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A Leak in the Lifeboat:

The effect of Medicaid managed care on the vitality of safety-net hospitals

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Abstract

States are increasingly adopting Medicaid managed care in efforts to address budgetary concerns. The intent is that by releasing Medicaid oversight to private organizations, competition will drive down healthcare expenditures so that savings may be passed to the state. Yet there are concerns that this competitive solution to cost savings might compromise safety-net hospitals. Managed care organizations cut costs by restricting the providers that enrollees are allowed to see. If movement in Medicaid patients disrupts safety-net hospitals' casemix, this could affect their ability to cross-subsidize care. This study estimates the impact of Medicaid managed care on safety-net hospitals by exploiting a Florida pilot program that required Medicaid recipients in five counties to enroll in managed care. The results suggest this mandate led to a small reduction in safety-net hospitals' average ratio of payment-to-cost. There is also some evidence that the effect on safety-net hospitals was disproportionate. This disproportionality was such that hospitals nearest the margin were pushed the furthest towards the edge.

Keywords

managed care; Medicaid; safety-net hospitals

1. Introduction

Safety-net hospitals play a vital role in the health system by providing disproportionate amounts of indigent care. Because of these populations' low reimbursement rates, safety-net hospitals operate with little financial leeway. Therefore, understanding the marginal impact of policies that might further reduce safety-net hospitals' bottom line is important to ensure these hospitals' viability.

One policy states are increasingly adopting is a mandate that Medicaid beneficiaries receive their coverage through managed care. This move to managed care has largely stemmed from efforts to reduce Medicaid spending. Under traditional Medicaid, the provision of public health insurance is coordinated by the state. Providers may choose to accept or decline Medicaid¹, and Medicaid beneficiaries are free to seek care from any provider willing to see them. Under Medicaid managed care, Medicaid coordination is outsourced to private

¹With the exception of emergency departments.

organizations who are frequently paid 95% of traditional Medicaid spending. However, with an incentive to generate profit, these organizations will attempt to trim spending by more. Managed care organizations do this by negotiating payment rates with providers and then imposing restrictions on which providers beneficiaries may see. As a result, the move from traditional Medicaid to Medicaid managed care breathes competition into the market but heightens the boundaries that restrict individuals' choice.

There have been concerns that states' adoption of Medicaid managed care may be weakening safety-net hospitals financially. If managed care organizations move enrollees to different settings of care, this could alter the composition of safety-net hospitals' caseloads. A disruption of safety-net hospitals' casemix could prevent their ability to cross-subsidize care. Yet, identifying the impact of Medicaid managed care on safety-net hospitals is difficult; states do not *at random* decide to provide managed care. Instead, the use of Medicaid managed care is correlated with other state characteristics which, on their own, might affect hospitals' finances. (Kim & Jennings, 2012) Moreover, there are individual characteristics which are associated with enrollment in Medicaid managed care. (Currie & Fahr, 2005) This selection makes it challenging to determine whether Medicaid managed care causes changes in hospitals' financial wellbeing, or whether observed changes are actually due to other factors unique to the regions/individuals choosing to participate. A further complication for current research is the fact that numerous healthcare reform “switches” are flipping in unison. In the United States, many of the Affordable Care Act's provisions are simultaneously hitting the market. Meanwhile, states, which are simultaneously being affected by federal reforms, are endogenously choosing if/how to reform their own Medicaid programs. This simultaneity makes it especially difficult to disentangle the causal effects of Medicaid managed care using data from recent years.

The experiment this study exploits to identify the impact of Medicaid managed care is the Florida Medicaid Reform Pilot Program. In 2006, the state of Florida began a comprehensive demonstration program that required Medicaid recipients living in five counties to enroll in managed care. For Medicaid recipients living in other counties, participation in managed care remained optional. This option had been allowed statewide prior to the demonstration. Florida's pilot program provides a useful setting to identify the effects of Medicaid managed care for two reasons. First, it was instituted in five counties within a single state. In this way, all state-level factors (which affect Medicaid provision, among other things) remained constant across reformed and non-reformed regions. Second, it occurred prior to the Affordable Care Act. This makes the view of Medicaid managed care less clouded by concurrent state and federal reforms.

Using administrative records on 29 million hospitalizations in Florida from 2000 through 2012, I employ difference-in-differences estimation to measure the direct and disproportionate impact on safety-net hospitals' average ratio of payment-to-cost. The difference-in-differences design is useful in that it allows me to “subtract out” secular shifts in hospitals' payment-to-cost ratios which come from secular shifts in hospitals' casemix. Importantly, patients with different insurances provide hospitals with different levels of reimbursement. Hence, population-level trends in insurance coverage independently influence hospitals' payer-mix, and subsequently their average payment-to-cost. These

trends, however, should not systematically vary across the reform's geographical boundaries. Thus, I subtract the observed (secular) change in *non*-reform counties' average payment-to-cost ratio from the change in reform counties' payment-to-cost ratio to isolate the impact of the reform.

The findings of this study suggest that Florida's expansion of Medicaid managed care weakened safety-net hospitals financially. These hospitals' average ratio of payment-to-cost fell by an estimated 1.6 percentage-points. There is also some evidence that the effect on safety-net hospitals was disproportionate. This effect heterogeneity was such the gap in safety-net hospitals' financial vulnerability relative to the rest of the hospital system widened as a result of the Medicaid managed care mandate.

