A Leak in the Lifeboat:

The effect of Medicaid managed care on safety-net hospitals

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This study examines the effect of Medicaid managed care on safety-net hospitals' financial health. This is a timely question as, under the Affordable Care Act, Medicaid Disproportionate Share Hospital payments (from which safety-nets directly benefit) have been reduced while nearly one-half of U.S. states have decided not to expand Medicaid (affecting the size of states' uninsured populations, largely served by safety-nets). Meanwhile, in seeming isolation, states have been moving towards greater use of managed care in Medicaid, which has been accused of leaking a major revenue source from safety-nets – namely, Medicaid patients. To estimate the effect of this type of Medicaid provision on safety-net hospitals, I take advantage of a Medicaid Reform Pilot Program which required certain Medicaid recipients living in five Florida counties to enroll in managed care. Using "differences-in-differences-in-differences" (DDD) estimation, I compare hospital compensation patterns (i) before vs. after the Pilot Program's launch, (ii) among patients who resided inside vs. outside of reform counties, and (iii) at safety-net vs. non-safety-net hospitals. In doing so, I gain an estimate for the effect of Medicaid managed care which is not biased by state selection into a reform program. The results suggest that the Florida mandate led to a sorting of Medicaid enrollees across hospital types on the bases of coverage type and health status. The result was a 1.6¢ decline in safety-nets' average level of reimbursement per $1 charged. However, this reduction was not significantly more pronounced relative to the 1.4¢ decline also felt by hospitals outside of the safety-net.

1. Introduction

In a Washington Post blog discussing the effects of the Affordable Care Act (ACA) on Atlanta's Grady Memorial Hospital, one of the largest safety-net facilities in the nation, "Grady officials say that the Affordable Care Act could now be the worst thing to happen to the hospital — an 121-year-old institution that's all too familiar with financial struggles." In a two-punch blow, the facility (which serves a disproportionately large share of uninsured patients) will simultaneously be hit by the state's decision not to expand Medicaid and the upcoming reductions to Medicaid Disproportionate Share Hospital (DSH) payments, which provide additional funds to facilities that care for large numbers of non-paying patrons. (Blau, 2013) In short, Grady will be left with no fewer uninsured patients at their doorstep and less money to care for them.

Similar to Georgia, 20 other states have chosen not to expand Medicaid under the ACA. Consequently, these states' safety-net hospitals are also expected to be financially constrained by the law. Alongside this decision, many states are moving towards a Medicaid structure based entirely on managed care. The thought is that if managed care organizations are able to redirect Medicaid enrollees away from expensive forms of care, then it may be possible for these savings to be passed to the state. Notably, this has the potential of steering Medicaid patients away from safety-nets. Therefore, given the squeeze that has recently been imposed on safety-nets by the ACA, it is important to carefully consider the marginal impact of Medicaid managed care on safety-nets before moving forward with further Medicaid managed care implementation.

To determine the effects of Medicaid managed care on safety-net hospitals' bottom line, one could easily compare compensation patterns across states with different levels of Medicaid managed care provision. The endogeneity concern which arises, however, is that states do not – at random – decide to provide managed care. That is, when a state selects into Medicaid

managed care provision, the decision is almost certainly correlated with other state-level factors which, on their own, affect hospitals' finances.[1] Because of this, it is difficult to determine whether managed care, itself, causes changes in financial wellbeing, or whether observed effects on financial health are actually due to other state-level factors unique to the region(s) choosing to reform. To account for this concern, this study takes advantage of a Medicaid Reform Pilot Program which was implemented in five counties within a single state. In this way, all state-level factors (which affect Medicaid provision, among other things) remain constant across reformed and non-reformed regions so that "treatment" is exogenous.

The specific experiment this study exploits is Florida's Medicaid Reform Pilot Program, which was first launched in 2006 in two counties and then expanded by three counties in 2007. Rather than offering Medicaid enrollees a traditional public insurance plan, particular enrollees living in the pilot counties were required to receive their coverage through either a Medicaid HMO or Provider Service Network (PSN)[2]. Because the Pilot Program began prior to any reductions in Medicaid DSH payments or (potential) ACA Medicaid expansions, I can use pre- versus post-launch differences in safety-net hospitals' compensation patterns to estimate the marginal effects of the reform. I can further control for any differences in the reform counties, themselves, by conditioning my findings on the outcomes among the individuals who lived

[1] According to a study by Kim and Jennings (2012), participation rates in Medicaid risk-based managed care programs are lower in states whose Medicaid coverage is less generous, states whose citizens are more ideologically liberal, states controlled by a unified Democratic party, and states with higher gross state product.

[2] PSNs are Florida-specific entities similar to Accountable Care Organizations (ACOs). According to the Florida Agency for Health Care Administration, "*A Provider Service Network (PSN) is a network established or organized and operated by a health care provider or group of affiliated health care providers, including minority physician networks and emergency room diversion programs that meet the requirements of Section 409.912 (4) (d) , F.S. (…) The health care providers must have a controlling interest in the governing body of the PSN*."

within the Pilot Program's boundaries but who visited non-safety-net hospitals.[3] And, to account for any trends which were unique to safety-net hospitals, but independent of the reform, I can condition my findings on the outcomes of individuals who visited safety-net hospitals, but who lived outside of the Pilot Program's boundaries. In this way, I am left with a triple-difference estimate.

The data used to conduct this analysis consists of individual-level observations on every hospital discharge in the state of Florida between the years 2000 and 2012. Because this dataset identifies patients' counties of residence, the specific hospitals from which patients were discharged, as well as year and quarter of discharge, I am able to precisely assign treatment classification (i.e., residency inside vs. outside of a reform county, safety-net vs. non-safety-net hospital, and pre- versus post-reform period). Additionally, because the dataset includes individuals' sources of payment and total gross charges, I am able to determine from whom, and how much, safety-net hospitals were expected to be compensated.

The results of this study suggest that, following the reform, two shifts occurred in the hospitalization patterns of the Medicaid recipients living in the reform counties. First, a shuffling of Medicaid enrollees emerged by which individuals with traditional coverage were sorted towards safety-net hospitals and individuals with managed care coverage were sorted towards non-safety-net hospitals. Second, relatively sick individuals were sorted towards safety-net hospitals and relatively healthy individuals were sorted towards non-safety-net hospitals.

In total, the evidence suggest that safety-net hospitals' average rate of reimbursement per dollar charged declined as a result of the reform. However, so too did the average rate received

[3] Not only does this particular comparison correct for the possibility that "reformed" individuals may have been systematically more or less sensitive to the reform, but it also allows me to estimate any disproportional effect the reform may have had on safety-nets, relative to other hospitals.

by non-safety-net hospitals. Contrary to previous speculations, safety-nets' leakage of Medicaid managed care patients alleviated some of the downward pressure on these hospitals' average rates of return; when Medicaid managed care patients left, they took with them their below average reimbursement rates, affectively pulling up safety-net hospitals' averages. The arrival of the low-paying Medicaid managed care individuals at other hospitals, conversely, put downward pressure on these facilities' average reimbursement rates. Safety-nets were negatively affected by an inflow of traditional Medicaid patients, who appear to have been crowded out of other hospitals. Like the managed care enrollees, these individuals were also tied to below average reimbursement rates. Additionally, safety-nets were further harmed by the heavier "charge" weights assigned to their post-reform Medicaid pool. That is, the qualitative shift in the kind of Medicaid patients that safety-nets received (i.e., sicker/costlier individuals) led to a unique downward pull on safety-nets' average rate of reimbursement per dollar charged.

In the end, it is estimated that, as a result of the Florida reform, safety-net hospitals were reimbursed 1.6¢ less per dollar charged, and non-safety-net hospitals were reimbursed 1.4¢ less per dollar charged. There was no statistically significant difference in these values. However, had the mandate required that all Medicaid enrollees receive their coverage through a managed care organization, this study projects that the reform would have resulted in no net effect on safety-nets' average reimbursement per dollar charged, and a 2.4¢ per dollar decrease in non-safety-nets' average reimbursement per dollar charged. That is, with no traditional Medicaid available to level the Medicaid population's dispersion across hospital types, the rift in the average reimbursement rate between safety-nets and non-safety-nets would narrow as the burden of caring for the publicly insured would shift in the direction of non-safety-net hospitals.

2. Background

2.1 The Affordable Care Act and Safety-Net Hospitals

On March 23, 2010, the Affordable Care Act (ACA) was signed into law by President Barack Obama. One of the hallmark features of the original bill was its mandated expansion of Medicaid to cover all individuals with incomes up to 133% of the federal poverty line. Among other things, it was thought that this provision would alleviate some of the financial burden felt by safety-nets, as many of their (non-paying) uninsured patients would be nudged under the umbrella of public insurance.

Given the presumed expansion of Medicaid, it was logical (at the time) to assume that once the ACA was fully implemented, safety-net hospitals would have less need for Medicaid DSH payments, which provide additional funds to facilities with high rates of uncompensated care. As a result, the ACA also prescribed significant cuts to all states' Medicaid DSH payments.[4] If all went as planned, safety-nets would merely have to deal with a three month lag before the Medicaid DSH cuts, set to begin October 1, 2013, would be offset by the tilt in their

[4] Unlike Medicare DSH payments (which were also cut under the ACA) where money moves directly from the federal government's hands to hospitals', Medicaid DSH payments are distributed to the states who then distribute the money to individual hospitals. Currently, the U.S. federal government distributes over $11B in Medicaid DSH allotments to the states, each year. The amount of funds allotted to each state in a particular year is capped at the larger of either that state's allotment the previous year or 12% of that state's Medicaid payments the current year. Once the money is received, states have a large degree of freedom in determining how their money will be distributed across "disproportionate share" hospitals. Under the ACA, aggregate Medicaid DSH payments (at the federal level) were originally set to be reduced by $500M in fiscal year 2014, $600M in FY2015 and FY2016, $1.8B in FY2017, $5B in FY2018, $5.6B in FY2019, and $4B in FY2020. To determine how these aggregate reductions would be allocated across the states, the law also required that the Secretary of the Department of Health and Human Services create a methodology for allocating the cuts. A part of the criteria for the methodology was that the reductions (as a percentage) be smaller in low-DSH states (i.e., those with Medicaid DSH payments accounting for less than 3% of total Medicaid payments), larger in states with low levels of uninsured individuals, larger in states that do not target DSH funds towards hospitals with high frequencies of Medicaid patients and/or uncompensated care, and it must account for the degree to which a state's DSH allotment was included in the budget neutral calculation for a Medicaid coverage expansion occurring under a Section 1115 waiver (i.e., a Medicaid "demonstration" waiver that allows a state to receive federal funds for implementing a Medicaid expansion that does not align with standard federal program rules).

patients' insurance coverage, set to begin January 1, 2014. The problem, however, was that for 25 states, the balance did not tilt.

Immediately following the passage of the ACA, over half of the U.S. states filed lawsuits against the federal government challenging the constitutionality of the law, and, in 2012, the ACA made its way to the Supreme Court. Among the few provisions to be struck down was the bill's mandated expansion of Medicaid. Instead, it was determined that every state could choose whether to expand its Medicaid program to cover individuals with incomes up to 133% of the federal poverty line. If expansion was chosen, then the federal government would match 100% of the cost of covering the state's new enrollees through 2016. After this point, support would gradually decline.

With the mandated expansion of Medicaid struck down, the implications of the upcoming Medicaid DSH cuts became a troublesome concern. For states which would not expand coverage, the original rationale for the Medicaid DSH cuts no longer held. Therefore, in late 2013, Medicaid DSH cuts (which were scheduled to take effect on October 1, 2013) were postponed until October 1, 2015. Though this delay would push back the timing of the impending blow, it would not lessen its impact. Instead, it was decided that, at that new implementation date, the initial Medicaid DSH reduction amount would be double the amount it otherwise would have been in that year.

