The Indirect Effects of Medicaid Payment Changes: Evidence of Spillovers to the Commercially Insured*

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Abstract

We examine whether effects of a Medicaid policy that discontinued payment for Early Elective Deliveries (EEDs) – a low-value mode of childbirth defined as a scheduled, non-medically necessary induction or cesarean section (c-section) before 39 weeks' gestation – spilled over to other payers and services. The policy was implemented in four states between January 1, 2014 and January 1, 2015, to incentivize lower use of EEDs, which are convenient for the physician and patient, but are not clinically beneficial. Previous research showed that in response to the policy, physicians significantly reduced the supply of EEDs in the Medicaid population (Dahlen et al., 2017; Allen and Grossman, 2019). Empirical studies have not examined indirect effects of the policy in the non-Medicaid population, including whether the observed change in Medicaid EED volume extended to the commercial sector. Using the Hospital Compare database in a difference-in-differences analysis, we assess whether the change in Medicaid payment policy impacted physician behavior across all payers. All-payer EEDs in treatment states declined 3.3% more than in control states post- relative to pre-policy implementation. Hospital Compare data do not allow us to disentangle the direct result within Medicaid and the spillover to privately insured patients; however, effects on all-payer EED rates did not vary with the Medicaid coverage rate, suggesting that there was an effect among commercially-insured patients. We find larger effects in areas with a higher share of for-profit hospitals, consistent with a stronger response to financial incentives among physicians in for-profit hospitals. We test for the presence of physician-induced demand in response to the Medicaid payment change, but find no evidence of this behavior.

Key Words: Physician payment; low-value care; perinatal care; Medicaid; private insurance; spillover effects; supplier-induced demand

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1 Introduction

Physician behavior is a key driver of health outcomes and spending, as many medical services are not directly demanded by a patient, but requested by a doctor on the patient's behalf (Chandra & Skinner, 2012; Ellis & McGuire, 1986; Kessler & McClellan, 1996; Smith, Saunders, Stuckhardt, & McGinnis, 2013). One contributor is the financial incentive that a doctor receives. It is well established that under the prevailing payment model, fee-for-service (FFS), physicians are influenced to overprescribe low-value care to maximize their income (Ellis & McGuire, 1986; Ikegami, 2015; Mendelson et al., 2017). Low-value care, defined as the provision of a medical procedure that provides little or no benefit to patients, has potential to cause harm or incur unnecessary costs; it also wastes healthcare resources and deviates from the social optimum (Brownlee et al., 2017; Brownlee, Saini, & Cassel, 2014; Mafi et al., 2017; Maratt et al., 2019; McGlynn et al., 2003). To address this concern, many states and payers have adopted policies that move payment systems away from traditional FFS towards reimbursement mechanisms that align spending with quality; these approaches often aim to discourage the provision of low-value care by incorporating lower physician earnings, or prices, for these services (MedPAC, 2019).

There are two relevant streams of literature on how physicians respond to price changes. The first is on spillovers, or the extent to which a financial incentive directed towards patients with one insurance type affects patients with other insurance types. Spillovers are difficult to predict, as there is debate over whether physicians adhere to "custom made" or "ready-to-wear" treatments (e.g. addressing an individual patient's needs on a case-by-case basis or treating a broad class of patients with a standardized "norm," respectively) (Frank et al., 2007). On one hand, doctors have demonstrated an inclination to treat a "modal" patient, rather than differentiate by insurer, to circumvent various costs (e.g. communication, cognition, coordination, and capability); this suggests that when a physician's dominant payer alters payment incentives, the behavioral response may spillover to other populations (Frank et al., 2007; Glied & Zivin, 2002; Tai-Seale, McGuire, & Zhang, 2007). On the other hand, empirical work also supports the notion that physicians customize care across patients, as varying payment rates from public and private insurers have led to significantly different utilization patterns, waiting times, and number of follow-up visits (Jürges, 2009; Lungen, Stollenwerk, Messner, Lauterbach, & Gerber, 2008; Newhouse & Marquis, 1978; Schwierz, Wübker, Wübker, & Kuchinke, 2011). Despite evidence in both areas, there is little understanding of the circumstances under which physicians use custom made versus ready-to-wear treatments. Developing a greater insight into the factors that lead to variation in spillovers is critical to understanding physician responses to payment reforms. The second area is physician-induced demand, where physicians, acting as agents on behalf of patients (who lack medical knowledge to make autonomous treatment decisions), request additional volume of services in response to negative income shocks (Evans, 1974; Mcguire & Pauly, 1991). Prior studies support this theory across Medicare and commercial services, particularly when procedure intensity is high and elective in nature (e.g., heart attack treatment, advanced imaging, and cesarean sections (c-sections)) (Clemens & Gottlieb, 2014; Coey, 2015; Foo, Lee, & Fong, 2017; Jonathan ; Gruber & Owings, 1996; Jacobson, Earle, Price, & Newhouse, 2010; Yip, 1998). In contrast, the limited literature on fee changes in Medicaid have found no evidence of inducement, arguing that non-Medicaid findings cannot be generalized since Medicaid patients are a small share of a physician's patient pool (Jon Gruber, Kim, & Mayzlin, 1999; Johnson & Rehavi, 2016). More research on inducement and other unintended consequences of incentives in the Medicaid context is imperative for developing state-level policies to reduce low-value care.

We build on the literature of physician behavior by exploring whether discontinuing Medicaid payment for Early Elective Deliveries (EEDs) – a low-value mode of childbirth defined as a scheduled, non-medically necessary induction or c-section prior to 39 weeks gestation – affects privately insured patients. Specifically, we address the following questions: (1) does the change in Medicaid payment prompt a payer-related spillover, in the form of a reduction in EEDs, in the commercial sector? (2) is there evidence of physician-induced demand, in the form of an increase in low-risk c-sections, which are less time-intensive and more profitable, but are also riskier, than vaginal deliveries? and (3) are variations in spillovers consistent with financial or reputational drivers?

In perinatal care, fee reductions associated with low-value EEDs emerged as a promising option to reduce long-term hospital costs. EEDs constituted nearly 20% of all U.S. hospital births in 2010, exceeding the patient safety target of 5% (Main et al., 2010). Because EEDs can be conveniently scheduled, and are paid at the same rate as full-term deliveries under FFS, there are strong incentives to continue their provision. Up to this point, providers were encouraged, by non-financial means, to reduce the provision of EEDs. In February 2013, the Choosing Wisely campaign, in conjunction with the American College of Obstetricians and Gynecologists (ACOG), released an official guideline discouraging EEDs, which pose significant dangers to mothers, including increased risk of infection and postpartum hemorrhage. EEDs have no known clinical benefits, but physicians may schedule them for convenience reasons, perceived liability concerns, or to relieve symptoms during the final stages of pregnancy (Choosing Wisely, 2013). EEDs often generate higher medical expenses compared to full-term, spontaneous births. Since Medicaid covers 45% of births, Medicaid programs in Georgia, Indiana, Missouri, and Mississippi adopted policies that discontinued physician reimbursement for EEDs between January 1, 2014 and January 1, 2015 (The Henry J. Kaiser Family Foundation, 2019). Physicians were only eligible to receive payment for births prior to 39 weeks gestation if it was properly documented as medically necessary, providing a financial incentive to reduce overuse. Supplying this low-value mode of delivery would forego the average $7,213 that a physician earns per Medicaid birth (Caughey et al., 2009). Several states adopted a range of other approaches. For example, Washington, Colorado, and Wisconsin implemented Medicaid pay-for-performance programs, which provided physicians with a bonus payment if they achieved a goal EED rate. Other states implemented non-financial approaches, such as voluntary “hard stop” initiatives, which encouraged hospitals to take a pledge to end the provision of EEDs, and quality improvement collaboratives, which were multi-stakeholder efforts to educate physicians and expecting mothers about the dangers of EEDs.

In this paper, we investigate spillover effects of the Medicaid payment policy on low-value care among the commercially insured, and compare effects to four groups with varying financial and non-financial incentives, including states with: (1) no policies aimed at curbing EEDs, (2) voluntary “hard stop” initiatives, (3) quality improvement collaboratives, and (4) Medicaid pay-for-performance programs with a bonus for reducing EEDs. To date, studies have measured the Medicaid payment policy’s direct effects within Medicaid; all are single state analyses with a difference-in-differences (DD) design (Allen & Grossman, 2019; Dahlen, Mccullough, Fertig, Dowd, & Riley, 2017). Dahlen et al. (2017) examined the impact of the 2011 Texas payment change, finding a 14% significant decline in the EED rate compared to control states, with a larger impact on minority patients. Birth outcomes also improved significantly, with birthweight increasing by 6 ounces (Dahlen et al., 2017). Allen and Grossman (2019) explored the effects of the Medicaid policy in South Carolina. The study found that the Medicaid payment policy (which was implemented in both commercial and Medicaid markets) reduced EEDs by 10.9%, while a voluntary hard stop policy led to a 12.7% decline in EEDs, relative to controls. The decline in EEDs was higher among Medicaid, rather than commercial patients (Allen & Grossman, 2019).

Byanova (2015) assessed the joint effects of a hard stop initiative and a Medicaid nonpayment policy, finding that Medicaid and non-Medicaid EEDs declined by 18.5% and 5.9%, respectively. The study also observed a 13.1% increase in the non-Medicaid total c-section rate, attributing this to demand inducement (Byanova, 2015). To date, empirical studies have not examined indirect effects of the policy in the non-Medicaid population in a multi-state analysis. Further, prior work has not directly compared the impact of the Medicaid payment policy to other financial and non-financial strategies for reducing EEDs. We aim to fill this gap by exploring whether effects of the Medicaid nonpayment policy spilled over to the commercial sector, and how these effects varied across policy approaches.

Using the Medicare Hospital Compare database in a DD analysis, we assess whether the change in Medicaid payment policy in four states (GA, IN, MO, and MS) impacted physician behavior in the commercial market relative to each comparison group, including states that adopted (1) no policy to reduce EEDs, (2) a hard stop policy, (3) a quality improvement collaborative, and (3) a Medicaid pay-for-performance program. First, we explore whether the observed change in Medicaid EED volume extended to the commercial sector. We find that all-payer EEDs in treatment states declined 3.3% more than in main control states post- relative to pre-policy implementation. The Medicaid payment policy also reduced all-payer EEDs by 3.9% and 3.6% compared to states with hard stop policies and pay-for-performance payment programs, respectively. Hospital Compare data do not allow us to disentangle the direct result within Medicaid and the spillover to privately insured patients; to gain insight into this question, we examine whether effects increase in geographic areas with a higher share of Medicaid patients. Effects did not vary with Medicaid rate, providing suggestive evidence that commercial patients were impacted by the policy. Next, we examine whether results aligned with the demand inducement hypothesis, under which the income lost from a drop in EED price would prompt an increase in other low-value services, including low-risk c-sections (Mcguire & Pauly, 1991). C-sections are reimbursed at an average rate 50% higher than vaginal deliveries, and are considered dangerous for low-risk women (Teleki, 2017). We do not find inducement-related spillovers in the commercial sector, as there were no significant changes in the rate of low-risk c-sections, and the magnitude of the effect did not change in areas where c-sections were more profitable. Finally, we evaluate whether areas with a higher share of for-profit versus non-profit hospitals respond differently to the policy, since we might expect non-profit hospitals to have reputational objectives, and for-profit hospitals to hold financial ones (Dranove, Garthwaite, & Ody, 2017; Horwitz, 2005; Newhouse, 1970). We find the policy response to be larger in areas with a higher share of for-profit hospitals, and smaller in areas with a higher proportion of non-profit hospitals. This may indicate a stronger response to financial incentives among physicians in a for-profit hospital setting. It may also point to barriers in changing physician practice patterns when their hospitals have reputational, rather than financial, objectives.

The Medicaid payment policy presents a novel opportunity to study the indirect effects of financial incentives on physician behavior, and to understand the impact at a system level. Studying a physician's financial incentives in the context of perinatal care has several advantages. First, perinatal care is clinically salient. Since 2013, average inpatient costs have increased 32%, while overall hospital spending has only grown by 4.8% (Kamal & Cox, 2018; Truven Health Analytics, 2013). Nonetheless, the U.S. experiences the highest maternal mortality rate of all developed countries (Carroll, 2017). Over half of U.S. hospitals lag below the national target in quality scores, suggesting that treatment patterns have significant room for improvement (Consumer Reports, 2017). Second, since repeat childbirth is unpredictable, and volume is relatively independent from physician influence (e.g. compared to services like elective arthroplasty, that require a pre-surgical appointment), the measured physician response is expected to stem directly from the payment reform as opposed to potential confounders (Carroll, Chernew, Fendrick, Thompson, & Rose, 2018). This increases the likelihood that results can apply in other settings. Finally, perinatal care possesses features that have been linked to stronger provider responses, including high variation in quality and costs, and presence of elective services with practical treatment substitutes (Chou et al., 2006). There are also validated quality measures through which to track overuse, allowing the direct physician response to be isolated.

This study makes several contributions to the literature. First, this analysis is one of the first to study payer- and treatment-related spillovers of financial incentives in the same setting. This examination lends insight into how physicians customize care, an important factor in understanding the full effect of reimbursement changes. Most studies on financial incentives examine direct effects within Medicare and private insurance, with minimal focus on indirect effects, especially in the context of Medicaid payment. Research on spillovers has examined whether physicians customize treatment to an individual patient, but rarely evaluates drivers of variation in these effects. Second, we provide a more precise measure of demand inducement by exploring whether the Medicaid payment policy affects low-risk, rather than total, c-section rate. This is an important distinction because low-risk c-sections capture inappropriate use of c-sections. Prior studies have focused on the total c-section rate, which limits the ability to make inferences about demand inducement, since c-sections are appropriate for risky births, but low-value for uncomplicated ones (Goer, Romano, & Sakala, 2012).