To maintain a viable hospital system, state policymakers should exercise caution before moving forward with Medicaid managed care. The Affordable Care Act has prescribed cuts to safety-net hospitals' subsidies which are set to take effect in 2018². If state-level reforms preemptively position safety-net hospitals nearer the margin, then the total effect could be severe.

The remainder of the paper proceeds as follows. Section 2 provides background information on Medicaid managed care and the pilot program this study considers. The data source and sample construction are described in Section 3. Sections 4 and 5 provide explanations of the identification and estimation procedures. Results are presented in Section 6 and discussed in Section 7. Section 8 ends with concluding remarks.

2. Background

2.1. Medicaid Managed Care and the Safety-Net

Since the 1990s, the use of managed care has grown in Medicaid. Currently, all U.S. states, with the exception of Alaska and Wyoming, offer some form of Medicaid managed care. Participation in managed care among the Medicaid population ranges from approximately 50-100% across states. (Medicaid, 2011) Spawning the growth in this type of Medicaid provision have been the arguments of cost savings and increased access to healthcare.

Traditional (fee-for-service) Medicaid finances enrollees' healthcare by offering to pay providers pre-specified amounts in exchange for particular medical services. Medicaid beneficiaries may seek care from any provider willing to provide their services in exchange for Medicaid's fee-for-service rate. Once the service is performed, money flows directly from the state to the provider. There are minimal instituted barriers to care.

Under Medicaid managed care, states contract managed care organizations to coordinate enrollees' healthcare and to provide reimbursement to providers. In return for this work, states typically pay managed care organizations using a capitated rate. This structure provides states with cost predictability. Additionally, because capitated rates are frequently

²Per the Medicare Access and CHIP Reauthorization Act of 2015.

set 5% below traditional fee-for-service rates, this provides an opportunity for states to reduce Medicaid spending.

In order for managed care organizations to be able to turn a profit with 95% of fee-for-service rates without reducing service quality, these organizations must manage Medicaid enrollees' healthcare consumption in a very efficient manner. (Grogan & Patashnik, 2003) In practice, this means directing Medicaid enrollees away from unnecessarily expensive forms of care. Managed care organizations achieve this feat by negotiating what they deem are acceptable reimbursement rates with providers and then facilitating Medicaid enrollees' movement towards these providers using in-network lists. Not only does this framework allow managed care organizations to lock-in favorable prices (Cutler et al., 2000), but it also gives managed care organizations leverage in dissuading inefficient types of healthcare consumption (e.g., unnecessary inpatient stays and emergency room visits).

On top of being financially preferable to states, Medicaid managed care has been touted for its potential to expand the set of doctors available to Medicaid enrollees. Because of the low reimbursement rates paid by traditional Medicaid programs, the number of providers willing to accept Medicaid patients was/is particularly limited. Consequently, if managed care organizations are able to negotiate contracts with providers which guarantee that the provider will treat Medicaid patients in return for the managed care organization's negotiated reimbursement rate, then this could possibly increase enrollees' access to care.

Although the presumed consequences of Medicaid managed care were optimistic when the program was initially established, there was concern that safety-net providers might be harmed. (Baxter & Mechanic, 1997; Gold, 1999; Hurley & Somers, 2003; Lipson & Naierman, 1996; Sparer & Brown, 2000) The reason for this concern centered around the notion that Medicaid managed care might change the composition of safety-net providers' caseloads (and thereby their sources of funding) such that it would reduce these providers' ability to cross-subsidize care. During the late 1990s and early 2000s, a number of case studies were conducted to measure the impact on safety-net facilities. (Gold et al., 1996; Grogan & Gusmano 1999; Holahan et al., 1998; Horton et al., 2001) These studies generally reported adverse effects. Consistent with financial strain on safety-net providers, other studies reported declines in access to care among uninsured individuals following the implementation of Medicaid managed care. (Cunningham 1999; Waitzkin et al., 2002) In a notable exception, however, it was reported in one study that, "safety-net providers are coping". (Haberer et al., 2005)

Since the mid-2000s, there has been a relative lull in the literature concerning the effects of Medicaid managed care on safety-net hospitals, with a complete absence in the field of economics. This study revisits the issue for two reasons. First, the literature continues to lack strong empirical evidence of the effects of Medicaid managed care on safety-net hospitals' financial condition. Recent studies within economics have challenged the claims that Medicaid managed care reduces spending, heightens enrollees' access to care, and promotes physical health. (Aizer et al., 2007; Duggan, 2004; Duggan & Hayford, 2013; Herring & Adams, 2011) Hence, it not implausible that this type of Medicaid provision is adversely impacting hospitals. Second, if Medicaid managed care does in fact harm safety-net

hospitals, then there is a renewed importance in determining the magnitude of the effect given upcoming federal reform.

The U.S. federal government currently provides safety-net hospitals with over \$11.6 billion in Medicaid Disproportionate Share Hospital Payments each year. (Federal Medicaid, 2014) These funds are provided to hospitals with large shares of uninsured and Medicaid patients to off-set uncompensated care. The Affordable Care Act (ACA) has prescribed cuts to Medicaid Disproportionate Share Hospital Payment which are scheduled to take effect in 2018.³ (Medicare Access, 2015) Importantly, these cuts are expected to heighten safety-net hospitals' financial vulnerability. Additionally, because these cuts will be felt almost exclusively by hospitals *inside* the safety-net, it is also likely that they will widen the gap that separates safety-net hospitals' financial vulnerability from that of the rest of the hospital system. Consequently, if the viability of safety-net hospitals is to be ensured, then it is important that state policymakers understand the marginal impact of instituting Medicaid managed care before moving forward with further implementation. This is so that the totality of state and federal reforms will not inadvertently push safety-net hospitals too far.