On January 1, 2014 – the deadline initially set for mandatory Medicaid expansion – only 25 states were on track to expand. Four states remained undecided as to their expansion decision, and the remaining 21 states had decided against expansion. (Where, 2014) For safety-net hospitals in the non-expansion states, the expected financial effects of the ACA were grim. In little more than a year, the supplemental payments these facilities were receiving would be

substantially cut and there would be no additional Medicaid coverage to off-set the missing money. Therefore, given this constraint, it has become increasing important for state policymakers to understand the implications of qualitatively reforming Medicaid, lest the additional nudge of these reforms push safety-nets over the edge. One such reform, which has the potential to substantially affect safety-nets, is Medicaid managed care.

2.2 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. Moreover, participation in managed care among the Medicaid populations ranges from approximately 50-100% across states.[5] Spawning the growth in this type of Medicaid provision have been the arguments of cost savings and increased access to healthcare. (Gold, 1999)

Traditional Medicaid finances enrollees' medical expenses by paying healthcare providers directly using fee-for-service rates. Under Medicaid managed care, however, states contract managed care organizations to do the leg work of reimbursing providers. In exchange for this service, states typically pay managed care organizations using a capitated rate. Not only does this structure provide states with cost predictability, but, because the capitated rates are generally set 5% below traditional fee-for-service rates, the states save money.

To incentivize managed care organizations to participate, it is generally argued that, if managed care organizations efficiently manage Medicaid enrollees' healthcare consumption (or at least more efficiently than the state), then contracted organizations should be able to turn a profit with 95% of fee-for-service payments. (Grogan & Patashnik, 2003) In practice, this means redirecting enrollees away from unnecessarily expensive forms of care, such as inpatient

[5] http://kff.org/medicaid/state-indicator/medicaid-managed-care-as-a-of-medicaid/

stays, specialty physicians, visits to emergency departments and, potentially, safety-nets. Additionally, because Medicaid managed care organizations are not required to pay providers federally regulated minimum Medicaid fee-for-service rates, and because hospitals are receiving lower rates from private insurance, it may be feasible for managed care organizations to negotiate more favorable reimbursement rates with hospitals. (Holahan et al., 1998)

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, the number of providers willing to accept Medicaid patients was/is particularly limited. Consequently, individuals enrolled in traditional Medicaid rely heavily on safety-nets as a source of care. If Medicaid managed care is able to bring formerly inaccessibly providers (to Medicaid enrollees) "in-network", then this encourages access to care.

Although, on the outset, the presumed consequences of Medicaid managed care were optimistic for states and enrollees, when Medicaid managed care initially rolled out, there was some concern that safety-nets would be hurt. The reason for this concern centered around the fact that safety-nets, who serve disproportionally high numbers of Medicaid and uninsured patients, are particularly dependent on Medicaid funds to cross-subsidize the care they provide to the uninsured. That is, if Medicaid managed care were to cause safety-nets to lose Medicaid patients to other facilities due to the imposition of in-network provider lists, then safety-nets' bottom line would unequivocally be hurt. (Hurley & Somers, 2003) Consistent with this fear, there emerged a new competition for Medicaid patients in response to Medicaid managed care. (Lipson & Naierman, 1996) In fact, some safety-nets went so far as to organize their own

managed care organizations in order to keep Medicaid patients in-house. (Sparer & Brown, 2000)

During the late 1990's and early 2000's, a number of (mostly qualitative) studies were conducted to assess the impact of Medicaid managed care on safety-nets. In New Mexico, where Medicaid managed care was mandated in 1997, Horton et al. (2001) found that safety-nets in rural areas, those not affiliated with larger hospitals, and those without recourse to charitable funding, were especially ill-equipped to buffer the effects of the reform. The authors also observed that the filtering of Medicaid money through the additional layer of managed care organizations resulted in lower Medicaid reimbursements for safety-nets. In Connecticut, where mandatory Medicaid manage care was first implemented in 1995, Grogan & Gusmano (1999) found that although most safety-nets reported the same or higher rates of reimbursement from Medicaid managed care organizations (compared to traditional Medicaid fee-for-service rates), some providers still reported losses as payments were, at times, entirely denied. In a survey of five states (California, Minnesota, New York, Oregon, and Tennessee), Gold et al. (1996) similarly found that Medicaid managed care does a poor job of protecting safety-nets. An additional concern in Michigan, noted by Holahan et al. (1998), was that Medicaid managed care enrollees who do not receive timely care from in-network providers may be incited to seek care from out-of-network safety-nets. In cases such as these, care delivered by safety-nets would go completely uncompensated, only reinforcing safety-nets' financial burden.

It is perhaps not surprising, given that safety-nets reportedly cross-subsidize the care they provide to the uninsured, that other studies have found a decline in access to care among the uninsured following the implementation of Medicaid managed care. Such an occurrence is consistent with an increased financial strain on safety-nets. Among these studies are

Cunningham (1999), which found that low-income uninsured individuals have less access to care in states with high Medicaid managed care penetration, and Waitzkin et al. (2002), which reported that uninsured individuals in New Mexico had greater difficulty accessing care following the state's Medicaid managed care mandate. In contrast to these results, however, Haberer et al. (2005), found no consistent evidence that Medicaid managed care diminishes the uninsured population's access to healthcare, concluding that "our findings suggest that safety-net providers are coping with the changes associated with Medicaid managed care."

Unfortunately, since the mid-2000's there has been a relative lull in the literature on the effects of Medicaid managed care on safety-nets, with a complete absence in economics. This paper revisits the issue, first, because the literature continues to lack strong empirical evidence on the effects of this type of Medicaid provision on the volume of patients leaked out of safety-nets and how this leakage affects safety-nets' bottom line. Second, if Medicaid managed care does in fact hurt safety-nets, then there is a renewed importance in determining the magnitude of the financial impact on safety-nets as, under the ACA, safety-nets are expected to be placed under particular financial strain. That is, if the viability of safety-nets is to be ensured, then it is imperative that states understand the marginal impact of instituting this type of Medicaid provision.

2.3 Theoretical Considerations

In the discussions surrounding the effects of Medicaid managed care on safety-net hospitals' financial health, there are two frequently overlooked considerations which this study wishes to incorporate. First, safety-nets' financial health has primarily been considered in the context of revenue; yet, revenue does not necessarily reflect profitability. Second, effects on revenue have primarily been considered through the avenue of patient volume; however, to the

extent that Medicaid managed care might manipulate the type of individuals that comprise a hospital's Medicaid pool, there is an additional avenue through which financial wellbeing might be affected.

To demonstrate these points, consider the following hospital revenue function for a single period:

$$Revenue = \sum_{i=1}^{n} N_i T_i R_i$$

Here, i denotes a particular payer source (e.g., self-pay, Medicaid, etc.), N_i represents number of encounters with $N_i>0$ for all i, T_i represents average total charge (in dollars) with $T_i>0$ for all i, and R_i represent average rate of reimbursement with $0<R_i\leq1$ for all i.

The current literature correctly points out that, for all payer sources, revenue moves in the same direction as encounters. That is, when encounters among any payer source rise or fall, so too does revenue:

$$\frac{\partial(Revenue)}{\partial N_j} = T_j R_j > 0$$

What the literature fails to discuss that if the average total charge associated with a particular payer type increases or decreases in response to Medicaid managed care, then this will also affect revenue. For example, if healthier Medicaid patients are increasingly kept out of hospitals due to a reform, then the average total charge associated with a hospital's new "average" Medicaid patient (who is now relatively sicker) will likely be higher. As shown below, non-randomness in the Medicaid managed care individuals that are directed away from hospitals, in terms of health, will have its own effect on revenue. Holding encounters constant, an increase in the average total charge associated with any one payer type will be an increase in revenue:

$$\frac{\partial(Revenue)}{\partial T_j} = T_j R_j > 0$$

However, it is incorrect to think that all effects which would serve to bolster revenue are necessarily preferable. For intuition, consider a hospital with two patients, one which provides a reimbursement rate of R_A=100% and the other which provides a reimbursement rate R_B=50%. If both A and B come to the hospital for a vaccine and are each charged $10, then this will result in $15 of revenue. If, however, A comes to the hospital for a vaccine and is charged $10, and B comes to the hospital for surgery and is charged $100,000, then this will result in $50,010 of revenue. While revenue is markedly higher in the latter scenario, it is not clear that this latter situation is financially preferable.

To express this dynamic, consider the following profit function:

$$Profit = \sum_{i=1}^{n} N_i T_i R_i - Total\ Costs$$

If we assume that the dollar amount that a hospital charges each patient is an inflated reflection of the hospital's total cost of caring for that patient (where the inflation is proportional across all patients), then this profit expression can be rewritten as:

$$Profit = \sum_{i=1}^{n} N_i T_i R_i - \beta \sum_{i=1}^{n} N_i T_i\,, \qquad assuming\ 0 < \beta < 1$$

When this is the case, we see that:

$$Profit < 0\ \ when\ \left(\frac{\sum_{i=1}^{n} N_i T_i R_i}{\sum_{i=1}^{n} N_i T_i}\right) < \beta$$

$$Profit = 0\ \ when\ \left(\frac{\sum_{i=1}^{n} N_i T_i R_i}{\sum_{i=1}^{n} N_i T_i}\right) = \beta$$

$$Profit > 0\ \ when\ \left(\frac{\sum_{i=1}^{n} N_i T_i R_i}{\sum_{i=1}^{n} N_i T_i}\right) > \beta$$

Importantly, the value in parentheses represents the hospital's average rate of reimbursement per dollar charged. Although this measure will not perfectly align with profitability, we can think of this metric as a more useful gauge by which to measure a hospital's financial wellbeing. That is, as a hospital's average rate of reimbursement per dollar charged increases, so too will the hospital's financial health.

Now consider the possible effects of a Medicaid managed care mandate on this metric:

$$y \equiv \frac{\sum_{i=1}^{n} N_i R_i T_i}{\sum_{i=1}^{n} N_i T_i}$$

If encounters among a particular payer source rise in response to Medicaid managed care, we might expect the following:

$$\frac{\partial y}{\partial N_j} = \frac{T_j}{\sum_{i=1}^{n} N_i T_i}\left(R_j - \frac{\sum_{i=1}^{n} N_i R_i T_i}{\sum_{i=1}^{n} N_i T_i}\right)$$

$$\frac{\partial y}{\partial N_j} > 0 \quad if\ R_j > \frac{\sum_{i=1}^{n} N_i R_i T_i}{\sum_{i=1}^{n} N_i T_i}$$

$$\frac{\partial y}{\partial N_j} < 0 \quad if\ R_j < \frac{\sum_{i=1}^{n} N_i R_i T_i}{\sum_{i=1}^{n} N_i T_i}$$

That is, a hospital's average rate of reimbursement per dollar charged (y) will be pulled down if its new-comer's reimbursement rate is below the existing hospital average. If, however, the new-comer brings with her a reimbursement rate that is above the existing hospital average, then the entire hospital average will be pulled up. A reversed effect will occur in response to a decrease in encounters.

