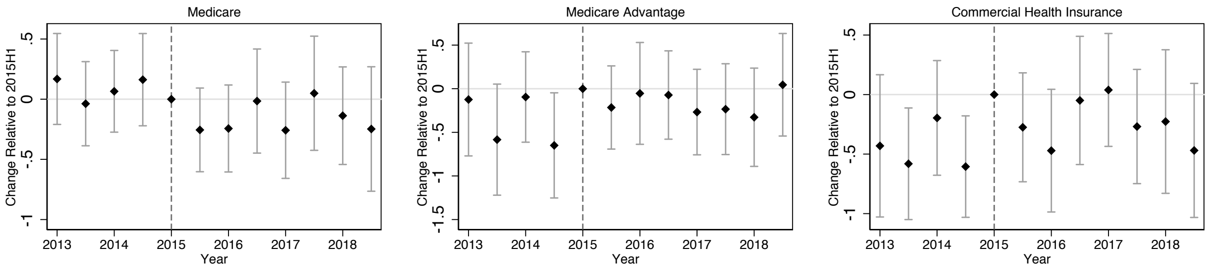
(b) Probability Discharged to a SNF



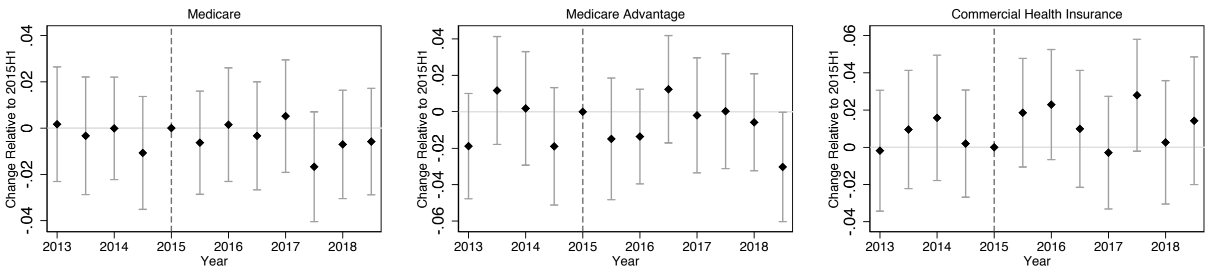
Notes: Each plot shows the event study coefficients and 95% confidence interval for outcomes pertinent to LEJR procedures. Sample includes physicians who perform both LEJR and related non-LEJR procedures (i.e., MDC-8). All changes are measured relative to 2015H1. Each regression additionally controls for patient demographics and hospital fixed effects. Standard errors are clustered by hospital. Dashed vertical line represents the six months immediately preceding the announcement of the CJR program and hospital assignment to the CJR program.

Appendix Figure B2: Event Study Patient Characteristics

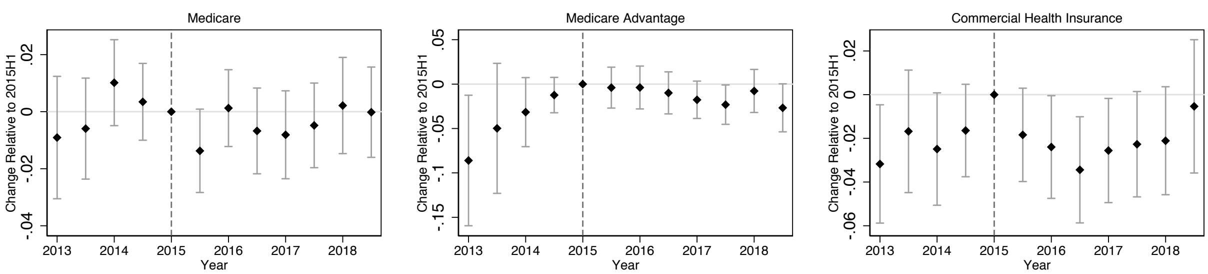
(A) Age



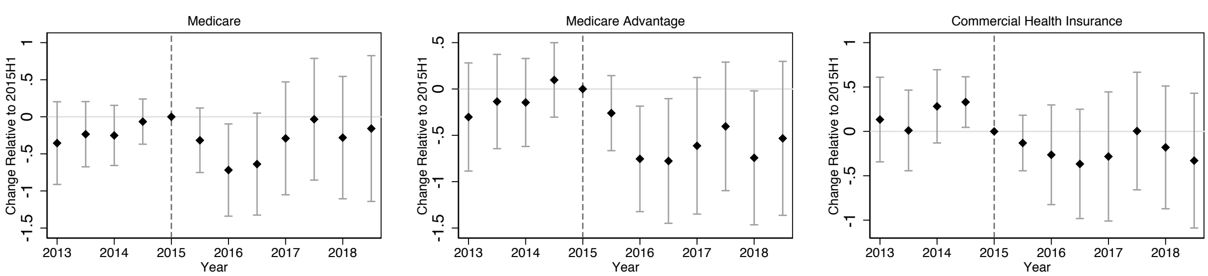
(B) Gender (female)



(C) Race (white)

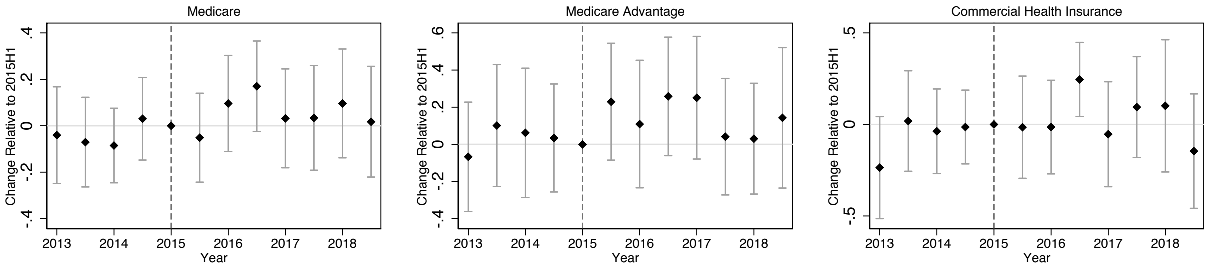


(D) Number of Other Diagnoses



Notes: Each plot shows the event study coefficients and 95% confidence interval for covariates of patients receiving LEJR procedures. All changes are measured relative to 2015H1. Each regression additionally controls for hospital fixed effects. Standard errors were clustered by hospital. Dashed vertical line represents the six months immediately preceding the announcement of the CJR program and hospital assignment to the CJR program.

Appendix Figure B3: Non-LEJR Event Studies for Length of Stay



Notes: Each plot shows the event study coefficients and 95% confidence interval for outcomes pertinent to non-LEJR procedures. Sample includes physicians who perform both LEJR and related non-LEJR procedures (i.e., MDC-8). All changes are measured relative to 2015H1. Each regression additionally controls for patient demographics and hospital fixed effects. Standard errors are clustered by hospital. Dashed vertical line represents the six months immediately preceding the announcement of the CJR program and hospital assignment to the CJR program.