2.2. Florida's Medicaid Reform Pilot Program

The policy change this study exploits to determine the impact of Medicaid managed care is Florida's Medicaid Reform Pilot Program. Five years prior to the enactment of the ACA, on October 19, 2005, Florida was granted permission by the Centers for Medicare and Medicaid Services to launch a comprehensive demonstration which would require the majority of Medicaid enrollees living in particular counties to receive their health insurance through a managed care organization. (Medicaid Reform, 2005) Although participation in Medicaid managed care was, at the time, optional for Medicaid enrollees state-wide, this mandate would bring a substantial number of Medicaid enrollees with traditional (fee-for-service) Medicaid coverage under the umbrella of managed care.

In Florida, the mandatory Medicaid managed care organizations took the form of either Medicaid Health Maintenance Organizations (HMOs) or Provider Service Networks (PSNs). The PSNs (networks unique to Florida) differed from the commercially licensed/contracted HMOs in that the PSNs were directly controlled and operated by Florida physicians. These organizations, which were largely formed around safety-net physicians, were primarily instituted as a concession to lobbyists who believed that Florida safety-net providers would be harmed by the Medicaid managed care mandate. Additionally, unlike the HMOs (which were only reimbursed by the state using a capitated rate), the PSNs could be reimbursed using either a fee-for-service or capitated rate. In practice, the PSNs widely favored fee-for-service.⁴ For intuition, Florida's PSNs strongly resembled Accountable Care Organizations (ACOs).

³Under the ACA, Medicaid Disproportionate Share Hospital (DSH) payments were originally set to be reduced by \$500M in FY2014, \$600M in FY2015 and FY2016, \$1.8B in FY2017, \$5B in FY2018, \$5.6B in FY2019, and \$4B in FY2020. Medicaid DSH cuts have since been delayed. In the revised schedule outlined in the Medicare Access and CHIP Reauthorization Act of 2015, Medicaid DSH allotments will be reduced by \$2B in FY2018, \$3B in FY2019, \$4B in FY2020, \$5B in FY2021, \$6B in FY2022, \$7B in FY2023, and \$8 in both FY2024 and FY2025.

⁴PSNs' fee-for-service rates were originally set to phase into capitated rates at a designated time. However, this change never occurred due to strong lobbying.

On July 1, 2006, the Florida Medicaid Reform Pilot Program took effect in Broward and Duval counties. The following year, on July 1, 2007, the pilot program expanded to include Baker, Clay and Nassau counties. The first two counties consisted of urban areas, including the cities of Fort Lauderdale and Jacksonville. The three expansion counties consisted of primarily rural areas surrounding Jacksonville. The location of the reform counties is shown in Figure 1.

Consistent with the federal standard for managed care, each of the reform counties offered their Medicaid enrollees at least two insurance plans to choose between. At the time of pilot program's launch, Medicaid enrollees living in Broward county (population ~1.75M) were allowed to choose between a set of ten HMOs and five PSNs, enrollees in Duval county (population ~0.85M) were allowed to choose between a set of four HMOs and three PSNs, and enrollees in Baker, Clay, and Nassau counties (the rural counties surrounding Duval) could each choose between one HMO and one PSN.⁵ (Bragdon, 2011) If an individual failed to select a plan, then he/she was automatically assigned one by Florida's Agency for Health Care Administration. For families with incomes below the Temporary Assistance for Needy Families (TANF) eligibility limit (i.e., 23% of the federal poverty line) with assets less than \$2,000, children (depending on age) whose family's income was <200% of the federal poverty line, and Medicaid beneficiaries receiving Supplemental Security Income, enrollment in one of the managed care plans was mandatory. According to the initial Medicaid reform application, managed care participation would also be required for the individuals eligible for both Medicare and Medicaid, "upon the development and inclusion of an integrated service delivery system for individuals aged 60 and older." Participation in managed care was optional for other Medicaid enrollees living within reform counties, as it was in the rest of the state.

The Florida Medicaid Reform Pilot Program continued to operate until August 1, 2014. Florida has since chosen to mandate Medicaid managed care statewide. A question which has remained unanswered, however, is if/how the expansion of Medicaid managed care affected safety-net hospitals financially. In an attempt to answer this question, I examine the impact of the Florida Medicaid Reform Pilot Program on safety-net hospitals' average payment-to-cost and compare it to the impact on other hospitals.

3. Data

The data used in this study come from the Agency for Health Care Administration, Florida Center for Health Information and Policy Analysis, and consists of individual records on all hospital discharges in the state of Florida from 2000 through 2012. Included in the dataset are patients' discharging facility, year and quarter of discharge, county of residence, payer source, gross charges, and information related to patients' conditions and diagnoses. Because safety-net status is not explicitly defined, I manually assign safety-net status to observations by matching discharging facilities to the list of hospitals that were members of the Safety

⁵Over time, the list of Medicaid HMOs and PSNs available to Medicaid enrollees fluctuated. There were also changes in enrollment breakdowns, with increasing relative enrollment in PSNs. Additionally, an "Opt Out" program was made available so that individuals could use (what would have been) the dollar value of their Medicaid benefits to enroll in employer-provided coverage. There was little participation in this program.