What if, in response to a mandate, relatively healthy Medicaid enrollees are kept out of hospitals, thereby increasing the average total amount charged to an individual in a hospital's Medicaid pool? While revenue will necessarily decrease in response to a negative shock to N_j,

we can see that non-randomness in the health of the exiting persons will generate additional implications for revenue. If we fix encounters, so as to understand the isolated effect of a change in T_j, we see that:

$$\frac{\partial y}{\partial T_j} = \frac{N_j}{\sum_{i=1}^{n} N_i T_i}\left(R_j - \frac{\sum_{i=1}^{n} N_i R_i T_i}{\sum_{i=1}^{n} N_i T_i}\right)$$

$$\frac{\partial y}{\partial T_j} > 0 \quad if\ R_j > \frac{\sum_{i=1}^{n} N_i R_i T_i}{\sum_{i=1}^{n} N_i T_i}$$

$$\frac{\partial y}{\partial T_j} < 0 \quad if\ R_j < \frac{\sum_{i=1}^{n} N_i R_i T_i}{\sum_{i=1}^{n} N_i T_i}$$

In other words, similar to before, if the average total charge which rises is attached to a payer type whose reimbursement rate exceeds the existing hospital average, then the hospital's average will be pulled up. Conversely, if the increased average total charge is tied to a payer type with a reimbursement rate below the existing hospital average, then the entire hospital average will be pulled down.

In summary, if a Medicaid managed care mandate results in a hospital losing Medicaid patients, then the effect of this leak on a hospital's financial wellbeing is ambiguous. If the hospital starts from a position of relative financial health, then it is possible that the loss of relatively low paying Medicaid patients could improve the financial standing of the hospital. Conversely, if the hospital starts from a position of sufficiently low financial health, then this exit of patients could harm the financial wellbeing of the facility. Similarly, if Medicaid managed care results in a qualitative shift in the healthiness of a hospital's Medicaid pool, then the effect on the hospital's financial wellbeing will again be conditional on the initial average return the hospital received per $1 charged. What we can say, however, is that the likelihood of a hospital being financially harmed by a sicker Medicaid pool (i.e., a pool whose average total

charge increases) will be greater for a hospital that starts from a position of better financial health. Whether the net effect of a Medicaid managed care mandate on a hospital's financial wellbeing differs across safety-net hospitals and non-safety-net hospitals is left as an empirical question.

2.4 Florida's Medicaid Reform Pilot Program

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

In Florida, the mandatory Medicaid managed care organizations took the form of either a Medicaid HMO or a PSN. 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 the Florida safety-nets would be harmed by the reform. 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, though, in practice, the PSNs widely favored fee-for-service.[6] (Pilot)

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

Beginning 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. (Florida Medicaid) While 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 these 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 launch, Medicaid enrollees 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.[7] If an individual failed to select a plan, then he/she was automatically assigned one by Florida's Agency for Health Care Administration. (Bragdon, 2011, Final Bill Analysis) For families with incomes below the TANF 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, participation was mandatory. According to the initial Medicaid reform application, managed care participation would also be required among the individuals eligible for both Medicare and Medicaid, "upon the development and inclusion of an integrated

[7] Over time, the list of Medicaid HMOs and PSNs available to Medicaid enrollees in particular counties fluctuated as some HMOs/PSNs found participation unaffordable and others chose to join. Additionally, an "Opt Out" program was made available to Medicaid enrollees so that qualified individuals could use (what would have been) the dollar value of their Medicaid benefits to enroll in employer-provided coverage. However, there was little participation in this program.

service delivery system for individuals aged 60 and older." (Florida Agency, 2005) Participation in managed care was optional for other Medicaid enrollees living with the reform counties, as it was in the rest of the state. (Medicaid, 2008)

The Florida Medicaid Reform Pilot Program continued to operate until August 1, 2014. At its closure, the consensus was that the experiment had been a success. Given the lower state expenditures (estimated to be $118M per year, FGA Letter), along with the higher rates of enrollee satisfaction, it was determined that mandated Medicaid managed care would be implemented state-wide. The question which remained unclear, however, was how the Medicaid managed care expansion had, and would be expected to, affect the state's safety-net hospitals. To address this question, I consider four retrospective outcomes of Florida's Medicaid Reform Pilot Program. These are: (i) how the number of hospitalizations within each payer source category changed, (ii) how the average total charge associated with each payer source category changed, (iii) how hospitals' revenues changed, and (iv) how hospitals' average rates of reimbursement per dollar charged changed.

3. Data

The data used in this study comes from the Agency for Health Care Administration, Florida Center for Health Information and Policy Analysis, and consists of patient-level observations for every hospital discharge in the state of Florida between the years of 2000 and 2012. Included in the dataset are patients' discharging facility, year and quarter of discharge, payer source, gross charges, information related to the patients' conditions and diagnoses, and demographic information (including county of residency). Because safety-net status is not explicitly defined in the dataset, safety-net treatment is manually assigned by matching

discharging facilities to the list of hospitals which are members of the Safety Net Hospital Alliance of Florida.[8] 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.[9] Second, only individuals that can be identified as Florida residents are retained[10]. The remaining sample consists of 29,927,715 observations.

For estimation, individual observations are collapsed to the level of county of residence, quarter of discharge, and hospital-type (i.e., safety-net versus non-safety-net). This leaves 6,968 effective cells: one for each of Florida's 67 counties, across 52 quarters of data, and across 2 hospitals types.

Presented in Table 1 are pre-reform summary statistics, stratified by reform county status and hospital type. Compared to non-reform counties, the five counties selected to participate in Florida's Medicaid Reform Pilot Program had relatively higher rates of safety-net hospitalizations prior to the reform. Additionally, as expected, safety-net hospitals received lower average rates of reimbursement relative to other facilities. While safety-nets received roughly $0.75 on each one dollar they charged, other facilities received roughly $0.80. To see this gap over time, the pre-reform trends in safety-nets' versus non-safety-nets' average

[8] http://safetynetsflorida.org/ (Accessed February 2014)

[9] Other sources of payment account for 5.2% of the full sample.

[10] Individuals without a recorded Florida county of residency account for 3.5% of the full sample

reimbursement rates are presented in Figure 2.[11] Throughout the period of 2000 to 2006, safety-nets' average return disparity consistently paralleled that of other hospitals.

To understand the source of the consistent disparity in safety-nets' versus other hospitals' average rate of reimbursement per dollar charged, Figure 3 shows each hospital types' distribution of encounters and total charges across payer source. Here, payer categories are arranged along the horizontal axis in order of increasing average reimbursement rate. As discussed previously, if reimbursement rates are assumed to be fixed, a hospital's average reimbursement per dollar charged will be a weighted function of the number of encounters, and the average total charge, associated with each payer type. Consequently, higher values on the left ends of the spectrum (which correspond to patient types with lower average rates of reimbursement) will result in weightier low payers. These individuals pull down hospitals' average return. For safety-nets, this sort of left-loaded pressure is visually evident. For both encounters and charges, safety-nets' distribution is relatively skewed (compared to non-safety-nets) towards patient types with low reimbursement rates.

4. Identification Strategy

To determine the effect of Florida's Medicaid Reform Pilot Program on safety-net hospitals, what I would like to know is:

$$\overbrace{E(y_1|Treat=1)}^{(A)} - \overbrace{E(y_0|Treat=1)}^{(B)} \quad (1)$$

where *Treat* is equal to one when, at the time of hospitalization, an individual is affected by Florida's Medicaid reform. The problem, however, is that both (A) and (B) cannot

[11] The reason for the slightly narrower gap between hospitals' reimbursement rates in Figure 2 relative to the average rates presented in Table 1 is because, in Table 1, average reimbursement rates are calculated at the individual level whereas, in Figure 2, these rates are calculated at the cell level.

simultaneously exist. That is, for an individual affected by the reform at the time of his/her hospitalization, the counterfactual of not being affected at that time will never materialize.

To overcome this obstacle, difference-in-differences (DD) estimation is one alternative identification procedure which can be used to approximate the above effect. A DD estimate allows the econometrician to estimate what is close to the true effect of treatment by using another similar population, which was not "treated" at time t, as a baseline comparison. To ensure that any dissimilarity between the two groups doesn't bias the one-to-one comparison, it then subtracts out any differences between the treated and untreated groups which existed at a time when neither group was eligible for treatment. In the case of the present study, one might do this by first narrowing the sample to include only safety-net observations and then using, as the baseline comparison group, individuals who lived outside of the reform counties. In a simple two-period model (where residence in a reform county is denoted *R* and the post-reform period is denoted *P*) this can be expressed as:

Among Safety-Nets:

Residence in Reformed vs. Non-Reformed County Difference, Post-Reform (C) — *Residence in Reformed vs. Non-Reform County Difference, Pre-Reform* (D)

$$\overbrace{[E(y|R=1, P=1) - E(y|R=0, P=1)]}^{(C)} - \overbrace{[E(y|R=1, P=0) - E(y|R=0, P=0)]}^{(D)} \quad (2)$$

or, alternatively:

Among Safety-Nets:

Pre- vs. Post-Reform Difference, Given Residence Within a Reform County (E) — *Pre- vs. Post-Reform Difference, Given Residence Outside of a Reform County* (F)

$$\overbrace{[E(y|R=1, P=1) - E(y|R=1, P=0)]}^{(E)} - \overbrace{[E(y|R=0, P=1) - E(y|R=0, P=0)]}^{(F)} \quad (3)$$

where the difference is interpreted as the effect of the reform among safety-nets serving patients from reform counties, less the effect which emerged among the safety-nets serving patients from non-reform counties.

While an improvement compared to equation (1)'s inability to capture the counterfactual effect of treatment, this procedure relies on a number of assumptions. First, it is assumed that the observed gap (in y) between the treated safety-nets (serving patients from reform counties) and control safety-nets (serving patients from outside of the reform counties) at time P=0 is the same gap which would have emerged at time P=1, had the reform not occurred. This "parallel worlds" assumption essentially requires (C) to equal (D) in the absence of reform so that the difference-in-differences remains unbiased. Alternatively, it is assumed that the observed jump (in y) among the control safety-nets at the time of the reform is the same jump which would have occurred among the treated safety-nets at the time of the reform, had the reform not taken place. That is, were it not for the Pilot Program, (E) would be equal to (F).

While it is relatively straightforward to graphically verify that the outcomes among the safety-nets serving reform county residents and the safety-nets serving non-reform county residents parallel each other, it is less simple to verify that reform counties were randomly selected so that the residents of reform counties are not relatively more or less sensitive to manipulation. If so, then the jump in (E) (i.e., the post-reform jump among only the reform county residents) will be relatively inflated/deflated so that the difference between (E) and (F) will not align with the difference between (A) and (B).

To account for the possibility that the reform county residents may have been innately more or less sensitive to the reform than the average individual, an additional outcome which I can condition on is the effect of the reform among individuals who lived inside of the reform

counties, after the reform, but were yet untreated in some way. Because this study is ultimately interested in the effects on safety-nets, non-safety-net hospitals provide one such possible comparison group. By observing the effects of the reform relative to this population, any potential confounding "sensitivity to treatment" effects can be subtracted out. This leaves me with a differences-in-differences-in-differences (DDD) estimate for the effect of the reform. However, it should be noted that this strategy estimates a slightly different effect than the impact of the reform on safety-nets directly. That is, this estimate tells me the disproportionate impact of the reform on safety-nets relative to other facilities. This is important as the non-safety-net "control" group may also have been affected by the mandate and it is possible that the effect on these hospitals may have been leveraged by the effect on safety-nets. For example, if, due to the reform, individuals moved between safety-net and non-safety-net hospitals, then safety-nets' loss of patients would be non-safety-nets' gain.

While using a DDD strategy as opposed to a DD strategy subtly alters the interpretation of the fully-differenced term (since it nets-out any reform effects felt by non-safety-net hospitals), it does provide an arguable more useful and holistic picture of the effects of Medicaid managed care on a state's hospital system. In the DDD framework, it is acknowledged that there exists a spread in the financial health of safety-nets and other hospitals. To the extent that safety-nets are safety-nets because they are different, this method captures the effect of Medicaid managed care on safety-nets' degree of delineation. From a policy standpoint, understanding the impact of Medicaid managed care on the degree of separation between different hospitals' compensation may be relatively more insightful than the isolated effects on the hospitals that begin from a safety-net state. This is because, under the ACA, current safety-nets' finances are expected to be disproportionately impacted in such a way as to widen the gap which separates

safety-nets from other hospitals. Consequently, if states are able to narrow this gap using their own reforms, then this is useful information.