We show that reducing Medicaid payment for low-value services provides two broad results: (1) it discourages overprovision of low-value care in the non-Medicaid population, and (2) spillovers to the non-Medicaid population are smaller than the direct effect within Medicaid. We further illustrate that physicians are generally more responsive to financial incentives when it aligns with hospital objectives. We also highlight important tools for incentive design, including that: (1) a financial penalty for low-value care can lead to stronger spillovers than a financial bonus, (2) mandatory financial incentives can be more effective than voluntary, non-financial ones, and (3) quality improvement programs with interdisciplinary collaboration and educational components may yield comparable effects to payment incentives. These comparisons provide greater understanding into payment reform, such as how to structure incentives, determine participation, and incorporate additional education and teamwork elements.

The paper proceeds as follows. Section 2 provides background on incentives and the policy landscape. Section 3 lays out the theoretical framework. Section 4 describes the analytic dataset and empirical strategy. Section 5 discusses the main results, Section 6 concludes.

2 Background: Policy Landscape on Perinatal Incentives

Since 2007, several states have adopted policies employing financial or non-financial incentives aimed at reducing EEDs across all payers, in an effort to generate perinatal care cost savings and improve birth outcomes. In this study, I evaluate the effect of a Medicaid policy change that stopped physician payment for EEDs. The first state to implement nonpayment for Medicaid EEDs was Texas in 2011. Since, ten other states have enacted the same policy (New York, New Mexico, Nevada, Montana, South Carolina, Louisiana, plus the four treatment states). Reducing or eliminating payments for a given service is advantageous from a policy perspective due to its simplicity. Methodologically, it is straightforward, which serves as a strong predictor for its effectiveness. In particular, studies show that incentives are most conducive to successful behavior change and decreased gaming when there exists a clear, one-to-one relationship between the behavior and reinforcement (Town, Wholey, Kralewski, & Dowd, 2004). Further, this payment policy largely retains a FFS structure, the preferred payment design among physicians (Bain, 2017; Ikegami, 2015). I compare the effects on EED rates of the elimination of payment policy to three other policies which have similar goals and offer either financial or non-financial incentives to physicians.

Another approach that leverages financial incentives is Medicaid pay-for-performance reimbursement, which offers a bonus payment to physicians who achieve a benchmark EED rate. In 2010, Washington launched the Safety Net Assessment Act, which gave hospitals a 1% increase in their Medicaid reimbursement for reducing EEDs from one year to the next (Association of State and Territorial Health Officials, 2014). Colorado adopted a similar program in 2011, called the Hospital Quality Incentive Payment (HQIP) Program. HQIP offers volume-adjusted payments based on Medicaid discharges and quality achievement on five performance measures (one of which is EEDs) (Colorado Medicaid, 2016). Wisconsin rolled out the Obstetric Medical Home (OBMH) program between 2011 and 2013, which pays the obstetrician an additional $1,000 for each Medicaid patient that attends ten prenatal visits and a postpartum visit within 60 days of birth. OBMH practitioners are given an additional $1,000 bonus per positive birth outcome, including full-term births (Agrawal, 2017). The comparison between discontinuing payment for EEDs and providing a bonus for EED performance in Medicaid applies behavioral economic principles of prospect theory, under which individuals value gains and losses of the same magnitude asymmetrically, as they lose more utility from a penalty than they gain from an equivalent bonus. From a behavioral perspective, this suggests that when faced with uncertainty (e.g. reimbursement changes for EEDs), financial penalties will be more effective than bonuses; physicians, aiming to avoid financial losses, will reduce the provision of low-value care, even if it means suboptimal expected utility (Kahneman & Tversky, 1979).

There are two main approaches that utilize non-financial incentives. The first is a "hard stop" policy, under which hospitals voluntarily pledge to ban EEDs by requiring hospital review and approval for any delivery before 39 weeks' gestation without documented indication. Eleven states (Arkansas, Arizona, California, Delaware, Iowa, Michigan, Minnesota, North Carolina, Oregon, Tennessee, and Oklahoma) implemented these initiatives between 2009 and 2013. This policy adds an effort-related cost to providing an EED to discourage its delivery. Although these programs target hospitals with a relatively higher share of Medicaid births, all hospitals are encouraged to participate. The pledge to reduce EEDs applies to the overall hospital rate, and is not payer-specific. The second approach is a quality improvement collaborative, which takes on a range of structures. They typically involve a coalition of professional, clinical, and non-governmental organizations rolling out educational awareness campaigns on the dangers of EEDs and low-risk c-sections for expecting mothers and delivering obstetricians. Some programs go a step further by requiring hospitals to report their EED rates, to track performance and promote accountability. This strategy is two-pronged, as it attempts to change the culture surrounding provision of EEDs through education and multi-stakeholder buy-in, while also publicly comparing physicians with their peers to enact social pressure and change norms.

3 Theoretical Framework

In this section, we develop a theoretical framework for understanding why variation in spillovers occurs in response to financial incentives, in the context of low-value services. In particular, we are interested in how physicians adjust the provision of care in the commercial sector in response to Medicaid nonpayment of a low-value service. We begin with a physician utility model in the style of Ellis and McGuire (1986), where a physician selects a quantity of services to maximize utility over profits (π) and patient well-being (B) (Ellis & McGuire, 1986). We extend the framework by considering how a physician may vary behavior across payers and services. In particular, our utility model U allows flexibility for a physician to choose a different quantity of care for Medicaid (x_m) and non-Medicaid (x_n) patients across services $j = 1,2, \dots, k$:

$$U = \sum_{j=1}^{K} \alpha_{\text{HOSP}}\left[B\left(x_{j,m}\right) + B\left(x_{j,n}\right)\right] + \delta\pi\left(x_{j,m}\right) + (1-\delta)\pi\left(x_{j,n}\right) \quad [1]$$

where profit is a function of the income for Medicaid and non-Medicaid services (ρ_m and ρ_n), characterized by net earnings after subtracting monetary costs from total reimbursement; non-financial, or implicit, effort-related costs (e); and quantity of care selected across Medicaid and non-Medicaid patients (x_m and x_n):

$$\pi(\rho, e; x_m, x_n) \quad [2]$$

The model adds two unique components. First, it considers the share of a physician's patients that are insured by Medicaid, δ, and the remaining proportion insured by non-Medicaid payers $(1-\delta)$. Second, it assumes that the weight a physician places on patient benefits varies by the type of hospital that a physician is employed. In practice, a physician's preference contains a weight for agency, α, which represents the marginal rate of substitution, or the rate at which the physician is willing to trade off one dollar of hospital profit for one dollar of patient benefit, such that $1 > \alpha > 0$. If a physician were to serve as perfect agent for the patient, then $\alpha = 1$, and then physician weights their profit equally to the patient's benefit. We assume that α varies with the index HOSP, or the type of hospital that a physician is employed (e.g. non-profit, for-profit, or public), and that the physician serves as a better agent when employed by a non-profit, relative to a for-profit, hospital ($\alpha_{\text{FORPROF}} < \alpha_{\text{NONPROF}}$).

For simplicity, we explore the case where k represents *E* for EEDs, *C* for low-risk c-sections, and *V* for full-term vaginal deliveries. The distinction between these services is that *E* and *C* are low-value, and *V* is high-value, so the implicit costs of providing *E* and *C* (e.g. concerns about harming the patient, uncertainty as to whether the service is appropriate) are relatively higher. Also, the marginal profit is higher for *C* than for the other services. The first-order conditions (FOCs) for Medicaid and non-Medicaid treatment are:

$$\alpha_{\text{HOSP}}\left[B'\left(x_{E,m}\right) + B'\left(x_{C,m}\right) + B'\left(x_{V,m}\right)\right] + \delta\left[\pi'\left(x_{E,m}\right) + \pi'\left(x_{C,m}\right) + \pi'\left(x_{V,m}\right)\right] = 0 \qquad [3]$$

$$\alpha_{\text{HOSP}}\left[B'\left(x_{E,n}\right) + B'\left(x_{C,n}\right) + B'\left(x_{V,n}\right)\right] + (1-\delta)\left[\pi'\left(x_{E,n}\right) + \pi'\left(x_{C,n}\right) + \pi'\left(x_{V,n}\right)\right] = 0 \qquad [4]$$

In general, we assume that $\pi'\left(x_{j,n}\right) > \pi'\left(x_{j,m}\right)$, or the marginal profit is higher for non-Medicaid patients for a given service, as prices tend to be higher on average. Assuming that marginal patient benefits are equal across payers (e.g. $B'\left(x_{E,m}\right) = B'\left(x_{E,n}\right)$, $B'\left(x_{C,m}\right) = B'\left(x_{C,n}\right)$, and $B'\left(x_{V,m}\right) = B'\left(x_{V,n}\right)$), and rearranging, we have:

$$\frac{\delta}{(1-\delta)} = \frac{\pi'(x_{E,n}) + \pi'(x_{C,n}) + \pi'(x_{V,n})}{\pi'(x_{E,m}) + \pi'(x_{C,m}) + \pi'(x_{V,m})} \qquad [5]$$

When the Medicaid payment policy is implemented, the marginal profit for Medicaid EEDs, $\pi'\left(x_{E,m}\right)$, drops significantly. Since the payment policy is only implemented in Medicaid, the marginal profit for non-Medicaid EEDs remains relatively higher, and the gap widens relative to pre-policy implementation: $\pi'\left(x_{E,m}\right) < \pi'\left(x_{E,n}\right)$. Intuitively, the volume of Medicaid EEDs will fall with the price, a result that has been demonstrated empirically (Allen & Grossman, 2019; Byanova, 2015; Dahlen et al., 2017). Since the right and left terms are equivalent, it follows that the total marginal profit among non-Medicaid services, $\pi'\left(x_{E,n}\right) + \pi'\left(x_{C,n}\right) + \pi'\left(x_{V,n}\right)$, increases with δ, or share of Medicaid patients. Based on this result, it is likely that the mix of services changes among the non-Medicaid population after implementation of the Medicaid payment policy, but whether and how physicians choose to substitute depends on the spillover mechanism.

Spillovers may arise through several mechanisms. One likely avenue is through use of ready-to-wear treatments, or common practice patterns across patients, regardless of insurance type (Frank et al., 2007). This may transpire as a physician's strategy to combat the challenges of customizing care for patients insured by different payers. It may also result from a physician learning new skills while treating Medicaid patients, and applying them to non-Medicaid patients (Baicker, Chernew, & Robbins, 2013; Chernew, Baicker, & Martin, 2010). In the model, this is represented by marginal profits for EEDs, $\pi'(x_{E,n})$, decreasing in δ because of implicit, non-monetary costs of continuing to provide the service. The magnitude of the response is expected to rise as a physician gains patients from Medicaid, with practice patterns converging to those for the "modal" patient (Glied & Zivin, 2002). Spillovers are also more likely to arise when patients have similar clinical reasons for seeking medical care, such as childbirth. In these circumstances, it is likely that the physician can use the same standards of care across patients, potentially leading to improvements in outcomes and cost savings (Chernew et al., 2010). If a reduction in non-Medicaid EEDs is accompanied by an increase in full-term vaginal deliveries, the spillover may be a welfare-improving, as it helps to offset services in which the marginal cost exceeds the net patient benefit (Baicker et al., 2013).

Another channel for spillovers is through physician-induced demand, under which physicians respond to negative income shocks by increasing volume or intensity of services, beyond the optimal amount (Mcguire & Pauly, 1991). Inducement is "costly" for the physician, in the sense that it may cause harm to the patient; thus, it will only occur when the profit margin is high and the time cost is low. Spillovers of a fee change in one market may be characterized by inducement in more profitable sectors, since the physician has alternate avenues through which to recoup income. In particular, inducement is likely to occur when a substitutable, more profitable, and less time intensive service exists through which to pursue inducement (Chernew et al., 2010; Mcguire & Pauly, 1991). In the model, we may observe physician-induced demand through the effect of the Medicaid payment policy on low-risk c-sections among non-Medicaid patients. In this setting, it is possible that the physician seeks to recover income lost for EEDs by increasing the volume of a more profitable service, such as low-risk c-sections. As the marginal profit for low-risk c-sections increases compared to its substitutes, inducement-related spillovers are also expected to rise. These effects are also likely to amplify when exposure to the fee change, measured by δ, increases, because physicians must induce more to account for a greater proportion of lost income. This spillover suggests that policy efforts to reduce spending in Medicaid may be cost-increasing for the commercial sector, due to higher profitability among privately insured patients.

The degree to which these spillovers occur may be influenced by the physician's hospital type. Non-profit hospitals have a distinct objective function, under which they aim to maximize quantity and quality, rather than focusing predominantly on profits (Newhouse, 1970). This is represented in our model by varying physician agency, α, by hospital type, where patient benefits are considered more important by physicians in a non-profit hospital setting ($\alpha_{\text{FORPROF}} < \alpha_{\text{NONPROF}}$). In general, this framework suggests that in the absence of financial incentives, non-profit hospitals maintain higher levels of quality, and lower supply of low-value services, relative to for-profits. However, it also implies that non-profit hospitals may be less attentive to changes in the financial environment, including payment incentives aimed at reducing low-value care. In contrast, physicians in for-profit hospitals, driven primarily by financial objectives, are likely to respond more strongly to changes in service profitability (Dranove et al., 2017; Horwitz, 2005).

In this analysis, we empirically test model predictions by investigating whether there were spillovers of EEDs and low-risk c-sections to the commercial sector after implementation of the Medicaid payment policy. If spillovers are prompted by ready-to-wear treatments, we might expect there to be a greater reduction in EEDs as policy exposure, or the share of Medicaid patients, rises, as this would signal convergence of physician practice patterns towards a modal patient. If spillovers arise from physician-induced demand, we might expect the Medicaid payment policy to prompt an increase in low-risk c-sections among the commercial population, as this service is less time consuming and more profitable compared to EEDs. Further, if physician-induced demand is present, these effects are anticipated to be higher in areas where c-sections are more profitable relative to vaginal deliveries, and where the magnitude of profit loss is greatest (e.g. in areas with more Medicaid patients). Finally, if variation in spillovers is driven by a physician's hospital setting, we might expect a stronger reduction in EEDs in areas with a greater share of for-profit, rather than non-profit and public, hospitals, where objectives are financial, and physicians are more likely to respond to changes in service profitability.