Net Hospital Alliance of Florida in 2014.⁶ The entire sample consists of 32,705,130 observations.

To facilitate estimation, two restrictions are made to the full sample. First, individuals whose primary expected source of payment was not either private insurance, Medicare, Medicaid (traditional and managed care), or “uninsured” are omitted.⁷ Second, only individuals who can be identified as Florida residents are retained.⁸ The remaining sample consists of 29,927,715 observations.

The primary question of interest in this study is: *Did Florida's Medicaid managed care mandate change safety-net hospitals' average ratio of payment-to-cost?* Importantly, a hospital's average payment-to-cost is a function of the hospital's casemix. Patients with different sources of payment (e.g., private insurance, Medicare, Medicaid, self-pay) provide hospitals with different levels of reimbursement. On average, private insurers reimburse hospitals most generously, followed by Medicare, Medicaid, and self-pay.

While the Florida data do not include payment-to-cost ratios for individual hospitalizations, they do include hospital identifiers and patients' principal source of payment. Hence, I can construct proxies for hospitals' average payment-to-cost using information on hospitals' payer-mix at particular points in time. I do this by assigning expected payment-to-cost ratios to individual hospitalizations and then averaging across hospitals' caseload. By doing this at different times within the same hospitals, I can obtain a series of snapshots to understand how given hospitals are affected. Again, while these averages are *proxies*, they are useful in that they allow me to measure the direction hospitals' payment-to-cost ratios move.

The values I assign to individual hospitalizations are based on national statistics. Specifically, for patient “i”, hospitalized in year “y”, with payer source “p”, the expected payment-to-cost ratio I assign to that observation is the mean payment-to-cost ratio across all U.S. hospitalizations covered by payer source “p” in year “y”. For hospitalizations covered by Medicaid, Medicare or private insurance, these statistics come from the American Hospital Association. (Trendwatch, 2015) The American Hospital Association does not supply information on payment-to-cost ratios for the uninsured. Hence, I rely on information from the Kaiser Family Foundation to assign payment-to-cost ratios to uninsured hospitalizations. (Coughlin et al., 2014) Due a lack of yearly information, I derive a single payment-to-cost ratio for the uninsured using information on aggregate healthcare payments and costs among the uninsured in 2013.⁹ I then use this ratio for all uninsured hospitalizations across the years 2000-2012.

The payment-to-cost ratios used in this study are intended to reflect aggregate payments made to the hospital. For Medicaid and Medicare encounters, this includes fill-in money from Disproportionate Share Hospital Payments. For uninsured encounters, this includes fill-in money from Disproportionate Share Hospital Payments in addition to funding from the

⁶<http://safetynetsflorida.org/> (Accessed February 2014)

⁷Other sources of payment account for 5.2% of the full sample.

⁸Individuals without a recorded Florida county of residency account for 3.5% of the full sample.

⁹A limitation of these uninsured statistics is that the payment and cost levels provided by the Kaiser Family Foundation include, but are not specific to, hospitals.

Veterans Administration, state/local programs, community health centers, Indian Health Service, the Ryan White Care Act, and Maternal and Child Health Title V Block Grants. A complete list of the payment-to-cost ratios I use are provided in Table 1.

A pre-reform comparison of the hospitalizations within safety-net and non-safety-net hospitals is provided in the top panel of Table 2. As might be expected, the average ratio of payment-to-cost was lower among hospitals inside than outside the safety-net (101.9% compared to 102.6%). Consistent with this disparity, the composition of safety-net hospitals' caseload was noticeably different. Outside the safety-net, roughly one in seven hospitalizations was covered by Medicaid and roughly one in twenty hospitalizations was uninsured. Inside the safety-net, approximately one in four hospitalizations was covered by Medicaid; approximately one in fifteen hospitalizations was uninsured.

Also provided in Table 2 is a pre-reform comparison of the hospitalizations among reform versus non-reform county residents. Residents of the reform counties varied slightly from residents of other counties in terms of their sources of payment. Among reform county residents, a relatively higher share of hospitalizations were covered by private insurance and a relatively lower share were covered by Medicare. Approximately 23% of the hospitalizations among reform county residents were covered by no insurance or Medicaid. This share was similar among Floridians living elsewhere.

The bottom panel of Table 2 provides a post-reform comparison of hospitals' casemix. As shown, there appear to be strong secular trends in hospitals' casemix. In both reform *and* non-reform counties, hospitals' share of privately insured encounters is noticeably smaller after the reform and the share of Medicaid encounters noticeably larger. To illuminate the underlying mechanism driving these changes, Figure 2 displays the trends in hospitals' volume of encounters across different payer groups. Over time, there is a slight overall downward trend in the volume of privately insured hospital visits and an upward trend in the volume of Medicaid visits. It also appears as though the growth in Medicaid hospitalizations among reform county residents does not keep pace with that in non-reform counties. This provides some suggestive evidence that the Florida Medicaid Reform Pilot program might have generated unique leverage on hospitals' average payment-to-cost. The following sections explore this possibility further.

4. Identification Strategy

To understand the impact of Florida's Medicaid Reform Pilot Program on safety-net hospitals, there are two types of effects I wish to identify. First, I would like to identify the extent to which safety-net hospitals were directly affected. Second, I would like to identify the extent to which safety-net hospitals were disproportionately affected.