In a simple two-period model, where *P* denotes the post-reform period, *R* denotes residency in a reform county, and *SN* denotes admittance to a safety-net, this DDD estimate can be written as:

Pre- vs. Post-Reform Difference, Less Any Changes in Non-Safety-Net Facilities, Given Residence Within a Reform County

$$\overbrace{\{[E(y|R=1, P=1, SN=1) - E(y|R=1, P=1, SN=0)] - [E(y|R=1, P=0, SN=1) - E(y|R=1, P=0, SN=0)]\}}^{(G)} - \underbrace{\{[E(y|R=0, P=1, SN=1) - E(y|R=0, P=1, SN=0)] - [E(y|R=0, P=0, SN=1) - E(y|R=0, P=0, SN=0]\}}_{(H)} \quad (4)$$

Pre- vs. Post-Reform Difference, Less Any Changes in Non-Safety-Net Facilities, Given Residence Outside of a Reform County

or, alternatively:

Pre- vs. Post-Reform Difference, Less Any Changes Among Residents of Non-Reformed Counties, Given Admittance to a Safety-Net

$$\overbrace{\{[E(y|R=1, P=1, SN=1) - E(y|R=0, P=1, SN=1)] - [E(y|R=1, P=0, SN=1) - E(y|R=0, P=0, SN=1)]\}}^{(I)} - \underbrace{\{[\ E(y|R=1, P=1, SN=0) - E(y|R=0, P=1, SN=0)] - [E(y|R=1, P=0, SN=0) - E(y|R=0, P=0, SN=0]\}}_{(J)} \quad (5)$$

Pre- vs. Post-Reform Difference, Less Any Changes Among Residents of Non-Reform Counties, Given Admittance to a Non-Safety-Net

Again, for clarity, this fully differenced equation expresses the extent to which safety-nets were *disproportionately* affected by the implementation of mandated Medicaid managed care relative to non-safety-net hospitals.

5. Estimation

The two period DDD estimate given by equations (4) and (5) can be empirically estimated as follows:

$$Y = \beta_0 + \beta_1 R + \beta_2 P + \beta_3 SN + \beta_4 (R*P) + \beta_5 (R*SN) + \beta_6 (P*SN) + \beta_7 (R*P*SN) + \varepsilon \quad (6)$$

Here, the coefficient β_7 is analogous to the triple difference. However, in the present study, the multi-period implementation of the Medicaid Reform Pilot Program complicates the definition of *P* in equation (6). This is because, since *P* switches on at different times in different reform counties, there is no clear switching threshold for non-reform counties. To address this, an amended version of equation (6) is estimated, 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 better capturing the effects of the multi-period implementation of the reform, this method of handling the "post" variable also accounts for time trends, which are likely to emerge across a period of 13 years. Cell-level demographic characteristics are further incorporated as control variables, yielding the following specification:

$$Y_{cqh} = \alpha_0 + \alpha_1 R_c + \alpha_2 Q'_q + \alpha_3 SN_h + \alpha_4 (R*P)_{cq} + \alpha_5 (R*SN)_{ch} + \alpha_6 (Q'*SN)_{qh} + \alpha_7 (R*P*SN)_{cqh} + X'_{cqh} + \mu \quad (7)$$

Here, 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, as opposed to a non-safety-net hospital. The vector X' contains information on cell-level demographics, including percent male, percent white, percent black, percent Hispanic, and mean age. Again, the unit of observation is the county/quarter/hospital-type cell, with these divisions denoted by subscripts c, q and h, respectively.

The dependent variable, y, is defined as both the natural log and the absolute value of:

- Number of Admissions: N_i
- Average Total Gross Charge[12]: T_i
- Revenue: $\sum_{i=1}^{5} N_i T_i R_i$
- Average Amount Reimbursed per Dollar Charged: $\frac{\sum_{i=1}^{5} N_i R_i T_i}{\sum_{i=1}^{5} N_i T_i}$

where subscript i denotes primary principal payer, with:

$$i = \begin{cases} 1 \text{ if uninsured} \\ 2 \text{ if traditional Medicaid} \\ 3 \text{ if Medicaid managed care} \\ 4 \text{ if Medicare} \\ 5 \text{ if private insurance} \end{cases}$$

and average reimbursement rates are set such that R_1=0.12, R_2=0.456, R_3=0.456, R_4=0.8, and R_5=1.

The reason for estimating the effect on both the unadjusted and log transformation of each dependent variable is so that the effect of the reform can be seen on the explicit gap in y between hospital types, as well as on the "momentum gap" – that is, the difference in the degree of forward motion between the two hospital types. It should be noted that these two measurements may move in opposite directions. For example, if safety-nets start with an average return of 10¢ per dollar, and non-safety-nets with 50¢ per dollar, then the initial gap will be -40¢. If, due to the reform, safety-nets' return grows by 100% to reach 20¢, and non-safety-nets' return grows by 50% to reach 75¢, then the post-reform gap will be -55¢. That is, although

[12] Total gross charge is defined in the data as, "The total of undiscounted charges for services rendered by the hospital excluding professional fees".

safety-nets will have moved forward more aggressively, the absolute distance of the gap will be wider. Consequently, when the dependent variable is defined as the absolute value of a hospital's average rate of reimbursement per dollar charged, the model (in this case) will generate a negative DDD coefficient (representing the effect on the explicit gap width), while a log transformation of the dependent variable will generate a positive DDD coefficient (representing the comparative strength of the forward movement among safety-nets).

The above R_i values are calculated using outside reimbursement rate statistics. First, as a baseline, it is assumed that hospitals receive full reimbursement from privately insured patients. While this does not require that private insurance companies cover 100% of charges, it does assume that hospitals are able to squeeze any excess (uncovered) charges out of the privately insured individuals, themselves. Next, the Medicare rate is assigned based on a 2009 Medicare Payment Advisory Commission report which states, "Medicare payment rates continue to be about 80 percent of private insurance payment rates as they have for the past decade." The Medicaid rate is calculated based on the Kaiser Foundation's Medicaid-to-Medicare Fee Index of 0.57 in the state of Florida.[13] (Medicare Payment, 2009; Medicaid-to-Medicare) Together with the 80% Medicare rate, this yields the 0.80*0.57=45.6% approximation for realized reimbursement from Medicaid enrollees.[14] The approximation for uninsured patients' rate of reimbursement is based on the U.S. Department of Health and Human Service's assertion that, "Uninsured families can on average afford to pay the full bills for only about 12% of the

[13] This figure is based on the 2012 Medicaid-to-Medicare Fee Index.

[14] Though it is possible that Medicaid managed care plans may pay hospitals higher/lower rates than traditional Medicaid, there is little evidence of the direction or extent of this difference. In fact, among experts in the field, there are internal inconsistencies as to whether Medicaid managed care provides Florida hospitals with higher or lower rates than traditional Medicaid. Therefore, it is assumed that, on average, these rates are comparable.

hospitalizations they might experience." (The Value, 2011) Though this assertion does not necessarily imply that the average reimbursement rate is 12%, if the uninsured families who cannot afford to pay the *full* bill provide close to zero compensation, then, among the uninsured, the average reimbursement rate will be close to 12%. Alternatively, 12% may be an under-approximation of the average reimbursement provided by an uninsured individual if the 12% of fully paid bills are those which are relatively small.

Again, the coefficient on the triple-interaction term, α_7, is the point-estimate of interest. This value indicates the effect of the reform on the disparity between safety-nets and other hospitals in terms of y. Again, for clarity, an explicit closing or narrowing of the gap may not be assumed when the dependent variable is log-transformed. In this case, the DDD estimate represents the relative aggressiveness of each hospital-types' forward movement. The effect on the absolute width of the gap can be ascertained by the sign on α_7, however, when the dependent variable is not log-transformed. For the measures in which safety-nets lag behind other hospitals, a positive α_7 will indicate that safety-nets have, to some degree, caught up, thereby narrowing the raw distance of the gap in y. For measures in which safety-nets lead other hospitals, a positive α_7 will indicate that safety-nets have extended their lead, thereby extending the raw distance of the gap in y.

6. Results

Presented in Table 2 are the DDD estimates for the effects of Florida's Medicaid reform on the frequency of admissions within each payer source category. The DDD estimates for the effects of the reform on the average total charge associated with each payer source category are

presented in Table 3. Together, the impacts on these "weights" are used to estimate the aggregate effects on hospitals' total revenue as well as their average rate of reimbursement per dollar charged. These aggregate estimates are given in Table 3. While the effects on the on log-transformed dependent variables will be primarily cited in the text, the effects on unadjusted measures are presented in the tables. Unless otherwise noted, references to the "gap" will refer, not to an explicit distance, but to the "momentum gap". That is, the gap in the percent effect of the reform on safety-nets relative to the percent effect on non-safety-nets.

Withstanding the Medicaid population living inside of the reform counties, the results suggest that the Florida mandate had no statistically significant effect on the volume of patients that safety-nets admitted relative to other hospitals. However, within the Medicaid population living inside of the reform's boundaries, there is evidence of a disproportionate change in patient volume among safety-nets. In response to the reform, the gap in safety-net versus non-safety-net Medicaid managed care admissions was affected by -249%. This divergence is further corroborated by Figure 4 which illustrates the DDD trends. While, among non-reform county residents, Medicaid managed care admissions across hospital types consistently parallel each other, among the subset of Floridians living in the reform counties, we see that Medicaid managed care hospitalizations rose sharply in non-safety-nets around the time of the reform's implementation. In fact, this effect was so pronounced as to cause the number of Medicaid managed care admissions in non-safety-nets to surpass that in safety-nets.

While imprecisely estimated, Table 3 also indicates a substantial effect (30%) on the gap in traditional Medicaid admissions. This effect is also supported by Figure 4. While, outside of the reform counties, traditional Medicaid enrollees' choice of hospital type appears unaffected throughout the reform's implementation, a decided shift occurs among reform county residents

immediately following the Pilot Program's implementation. Specifically, traditional Medicaid enrollees living in reform counties appear to exhibit a more pronounced tendency to select a safety-net hospital over a non-safety-net hospital following the reform.

It should be noted that, that while imprecisely estimated, the results in Table 2 also suggest an effect on "uninsured" individual's choice of hospital. However, because insurance, here, is defined as primary principal payer, this may not be indicative of an effect among individuals who were truly uninsured. If a managed care plan, for example, did not cover a particular hospitalization for whatever reason, then the associated bill would have been coded as "uninsured". Consequently, although unverifiable, the 21% decrease in safety-nets' relative number of uninsured hospitalizations would be consistent with an increased number of out-of-network occurrences at non-safety-nets, who were receiving Medicaid managed care enrollees.

To determine the effect of the Florida reform on the disparity between safety-nets' and non-safety-nets' patients' physical health, the DDD effects on average total charges are presented in Table 3. According to these estimates, the Florida reform resulted in a 19% change in the gap between the total charge associated with a safety-net's average traditional Medicaid patient and a non-safety-nets' average traditional Medicaid patient. If we assume that total charges are a reflection of the acuteness of a patient's condition, then this result is consistent with safety-nets being dealt a relatively sicker pool of traditional Medicaid enrollees. Additionally, there is also some evidence of health-based sorting among "uninsured" reform county residents. Specifically, the results suggest that routing to a safety-net versus a non-safety-net hospital was effected in such a way as to increase safety-nets' relative charge to the "uninsured" population by 11%. Again, though not verifiable, this might be explained by non-randomness in the out-of-network incidences among the Medicaid managed care patients who

were directed to non-safety-nets. If, among the managed care population, there was a greater incidence of denied coverage among the hospitalizations that were tied to relatively large bills, then this could explain the 10% effect on the T_1 gap. While admittedly noisy, a visual representation of the DDD effects on the average total charges to Medicaid managed care and traditional Medical patients is shown in Figure 5.