4 Methods

4.1.1 Analytic Data Set

The primary data for this analysis is Medicare's Hospital Compare Database from 2013 and 2015 to 2017, a comprehensive, hospital-level database containing quality measures reported as part of a mandatory initiative, the Medicare Inpatient Quality Reporting (IQR) Program. We use these data to measure the main outcome, EED rate. Hospital Compare was formed through a public-private collaboration between Medicare and the Hospital Quality Alliance in 2002, with the goal to improve quality of U.S. hospitals by making information on hospital performance publicly accessible to consumers. Hospital Compare data are collected and updated from hospitals on a quarterly basis; all hospitals are required to submit rates for a set of quality measures, except critical access hospitals, which may voluntarily submit their data. Hospital Compare data confer several advantages. First, all measures are validated and endorsed by the National Quality Forum (NQF), the only consensus-based healthcare organization in the U.S., as defined by the Office of Management and Budget. NQF endorsement is the gold standard for quality metrics, as it uses a transparent, evidence-based, and consensus-based process driven by experts. Second, the mandatory nature of the Hospital Compare data allow longitudinal, hospital-level data to be easily accessible for research. Unlike voluntary reporting, this ensures that there are no systematic omissions in the data set, which improves representativeness and generalizability. Finally, Hospital Compare relies on multiple data elements, including administrative data and medical records, which can improve completeness and accuracy. The data also have some limitations. All measures are aggregated to a single all-payer rate, so insurer- and physician-specific rates cannot be identified. Further, since hospitals are responsible for self-reporting data, differences in hospital size, types of patients, and sampling strategies may reduce standardization, and thus, measure precision may be limited. Finally, Hospital Compare began publicly reporting perinatal care measures in 2013, which limits the length of the pre-policy period (Centers for Medicare and Medicaid Services, 2016; National Quality Forum, 2019).

To create the analytic data set, we merge the Hospital Compare database to several other data sources. All data sets are aggregated to the Metropolitan Statistical Area level and linked using a concatenated State-MSA identifier to obtain a consistent unit of analysis. State-MSA is the smallest indicator in common across all data sets. To aggregate the Hospital Compare data, we use the Department of Housing and Urban Development's U.S. Postal Service's Zip Code Crosswalk. Since Zip Codes often overlaps the boundaries of multiple MSAs, duplicate Zip Code records exist in the crosswalk (one per Zip Code-MSA pair). To ensure that the EED rate is proportionately counted within and across MSAs, and to account for population differences within a Zip Code, we multiply each EED numerator and denominator by the ratio of residential addresses as a weight. A residential ratio weight is available for each Zip Code-MSA pair, with the sum of weights for each Zip Code equaling 1.0 (Wilson & Din, 2018).

For the second outcome variable, low-risk c-sections, we use the Truven Marketscan Commercial Claims Database from 2010 to 2017, which links paid claims and encounter data with detailed patient information across sites and types of providers over time. Although the database is a convenience sample of enrollees in commercial health plans and large self-insured firms that opt to provide their data, the MarketScan data includes proprietary commercial claims (employer and health plan) from over 36 million patient hospital discharges (Johns Hopkins, 2016). Only commercial claims are included to enable identification of a true change in low-risk c-sections among the privately insured. These data are collected across broad geographic areas to represent treatment patterns and costs in the U.S. We use several maternal and clinical characteristics as covariates. One limitation is that a major insurer dropped out of the MarketScan data in 2015. To avoid differential selection into the database over time, we limit our sample to the employer population, which remains stable over the study period.

For the remaining covariates, we link to additional data sources, including the U.S. Census Bureau's American Community Survey (ACS), the Health Resources and Services Administration's Area Health Resource File (AHRF), the American Hospital Association (AHA) Annual Survey, and the National Practitioners Data Bank (NPDB). The ACS has publicly available data on demographic and employment characteristics for all counties. AHRF includes data on health professions and facilities, hospital utilization, and spending. Both are extracted at the county-level, and then aggregated and linked at the State-MSA level. The AHA data contains hospital-level information, so we collapse it to the State-MSA level using a county to MSA crosswalk, and then link it to the main data set. The AHA Survey is the most widely used database for hospital-level information with an average response rate of 83%. The AHA estimates certain measures for non-reporting hospitals, or those that submit incomplete survey information, using U.S. Census and other national-level data sources. Finally, we use the state-level NPDB, a web-based repository of reports containing information on state-level medical malpractice payments (AHA, 2018; HRSA, 2019; NPDB, 2019; U.S. Census Bureau, 2018).

The final analytic sample consists of 3,951 State-MSA-quarters between 2013 and 2017. This includes 553 State-MSA-quarters among treatment states; 436 State-MSA-quarters among states with no EED-related policies; 1,135 State-MSA-quarters among states with hard stop policies; 1,352 State-MSA-quarters among states with quality improvement coalitions; and 475 State-MSA-quarters among states with Medicaid pay-for-performance programs.

4.1.2 Empirical Strategy

The identification strategy is a DD framework comparing treatment states to states that implemented (1) no policy to curb EEDs, (2) a voluntary hard stop policy, (3) an EED quality improvement collaborative, and (4) a Medicaid pay-for-performance program, over one pre-implementation year (2013) and three post-implementation years (2015-2017). The EED Medicaid policy acts as a source of exogenous variation, such that physicians in GA, IN, MO, and MS are subject to discontinued reimbursement for Medicaid EEDs, while control states are not, leading to a quasi-experimental design. This approach is modeled on earlier work comparing direct effects of the Medicaid payment policy on EED rate, total c-section rate, and birth outcomes within a single state (Allen & Grossman, 2019; Byanova, 2015; Dahlen et al., 2017). Each state implemented the Medicaid payment policy between January 1, 2014 and January 1, 2015. Due to the variation in treatment timing, we begin our post period in 2015; this allows us to avoid biasing estimates by including later-treated states in the comparison group before treatment begins (Goodman-Bacon, 2018).

Our control groups represent types of initiatives aimed at curbing EEDs in other states. This strategy is twofold. First, it mitigates the threat of contaminating the true effect of the Medicaid payment policy of interest. It also enables comparing the Medicaid payment policy to other incentive structures, both financial and non-financial. The policies in each comparison group were implemented in all states prior to 2014. The main control group includes eight states with no policies in place to reduce EEDs (ID, ME, NE, NJ, RI, ND, SD, VA, and WY). The Medicaid pay-for-performance comparison group has three states (WA, CO, and WI). For non-financial incentives, the hard stop policy group includes eleven states (AR, UT, DE, IA, MA, MI, MN, NC, OR, TN, and OK), and the quality improvement initiative group has eleven states (AL, AZ, CA, CT, FL, IL, KS, WV, OH, NH, and VT).

The DD identification strategy relies on the untestable assumption that treatment states would have similar trends to the control group if the policy had not been implemented. If the trends are parallel in the pre-policy period, even if there is a difference in magnitude, we assume that differential changes across groups post-implementation are driven by the policy, rather than inherent differences between regions. For each control group, we compare pre-Medicaid payment policy trends in EEDs and low-risk c-sections to trends in the treatment states; all groups have statistically similar trends. We exclude Pennsylvania from the quality improvement comparison group due to dissimilar pre-policy patterns in the outcomes.

We estimate the impact of the Medicaid payment policy using the following equation:

$$Y_{mst} = \beta_0 + \beta_1 \cdot \text{Treat}_s + \beta_2 \cdot \text{Post}_t + \beta_3 \cdot \text{Treat}_s \cdot \text{Post}_t + T_t + \vartheta \cdot Z_m + \mu \cdot V_s + \varepsilon_{mst} \quad [6]$$

In [6], β_3 is the coefficient of interest, and it represents the aggregate effect of the Medicaid payment policy. Y_{mst} is the expected value of the outcome. It is indexed by State-MSA *m*; in state *s*; at time *t*, which is representative of pre/post policy implementation. Treat is a binary variable that denotes the presence of the Medicaid payment policy, and Post is a binary variable that indicates the policy post-period. V_s and T_t are state and year fixed-effects, respectively. Z_m is a vector of time-varying State-MSA-level controls, which account for maternal characteristics, healthcare factors, and demographic and economic variables that may influence physician practice patterns. Maternal characteristics, drawn from the MarketScan data, include the percent of commercially insured mothers that are over 35 years old, the percent with a hospital length of stay over four days (the number of days typically covered by the insurer), and cost sharing quartile bins (National Conference of State Legislatures, 2020). Healthcare factors, extracted from AHA, AHRF, NPDB, and the MarketScan databases, include hospital characteristics (percent of hospitals that are non-profit, percent of hospitals that provide obstetric services, beds per 1,000, and percent of patients that are insured by Medicaid), practitioner information (primary care practitioners per 1,000), and financial attributes (average price differential between c-sections and vaginal deliveries among privately insured bins (<\$0, \$0-\$5,000, and ≥\$5,000), and malpractice risk, defined as the average obstetric-related malpractice payout). Demographic characteristics from the ACS include percent of the population with less than a high school education, percent of the population with more than a college education, and percent of the population that is Black. Finally, economic characteristics from ACS and AHRF refer to percent uninsurance, percent unemployment, and percent poverty. We add dummy variables for the number of waves that a State-MSA appears in the data. To account for covariance in standard errors between time periods by geographic area, standard errors are clustered at the State-MSA level. All models are estimated using Ordinary Least Squares.

The primary outcome is the all-payer EED rate, defined as the percent of patients in the State-MSA-quarter with elective vaginal deliveries or elective cesarean births between ≥ 37 and < 39 weeks' gestation completed, excluding individuals with conditions justifying elective delivery prior to 39 weeks. Justifiable conditions include comorbidities in the prenatal period (e.g. hypertension, diabetes, eclampsia, breech, and fetal abnormalities), and pregnancy complications (e.g. prolonged labor, fetal distress, or premature rupture of membranes) (Glantz, 2005). We measure this rate using the Joint Commission's Perinatal Care-01 (PC-01) methodology. Hospital Compare mandated that all hospitals with annual births totaling 1,100 or more submit PC-01 for public reporting, beginning January 1, 2013 (Joint Commission, 2019). We rely on this measure instead of commercial claims because International Classification of Diseases, Ninth Revision (ICD-9) codes lack the granularity needed to properly measure gestational age for EEDs. ICD-9 provides a single code for 37 or more completed weeks gestation, making it impossible to identify early-term births that occur between 37 and 39 weeks. The second outcome in this study is low-risk c-sections, defined as the percent of nulliparous women with a term, singleton baby in a vertex position delivered by c-section. We follow the methodology developed by AHRQ, using Inpatient Quality Indicator (IQI) 33 (Agency for Healthcare Quality and Research, 2016). PC-01 and IQI 33 aggregate a rolling four-quarter measure rate, to minimize the extent to which low denominators create noise and minimize the ability to capture unbiased trends. Both measures are endorsed by the NQF as a consensus standard for hospital care.

Since PC-01 is all-payer, we cannot identify the direct result within Medicaid versus the spillover to privately insured patients, so we conduct additional analysis to gain insight into this question by examining whether effects increase in geographic areas with a high share of Medicaid patients, split at the median level. We expect that if direct effects in Medicaid spilled over to the commercial sector, then the reduction in EEDs will remain constant across different levels of the policy exposure, or the share of Medicaid patients in a given area.

We pursue a variety of robustness checks. First, we repeat our analyses using multiple group propensity score weights proposed by Stuart et al. (2014). Propensity scores aim to create a control group similar to the treatment, based on observed covariates. A main concern with the DD approach is that selection into the treatment group may be confounded by baseline characteristics, which can lead to biased estimates. This can occur if (1) treatment and comparison groups vary in ways that impact trends over time, or (2) within-group composition changes over time. By weighting units on a set of baseline factors, propensity scores aim to replicate the pre-intervention values of the outcome's determinants. It is advantageous for its potential to reduce extrapolation of the counterfactual, aggregate a potentially large number of confounders into a simple scalar, and implement without use of the outcome variable, detaching the study design from the analysis. This approach can promote feasible, more robust, and less biased estimates (Stuart et al., 2014).

In cases where treatment units enact a policy based on high or low values of the outcome, however, propensity scores can introduce regression to the mean bias by assigning higher weights to groups with extreme outcome values (Daw & Hatfield, 2018). Our analysis is susceptible to this issue because treatment states have relatively higher outcome values in the pre-period, indicating that unobservable characteristics associated with these high rates may contribute to treatment adoption. For this reason, we are faced with trading off good covariate balance (when using propensity score weighting) and minimizing regression to the mean bias (without propensity scores). We use an unweighted approach for the main analysis, but re-run models using multiple group propensity score weights to assess whether models are robust to changes in covariate balance. Second, we re-run analyses using alternate treatment groups by dropping one treatment state at a time. Third, we re-estimate our models using individual quarterly rates for EEDs and low-risk c-sections (as opposed to a rolling four-quarter measure period). Fourth, we re-run analyses excluding all State-MSAs that did not report outcomes for all waves of data, likely due to select hospitals churning above and below the 1,100 birth threshold for PC-01 reporting. Fifth, we test whether results are sensitive to inclusion of non-metropolitan areas. Finally, we repeat the analyses as an event study to determine if results are robust to standardizing timing of policy implementation across states.