To motivate this dual identification, it is important to reiterate that safety-net hospitals are dissimilar from other hospitals in terms of their financial vulnerability. While safety-net hospitals operate near the margin, other hospitals operate a further distance away. A clearly important question is whether safety-net hospitals' financial vulnerability increases or decreases relative to its initial position (i.e., the direct effect). A similarly notable outcome is

whether safety-net hospitals' dissimilarity becomes more or less pronounced. If there is no disproportionate effect on safety-net hospitals, then the gap between safety-net and other hospitals' average ratio of payment-to-cost will remain unaffected. If, however, the impact on safety-net hospitals is disproportionate, then the gap will widen or narrow.

To identify the direct effect of Medicaid managed care on safety-net hospitals, I use a standard difference-in-differences approach. This identification strategy exploits the existence of individuals who were unaffected by the Florida reform in the post-intervention period. To ensure that a one-to-one comparison of the treatment and control groups in the post-intervention period is not biased by an underlying dissimilarity between groups, it subtracts from the two groups' post-intervention difference in outcomes their difference before intervention. Alternatively, to ensure that a pre/post comparison within the treatment group is not biased by a simultaneous external shock, it subtracts from the treatment group's pre/post difference in outcomes any pre/post difference in the control group.

In the present study, the treatment group consists of individuals who were residents of a reform county at the time of their hospitalization. The control group consists of hospital patients who lived in any other Florida county. The pre-intervention period includes quarters before the pilot program's implementation; the post-intervention period includes quarters after. The dependent variable, y , is a given patient's expected payment-to-cost. Hence the average y across a subset of patients, $E(y)$, corresponds to the average payment-to-cost across that particular casemix. In a simple two-period model where residence in a reform county is denoted R and the post-reform period is denoted P , the difference-in-differences effect can be expressed as follows:

$$\begin{array}{c}
 \text{Within Safety-Net Hospitals} \\
 \begin{array}{cc}
 \text{Difference between} & \text{Difference between} \\
 \text{reform and non-reform county residents,} & \text{reform and non-reform county residents,} \\
 \text{post-reform} & \text{pre-reform}
 \end{array} \\
 \begin{array}{cc}
 \underbrace{\hspace{10em}} & \underbrace{\hspace{10em}} \\
 [E(y|R=1, P=1) - E(y|R=0, P=1)] & - [E(y|R=1, P=0) - E(y|R=0, P=0)]
 \end{array}
 \end{array}
 \tag{1}$$

The resultant estimate is interpreted as the direct effect of the Florida Medicaid Reform Pilot Program on safety-net hospitals' y . That is, their average ratio of payment-to-cost.

To identify the disproportionate effect of Medicaid managed care on safety-net hospitals, I can use the structure of equation (1) to construct an analogous difference-in-differences estimate among only non-safety-net hospitals. This leaves me with one estimate for the direct effect of the Medicaid Reform Pilot Program on safety-net hospitals' average payment-to-cost ($DD_{SN=1}$) and one estimate for the direct effect of the Medicaid Reform Pilot Program on other hospitals' average payment-to-cost ($DD_{SN=0}$). I can then subtract $DD_{SN=0}$ from $DD_{SN=1}$ to determine whether any of the effect felt by safety-net hospitals was exclusive to these facilities. Again, in a simple two-period model where SN now represents hospitalization in a safety-net, this can be expressed as follows:

$$\begin{array}{c}
 \text{Within Safety-Net Hospitals} \\
 \begin{array}{cc}
 \text{Difference between} & \text{Difference between} \\
 \text{reform and non-reform county residents,} & \text{reform and non-reform county residents,} \\
 \text{post-reform} & \text{pre-reform}
 \end{array} \\
 \begin{array}{cc}
 \{[E(y|R=1, P=1, SN=1) - E(y|R=0, P=1, SN=1)] - [E(y|R=1, P=0, SN=1) - E(y|R=0, P=0, SN=1)]\} & \\
 - & \\
 \{[E(y|R=1, P=1, SN=0) - E(y|R=0, P=1, SN=0)] - [E(y|R=1, P=0, SN=0) - E(y|R=0, P=0, SN=0)]\} & \\
 \begin{array}{cc}
 \text{Difference between} & \text{Difference between} \\
 \text{reform and non-reform county residents,} & \text{reform and non-reform county residents,} \\
 \text{post-reform} & \text{pre-reform}
 \end{array}
 \end{array} \\
 \text{Within Non-Safety-Net Hospitals}
 \end{array}$$

(2)

If the resultant difference in equation (2) is zero, this will suggest that the Florida Medicaid Reform Pilot Program had no disproportionate effect on safety-net hospitals. If the resultant difference is not zero, this will indicate a disproportionate effect on safety-net hospitals. For the measures in which safety-net hospitals lag behind other hospitals (e.g., ratio of payment-to-cost), a positive value will indicate that safety-net hospitals caught up, narrowing the gap in y . A negative value will indicate that safety-net hospitals fell further behind, elongating the gap in y .

For clarity, it should be noted that identification in equation (2) is not based on triple-difference estimation, although the equation is structurally identical. This is because triple-difference estimation exploits three dimensions of control. Here, I exploit only two dimensions of control: in the time dimension, the pre-reform period, and in the space dimension, residency in a non-reform county. The third dimension, hospital type, offers no control group. Both safety-net and non-safety-net hospitals were immune to treatment from the Florida reform. Consequently, identification remains rooted in double-difference estimation, and the introduction of the third dimension is used for an alternative purpose. That is, to change the effect that is being identified, not the source of identification.