Finally, of primary interest, the DDD results for the effect of the Florida reform on the safety-net versus non-safety-net gap in financial wellbeing is presented in Table 4. These results suggest that the Florida reform had no disproportionate impact on safety-nets' total revenue or average rate of reimbursement per dollar charged. The associated DDD trends are depicted in Figures 6 and 7, respectively.

Given the previous finding that traditional Medicaid enrollees (who were exempt from the managed care mandate) may have been affected by the reform in a reversed faction, relative to the non-exempt population, this null result may not be surprising. That is, if crowd-out occurred such that the affected enrollees – who were pushed towards non-safety-nets – inadvertently pushed out some of the unaffected enrollees, then this sort of flip-flop of Medicaid patients across hospitals could have plausibly served to shield the existing safety-net versus non-safety-net reimbursement gap.

To gauge whether the effects of the Florida reform might have been different had the managed care mandate been 100% inclusive, I conduct an extrapolation exercise in which I use the observed effects of the reform on only the "affected" Medicaid enrollees to predict what would have occurred in Florida had no reform county residents been allowed to retain traditional Medicaid coverage. To do this, I first take reform county cells in post-reform quarters and, within each of these cells, recode every traditional Medicaid hospitalization as Medicaid

managed care. I then distribute the recoded hospitalizations (at the county/quarter level) across safety-nets and non-safety-nets based on the observed proportion of Medicaid managed care hospitalizations which actually occurred at safety-nets and non-safety-nets in that county/quarter. For example, for Broward county in 2010 Q1, the original coding of:

County	***Quarter***	***Safety-Net***	***Traditional Medicaid Admissions***	***Medicaid Managed Care Admissions***
Broward	2010 Q1	1	5,690	1,859
Broward	2010 Q1	0	2,384	1,427

was transformed to:

County	***Quarter***	***Safety-Net***	***Traditional Medicaid Admissions***	***Medicaid Managed Care Admissions***
Broward	2010 Q1	1	0	$1{,}859 + ((5{,}690+2{,}384)* \frac{1{,}859}{1{,}859+1{,}427})$
Broward	2010 Q1	0	0	$1{,}427 + ((5{,}690+2{,}384)* \frac{1{,}427}{1{,}859+1{,}427})$

The estimates for the effect of Medicaid managed care under this regime are given in the last column of Table 4. Using this hypothetical exercise, it is estimated that the effect of a comprehensive Medicaid managed care program would have been a +$0.028 per dollar impact on the absolute value of the safety-net versus non-safety-net gap in average reimbursement per dollar charged. This corresponds to 3.5% more forward movement in safety-nets' per dollar reimbursement rate. A visual representation of this effect is shown in Figure 8. Here, we see that, compared to the realized outcome, safety-nets' per dollar return is propped up when traditional Medicaid coverage is disallowed and the entire Medicaid population is assumed to exhibit the same hospital sorting behavior as the managed care population.

7. Discussion

Given the findings of this study which suggest that Medicaid managed care did not intensified the relative burden of uncompensated care carried by safety-nets in Florida, there are two issues which merit discussion. First, it is important to consider the isolated impact of the reform on safety-nets, independent of other facilities. Because the DDD estimates represent the effects of the reform on the spread between safety-nets and other facilities, they do not indicate the direction that either hospital type was independently moving. To give an illustration, if we think of a race between safety-nets and other hospitals, and we position the two "runners" facing the same direction and allow the runners to start from different positions, then the DDD estimates tell us by how many more/fewer steps safety-nets traveled forward by the end of the race; they do not tell us is the direction that either runner was moving. Therefore, a zero effect on the gap between the average reimbursement rate received by a safety-net and non-safety-net does not necessarily imply a zero effect on safety-nets; safety-nets may still have finished the race behind their own starting position. Second, in order to gauge the generalizability of these findings, the specific characteristics of Florida's reform which distinguish it from other states' implementations of Medicaid managed care are considered.

First, to measure the isolated effects of the Florida reform on safety-net hospitals, Tables 5, 6 and 7 present the DD estimates for the effects of the Pilot Program on each of the previously estimated outcomes. As discussed, DD estimates (as given by equations (2) and (3)) represent the impact of the reform on individual facility-types, irrespective of other facilities' outcomes. As shown in Table 5, the enlarged gap in Medicaid managed care hospitalizations estimated in the DDD model was the result of a two-way movement of patients. In addition to non-safety-nets' number of Medicaid managed care hospitalizations increasing (by 125%), safety-nets'

number of Medicaid managed care hospitalization simultaneously decreased (by 125%). Yet, the same two-way reinforcement was not the case for the enlarged traditional Medicaid admission gap across hospital types. What we see here is that while the number of traditional Medicaid admissions at non-safety-nets was unaffected by the reform, safety-nets' number of traditional Medicaid admissions rose by 25%. This is insightful as it reveals a broader effect of Medicaid managed care implementation on safety-nets than the construction of a one-way exit. Additionally, the isolated 34% increase in non-safety-nets' "uninsured" hospitalizations provides some suggestive evidence that the previously estimated enlargement of the N_1 gap can be explained by inadvertent out-of-network hospitalizations among the Medicaid managed care individuals who were increasingly appearing at non-safety-nets.

Given the evidence that safety-nets gained traditional Medicaid patients as a result of the Florida reform, the original presumption that the acuity of safety-nets' patients would only be altered by a leakage of relatively sick managed care enrollees is now unfounded. In fact, Table 6 suggests that another mechanism was in play. As shown, while there was no statistically significant effect on the average charge associated with non-safety-nets' Medicaid managed care patients, the average charge to a safety-net's Medicaid managed care patient rose by 13%. Similarly, among safety-nets, the average total charge associated with a traditional Medicaid patient grew by 9.5% in response to the reform. This latter effect could be explained by either an outflow of relatively healthy Medicaid patients and/or an inflow of relatively sick traditional Medicaid patients. It is here that the isolated effect on non-safety-nets becomes insightful. Post-reform, we see that average total charge associated with a traditional Medicaid patient admitted to a non-safety-net declined by 12%. Given that non-safety-nets did not serve more traditional Medicaid patients, this result can only be explained by a health-based exchange of traditional

Medicaid enrollees. That is, relatively sick traditional Medicaid enrollees being "traded" for relatively healthy traditional Medicaid enrollees. In other words, these results suggest that in addition to the apparent two-way movement of Medicaid patients across hospital types based on type of coverage, there was a two-way movement of Medicaid patients across hospital types based on level of acuity. This is important as it refutes the presumption that safety-nets are only affected by an exit of relatively healthy Medicaid managed care individuals.

Finally, as shown in Table 7, it is estimated that the isolated effect of Florida's reform on safety-nets' financial health was a 1.6¢ reduction in the average reimbursement these hospitals received on each $1 they charged; this corresponds to a 2.2% decline. The isolated effect on non-safety-nets was a 1.4¢ reduction in the average reimbursement per $1 charged; this corresponds to a 1.7% decline. In other words, the null effect of the Florida reform on the safety-net versus non-safety-net reimbursement gap was not because the reform had no effect on hospitals. Rather, it was because the reform had an equally adverse effect on all hospital types. Had the reform not allowed eradicated traditional Medicaid coverage, however, this likely would not have been the case. Instead, this study projects that a 100% Medicaid managed care mandate would have left safety-nets' average level of reimbursement per dollar charged unaltered, while bringing down this rate by 2.4¢ in other facilities. That is, the projected narrower reimbursement gap estimated in the DDD model would occur at the expense of non-safety-net hospitals, whose compensation would be pulled down to more closely mimic safety-nets'.

Although the above results may appear to paint a grim picture of the effects of Medicaid managed care, it should be reiterated that these results do not necessarily support or refute the overall merit of Medicaid managed care. They say nothing as to whether this Medicaid structure saves the state money, increases enrollee satisfaction, increases healthcare access/utilization, or

affects public health. They also do not speak to the effects of the reform on healthcare providers that work in outpatient settings. They only evaluate the effects of the reform on hospitals, and, specifically, on the compensation that hospitals receive from the portion of their patients that are admitted.

Finally, to keep these results in perspective, it is important to reiterate that the outcomes presented this study are specific to one state's implementation of Medicaid managed care, and this one state's implementation of Medicaid managed care will not perfectly align with other states'. Therefore, in order to guard against loose extrapolations, and so that these results may be usefully applied in other contexts, two distinguishing factors of Florida's mandated Medicaid managed care program are highlighted. The first factor is related to the marginal impact of Florida's mandate. The second is related to the particular types of managed care plans offered in Florida.

First, as discussed previously, when Medicaid managed care was mandated in Florida, the pilot counties did not start from a point of 0% participation in Medicaid managed care and then move to a rate of 100% participation. Prior to the reform, the option of managed care was available to Medicaid enrollees (state-wide), and, even after the reform, particular individuals living in the reform counties were exempt from the reform's mandate. According to Coughlin et al. (2008):

> *"Prior to reform, statewide, only a third of Medicaid recipients were enrolled in a capitated managed care plan. The extent of managed care enrollment before the demonstration, however, varied from a virtual absence in some counties to as much as 80 percent in others. Medicaid enrollment in capitated managed care in*

the two initial pilot counties (Broward and Duval) was, respectively, 59 percent and 50 percent."

Then, in June of 2008, the Office of Program Policy Analysis and Government Accountability of the Florida Legislature reported:

"A little more than a year and a half after implementing Reform in Broward and Duval counties, 187,264 Medicaid beneficiaries were enrolled in a Reform health plan, which represented 65% of all Medicaid beneficiaries in those counties." (Medicaid, 2008)

Together, this would suggest that the reform resulted in an approximately 6 to 15 percentage-point increase in managed care enrollment in Broward and Duval counties. The Florida Legislature went on to report a post-reform 55% enrollment in managed care among Medicaid beneficiaries in the expansion counties (Baker, Clay, and Nassau). If the state-wide 33% pre-reform Medicaid managed care enrollment level is assumed in these counties, this would suggest that the reform resulted in an approximately 20 percentage-point increase in Medicaid managed care participation in Florida's three expansion counties.[17] Therefore, the results of this study should be interpreted as the marginal impact of mandating Medicaid managed care, when the margin is a roughly 6 to 20 percentage-point expansion in coverage, starting from an enrollment level of roughly 30-60%.

Second, as opposed to only allowing HMOs to participate in their Medicaid managed care program, Florida also allowed Provider Service Networks to participate. Because these organizations were largely formed around safety-nets, it is likely that Florida safety-nets were

[17] Because managed care plans steer enrollees away from inpatient care, county-level participation in Medicaid managed care cannot accurately be measured using hospital discharge data.

better equipped to ward off the financial effects of Medicaid managed care than those in states which do not intentionally incorporate safety-nets into managed care contracts. Additionally, Florida gave Medicaid enrollees the ability to replace their HMO/PSN plans when more attractive plans became available, and gave unprofitable managed care organizations the ability to drop out. This created much churning in the market as enrollees moved between available options. This also allowed the better performing networks to gain market share. Recently, there is evidence that, in Florida, PSNs (which were not forcefully tied to capitated reimbursement rates) have fared better than have HMOs. According to one University of Florida study, PSNs save approximately $7 more, per member, per month than do Medicaid HMOs. Additionally, enrollees covered by PSNs report slightly higher levels of satisfaction than those covered by HMOs. (Florida's Medicaid reform pilot) While, as of June 2008, the ratio of HMO to PSN coverage (among pilot county enrollees) was 73% to 27%, by August 2011, this ratio had fallen to 53% to 47%. (Bragdon, 2011) Therefore, the results of this study should be interpreted as the effects of a Medicaid managed care system that is highly comprised of relatively "safety-net friendly" managed care organizations, and, importantly, safety-net friendly organizations not tied to capitated rates.