5 Results

5.1.1 Descriptive Statistics and Validity of Study Design

Table 1 summarizes State-MSA characteristics in treatment and control states, before and after implementation of the Medicaid payment policy. Differences in maternal and healthcare characteristics are relatively small in the pre-policy period. Prior to the Medicaid payment policy, treatment states had fewer mothers over 35 years old, more births with LOS over 4 days, and greater average cost sharing, compared to the main control group. Treatment states also had more Medicaid patients, fewer non-profit hospitals, fewer hospitals that provide obstetric care, lower density of primary care physicians, and a smaller price differential between c-sections and vaginal deliveries among the commercial population. The average obstetric malpractice payout was also significantly smaller. Hospital beds per 1,000 were consistent between the groups. Demographic and economic differences were greater. On average, the population in treatment states were less educated, and more likely to be Black, uninsured, unemployed, or impoverished compared to the main control group. Differences in the other control groups are similar for most variables; however, we observe a few contrasts. The quality improvement control group was marginally less educated than the treatment group. The pay-for-performance control group had slightly fewer mothers over 35 years old and fewer births with LOS over four days. Quality improvement and pay-for-performance groups had a higher share of Medicaid patients; these groups, along with the hard stop policy group, also had fewer hospital beds per 1,000.

There is little evidence of differential changes in State-MSAs after the Medicaid payment policy is implemented. The gap in maternal age, as well as price differential between c-sections and vaginal deliveries, decreased, while differences in the Medicaid share increased. Average percent cost sharing rose across all intervention groups, while the uninsurance rate dropped. The only variable with substantial changes was average obstetric malpractice payouts, which varied across all groups; treatment and quality improvement groups increased, while the other control groups decreased. In general, gaps between groups declined. Otherwise, maternal, healthcare, demographic, and economic characteristic evolved similarly over time.

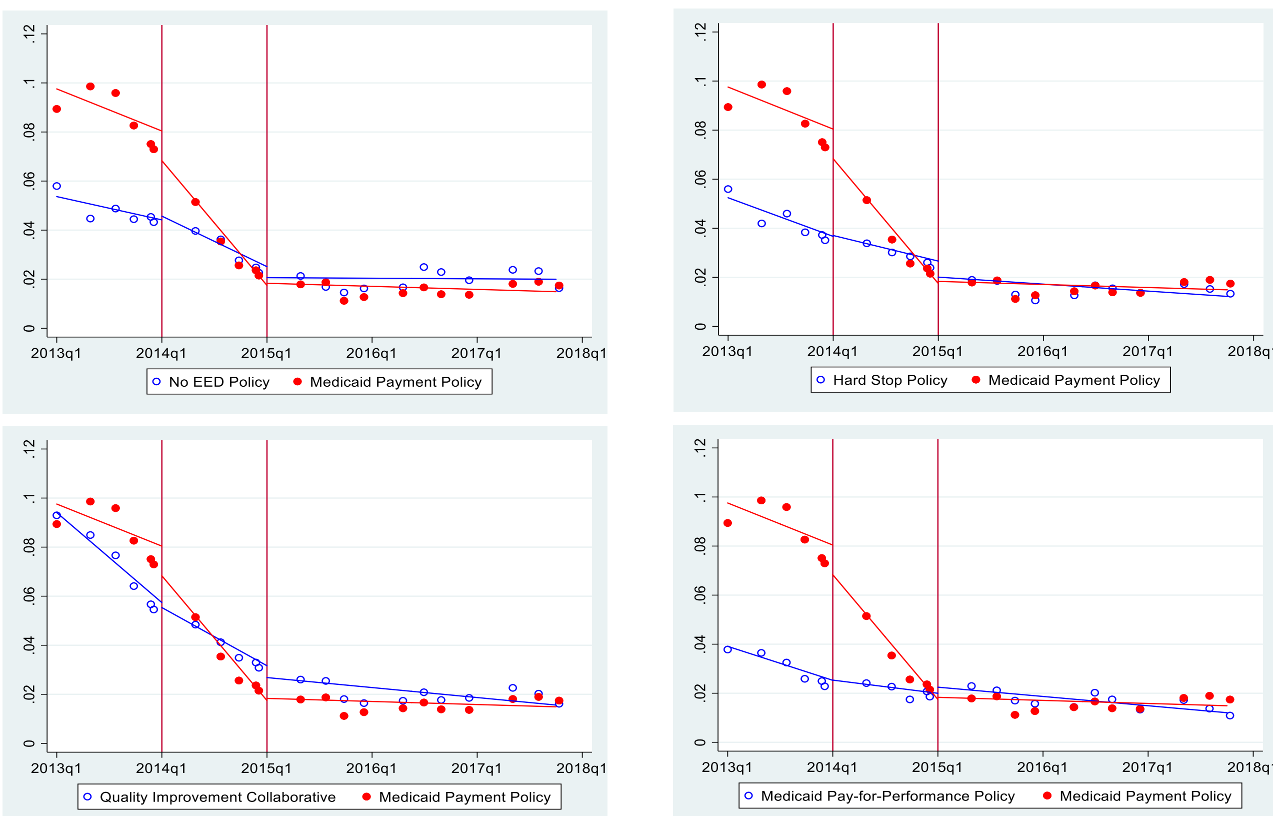
Table 1. Summary Statistics of Treatment and Control States, Before and After Medicaid Payment Policy

	Pre Medicaid Payment Policy (2013)						Post Medicaid Payment Policy (2015-2017)					
	Treatment	Main	P Val	Hard Stop	QI	P4P	Treatment	Main	P Val	Hard Stop	QI	P4P
Maternal Characteristics												
% Maternal Age 35+	10.86	13.35	0.09	13.05	14.18	10.73	11.80	13.54	0.04	12.78	13.75	12.91
% LOS > 4	3.64	3.27	0.55	4.26	3.99	3.42	4.37	3.57	0.05	4.52	4.96	3.35
Avg. % Cost Sharing	15.57	15.23	0.57	14.57	11.76	14.05	16.01	17.07	0.01	17.33	13.50	14.48
Healthcare Characteristics												
% Medicaid	15.38	14.79	0.47	14.30	17.26	15.41	15.80	14.21	0.00	16.68	19.96	17.05
% Hospitals Non-Profit	48.49	56.59	0.05	60.83	53.05	66.23	49.54	54.55	0.03	58.51	51.66	68.37
% Hospitals Provide Obstetric Services	39.98	43.85	0.18	52.82	48.54	62.16	39.66	39.39	0.88	50.66	72.65	58.42
Beds per 1,000	2.90	2.91	0.96	2.15	2.11	1.72	2.96	3.05	0.45	2.35	2.16	1.73
PCPs per 1,000	0.68	0.85	0.00	0.82	0.72	0.81	0.67	0.80	0.00	0.82	0.73	0.79
Avg. Price Differential												
< $0	1.48	0.93	0.70	3.24	3.88	1.71	3.83	3.34	0.73	2.68	4.52	2.23
$0 - $5,000	70.37	42.06	0.00	55.76	51.04	31.62	60.53	49.85	0.00	55.43	47.98	35.47
≥ $5,000	28.15	57.01	0.00	41.01	45.07	66.67	35.65	46.81	0.00	41.89	47.49	62.29
Avg. OB Malpractice Payout ($)	396,131	736,068	0.00	710,130	523,200	1,209,316	499,994	336,116	0.00	525,782	565,946	394,779
Demographic Characteristics												
% Less Than HS Education	13.74	10.02	0.00	10.58	13.82	9.80	12.64	8.96	0.00	9.81	13.00	8.88
% More Than College Education	25.00	30.81	0.00	29.67	26.06	28.99	27.08	32.82	0.00	30.94	27.72	30.95
% Population Black	22.04	8.56	0.00	12.70	11.86	3.51	22.13	8.59	0.00	12.67	11.98	3.62
Economic Characteristics												
% Uninsured	18.52	14.81	0.00	13.97	16.94	13.88	13.20	10.28	0.00	8.66	9.60	7.17
% Unemployed	7.77	6.08	0.00	7.08	8.61	7.17	5.01	4.21	0.00	4.55	6.09	4.58
% Poverty	18.23	13.61	0.00	16.07	16.76	13.26	16.09	12.25	0.00	14.24	15.00	11.66
State-MSA-Quarters	135	107		278	335	117	418	329		857	1,017	358
# of States	4	8		11	11	3	4	8		11	11	3

Notes: Sample estimates are from the Hospital Compare Database in 2013; 2015-2017. Maternal characteristics are from the Truven MarketScan commercial claims database, using data from births during the study period. Healthcare characteristics are from the AHA Annual Survey, AHRF, and the NPDB. Average price differential represents the mean difference in reimbursement between c-sections and vaginal deliveries among births in the Truven MarketScan data. Demographic and economic characteristics are from the U.S. Census ACS. Control groups are defined by policy type, including: (1) main control group with no EED policy (8 states: ID, ME, NE, NJ, RI, ND, SD, VA, WY); (2) hard stop policy group (11 states: AR, UT, DE, IA, MA, MI, MN, NC, OR, TN, OK)); (3) quality improvement collaborative (11 states: AL, AZ, CA, CT, FL, IL, KS, WV, OH, NH, and VT); and (4) Medicaid pay-for-performance group (3 states: WA, CO, WI). P-values are from unpaired t-tests of variable means between the treatment and main control groups.

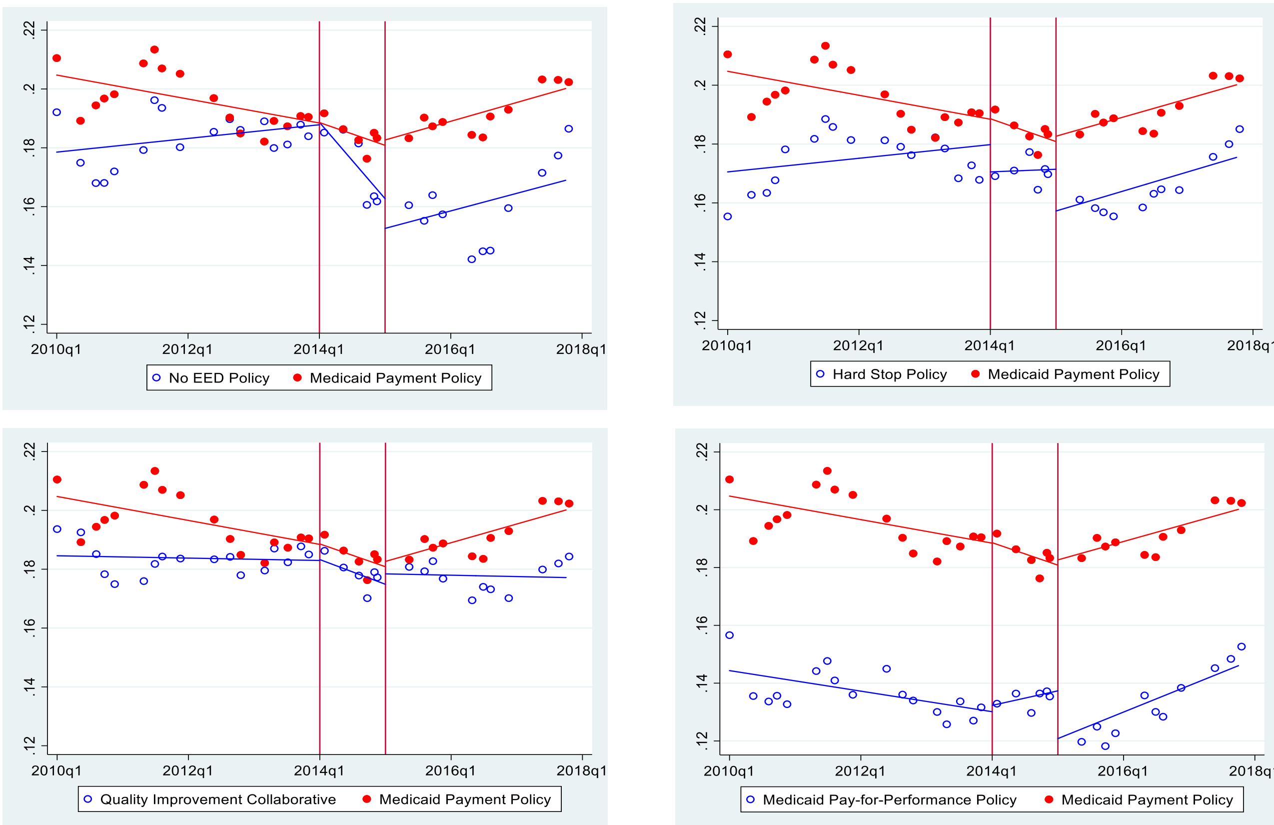
As discussed earlier, the DD approach relies on the assumption that trends in treatment states and control states would remain similar in the absence of the treatment policy. Although we cannot test this directly, we analyze pre-implementation trends to assess validity of the analysis. Figures 1 and 2 plot unadjusted quarterly means of EEDs and low-risk c-sections, respectively, in treatment and control states, where the red lines represent outcome trends in the treatment group and the blue lines represent outcome trends in each control group. Visual inspection suggests that EED variables followed similar pre-policy trends in treatment and control groups (Figure 1), while other outcomes are noisier (Figure 2). To verify that trends are statistically similar, we run formal tests to assess differential pre-trends (Table A1). We run the same specification as in the main model, but limit inclusion to the pre-policy period. The coefficient of interest is the interaction between treatment and a linear quarter-year time trend. We find no statistically significant differences in trends, and the magnitude of the differences are all less than 1%. These results suggest that the identification strategy is valid.

Figure 1. Trends in EEDs in Treatment and Control States



Notes: Sample estimates for EEDs are from Hospital Compare in 2013; 2015-2017. Sample estimates for Low-risk c-sections from Truven MarketScan claims, using data from births between 2010-2013 and 2015-2017. Data points are unadjusted, quarterly means. Control groups are defined by policy type, including: (1) main control group with no EED policy (8 states: ID, ME, NE, NJ, RI, ND, SD, VA, WY); (2) hard stop policy group (11 states: AR, UT, DE, IA, MA, MI, MN, NC, OR, TN, OK)); (3) quality improvement collaborative (11 states: AL, AZ, CA, CT, FL, IL, KS, WV, OH, NH, and VT); and (4) Medicaid pay-for-performance group (3 states: WA, CO, WI).