The validity of the difference-in-differences design relies on the assumption of “parallel worlds”. While this assumption cannot be formally test, as there is a counterfactual we cannot observe, plotting the trends between the treatment and control groups is useful to gauge the plausibly that this assumption is satisfied. In this spirit, Figure 3 displays safety-net and other hospitals' average ratio of payment-to-cost over time. Within safety-net hospitals, this ratio appears to diverge between the treatment and control groups around the time of the intervention, but the treatment and control groups' ratios track fairly consistently before and after the split at the intervention threshold. This visual representation of the reform's effects provides cursory evidence that difference-in-differences estimation is a valid approach for identifying the impact of treatment.

5. Estimation

The standard regression forms of equations (1) and (2) are respectively specified as follows:

$$y = \alpha_0 + \alpha_1 R + \alpha_2 P + \alpha_3 (R * P) + \varepsilon \quad (3)$$

$$y = \beta_0 + \beta_1 R + \beta_2 P + \beta_3 SN + \beta_4 (R * P) + \beta_5 (R * SN) + \beta_6 (P * SN) + \beta_7 (R * P * SN) + \mu \quad (4)$$

When equation (3) is estimated on the sample of only encounters at safety-net hospitals, the coefficient α_3 will align with difference in conditional expectations given by equation (1). When equation (3) is estimated on the pooled sample of encounters at all hospitals, the coefficient β_7 will align with the difference in conditional expectations given by equation (2).

In the present study, the multi-period implementation of the Medicaid Reform Pilot Program complicates the definition of P in the generic equations (3) and (4). This is because P switches on at different times in the reform counties, making no clear switching threshold for non-reform counties. To address this, I estimate amended versions of equations (3) and (4) in which P is replaced by a vector of quarter-of-discharge dummy variables (Q) any time the “post” term is independent of R . In addition to capturing the multi-period implementation of the reform, this “roll-out” of treatment aids with identification as the treatment threshold is now independent of a single point in time. This method of handling the “post” variable also accounts for time trends, which are likely to emerge across a period of 13 years. This yields the following specifications:

$$y = \delta_0 + \delta_1 R + \delta_2 Q' + \delta_3 (R * P) + \zeta \quad (5)$$

$$y = \rho_0 + \rho_1 R + \rho_2 Q' + \rho_3 SN + \rho_4 (R * P) + \rho_5 (R * SN) + \rho_6 (Q' * SN) + \rho_7 (R * P * SN) + \lambda \quad (6)$$

The dependent variable, y , is the expected ratio of payment-to-cost for the hospitalization. Again, R is a binary variable indicating residency in a county that was reformed in either 2006 or 2007, Q is a vector of quarter-of-discharge indicators, and SN is a binary variable indicating discharge from a safety-net hospital. Similar to before, the coefficient δ_3 represents the direct effect of the Florida reform on safety-net hospitals' average ratio of payment-to-cost across their caseload. The coefficient ρ_7 represents the disproportionate effect of the Florida reform on safety-net hospitals' average ratio of payment to cost across their caseload.

For all regressions, standard errors are clustered by patients' county of residence. This is to ensure that the precision of the difference-in-differences estimates is leveraged by the finer units of observation beneath the level in which the policy variation occurs. There are 67 counties in Florida.

6. Results

6.1. Main Estimates

Presented in Table 3 are the main difference-in-differences estimates for the effects of the Florida reform. Again, a graphical depiction of the changes in hospitals' payment-to-cost ratios is provided in Figure 3. These each provide suggestive evidence that the expansion of Medicaid managed care heightened safety-net hospitals' financial vulnerability.

The difference-in-differences point-estimate for the direct effect on safety-net hospitals is statistically significant and negative. This point-estimate indicates that safety-net hospitals' average ratio of payment-to-cost fell by 1.6 percentage-points, consistent with the timing and location of the Florida reform. Similarly, Figure 3 is suggestive of an adverse effect. While payments to safety-net hospitals from reform county residents generally exceeded costs before the expansion of Medicaid managed care, payments began to dip below costs around the time of the pilot program's implementation.

The difference-in-differences point-estimate for the disproportionate effect on safety-net hospitals is also statistically significant and negative. This point-estimate indicates that the average ratio of payment-to-cost fell by an estimated 1.8 percentage-points more among the hospitals inside the safety-net than among the hospitals outside the safety-net. The magnitude of this estimate implies that the financial strain that was generated by the expansion of Medicaid managed care was felt exclusively by safety-net hospitals. The trends shown in Figure 3 are consistent with this result. As shown, the disproportionate effect on safety-net hospitals was such that gap separating safety-net hospitals' financial vulnerability from other hospitals' vulnerability widened in response to the Medicaid managed care mandate.

6.2. Robustness Checks

A possible concern which might arise upon inspection of Figure 3 is that there is a noticeable dip in the outcome among safety-net hospitals serving “treated” patients in the first quarter of 2012. To verify this outlier is not driving the results, I cut the data after December 31, 2011 and repeat the estimation for only hospitalizations between 2000 and 2011. The results following this exclusion are presented in the first column of Table 4. As shown, the results are largely robust to the omission of 2012 data. This suggests that the 2012 Q1 payment-to-cost outlier did not independently generate the estimated (disproportionate) reduction in safety-net hospitals' average payment-to-cost.