8. Conclusion

Safety-net hospitals run a tight a ship. With high levels of uncompensated care, these facilities are particularly sensitive to healthcare reforms which would threaten to further reduce their bottom line. Recently, federal regulations have pushed safety-nets closer to the edge by reducing Disproportionate Share Hospital payments. On top of this, states are increasingly implementing Medicaid managed care as a means of saving money. Yet, these state-level reforms also have the potential to nudge safety-nets closer to the edge by altering their mix of

patients to include a greater proportion of low/non-paying individuals. Whether the imposition of Medicaid managed care will push safety-nets past their breaking point depends on the magnitude of its effect. This paper, therefore, estimates the magnitude of the effect of Medicaid managed care on safety-nets' compensation differential, compared to other facilities.

In 2006, the state of Florida began an experiment in which they required a subset of Medicaid recipients in five counties to enroll in managed care. In the rest of the state, enrollment continued to be optional. Using data on all hospital discharges in the state of Florida between the years of 2000 and 2012, I employ triple-difference estimation to measure the effects of this experiment on Florida's safety-net hospitals.

The results of this study suggest that Florida hospitals were harmed by the institution of Medicaid managed care, but not via the mechanism previously assumed. Although the evidence suggests that Medicaid managed care enrollees were directed towards other hospitals, it was an inflow of relatively sick traditional Medicaid patients (who were exempt from the mandate) that harmed safety-nets financially. Because these individuals brought with them below average reimbursement rates, safety-nets' average return per dollar charged was pulled down. Yet, a similar effect was also felt by non-safety-net hospitals following an inflow of low-paying Medicaid managed care patients. Comparatively, neither effect was stronger than the other, leaving the gap between safety-nets' and other hospitals' financial wellbeing unaltered. Had no Medicaid enrollees been exempt from managed care, however, this study projects that safety-nets would have felt no impact on their financial wellbeing, whereas the intensified inflow of Medicaid patients at other hospitals would have put additional negative pressure on these facilities' finances. That is, a Medicaid system rooted entirely in managed care would have

likely narrowed the gap between safety-nets' and other hospitals' rates of reimbursement – but not by pulling safety-nets up; rather, by pulling other hospitals down.

The difficulty in measuring the effects of healthcare reform is that, frequently, reform "switches" are flipped in unison so that researchers cannot disentangle the effects of one policy change compared to another. In the U.S., many of the ACA's healthcare reforms 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. At the tail end of these changes, it is fairly easy to determine whether a portion of the healthcare machine has broken down; it is virtually impossible to determine which flipped switch is to be credited for the effect. This study, however, identifies a way of cleanly estimating the effects of "turning on" one particular state-level reform. What the results of this study suggest is that, when a state chooses to implement Medicaid managed care, no Pareto improvement will occur in the financial wellbeing of the state's hospitals. Rather, the move to managed care will generate an additional weight on the state's hospital system, with this weight rolling towards non-safety-net hospitals as coverage becomes more expansive.

Figure 1. Map of Reform Counties

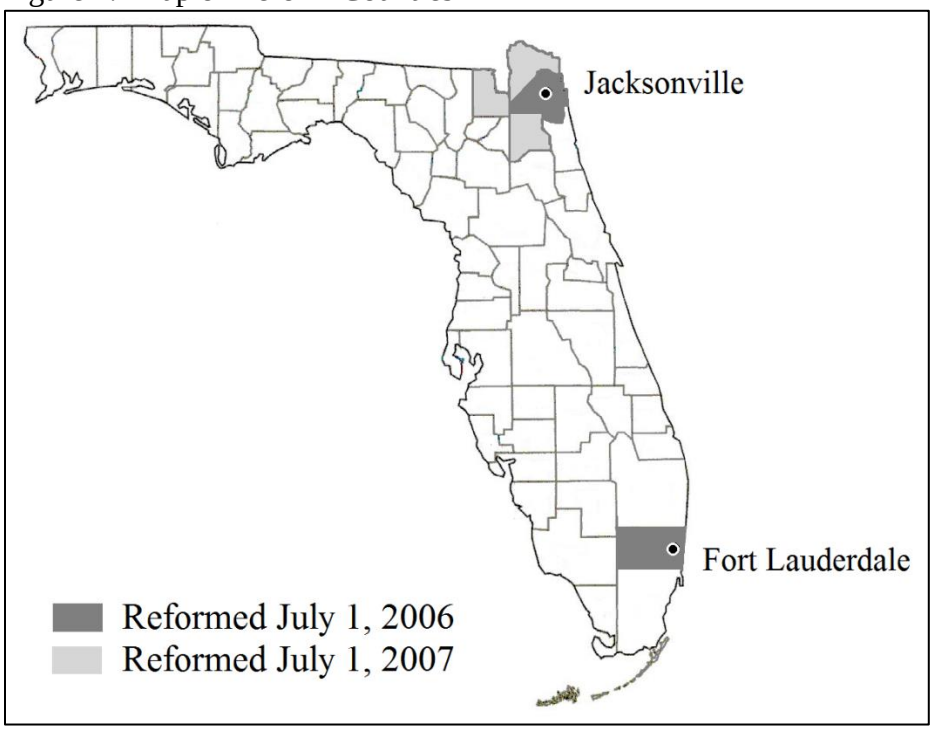


Figure 2.

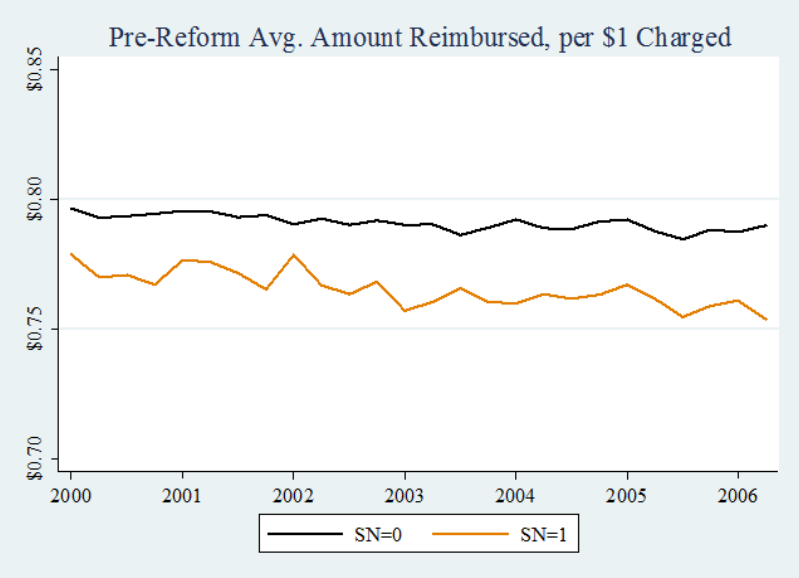


Figure 3.

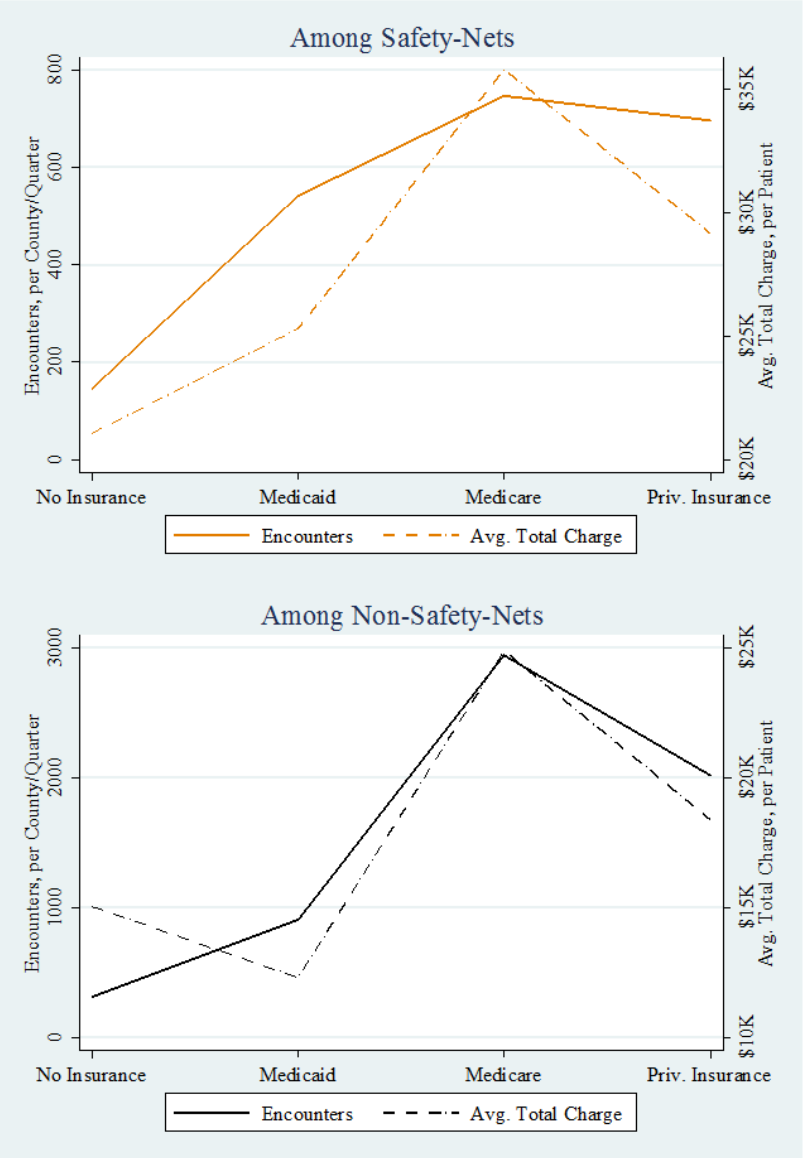


Figure 4.