Figure 2. Trends in Low-Risk C-Sections in Treatment and Control States



Notes: Sample estimates for EEDs are from Hospital Compare in 2013; 2015-2017. Sample estimates for Low-risk c-sections from Truven MarketScan claims, using data from births between 2010-2013 and 2015-2017. Data points are unadjusted, quarterly means. Control groups are defined by policy type, including: (1) main control group with no EED policy (8 states: ID, ME, NE, NJ, RI, ND, SD, VA, WY); (2) hard stop policy group (11 states: AR, UT, DE, IA, MA, MI, MN, NC, OR, TN, OK)); (3) quality improvement collaborative (11 states: AL, AZ, CA, CT, FL, IL, KS, WV, OH, NH, and VT); and (4) Medicaid pay-for-performance group (3 states: WA, CO, WI).

5.1.2 Effect of Medicaid Payment Policy on EEDs

We first assess the effect of the Medicaid payment policy on EEDs, the primary target of the policy. Regression estimates of [6] are displayed in Table 2. Relative to the main control group, we find that EEDs decreased by 3.3% after the policy was implemented. Effects were relatively larger compared to the hard stop policy group and the pay-for-performance group, with EEDs declining by 3.9% and 3.6%, respectively. We estimate a smaller, insignificant decrease in EEDs compared to quality improvement programs. These results show that the Medicaid payment policy achieved its intended goal of reducing statewide all-payer EEDs, with a response that is stronger than a voluntary non-financial incentive and a financial bonus, but not an education-driven collaborative approach.

Table 2. Spillover Effects of Medicaid Payment Policy on Low-Value Care Outcomes

	Main	Hard Stop	QI	P4P
Early Elective Deliveries				
Treatment * Post	-0.033** (0.016)	-0.039** (0.016)	-0.014 (0.016)	-0.036** (0.015)
N	989	1,688	1,905	1,028
Dependent Variable Mean: % (SD)				
Pre: Treatment Mean	9.26 (9.50)	9.01 (9.29)	9.01 (9.29)	9.01 (9.29)
Pre: Control Mean	4.96 (4.83)	4.60 (7.91)	8.01 (8.45)	3.39 (3.20)
Post: Treatment Mean	1.67 (1.90)	1.67 (1.90)	1.67 (1.90)	1.67 (1.90)
Post: Control Mean	2.03 (2.77)	1.61 (1.94)	2.12 (2.12)	1.73 (1.59)
Low-Risk C-Sections				
Treatment * Post	0.012 (0.012)	0.001 (0.009)	0.000 (0.008)	0.005 (0.012)
N	1,773	3,021	3,421	1,827
Dependent Variable Mean (SD)				
Pre: Treatment Mean	19.80 (6.67)	19.80 (6.67)	19.80 (6.67)	19.80 (6.67)
Pre: Control Mean	18.17 (9.23)	17.59 (7.81)	18.38 (6.20)	13.76 (7.05)
Post: Treatment Mean	19.08 (6.36)	19.08 (6.36)	19.08 (6.36)	19.08 (6.36)
Post: Control Mean	16.19 (6.84)	16.61 (6.61)	17.92 (6.91)	13.36 (5.27)

Notes: Sample estimates for EEDs from the Hospital Compare Database in 2013 and 2015-2017. Sample estimates for low-risk c-sections are from Truven MarketScan claims, using data from births between 2010-2013 and 2015-2017. Table cells include DD coefficients with standard errors in parentheses. Standard errors are clustered at the State-MSA level. Control groups are defined by policy type, including: (1) main control group with no EED policy (8 states: ID, ME, NE, NJ, RI, ND, SD, VA, WY); (2) hard stop policy group (11 states: AR, UT, DE, IA, MA, MI, MN, NC, OR, TN, OK)); (3) quality improvement collaborative (11 states: AL, AZ, CA, CT, FL, IL, KS, WV, OH, NH, and VT); and (4) Medicaid pay-for-performance group (3 states: WA, CO, WI). Covariates include all variables in Table 1, plus year and state fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Next, we study whether the effect of the Medicaid payment policy varies by policy exposure, or the share of Medicaid patients. We find no evidence of differential changes in EEDs between areas with a higher versus lower proportion of Medicaid patients. When comparing the Medicaid payment policy to the main control group, there is a slightly greater, but insignificant, decline in EEDs of 1.2% among areas with fewer Medicaid patients. There are no significant differences between areas with higher and lower Medicaid shares relative to the other comparison groups. Since changes in the all-payer rate of EEDs do not vary by the share of Medicaid patients, our analysis suggests that both commercial and Medicaid patients were affected by the Medicaid payment change, and is suggestive of a spillover to the privately insured.

To understand what is driving the decrease in EEDs, we estimate whether effects of the Medicaid payment policy on EEDs vary across providers and areas with different financial and reputational characteristics. To do so, we modify [6] by adding an interaction term that multiplies treatment, post-policy period, and a high value of the financial or reputational characteristic of interest, and an interaction of treatment, post-period, and a low value of the financial or reputational characteristic of interest. We then take the difference of the two new coefficients using a linear combination.

We first examine whether there are heterogeneous effects by price differential between c-sections and vaginal deliveries in the commercial sector within a given geographic State-MSA. A lower price differential between c-sections and vaginal deliveries provides a relatively greater financial incentive to deliver via traditional vaginal delivery, which suggests that combined incentives to reduce EEDs and c-sections may yield a stronger response among providers. Consistent with this hypothesis, we find a greater, but insignificantly different, decline in EEDs among areas with a lower price difference between c-sections and vaginal deliveries in the main control, hard stop, and quality improvement comparison groups. This difference is marginally significant between treatment states and pay-for-performance states; areas with a lower price differential exhibit a 4.7% greater decline in EEDs compared to areas with a higher price differential. When limiting to areas with a smaller c-section to vaginal delivery price difference, the treatment group exhibits a 5.0% and 6.2% significant decline in EEDs relative to the hard stop and pay-for-performance policy groups, respectively (Table 4).

Finally, we assess whether effects of the Medicaid payment policy vary between areas with a higher versus lower proportion of for-profit hospitals (Table 5). We find the reduction in EEDs to be greater in areas with more for-profit hospitals among states with the Medicaid payment policy relative to comparison groups, but this difference is only significant compared to states with hard stop and pay-for-performance initiatives (6.5% and 7.7%, respectively). These results are consistent with our conceptual model, which suggests that physicians in for-profit hospitals may be more responsive to changes in service profitability compared to physicians in non-profit hospitals.

Table 3. Spillover Effects by Policy Exposure: High vs. Low Share of Medicaid Patients

	Main	**Hard Stop**	**QI**	**P4P**
Early Elective Deliveries				
Treatment * Post * High Medicaid	-0.026 (0.026)	-0.048* (0.025)	-0.035 (0.025)	-0.041* (0.024)
Treatment * Post * Low Medicaid	-0.037* (0.020)	-0.031 (0.020)	0.012 (0.023)	-0.033 (0.020)
Difference	0.012 (0.033)	-0.017 (0.031)	-0.047 (0.034)	-0.008 (0.033)
N	989	1,688	1,905	1,028
Dependent Variable Mean: % (SD)				
High Medicaid				
Pre: Treatment	10.29 (10.86)	10.29 (10.86)	10.29 (10.86)	10.29 (10.86)
Pre: Control	5.81 (4.33)	4.02 (5.14)	6.71 (7.04)	3.31 (3.18)
Post: Treatment	1.94 (1.95)	1.94 (1.95)	1.94 (1.95)	1.94 (1.95)
Post: Control	1.76 (1.67)	1.72 (1.64)	1.98 (1.80)	1.58 (1.25)
Low Medicaid				
Pre: Treatment	8.18 (7.76)	8.18 (7.76)	8.18 (7.76)	8.18 (7.76)
Pre: Control	4.46 (5.07)	5.21 (10.03)	10.29 (10.09)	3.45 (3.25)
Post: Treatment	1.39 (1.82)	1.39 (1.82)	1.39 (1.82)	1.39 (1.82)
Post: Control	2.18 (3.23)	1.51 (2.20)	2.37 (2.57)	1.84 (1.81)
Low-Risk C-Sections				
Treatment * Post * High Medicaid	0.003 (0.018)	-0.002 (0.012)	-0.002 (0.012)	0.001 (0.018)
Treatment * Post * Low Medicaid	0.009 (0.014)	0.004 (0.012)	0.003 (0.012)	0.004 (0.014)
Difference	-0.007 (0.022)	-0.006 (0.016)	-0.005 (0.016)	-0.003 (0.022)
N	1,773	3,021	3,421	1,827
Dependent Variable Mean % (SD)				
High Medicaid				
Pre: Treatment	21.66 (6.76)	21.66 (6.76)	21.66 (6.76)	21.66 (6.76)
Pre: Control	19.34 (8.60)	17.56 (6.38)	18.11 (5.86)	14.81 (6.90)
Post: Treatment	20.92 (5.95)	20.92 (5.95)	20.92 (5.95)	20.92 (5.95)
Post: Control	17.25 (6.85)	16.82 (5.78)	17.52 (6.00)	14.77 (5.01)
Low Medicaid				
Pre: Treatment	17.81 (5.98)	17.81 (5.98)	17.81 (5.98)	17.81 (5.98)
Pre: Control	17.46 (9.53)	17.63 (9.03)	18.89 (6.76)	12.95 (7.08)
Post: Treatment	17.24 (6.24)	17.24 (6.24)	17.24 (6.24)	17.24 (6.24)
Post: Control	15.59 (6.78)	16.40 (7.35)	18.64 (8.24)	12.29 (5.23)

Notes: Sample estimates for EEDs are from Hospital Compare in 2013 and 2015-2017. Sample estimates for low-risk c-sections from Truven MarketScan claims, using data from births between 2010-2013 and 2015-2017. Table cells include DDD coefficients with standard errors in parentheses. Standard errors are clustered at the State-MSA level. High Medicaid share is determined by a threshold of ≥ median in the pre-policy period (2010-2013) to reduce endogeneity. Control groups are defined by policy type, including: (1) main control group with no EED policy (8 states: ID, ME, NE, NJ, RI, ND, SD, VA, WY); (2) hard stop policy group (11 states: AR, UT, DE, IA, MA, MI, MN, NC, OR, TN, OK)); (3) quality improvement collaborative (11 states: AL, AZ, CA, CT, FL, IL, KS, WV, OH, NH, and VT); and (4) Medicaid pay-for-performance group (3 states: WA, CO, WI). Covariates include all variables in Table 1, plus year and state fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table 4. Variation in Spillover Effects by High vs. Low Commercial Price Differential Between C-Sections and Vaginal Deliveries

	Main	Hard Stop	QI	P4P
Early Elective Deliveries				
Treatment * Post * High Price Diff.	-0.017 (0.017)	-0.020 (0.018)	-0.005 (0.017)	-0.015 (0.016)
Treatment * Post * Low Price Diff.	-0.022 (0.032)	-0.050** (0.024)	-0.017 (0.025)	-0.062*** (0.022)
Difference	0.004 (0.037)	0.030 (0.030)	0.012 (0.030)	0.047* (0.027)
N	989	1,688	1,905	1,028
Dependent Variable Mean: % (SD)				
High Price Difference				
Pre: Treatment	6.71 (7.53)	6.71 (7.53)	6.71 (7.53)	6.71 (7.53)
Pre: Control	4.04 (3.74)	3.65 (5.41)	6.48 (7.11)	3.30 (3.35)
Post: Treatment	1.40 (1.99)	1.40 (1.99)	1.40 (1.99)	1.40 (1.99)
Post: Control	2.05 (2.60)	1.63 (1.67)	2.05 (1.84)	1.60 (1.36)
Low Price Difference				
Pre: Treatment	11.07 (10.35)	11.07 (10.35)	11.07 (10.35)	11.07 (10.35)
Pre: Control	8.15 (6.63)	5.56 (9.74)	9.92 (9.55)	3.99 (1.94)
Post: Treatment	1.86 (1.82)	1.86 (1.82)	1.86 (1.82)	1.86 (1.82)
Post: Control	1.96 (3.30)	1.59 (2.19)	2.21 (2.42)	2.55 (2.50)
Low-Risk C-Sections				
Treatment * Post * High Price Diff.	0.013 (0.015)	0.007 (0.011)	-0.005 (0.012)	0.007 (0.014)
Treatment * Post * Low Price Diff.	0.012 (0.023)	-0.005 (0.013)	0.002 (0.011)	-0.004 (0.027)
Difference	0.001 (0.027)	0.011 (0.017)	-0.007 (0.016)	0.012 (0.030)
N	1,773	3,021	3,421	1,827
Dependent Variable Mean % (SD)				
High Price Difference				
Pre: Treatment	18.58 (7.43)	18.58 (7.43)	18.58 (7.43)	18.58 (7.43)
Pre: Control	17.94 (8.43)	16.15 (7.94)	17.85 (6.18)	14.09 (7.14)
Post: Treatment	17.91 (7.27)	17.91 (7.27)	17.91 (7.27)	17.91 (7.27)
Post: Control	16.13 (6.54)	14.53 (5.65)	17.71 (7.04)	13.63 (5.18)
Low Price Difference				
Pre: Treatment	20.70 (5.91)	20.70 (5.91)	20.70 (5.91)	20.70 (5.91)
Pre: Control	18.92 (11.48)	19.08 (7.40)	19.00 (6.16)	11.56 (6.03)
Post: Treatment	19.94 (5.47)	19.94 (5.47)	19.94 (5.47)	19.94 (5.47)
Post: Control	16.42 (7.79)	18.72 (6.85)	18.19 (6.74)	11.62 (5.59)