To further test the robustness of this study's findings, I again repeat the estimation, but weight all regressions by the total gross charge for the hospitalization. Importantly, the main results reflect the impact on an average *per hospitalization*. If costs were systematically higher within particular payer groups, then the average per hospitalization will not align with the average *per \$1 of cost*. While weighting observations by the cost of care would be ideal, cost information is not provided in the Florida dataset. I therefore weight all regressions by charges, a noisy proxy for cost. As shown in the second column of Table 4, the results are also robust to this adjustment. Weighted regressions generate difference-in-differences point-

estimates that differ from previous estimates by no more than 0.3 percentage-point. All effects move in the same direction.

It is possible that the national payment-to-cost ratios imputed for particular payer-groups in the main analysis may not generalize to Florida. If this is the case, then the above estimates may misstate the impact of the Florida reform. To gauge whether this is the case, I relax the previous specific payment-to-cost assumptions and instead rank individual hospitalizations according to the overall generosity of the principal payer. Hospitalizations covered by self-payment are assigned as score of 1, hospitalizations covered by Medicaid are assigned a score of 2, hospitalizations covered by Medicare are assigned a score of 3, and hospitalizations covered by private insurance are assigned a score of 4. I then repeat the estimate using this ranking as the dependent variable. This allows me to measure whether the reform had any effect on the average payer-source tier across safety-net hospitals' caseload. We would expect negative coefficients to correspond to narrower hospital margins. As shown in the third column of Table 4, safety-net hospitals' average payer-source tier falls by an estimated 0.077 levels, consistent with the timing and location of the reform. This suggests a direct negative effect on safety-net hospitals' margins. The estimate for the disproportionate effect on safety-net hospitals is also negative, but is statistically insignificant.

As a final check of robustness, I again repeat the estimation, but change the dependent variable to an indicator for a "high payer". Again, this robustness check is to assess the sensitivity of the results to the payment-to-cost ratios imputed in the main analysis. If high-payers provide relatively higher payment-to-cost ratios, then a larger share of high-payers in a hospital's casemix will likely correspond to decreased financial strain on the hospital. I define high-payers using three alternative cuts:

- i. patients with private insurance (as opposed to Medicare/Medicaid/uninsured)
- ii. patients with private insurance/Medicare (as opposed to Medicaid/uninsured)
- iii. patients with private insurance/Medicare/Medicaid (as opposed to uninsured)

The results using this outcome generally conform with previous findings. As shown in the final columns of Table 4, the difference-in-differences estimates for the direct effect on safety-net hospitals consistently indicate that the reform decreased safety-net hospitals' average share of high-payers across all definitions of the variable. However, the difference-in-differences estimates for the disproportionate effect on safety-net hospitals are imprecise. For the one high-payer classification that generates a statistically significant point-estimate at the 90% confidence level, it is estimated that safety-net hospitals' share of high-payers decreased by disproportionately more than in other hospitals.

7. Discussion

The principal finding of this study is that the use of Medicaid managed care increases safety-net hospitals' financial vulnerability. This form of Medicaid provision is estimated to reduce safety-net hospitals' average ratio of payment-to-cost. There is also some evidence that it

exacerbates the gap that separates safety-net hospitals' vulnerability from the rest of the hospital system.

There are certain limitations the reader should bear in mind when interpreting the results of this study. First, it is important to note that these results are specific to one state's implementation of Medicaid managed care. One state's implementation of Medicaid managed care will not perfectly align with all others'. For the reader interested in generalizing these results to other settings, it is important to carefully consider the similarities between the comparator's hospital system and Medicaid program and those of Florida at the time of reform. Additionally, it is important to point out that the dependent variable in the main analysis is constructed using national payment-to-cost ratios for various payer groups. Assigning these mean values to individual hospitalizations in the Florida dataset introduces some level of fuzziness in the estimation. Furthermore, these mean values must be consistent with Florida patients' (unobserved) payment-to-cost ratios for the findings of this study not to be compromised. While testing this assumption is not feasible with the data at hand, alternative measures of financial stability consistently point towards the conclusion that safety-net hospitals were financially weakened as a result of Florida's Medicaid managed care mandate. Finally, I cannot exclude the possibility that reform counties systematically differed from non-reform counties in such a way that the hospitals their residents visited would have experienced a unique change in their payment-to-cost regardless of the Medicaid managed care mandate.

In spite of these limitations, however, this study provides a needed contribution to the economic literature. The viability of safety-net hospitals is fundamental to the delivery of healthcare. Yet the impact of Medicaid managed care on these hospitals has received little attention. The medical literature has highlighted potential reasons for concern, but these concerns have not yet been supported by strong evidence. Therefore, states continue to move forward with Medicaid managed care in seeming isolation of upcoming cuts to safety-net hospitals' subsidies. If this move is leaving safety-net hospitals in a weakened condition, then state policymakers should be aware. Further investigation is needed.

8. Conclusion

With high levels of uncompensated care, safety-net hospitals are particularly sensitive to healthcare reforms that may threaten their bottom line. The federal government has recently prescribed cuts to Medicaid Disproportionate Share Hospital payments, which are used to subsidize safety-net hospitals. In seeming isolation, states are increasingly adopting Medicaid managed care in attempts to reduce Medicaid spending. There has been speculation that this form of Medicaid provision is nudging safety-net hospitals nearer the edge, but strong empirical evidence is lacking.