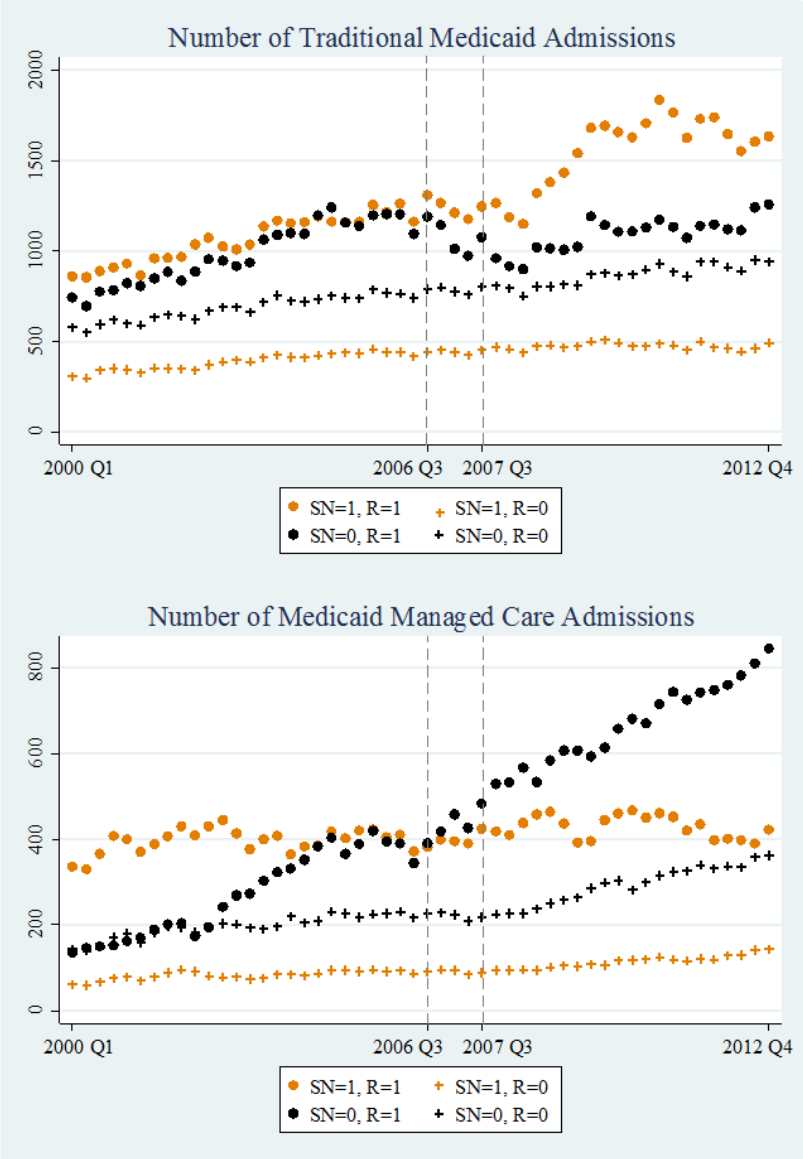
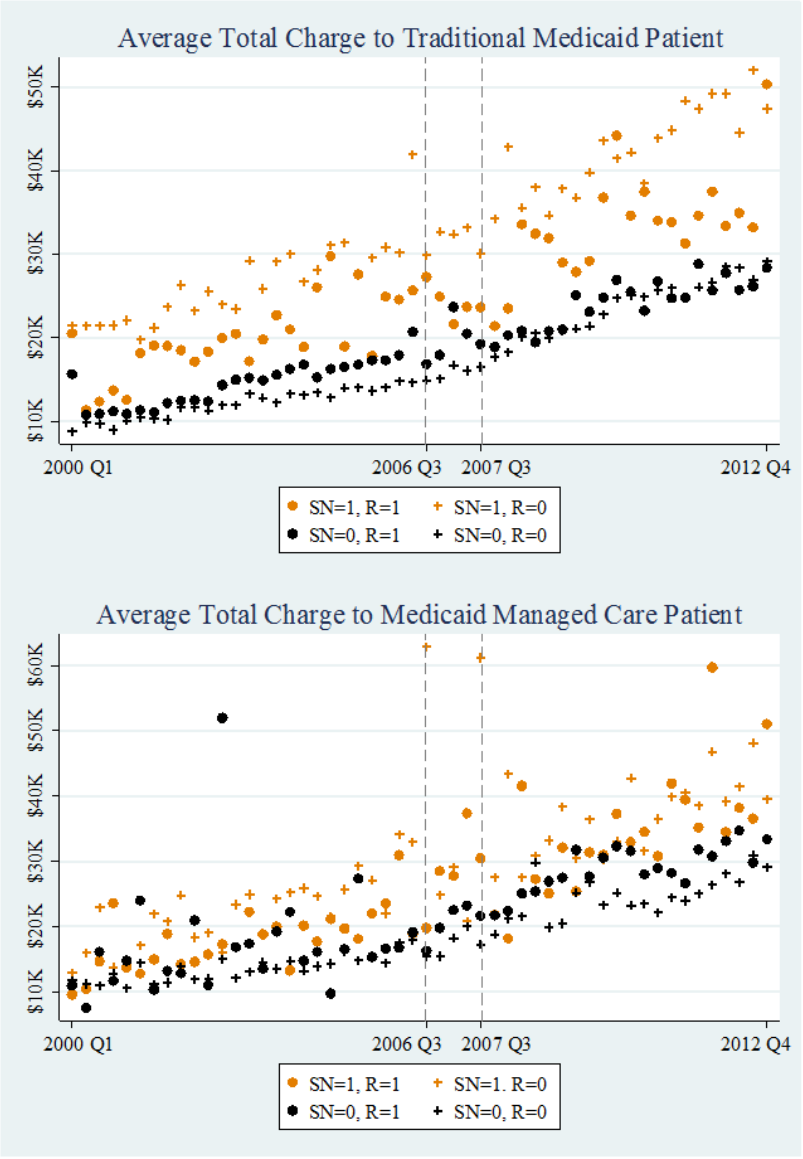
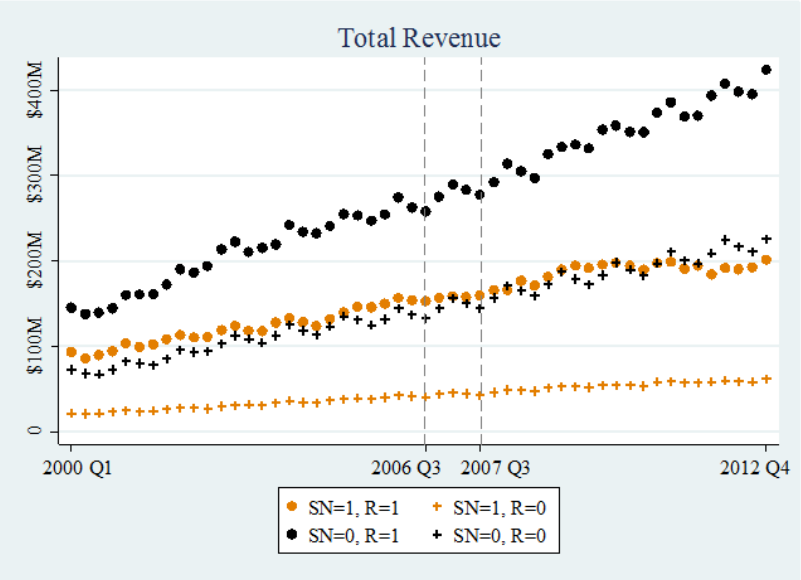


Figure 5.



Values are per county.

Figure 6.



Values are per county.

Figure 7.

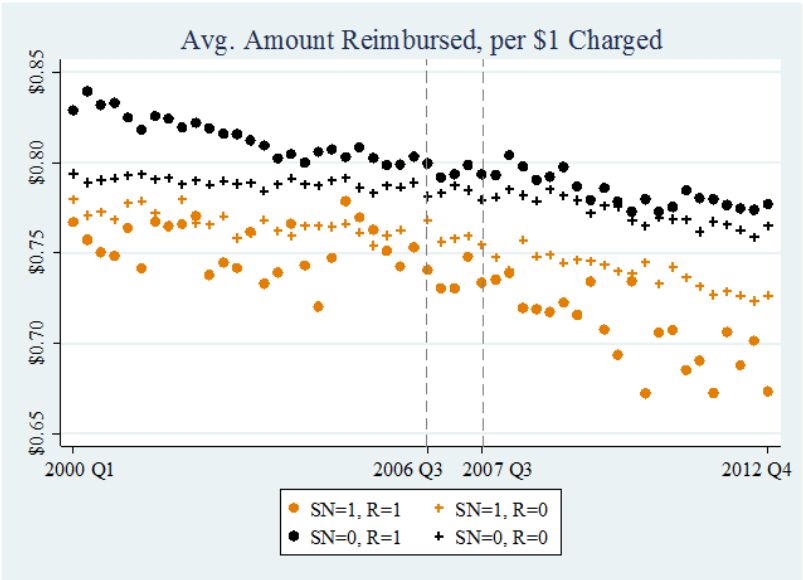


Figure 8.

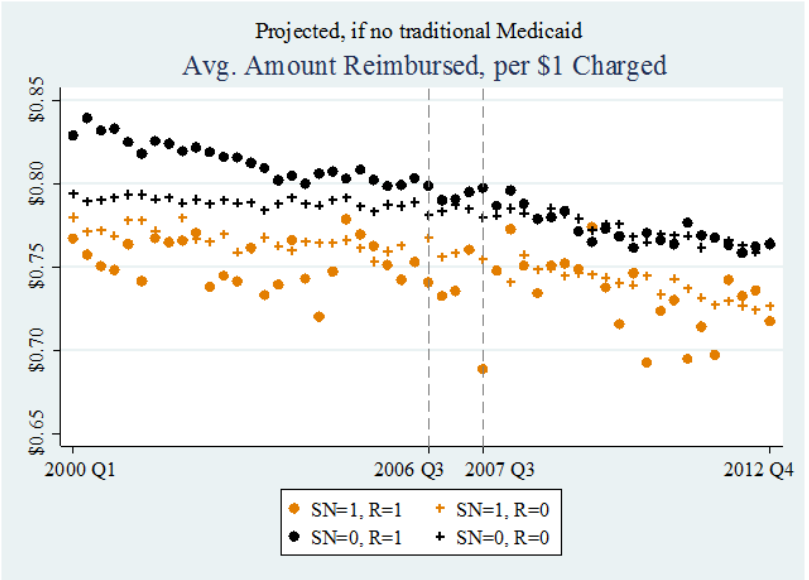


Table 1. Pre-Reform Summary Statistics

	Non-Reform Counties		Reform Counties	
	Aggregate Mean		**Aggregate Mean**	
	If SN=1	If SN=0	If SN=1	If SN=0
Hospital Discharges from Safety-Nets	**23%**		**40%**	
Healthcare Utilization				
Uninsured	**5%**		**7%**	
	6%	5%	10%	5%
Traditional Medicaid	**14%**		**12%**	
	22%	12%	15%	9%
Medicaid Managed Care	**4%**		**4%**	
	5%	3%	6%	3%
Medicare	**45%**		**39%**	
	35%	49%	36%	41%
Private Insurance	**32%**		**38%**	
	33%	31%	32%	41%
Avg. Total Charge, per Patient	**$22,779 (SD=$6,133)**		**$23,946 (SD=$5,009)**	
	$23,422 (SD=$7,675)	$22,587 (SD=$5,575)	$23,484 (SD=$3,798)	$24,250 (SD=$5,646)
Avg. Reimbursement, per $1 Charged	**$0.79 (SD=$0.03)**		**$0.78 (SD=$0.04)**	
	$0.76 (SD=$0.05)	$0.80 (SD=$0.02)	$0.74 (SD=$0.03)	$0.81 (SD=$0.01)
Demographics				
Male	**42%**		**41%**	
	43%	42%	42%	41%
White	**83%**		**73%**	
	76%	85%	67%	76%
Black	**14%**		**23%**	
	20%	12%	29%	20%
Hispanic	**15%**		**8%**	
	16%	15%	12%	5%
Average Age	**51 years (SD=7 years)**		**49 years (SD=4 years)**	
	43 years (SD=7 years)	54 years (SD=5 years)	45 years (SD=2 years)	51 years (SD=4 years)
Top 5 Principal ICD-9 Diagnoses [1]				
Most Frequent	V30	V30	V30	V30
Second Most Frequent	414	414	428	414
Third Most Frequent	786	786	786	786
Fourth Most Frequent	996	428	414	296
Fifth Most Frequent	486	486	296	428
Number of Discharges	2,813,095	9,390,301	898,276	1,366,149

[1] *Decimal places were dropped from ICD-9 codes so that codes represent three-digit ICD-9 diagnosis category. V30 - Single liveborn; 296 - Episodic mood disorders; 414 - Other forms of chronic ischemic heart disease; 428 - Heart failure; 486 - Pneumonia, organism unspecified; 786 - Symptoms involving respiratory system and other chest symptoms; 996 - Complications peculiar to certain specified procedures*