Notes: Sample estimates for EEDs are from Hospital Compare in 2013 and 2015-2017. Sample estimates for low-risk c-sections from Truven MarketScan claims, using data from births between 2010-2013 and 2015-2017. Table cells include DDD coefficients with standard errors in parentheses. Standard errors are clustered at the State-MSA level. Price differential represents the mean difference in reimbursement between c-sections and vaginal deliveries among births in the Truven MarketScan data. A high price differential is determined by a threshold of ≥ median in the pre-policy period (2010-2013) to reduce endogeneity. Control groups are defined by policy type, including: (1) main control group with no EED policy (8 states: ID, ME, NE, NJ, RI, ND, SD, VA, WY); (2) hard stop policy group (11 states: AR, UT, DE, IA, MA, MI, MN, NC, OR, TN, OK)); (3) quality improvement collaborative (11 states: AL, AZ, CA, CT, FL, IL, KS, WV, OH, NH, and VT); and (4) Medicaid pay-for-performance group (3 states: WA, CO, WI). Covariates include all variables in Table 1, plus year and state fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table 5. Variation in Spillover Effects by Hospital Type: High vs. Low Share of For-Profit Hospitals

	Main	Hard Stop	QI	P4P
Early Elective Deliveries				
Treatment * Post * High % For-Profits	-0.053* (0.029)	-0.073*** (0.026)	-0.026 (0.028)	-0.080*** (0.026)
Treatment * Post * Low % For-Profits	-0.014 (0.014)	-0.008 (0.015)	-0.001 (0.013)	-0.004 (0.015)
Difference	-0.039 (0.032)	-0.065** (0.030)	-0.025 (0.031)	-0.077** (0.030)
N	989	1,688	1,905	1,028
Dependent Variable Mean: % (SD)				
High % For-Profit Hospitals				
Pre: Treatment	13.32 (11.36)	13.32 (11.36)	13.32 (11.36)	13.32 (11.36)
Pre: Control	6.70 (5.84)	5.05 (4.22)	10.52 (9.56)	1.97 (1.52)
Post: Treatment	1.74 (1.86)	1.74 (1.86)	1.74 (1.86)	1.74 (1.86)
Post: Control	1.85 (1.92)	1.65 (1.70)	2.25 (1.94)	1.14 (1.12)
Low % For-Profit Hospitals				
Pre: Treatment	5.49 (5.07)	5.49 (5.07)	5.49 (5.07)	5.49 (5.07)
Pre: Control	3.84 (3.68)	4.39 (9.13)	6.00 (6.83)	3.66 (3.37)
Post: Treatment	1.59 (1.94)	1.59 (1.94)	1.59 (1.94)	1.59 (1.94)
Post: Control	2.15 (3.22)	1.60 (2.05)	2.02 (2.25)	1.85 (1.65)
Low-Risk C-Sections				
Treatment * Post * High % For-Profits	-0.001 (0.015)	-0.026** (0.014)	-0.011 (0.012)	0.000 (0.014)
Treatment * Post * Low % For-Profits	0.023 (0.015)	0.022** (0.010)	0.010 (0.011)	0.018 (0.013)
Difference	-0.024 (0.022)	-0.048*** (0.017)	-0.021 (0.015)	-0.019 (0.017)
N	1,773	3,021	3,421	1,827
Dependent Variable Mean % (SD)				
High % For-Profit Hospitals				
Pre: Treatment	21.39 (6.85)	21.39 (6.85)	21.39 (6.85)	21.39 (6.85)
Pre: Control	16.34 (8.12)	17.67 (6.54)	19.74 (5.72)	13.58 (6.37)
Post: Treatment	18.97 (6.60)	18.97 (6.60)	18.97 (6.60)	18.97 (6.60)
Post: Control	14.74 (6.58)	17.75 (5.91)	18.90 (6.60)	12.50 (3.23)
Low % For-Profit Hospitals				
Pre: Treatment	18.19 (6.09)	18.19 (6.09)	18.19 (6.09)	18.19 (6.09)
Pre: Control	19.31 (9.70)	17.56 (8.33)	17.25 (6.35)	14.80 (6.90)
Post: Treatment	19.18 (6.15)	19.18 (6.15)	19.18 (6.15)	19.18 (6.15)
Post: Control	17.17 (6.85)	16.07 (6.86)	17.13 (7.05)	13.53 (5.58)

Notes: Sample estimates for EEDs are from Hospital Compare in 2013 and 2015-2017. Sample estimates for low-risk c-sections are from Truven MarketScan claims, using data from births between 2010-2013 and 2015-2017. Table cells include DDD coefficients with standard errors in parentheses. Standard errors are clustered at the State-MSA level. High for-profit hospital share is determined by a threshold of ≥ median in the pre-policy period (2010-2013) to reduce endogeneity. An area with a high percent of for-profit hospitals has a low percent of non-profit and public hospitals. Control groups are defined by policy type, including: (1) main control group with no EED policy (8 states: ID, ME, NE, NJ, RI, ND, SD, VA, WY); (2) hard stop policy group (11 states: AR, UT, DE, IA, MA, MI, MN, NC, OR, TN, OK)); (3) quality improvement collaborative (11 states: AL, AZ, CA, CT, FL, IL, KS, WV, OH, NH, and VT); and (4) Medicaid pay-for-performance group (3 states: WA, CO, WI). Covariates include all variables in Table 1, plus year and state fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

5.1.3 Effect of Medicaid Payment Policy on Low-Risk C-Sections

In Table 3, we study the effect of the Medicaid payment policy on supply of low-risk c-sections, to explore whether physician-induced demand is present in the commercial sector. If physicians are inducing demand to recoup profit lost from EEDs, we would expect to see an increase in low-risk c-sections in treatment states. We find little evidence of changes in low-risk c-sections. There is a small, statistically insignificant increase of 1.2% in low-risk c-sections among the treatment group compared to states with no EED policies. Because low-risk c-sections are declining across all groups, we can interpret this as a smaller decrease among treatment versus main control states. Changes in low-risk c-sections relative to hard stop, quality improvement, and pay-for-performance comparison groups are insignificant, and are at or below 0.1%.

We further test whether there is evidence of physician-induced demand by exploring whether the increase in low-risk c-sections is greater in areas with higher policy exposure (measured by the share of Medicaid patients), and in areas where c-sections are more profitable (measured by the price differential between c-sections and vaginal deliveries in the commercial sector). If physicians replace the volume of EEDs with low-risk c-sections among the privately insured, this suggests a positive income effect. We expect these effects may be greater in areas with higher policy exposure and/or where c-sections are relatively more profitable.

As with the EED analysis, we do so by comparing a three-way interaction between indicators for the treatment, post-period, and high policy exposure to a three-way interaction between indicators for the treatment, post-period, and low policy exposure, using a linear combination. We compare equivalent three-way interactions for a high versus low price differential between c-sections and vaginal deliveries. We find no evidence of physician-induced demand. Results in Table 3 indicate that low-risk c-sections decline to a greater extent in the treatment group in areas with a higher share of Medicaid patients, but these differences are small (below 1.0%), and insignificant across all comparison groups. This suggests that the policy's effect on the low-risk c-section rate does not rise with policy exposure, which would be expected under the demand inducement hypothesis.

We also test whether the change in low-risk c-sections within treatment states is greater in areas where c-sections are more profitable compared to vaginal deliveries. Table 4 shows that there are no differences in the policy's effect on low-risk c-sections in areas with higher versus lower price differentials. There are small, but insignificant, increases of low-risk c-sections in areas with high, rather than low, price differences between c-sections and vaginal deliveries, in treatment versus control states for all comparison groups except for the quality improvement group. These changes are minor, and range from 0.1% when comparing treatment states to the main control group, to 1.2% when comparing treatment states to the pay-for-performance group. Taken together, this demonstrates that the Medicaid payment policy does not prompt unintended spillovers across other low-value services, such as low-risk c-sections. Since low-risk c-sections do not increase by the degree of policy exposure or profitability of c-sections, there is no evidence that physicians practice gaming in response to profit-driven factors.

5.1.4 Robustness Checks

In this section, we assess robustness of our results by pursuing a variety of specification checks. First, we repeat our analyses using multiple group propensity score weights proposed by Stuart et al. (2014). In Table A3, we measure whether covariate balance improved after applying propensity score weights via the standardized mean difference (SMD) between treatment and control means. We construct multiple group propensity score weights separately in the pre- and post-implementation periods, using logistic regression models. We observe significantly improved balance with propensity scores. Although there is no hard threshold for SMDs, most studies recommend a maximum difference of 0.25, and ideally, 0.10 (Stuart, Lee, & Leacy, 2013). SMDs are below 0.25 for all covariate differences between the treatment and hard stop policy and quality improvement groups. Still, the majority of SMDs are below 0.25 for the main control and pay-for-performance comparison groups. We find that results are not sensitive to propensity score weighting. The estimated reduction in EEDs was similar in magnitude and direction to the main analysis across three of the four control groups. Treatment states experienced a 3.3% and 3.0% reduction in EEDs relative to main control and pay-for-performance states, respectively (compared to 3.3% and 3.6% in the main models); p-values increased slightly, but were still marginally significant, below 0.10. The decline in EEDs remained insignificant in treatment states compared to the quality improvement group, and the effect size declined in the hard stop group (Table A2).

Next, we re-run analyses using alternate treatment groups, each with one of the treatment, states omitted to assess whether a single state disproportionately influenced results. We find estimated reductions in EEDs to range from 2.3% to 4.5% in the main control group (compared to

3.3% in the main analysis), although models without GA or MS were no longer significant. All other control group comparisons had estimates comparable in direction and magnitude to the main analysis (Table A5). Third, we re-estimate our models using individual quarterly rates for EEDs and low-risk c-sections (as opposed to a rolling four-quarter measure period). We find that the models were not sensitive to the alternate measurement specification (Table A4). Fourth, we re-run our models with non-metropolitan State-MSAs, finding that results are robust to inclusion of these observations (Table A6). Fifth, we re-run analyses excluding all State-MSAs that did not report outcomes for all waves of data, likely due to select hospitals churning above and below the 1,100 birth threshold for PC-01 reporting (Table A7). The change in EEDs among treatment states was negative relative to all control groups. EEDs declined in control states by 2.8% compared to the main control group (versus 3.3% in the main model), but this result was no longer significant. Results were consistent for all other comparison groups, with EEDs dropping by 4.0% and 3.3% in treatment states compared to the hard stop and pay-for-performance groups, respectively (compared to 3.9% and 3.6%, respectively); differences remained significant. Consistent with the main analysis, changes in EEDs were null relative to the quality improvement group. In general, this suggests that missing values of the outcome, driven by variation in public reporting requirements over time, did not systematically impact results. Finally, we repeat the analyses as an event study to determine if results are robust to standardizing timing of policy implementation across states. Results were consistent in both direction and magnitude to the main analysis (Table A8; Figures A1 and A2).

In line with the main analysis, robustness checks showed no significant changes in low-risk c-sections across all specifications, and the magnitude and direction remained fairly constant.

6 Discussion

In this paper, we study the indirect effects of a Medicaid nonpayment incentive implemented between January 1, 2014 and January 1, 2015 for EEDs, compared to other financial and non-financial incentives. We compare how the Medicaid payment policy impacted use of low-value childbirth deliveries in the commercial market relative to states with other Medicaid policies, including: no policy aimed at reducing EEDs, a hard stop policy, a quality improvement collaborative, or a pay-for-performance reform.

First, we explore whether the Medicaid payment policy prompted a reduction of EEDs among privately insured patients. We find that the Medicaid payment policy in GA, IN, MO, and MS led to a 3.3% decline in all-payer EEDs compared to states with no EED policy. The Medicaid payment policy also reduced all-payer EEDs by 3.9% and 3.6% compared to states with a hard stop policy and pay-for-performance payment program, respectively. Effects did not vary between geographic areas with a higher versus lower share of Medicaid patients. This provides suggestive evidence that both Medicaid and commercial patients were impacted by the policy, and indicates a positive spillover to the privately insured population.

Next, we assess whether there was evidence of physician-induced demand by examining whether there was an increase in low-risk c-sections. We do not find statistically significant changes in the rate of privately insured low-risk c-sections. Effects do not increase in areas with high policy exposure or in areas where c-sections are more profitable, suggesting that physicians do not substitute the volume of EEDs with low-risk c-sections. Taken together, this shows no evidence of physician-induced demand, which would be expected when profit decline is highest (in areas with a greater proportion of Medicaid patients) and time costs are lowest (in areas with a higher net return of profit in the private sector). Sensitivity tests support our main conclusions.

Finally, we explore whether the variation in spillovers was consistent with financial or reputational drivers. We find changes to be mostly consistent with financial drivers. We test whether the decline in EEDs was greater in areas with a higher share of for-profit hospitals, which would be expected if physician respond more strongly to incentives that align with hospital objectives. We find the reduction in EEDs to be 6.5% and 7.7% greater in areas with more for-profit hospitals among treatment states relative to hard stop and pay-for-performance comparison groups, respectively. One explanation is that physicians in for-profit hospitals, driven primarily by financial objectives, respond more strongly to changes in financial incentives than those in non-profits. In prior studies, for-profit hospitals demonstrated a more significant reaction to changes in the financial environment by offering a greater volume of profitable services (Dranove et al., 2017; Horwitz, 2005). As Medicaid EEDs become less profitable, it is possible that for-profit hospitals respond by reducing the provision of EEDs across all patients. Another explanation is that non-profit hospitals prioritize reputational objectives, regardless of whether financial incentives are present (Newhouse, 1970). In our case, areas with a higher proportion of non-profit hospitals have lower EED rates in both pre- and post-policy periods, suggesting that while these hospitals may be less attentive to changes in the financial environment, they continually retain lower rates of low-value care to advance prestige. However, less intense changes by physicians in these hospitals may also point to barriers in changing practice patterns. We also test whether areas with a lower commercial price differential between c-sections and vaginal deliveries experienced a greater decline in EEDs. In general, we estimate a larger, but insignificant, decrease in EEDs in areas with a lower price difference, except in the comparison to pay-for-performance states, where EEDs declined by 4.7% more in areas where c-sections were less profitable, and this difference was marginally significant. This is notable because a lower price differential between c-sections and vaginal deliveries provides a relatively greater financial incentive to deliver via traditional vaginal delivery; thus, combined incentives to reduce both EEDs and c-sections may yield a stronger response among providers.