This study uses a quasi-experimental design to examine the impact of Medicaid managed care on the financial wellbeing of safety-net hospitals. In 2006, the state of Florida began an experiment in which they required Medicaid recipients living in five counties to enroll in managed care. Using 2000-2012 Florida hospital discharge data, I employ difference-in-differences estimation to measure the consequences of this reform.

The results suggest that Florida's Medicaid managed care mandate led to a reduction in safety-net hospitals' average ratio of payment-to-cost. Moreover, there is some evidence that the impact on safety-net hospitals was disproportionate, exacerbating safety-net hospitals' financial inequality. Given upcoming cuts to safety-net hospitals' subsidies, state policymakers should carefully consider the marginal impact of instituting Medicaid managed care within the context of their own states. If the marginal impact is sufficiently adverse, then the totality of state and federal reform could reduce vulnerable populations' access to care even more by compromising safety-net hospitals.

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- Table 4.4: Aggregate Hospital Payment-to-Cost Ratios for Private Payers, 1993-2013. , editor. Trendwatch Chartbook 2015. American Hospital Association; 2015. Available from: <http://www.aha.org/research/reports/tw/chartbook/2015/table4-4.pdf>
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Figure 1. Map of Reform Counties

Figure 2. Trends in Hospital Volumes

Figure 3. Trends in Hospitals' Average Ratio of Payment-to-Cost

Table 1**Payment-to-Cost Ratios**

	Private Insurance	Medicare	Medicaid	Uninsured
2000	115.7%	99.1%	94.5%	73.9%
2001	116.5%	98.4%	95.8%	73.9%
2002	119.0%	97.9%	96.1%	73.9%
2003	122.3%	95.3%	92.3%	73.9%
2004	128.9%	91.9%	89.9%	73.9%
2005	129.4%	92.3%	87.1%	73.9%
2006	130.3%	91.3%	85.8%	73.9%
2007	132.2%	90.6%	87.9%	73.9%
2008	128.3%	90.9%	88.7%	73.9%
2009	134.1%	90.1%	89.0%	73.9%
2010	133.5%	92.4%	92.8%	73.9%
2011	134.5%	91.4%	94.7%	73.9%
2012	148.9%	85.9%	88.9%	73.9%

Table 2
Comparisons of Hospitalizations

	<i>By Hospital Type:</i>		<i>By Treatment Group:</i>	
	Safety-Net Hospitals	Other Hospitals	Reform Counties	Other Counties
<i>A. Pre-Reform</i>				
Caseload Composition:				
Private Insurance	32.7%	32.6%	37.8%	31.7%
Medicare	35.0%	47.6%	39.4%	45.3%
Medicaid	25.4%	14.7%	15.6%	17.8%
Uninsured	6.8%	5.1%	7.3%	5.2%
Average Payment-to-Cost	101.9%	102.6%	103.4%	102.2%
N=	3,711,371	10,756,450	2,264,425	12,203,396
<i>B. Post-Reform</i>				
Caseload Composition:				
Private Insurance	27.4%	26.1%	29.0%	26.0%
Medicare	35.3%	49.4%	41.8%	46.4%
Medicaid	29.7%	18.2%	20.3%	21.4%
Uninsured	7.6%	6.3%	8.9%	6.2%
Average Payment-to-Cost	101.3%	101.0%	101.8%	100.9%
N=	3,420,558	9,707,513	2,021,259	11,106,812

Pre-reform period includes January 1, 2000 through June 30, 2006, and post-reform period includes July 1, 2007 through December 31, 2012; phase-in period excluded.

Table 3**Main Estimates**

Payment-to-Cost	
<i>A. Direct Effect on Safety-Net Hospitals</i>	
$DD_{SN=1}$	-0.016 *** (0.002) N = 7,748,173
<i>B. Disproportionate Effect on Safety-Net Hospitals</i>	
$DD_{SN=1} - DD_{SN=0}$	-0.018 *** (0.003) N=29,927,715

Values in parentheses are robust standard errors clustered by patients' county of residence (67 clusters).

*
if p 0.10,

**
if p 0.05,

if p 0.01

Table 4

Robustness Checks

	Share of High-Payers:			
	Payment-to-Cost (2012 Excluded)	Payment-to-Cost (Weighted)	Avg. Payer-Source Tier in the Casemix	Definition (i) Definition (ii) Definition (iii)
<i>A. Direct Effect on Safety-Net Hospitals</i>				
$DD_{SN=1}$	-0.014 *** (0.002) N=7,139,030	-0.014 *** (0.002) N=7,735,724	-0.077 *** (0.026) N=7,748,173	-0.029 ** (0.012) N=7,748,173 -0.035 *** (0.010) N=7,748,173 -0.013 ** (0.006) N=7,748,173
<i>B. Disproportionate Effect on Safety-Net Hospitals</i>				
$DD_{SN=1} - DD_{SN=0}$	-0.014 *** (0.003) N=27,497,086	-0.015 *** (0.003) N=29,913,805	-0.033 (0.045) N=29,927,715	0.003 (0.020) N=29,927,715 -0.027 * (0.015) N=29,927,715 -0.008 (0.012) N=29,927,715

Values in parentheses are robust standard errors clustered by patients' county of residence (67 clusters).

Observations with missing charges are dropped in the weighted regression.

* if p 0.10,

** if p 0.05,

*** if p 0.01