Table 2. DDD Effects on Number of Admissions

	Number of Uninsured Admissions		Number of Traditional Medicaid Admissions		Number of Medicaid Managed Care Admissions		Number of Medicare Admissions		Number of Private Insured Admissions	
	N_1	$\ln(N_1)$	N_2	$\ln(N_2)$	N_3	$\ln(N_3)$	N_4	$\ln(N_4)$	N_5	$\ln(N_5)$
Reform County (R)	382.6 ***	1.273 ***	707.5 ***	0.988 ***	194.7 ***	1.692 ***	2612.3 ***	1.243 ***	3060.7 ***	1.620 ***
	(36.3)	(0.111)	(50.4)	(0.087)	(21.3)	(0.197)	(308.6)	(0.096)	(308.9)	(0.101)
Quarter Dummies (Q')	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Safety-Net (SN)	-96.0	-1.395 ***	59.6	-0.550 **	-95.5	-2.646 ***	-840.2	-0.649 **	-17.3	-0.865 ***
	(94.1)	(0.473)	(204.4)	(0.281)	(83.4)	(0.602)	(582.5)	(0.283)	(309.9)	(0.293)
(R*P)	180.3 ***	0.359 **	-51.6	0.064	309.9 ***	1.329 ***	791.0 *	0.223	-81.4	0.118
	(67.3)	(0.159)	(79.8)	(0.129)	(53.5)	(0.228)	(478.4)	(0.138)	(404.4)	(0.142)
(R*SN)	104.6	0.020	-312.6 ***	-0.352 **	26.7	3.478 ***	-1424.6 ***	-0.496 ***	-1866.4 ***	-1.047 ***
	(99.7)	(0.219)	(102.4)	(0.174)	(32.4)	(0.274)	(399.7)	(0.178)	(412.2)	(0.183)
(Q'*SN)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
(R*P*SN)	**96.6**	**-0.218**	**519.9 *****	**0.295**	**-270.1 *****	**-2.492 *****	**-402.9**	**-0.084**	**-32.7**	**-0.012**
	(178.3)	**(0.310)**	**(173.4)**	**(0.237)**	**(75.2)**	**(0.322)**	**(609.7)**	**(0.248)**	**(543.7)**	**(0.252)**
Constant	133.3	4.548 ***	-1637.6 ***	7.428 ***	-135.7	0.684	733.9	4.551 ***	761.1	8.554 ***
	(203.7)	(1.170)	(580.0)	(0.893)	(194.9)	(1.958)	(1645.6)	(0.902)	(1130.0)	(0.846)
Full Set of Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.329	0.290	0.428	0.273	0.400	0.237	0.341	0.406	0.349	0.304
Observations	6,968	6,968	6,968	6,968	6,968	6,968	6,968	6,968	6,968	6,968

Controls include county/quarter/hospital-type shares of male, white and black (versus "other race"), Hispanic, and average age.
Cells with N_i=0 are replaced with 0.0001 to prevent missing observations.
*Values in parentheses are robust standard errors. Stars indicate statistical significance where * if p≤0.10, ** if p≤0.05, and *** if p≤0.01.*

Table 3. DDD Effects on Average Total Charges

	Average Total Charge to Uninsured Patient		Average Total Charge to Trad. Medicaid Patient		Average Total Charge to Medicaid MC Patient		Average Total Charge to Medicare Patient		Average Total Charge to Priv. Insured Patient	
	T_1	$\ln(T_1)$	T_2	$\ln(T_2)$	T_3	$\ln(T_3)$	T_4	$\ln(T_4)$	T_5	$\ln(T_5)$
Reform County (R)	$4,238 ***	0.304 ***	$3,612 ***	0.272 ***	$4,012 **	0.275 ***	$3,713 ***	0.185 ***	$387	0.038 **
	(526)	(0.026)	(481)	(0.024)	(1,612)	(0.048)	(559)	(0.017)	(356)	(0.016)
Quarter Dummies (Q')	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Safety-Net (SN)	$2,548	0.156 **	$13,186 ***	0.258 ***	$8,991	0.132	$2,686	0.005	$216	-0.037
	(2,260)	(0.071)	(4,542)	(0.083)	(5,649)	(0.115)	(3,126)	(0.054)	(2,601)	(0.058)
(R*P)	$691	-0.047	-$1,362 **	-0.100 ***	$1,105	0.092 *	$1,878 **	-0.010	$89	0.019
	(889)	(0.035)	(646)	(0.032)	(1,727)	(0.054)	(901)	(0.023)	(559)	(0.020)
(R*SN)	-$5,545 ***	-0.267 ***	-$7,560 ***	-0.299 ***	-$6,929 ***	-0.181 ***	-$6,291 ***	-0.236 ***	$2,161 **	0.082 **
	(885)	(0.040)	(1,207)	(0.044)	(2,143)	(0.065)	(968)	(0.027)	(1,088)	(0.033)
	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
(R*P*SN)	**$1,361**	**0.106 ***	**$409**	**0.190 ***	**-$164**	**0.039**	**-$6,749 ***	**0.005**	**$657**	**0.021**
	(1,704)	**(0.055)**	**(1,673)**	**(0.055)**	**(2,990)**	**(0.079)**	**(1,411)**	**(0.035)**	**(1,744)**	**(0.044)**
Constant	$18,967 ***	9.396 ***	$46,586 ***	10.165 ***	$30,485 **	9.369 ***	$83,709 ***	11.114 ***	$34,808 ***	9.934 ***
	(7,093)	(0.223)	(9,724)	(0.249)	(12,257)	(0.285)	(7,282)	(0.151)	(6,529)	(0.151)
Full Set of Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.267	0.389	0.343	0.506	0.083	0.284	0.513	0.589	0.449	0.574
Observations	6,863	6,863	6,955	6,955	5,870	5,870	6,960	6,960	6,965	6,965

Controls include county/quarter/hospital-type shares of male, white and black (versus "other race"), Hispanic, and average age.
Smaller sample sizes across columns are due to missing T_i values when N_i=0. No observations are lost due to T_i=0.
*Values in parentheses are robust standard errors. Stars indicate statistical significance where * if p≤0.10, ** if p≤0.05, and *** if p≤0.01.*

Table 4. DDD Effects on Financial Health

	Total Revenue		Avg. Amount Reimbursed, per $1 Charged		*Projected, if no trad. Medicaid* Avg. Amount Reimbursed, per $1 Charged	
	Rev	ln(Rev)	Reimb	ln(Reimb)	Prj Reimb	ln(Prj Reimb)
Reform County (R)	$160M ***	1.462 ***	$0.026 ***	0.033 ***	$0.026 ***	0.033 ***
	(14.2M)	(0.104)	(0.002)	(0.002)	(0.002)	(0.002)
Quarter Dummies (Q')	Yes	Yes	Yes	Yes	Yes	Yes
Safety-Net (SN)	-$85.6M **	-0.728 **	-$0.035 ***	-0.049 ***	-$0.034 ***	-0.047 ***
	(42.7M)	(0.286)	(0.006)	(0.009)	(0.006)	(0.009)
(R*P)	$81.4M ***	0.170	-$0.012 ***	-0.014 ***	-$0.022 ***	-0.028 ***
	(28.8M)	(0.146)	(0.002)	(0.002)	(0.002)	(0.002)
(R*SN)	-$106M ***	-0.816 ***	-$0.035 ***	-0.044 ***	-$0.035 ***	-0.045 ***
	(18.6M)	(0.178)	(0.004)	(0.005)	(0.004)	(0.005)
(Q'*SN)	Yes	Yes	Yes	Yes	Yes	Yes
(R*P*SN)	**-$20M**	**0.007**	**-$0.006**	**-0.011**	**$0.028 *****	**0.035 *****
	(37.5M)	**(0.249)**	**(0.005)**	**(0.007)**	**(0.005)**	**(0.007)**
Constant	$153M	17.961 ***	$0.813 ***	-0.204 ***	$0.815 ***	-0.202 ***
	(100M)	(0.788)	(0.020)	(0.027)	(0.020)	(0.027)
Full Set of Controls	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.374	0.338	0.241	0.238	0.223	0.220
Observations	6,968	6,968	6,968	6,968	6,968	6,968

Controls include county/quarter/hospital-type shares of male, white and black (versus "other race"), Hispanic, and average age.
*Values in parentheses are robust standard errors. Stars indicate statistical significance where * if p≤0.10, ** if p≤0.05, and *** if p≤0.01.*

Table 5. DD Effects on Number of Admissions

	Number of Uninsured Admissions		Number of Traditional Medicaid Admissions		Number of Medicaid Managed Care Admissions		Number of Medicare Admissions		Number of Private Insured Admissions	
	N_1	$\ln(N_1)$	N_2	$\ln(N_2)$	N_3	$\ln(N_3)$	N_4	$\ln(N_4)$	N_5	$\ln(N_5)$
DD Effect Among Only SN=1	256.6	0.028	411.8 ***	0.250	28.5	-1.251 ***	328.6	0.045	-126.4	0.019
	(166.3)	(0.261)	(154.1)	(0.185)	(55.8)	(0.228)	(432.4)	(0.198)	(395.8)	(0.200)
DD Effect Among Only SN=0	176.4 ***	0.341 **	-32.1	0.034	306.4 ***	1.248 ***	573.7	0.189	-181.8	0.075
	(64.3)	(0.137)	(83.7)	(0.112)	(51.5)	(0.218)	(411.2)	(0.120)	(373.5)	(0.124)
Full Set of Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Controls include county/quarter/hospital-type shares of male, white and black (versus "other race"), Hispanic, and average age.
*Values in parentheses are robust standard errors. Stars indicate statistical significance where * if p≤0.10, ** if p≤0.05, and *** if p≤0.01.*

Table 6. DD Effects on Average Total Charges

	Average Total Charge to Uninsured Patient		Average Total Charge to Trad. Medicaid Patient		Average Total Charge to Medicaid MC Patient		Average Total Charge to Medicare Patient		Average Total Charge to Priv. Insured Patient	
	T_1	$\ln(T_1)$	T_2	$\ln(T_2)$	T_3	$\ln(T_3)$	T_4	$\ln(T_4)$	T_5	$\ln(T_5)$
DD Effect Among Only SN=1	$2,203	0.059	-$528	0.095 **	$1,152	0.133 **	-$4,650 ***	-0.002	$786	0.038
	(1,482)	(0.043)	(1,588)	(0.046)	(2,499)	(0.060)	(1,091)	(0.026)	(1,709)	(0.040)
DD Effect Among Only SN=0	$590	-0.052	-$1,852 ***	-0.124 ***	$837	0.074	$1,153	-0.022	-$413	0.002
	(831)	(0.033)	(637)	(0.031)	(1,706)	(0.053)	(849)	(0.021)	(493)	(0.018)
Full Set of Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Controls include county/quarter/hospital-type shares of male, white and black (versus "other race"), Hispanic, and average age.
*Values in parentheses are robust standard errors. Stars indicate statistical significance where * if p≤0.10, ** if p≤0.05, and *** if p≤0.01.*

Table 7. DD Effects on Financial Health

	Total Revenue		Avg. Amount Reimbursed, per $1 Charged		*Projected, if no trad. Medicaid* Avg. Amount Reimbursed, per $1 Charged	
	Y	ln(Y)	Y	ln(Y)	Y	ln(Y)
DD Effect Among Only SN=1	$56.4M **	0.081	-$0.016 ***	-0.022 ***	$0.007	0.010
	(27M)	(0.186)	(0.004)	(0.006)	(0.005)	(0.007)
DD Effect Among Only SN=0	$70.7M ***	0.116	-$0.014 ***	-0.017 ***	-$0.024 ***	-0.031 ***
	(25M)	(0.129)	(0.002)	(0.002)	(0.002)	(0.003)
Full Set of Controls	Yes	Yes	Yes	Yes	Yes	Yes

Controls include county/quarter/hospital-type shares of male, white and black (versus "other race"), Hispanic, and average age.
*Values in parentheses are robust standard errors. Stars indicate statistical significance where * if p≤0.10, ** if p≤0.05, and *** if p≤0.01.*

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