As several states and payers continue to debate nonpayment policies for low-value care, our analysis suggests that these incentives can be successful on a large scale. The magnitude of our results, however, indicates that Medicaid reimbursement may have a relatively modest spillover to commercially insured patients, relative to the direct effect within Medicaid. Our estimate of a 3.3% decline in EEDs is significantly smaller than those observed in Medicaid populations in Texas (14%) and South Carolina (10.9%) (Allen & Grossman, 2019; Dahlen et al., 2017). The effect size is comparable to evaluations of the non-Medicaid population in Texas, which also found significantly larger effects in Medicaid versus non-Medicaid EEDs. The difference in Texas was attributed to the Medicaid population facing both hard stop and reimbursement changes, while the non-Medicaid population faced only the hard stop policy, rather than a direct spillover (Byanova, 2015). In contrast, we do not find that the Medicaid payment policy affects the rate of low-risk c-sections in the commercial sector. Prior work found a 13.1% increase in non-Medicaid c-sections in Texas, with effects concentrated in hospitals that had a greater share of Medicaid births (Byanova, 2015). This difference may be due to tracking the rate of low-risk c-sections, as opposed to total c-sections. Notably, our study is the first multi-state analysis of this kind. Our results contribute to a growing empirical literature on financial incentives and physician behavior, showing that incentives can lead to positive spillovers across payers.

Our results have several implications for efforts to expand non-FFS payment reforms. First, our analysis indicates that discontinuing payment for a low-value service has potential to reduce unwanted physician behaviors, compared to the status quo. Our work also suggests that financial nonpayment has potential to yield greater improvements in quality than a range of other financial and non-financial incentives. The largest gains among treatment states were made relative to hard stop policies. Hard stop initiatives are voluntary and strategies vary between hospitals. In contrast, the Medicaid payment policy rollout is standardized within states by Medicaid programs, and participation is mandatory. Thus, mandatory programs may have greater potential to engage providers with the highest levels of low-value care. Literature focused on mandatory incentive programs is limited; in general, studies conclude that spending reductions are greater in voluntary programs relative to mandatory programs, but effects on quality are inconclusive (Dummit et al., 2016; Finkelstein, Ji, Mahoney, & Skinner, 2018; Liao, Sommers, & Navathe, 2018; Navathe et al., 2018, 2017). Our study offers preliminary evidence that low-value supply patterns may be more effectively reduced under mandatory, rather than voluntary, policies. Further, these results also offer initial evidence that imposing clear, coordinated goals across health systems may lead to larger declines in low-value treatments.

Second, given significant reductions in EEDs in the Medicaid nonpayment policy compared to pay-for-performance programs, results indicate that financial penalties may lead to greater spillovers than financial bonuses. These results are consistent with prospect theory in behavioral economics, which posits that providers value gains and losses of the same magnitude asymmetrically, and will respond more strongly to penalties in order to avoid financial losses (Kahneman & Tversky, 1979). Empirical evidence generally supports this notion, but this is the first study to explore spillovers across payers under this framework (Rizzo et al., 2002).

Finally, Medicaid nonpayment for EEDs is comparable in reducing statewide EEDs to quality improvement collaboratives. Since quality improvement programs target behavior changes across several payers, and the payment policy of interest is limited to Medicaid, this suggests that spillovers to commercial patients may be stronger among the nonpayment reform. However, it may also highlight how a multi-pronged approach, with patient and provider education, stakeholder engagement, and advancement in performance measurement for quality maternal care, can prompt similar results to Medicaid payment reform.

Our analysis has several limitations and suggests potential avenues for future work. First, we cannot identify physicians or payers in the Hospital Compare or MarketScan databases. This limits our ability to identify spillovers to the commercially insured population, and to attribute effects to changes in physician, as opposed to broader health system, behavior. It will be useful for future work to address this gap and examine these effects at the provider level across different insurance types. Exploring this area would also enable a better understanding of physician characteristics that drive variation in policy effects, and the potential mechanism behind behavioral changes. Second, we are only able to identify all-payer, rather than commercial insurance, EED rates. This limits the ability to make inferences about spillovers to the privately insured. We argue that since policy effects do not vary by the share of Medicaid patients, this suggests a possible effect on non-Medicaid births. To be certain, additional research focusing on commercial-specific EED rates, is needed. Next, we focus on spillovers of a Medicaid nonpayment policy in childbirth, so results may not be generalizable to other clinical areas. We argue that perinatal care has characteristics that reduce potential for effects to stem from confounders, which can increase the extent to which results apply to other settings. Additional work is needed to strengthen this claim. Finally, the DD design rests on the assumption that no unobserved factors contribute to the observed effect. We address this concern with several robustness checks. However, there are still issues of covariate balance and minimal availability of data in the pre-policy period that weaken interpretation of results.

Non-FFS payment reforms are becoming increasingly salient, but there are remaining questions about how to design incentives that promote high-value care and generate cost savings. We present evidence that Medicaid incentives can be effective in reducing low-value care in the commercial sector, without prompting unintended consequences such as physician-induced demand. While our results are specific to perinatal care, our study provides general guidance about financial and non-financial design. Continuing to build an understanding of these incentives and their indirect effects, especially surrounding variation in success, is imperative for future work.

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8 Appendix

Table A1. Tests for Equality of Pre-Medicaid Payment Policy Trends in Low-Value Care Outcomes

	Main	**Hard Stop**	**QI**	**P4P**
Early Elective Deliveries (2013)				
Treatment * Quarter	0.0035 (0.0046)	0.0041 (0.0046)	0.0065 (0.0054)	0.0017 (0.0044)
N	242	413	470	252
Dependent Variable Mean: % (SD)				
Treatment Mean	9.26 (9.50)	9.01 (9.29)	9.01 (9.29)	9.01 (9.29)
Control Mean	4.96 (4.83)	4.60 (7.91)	8.01 (8.45)	3.39 (3.20)
Low-Risk C-Sections (2010-2013)				
Treatment * Quarter	-0.0006 (0.0021)	-0.0016 (0.0011)	-0.0009 (0.0010)	0.0007 (0.0016)
N	1,026	1,746	1,986	1,051
Dependent Variable Mean (SD)				
Treatment Mean	19.80 (6.67)	19.80 (6.67)	19.80 (6.67)	19.80 (6.67)
Control Mean	18.17 (9.23)	17.59 (7.81)	18.38 (6.20)	13.76 (7.05)

Notes: Sample estimates for EEDs from the Hospital Compare Database in 2013. Sample estimates for low-risk c-sections are from Truven MarketScan claims, using data from births between 2010-2013. Table cells include regression coefficients with standard errors in parentheses. Standard errors are clustered at the State-MSA level. Control groups are defined by policy type, including: (1) main control group with no EED policy (8 states: ID, ME, NE, NJ, RI, ND, SD, VA, WY); (2) hard stop policy group (11 states: AR, UT, DE, IA, MA, MI, MN, NC, OR, TN, OK)); (3) quality improvement collaborative (11 states: AL, AZ, CA, CT, FL, IL, KS, WV, OH, NH, and VT); and (4) Medicaid pay-for-performance group (3 states: WA, CO, WI). Covariates include all variables in Table 1, plus year and state fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table A2. Spillover Effects of Medicaid Payment Policy: Propensity Score Weighted Approach

	Main	Hard Stop	QI	P4P
Early Elective Deliveries				
Treatment * Post	-0.033* (0.019)	-0.018 (0.019)	0.006 (0.018)	-0.030* (0.015)
N	989	1,688	1,905	1,028
Low-Risk C-Sections				
Treatment * Post	0.000 (0.014)	-0.003 (0.012)	-0.006 (0.012)	0.011 (0.018)
N	1,773	3,021	3,421	1,827

Notes: Sample estimates for EEDs from the Hospital Compare Database in 2013 and 2015-2017. Sample estimates for low-risk c-sections are from Truven MarketScan claims, using data from births between 2010-2013 and 2015-2017. Table cells include DD coefficients with standard errors in parentheses. Standard errors are clustered at the State-MSA level. Control groups are defined by policy type, including: (1) main control group with no EED policy (8 states: ID, ME, NE, NJ, RI, ND, SD, VA, WY); (2) hard stop policy group (11 states: AR, UT, DE, IA, MA, MI, MN, NC, OR, TN, OK)); (3) quality improvement collaborative (11 states: AL, AZ, CA, CT, FL, IL, KS, WV, OH, NH, and VT); and (4) Medicaid pay-for-performance group (3 states: WA, CO, WI). Covariates include all variables in Table 1, plus year and state fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table A3. Covariate Balance Before and After Multiple Group Propensity Score Weights

	Initial Balance			**Propensity Score Balance**		
	Treated	Control	SMD	Treated	Control	SMD
Main Control						
Pre-Policy						
% Less Than HS Education	0.62	0.32	0.64	0.62	0.71	-0.18
% More Than College Education	0.40	0.72	-0.67	0.40	0.31	0.18
% Population Black	0.70	0.35	0.74	0.70	0.76	-0.12
% Uninsured	0.65	0.28	0.78	0.65	0.70	-0.10
% Unemployed	0.57	0.13	1.02	0.57	0.65	-0.18
% Poverty	0.65	0.25	0.88	0.65	0.71	-0.13
% Hospitals Non-Profit	0.41	0.55	-0.29	0.41	0.35	0.11
% Hospitals Provide OB Services	0.49	0.44	0.09	0.49	0.77	-0.58
Beds per 1,000	0.62	0.73	-0.22	0.62	0.79	-0.36
PCPs per 1,000	0.38	0.63	-0.51	0.38	0.33	0.10
% Maternal Age 35+	0.45	0.48	-0.05	0.45	0.32	-0.27
Avg. % Cost Sharing	0.30	0.44	-0.29	0.30	0.63	-0.67
Post-Policy						
% Less Than HS Education	0.65	0.27	0.82	0.65	0.89	-0.51
% More Than College Education	0.45	0.75	-0.65	0.45	0.14	0.66
% Population Black	0.71	0.34	0.81	0.71	0.85	-0.30
% Uninsured	0.82	0.61	0.47	0.82	0.94	-0.27
% Unemployed	0.50	0.15	0.81	0.50	0.82	-0.73
% Poverty	0.63	0.23	0.88	0.63	0.89	-0.58
% Hospitals Non-Profit	0.38	0.53	-0.30	0.38	0.12	0.53
% Hospitals Provide OB Services	0.44	0.35	0.19	0.44	0.87	-0.88
Beds per 1,000	0.65	0.73	-0.18	0.65	0.87	-0.48
PCPs per 1,000	0.40	0.71	-0.66	0.40	0.15	0.53
% Maternal Age 35+	0.47	0.49	-0.04	0.47	0.47	0.02
Avg. % Cost Sharing	0.50	0.43	0.13	0.50	0.55	-0.10
Hard Stop						
Pre-Policy						
% Less Than HS Education	0.62	0.52	0.21	0.62	0.62	0.00
% More Than College Education	0.40	0.53	-0.26	0.40	0.45	-0.10
% Population Black	0.70	0.47	0.48	0.70	0.68	0.04
% Uninsured	0.65	0.56	0.18	0.65	0.63	0.03
% Unemployed	0.57	0.66	0.19	0.57	0.54	0.06
% Poverty	0.65	0.64	0.01	0.65	0.60	0.10
% Hospitals Non-Profit	0.41	0.51	-0.22	0.41	0.29	0.23

% Hospitals Provide OB Services	0.49	0.65	-0.35	0.49	0.50	-0.03
Beds per 1,000	0.62	0.48	0.28	0.62	0.61	0.03
PCPs per 1,000	0.38	0.56	-0.37	0.38	0.33	0.10
% Maternal Age 35+	0.45	0.57	-0.23	0.45	0.44	0.02
Avg. % Cost Sharing	0.30	0.60	-0.62	0.30	0.29	0.03
Post-Policy						
% Less Than HS Education	0.65	0.53	0.25	0.65	0.65	0.01
% More Than College Education	0.45	0.52	-0.16	0.45	0.47	-0.04
% Population Black	0.71	0.47	0.50	0.71	0.71	0.01
% Uninsured	0.82	0.41	0.94	0.82	0.83	-0.01
% Unemployed	0.50	0.52	-0.04	0.50	0.44	0.12
% Poverty	0.63	0.58	0.11	0.63	0.59	0.08
% Hospitals Non-Profit	0.38	0.48	-0.20	0.38	0.31	0.14
% Hospitals Provide OB Services	0.44	0.60	0.33	0.44	0.41	0.05
Beds per 1,000	0.65	0.50	0.31	0.65	0.67	-0.05
PCPs per 1,000	0.40	0.57	-0.34	0.40	0.41	-0.03
% Maternal Age 35+	0.47	0.55	-0.15	0.47	0.46	0.02
Avg. % Cost Sharing	0.50	0.48	0.04	0.50	0.42	0.15
QI						
Pre-Policy						
% Less Than HS Education	0.62	0.39	0.47	0.62	0.61	0.03
% More Than College Education	0.40	0.56	-0.33	0.40	0.38	0.04
% Population Black	0.70	0.70	0.00	0.70	0.72	-0.05
% Uninsured	0.65	0.30	0.74	0.65	0.58	0.14
% Unemployed	0.57	0.43	0.28	0.57	0.47	0.20
% Poverty	0.65	0.41	0.49	0.65	0.54	0.22
% Hospitals Non-Profit	0.41	0.53	-0.24	0.41	0.38	0.05
% Hospitals Provide OB Services	0.49	0.47	0.03	0.49	0.44	0.09
Beds per 1,000	0.62	0.57	0.11	0.62	0.58	0.08
PCPs per 1,000	0.38	0.54	-0.34	0.38	0.33	0.10
% Maternal Age 35+	0.45	0.49	-0.08	0.45	0.48	-0.06
Avg. % Cost Sharing	0.30	0.59	-0.60	0.30	0.39	-0.18
Post-Policy						
% Less Than HS Education	0.65	0.41	0.51	0.65	0.62	0.07
% More Than College Education	0.45	0.56	-0.23	0.45	0.44	0.01
% Population Black	0.71	0.71	0.01	0.71	0.75	-0.09
% Uninsured	0.82	0.42	0.90	0.82	0.83	-0.01
% Unemployed	0.50	0.53	-0.05	0.50	0.47	0.07
% Poverty	0.63	0.41	0.46	0.63	0.59	0.08

% Hospitals Non-Profit	0.38	0.52	-0.28	0.38	0.30	0.17
% Hospitals Provide OB Services	0.44	0.49	-0.09	0.44	0.46	-0.05
Beds per 1,000	0.65	0.58	0.14	0.65	0.62	0.06
PCPs per 1,000	0.40	0.56	-0.31	0.40	0.37	0.06
% Maternal Age 35+	0.47	0.51	-0.07	0.47	0.47	0.01
Avg. % Cost Sharing	0.50	0.60	-0.21	0.50	0.51	-0.02
P4P						
Pre-Policy						
% Less Than HS Education	0.62	0.17	1.05	0.62	0.83	-0.49
% More Than College Education	0.40	0.63	-0.48	0.40	0.16	0.49
% Population Black	0.70	0.07	1.72	0.70	0.36	0.93
% Uninsured	0.65	0.28	0.78	0.65	0.58	0.15
% Unemployed	0.57	0.37	0.41	0.57	0.81	-0.50
% Poverty	0.65	0.16	1.15	0.65	0.53	0.29
% Hospitals Non-Profit	0.41	0.70	-0.62	0.41	0.41	-0.02
% Hospitals Provide OB Services	0.49	0.73	-0.52	0.49	0.60	-0.23
Beds per 1,000	0.62	0.33	0.60	0.62	0.60	-0.04
PCPs per 1,000	0.38	0.53	-0.31	0.38	0.16	0.44
% Maternal Age 35+	0.45	0.44	0.02	0.45	0.43	0.04
Avg. % Cost Sharing	0.30	0.55	-0.51	0.30	0.23	0.16
Post-Policy						
% Less Than HS Education	0.65	0.21	0.99	0.65	0.34	0.71
% More Than College Education	0.45	0.56	-0.23	0.45	0.30	0.29
% Population Black	0.71	0.07	1.75	0.71	0.28	1.19
% Uninsured	0.82	0.12	1.96	0.82	0.60	0.62
% Unemployed	0.50	0.39	0.23	0.50	0.20	0.61
% Poverty	0.63	0.16	1.11	0.63	0.57	0.13
% Hospitals Non-Profit	0.38	0.69	-0.64	0.38	0.57	-0.39
% Hospitals Provide OB Services	0.44	0.75	-0.68	0.44	0.28	0.34
Beds per 1,000	0.65	0.30	0.74	0.65	0.56	0.19
PCPs per 1,000	0.40	0.50	-0.30	0.40	0.46	-0.13
% Maternal Age 35+	0.47	0.50	-0.06	0.47	0.50	-0.05
Avg. % Cost Sharing	0.50	0.67	-0.36	0.50	0.49	0.01

Notes: Propensity scores constructed using logistic regression. Covariate balance refers to a binary indicator for each variable indicating the proportion ≥ median. Select covariates excluded due potential influence by the Medicaid payment policy in the post-period, including Medicaid share, commercial price difference, and OB malpractice payout. Control groups are defined by policy type, including: (1) main control group with no EED policy (8 states: ID, ME, NE, NJ, RI, ND, SD, VA, WY); (2) hard stop policy group (11 states: AR, UT, DE, IA, MA, MI, MN, NC, OR, TN, OK)); (3) quality improvement collaborative (11 states: AL, AZ, CA, CT, FL, IL, KS, WV, OH, NH, and VT); and (4) Medicaid pay-for-performance group (3 states: WA, CO, WI). The estimate of interest is the Standardized Mean Difference (SMD), which provides an independent comparison between treated and control means.

Table A4. Spillover Effects of Medicaid Payment Policy: Individual Quarterly Measures of Low-Value Care Outcomes

	Main	Hard Stop	QI	P4P
Early Elective Deliveries				
Treatment * Post	-0.030* (0.015)	-0.036*** (0.013)	-0.013 (0.013)	-0.041*** (0.013)
N	989	1,688	1,905	1,028
Low-Risk C-Sections				
Treatment * Post	0.013 (0.014)	0.004 (0.009)	0.005 (0.008)	0.010 (0.011)
N	1,773	3,021	3,421	1,827

Notes: Sample estimates for EEDs from the Hospital Compare Database in 2013 and 2015-2017. Sample estimates for low-risk c-sections are from Truven MarketScan claims, using data from births between 2010-2013 and 2015-2017. Table cells include DD coefficients with standard errors in parentheses. Standard errors are clustered at the State-MSA level. Control groups are defined by policy type, including: (1) main control group with no EED policy (8 states: ID, ME, NE, NJ, RI, ND, SD, VA, WY); (2) hard stop policy group (11 states: AR, UT, DE, IA, MA, MI, MN, NC, OR, TN, OK)); (3) quality improvement collaborative (11 states: AL, AZ, CA, CT, FL, IL, KS, WV, OH, NH, and VT); and (4) Medicaid pay-for-performance group (3 states: WA, CO, WI). Covariates include all variables in Table 1, plus year and state fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table A5. Spillover Effects of Medicaid Payment Policy: Alternate Treatment Groups

	Main	**Hard Stop**	**QI**	**P4P**
Early Elective Deliveries				
Treatment * Post				
No MO	-0.44** (0.019)	-0.048*** (0.018)	-0.023 (0.019)	-0.041** (0.017)
No MS	-0.023 (0.016)	-0.025* (0.015)	0.002 (0.015)	-0.033** (0.015)
No IN	-0.045** (0.019)	-0.057*** (0.019)	-0.031 (0.020)	-0.064*** (0.022)
No GA	-0.028 (0.019)	-0.033 (0.020)	-0.010 (0.020)	-0.033* (0.016)
Low-Risk C-Sections				
Treatment * Post				
No MO	0.011 (0.012)	0.016 (0.009)	0.002 (0.009)	0.007 (0.012)
No MS	0.015 (0.012)	0.004 (0.009)	0.001 (0.009)	0.008 (0.012)
No IN	0.012 (0.014)	0.000 (0.010)	-0.002 (0.010)	0.001 (0.014)
No GA	-0.001 (0.011)	-0.006 (0.009)	-0.003 (0.009)	-0.003 (0.011)

Notes: Sample estimates for EEDs from the Hospital Compare Database in 2013 and 2015-2017. Sample estimates for low-risk c-sections are from Truven MarketScan claims, using data from births between 2010-2013 and 2015-2017. Table cells include DD coefficients with standard errors in parentheses. Standard errors are clustered at the State-MSA level. Control groups are defined by policy type, including: (1) main control group with no EED policy (8 states: ID, ME, NE, NJ, RI, ND, SD, VA, WY); (2) hard stop policy group (11 states: AR, UT, DE, IA, MA, MI, MN, NC, OR, TN, OK)); (3) quality improvement collaborative (11 states: AL, AZ, CA, CT, FL, IL, KS, WV, OH, NH, and VT); and (4) Medicaid pay-for-performance group (3 states: WA, CO, WI). Covariates include all variables in Table 1, plus year and state fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table A6. Spillover Effects of Medicaid Payment Policy: Inclusion of Non-Metro State-MSAs

	Main	**Hard Stop**	**QI**	**P4P**
Early Elective Deliveries				
Treatment * Post	-0.035** (0.016)	-0.037*** (0.015)	-0.017 (0.015)	-0.039*** (0.014)
N	1,117	1,872	2,115	1,124
Low-Risk C-Sections				
Treatment * Post	0.011 (0.010)	0.002 (0.008)	0.001 (0.008)	0.006 (0.011)
N	1,985	3,341	3,795	1,995

Notes: Sample estimates for EEDs from the Hospital Compare Database in 2013 and 2015-2017. Sample estimates for low-risk c-sections are from Truven MarketScan claims, using data from births between 2010-2013 and 2015-2017. Table cells include DD coefficients with standard errors in parentheses. Standard errors are clustered at the State-MSA level. Control groups are defined by policy type, including: (1) main control group with no EED policy (8 states: ID, ME, NE, NJ, RI, ND, SD, VA, WY); (2) hard stop policy group (11 states: AR, UT, DE, IA, MA, MI, MN, NC, OR, TN, OK)); (3) quality improvement collaborative (11 states: AL, AZ, CA, CT, FL, IL, KS, WV, OH, NH, and VT); and (4) Medicaid pay-for-performance group (3 states: WA, CO, WI). Covariates include all variables in Table 1, plus year and state fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table A7. Spillover Effects of Medicaid Payment Policy: Exclusion of MSAs with Attrition

	Main	**Hard Stop**	**QI**	**P4P**
Early Elective Deliveries				
Treatment * Post	-0.028 (0.018)	-0.040*** (0.014)	-0.006 (0.015)	-0.033** (0.015)
N	800	1,328	1,552	848
Low-Risk C-Sections				
Treatment * Post	0.017 (0.012)	0.009 (0.009)	0.010 (0.008)	0.004 (0.011)
N	1,400	2,324	2,716	1,484

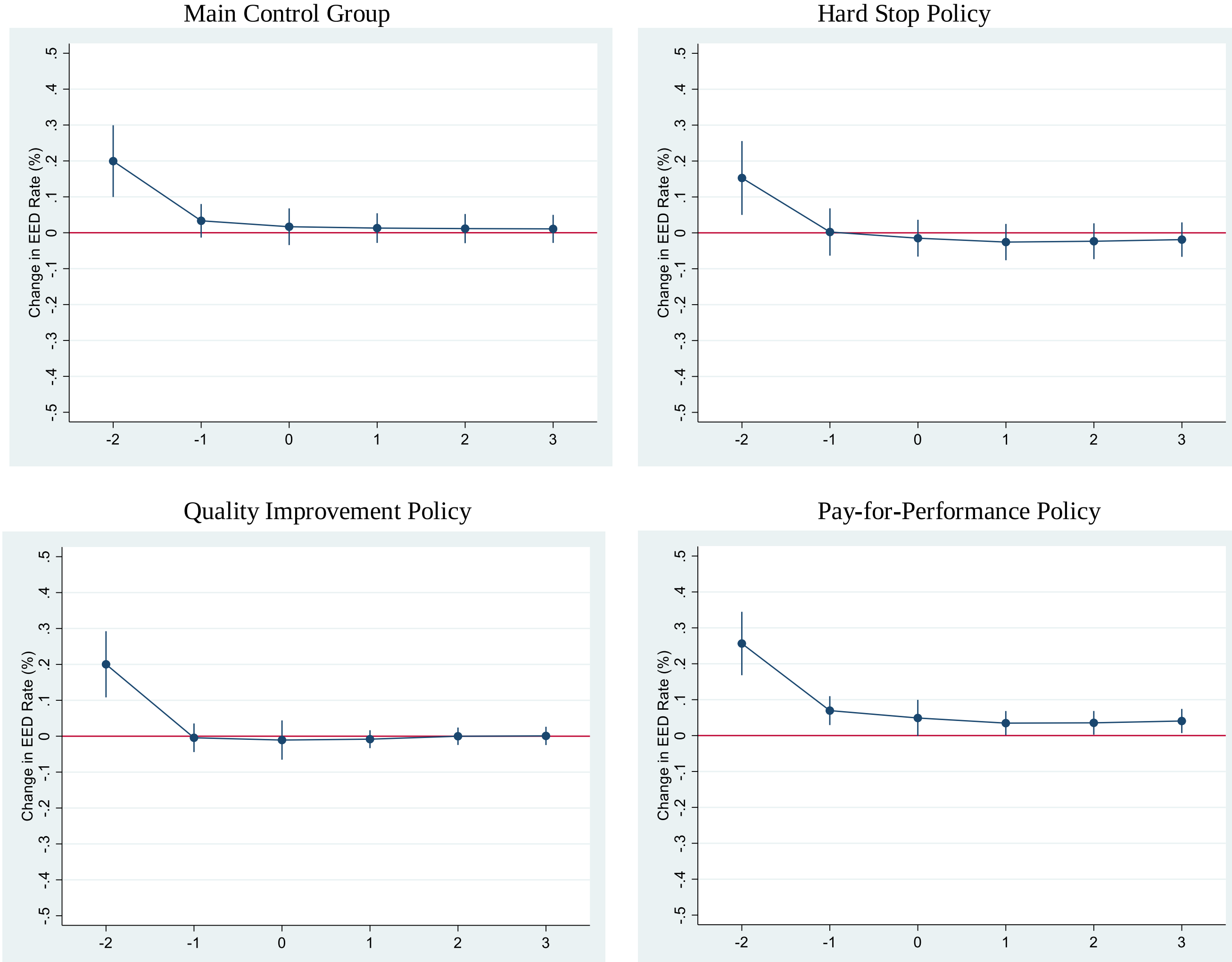
Notes: Sample estimates for EEDs from the Hospital Compare Database in 2013 and 2015-2017. Sample estimates for low-risk c-sections are from Truven MarketScan claims, using data from births between 2010-2013 and 2015-2017. Table cells include DD coefficients with standard errors in parentheses. Standard errors are clustered at the State-MSA level. Control groups are defined by policy type, including: (1) main control group with no EED policy (8 states: ID, ME, NE, NJ, RI, ND, SD, VA, WY); (2) hard stop policy group (11 states: AR, UT, DE, IA, MA, MI, MN, NC, OR, TN, OK)); (3) quality improvement collaborative (11 states: AL, AZ, CA, CT, FL, IL, KS, WV, OH, NH, and VT); and (4) Medicaid pay-for-performance group (3 states: WA, CO, WI). Covariates include all variables in Table 1, plus year and state fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table A8. Spillover Effects of Medicaid Payment Policy: Event Study

	Main	**Hard Stop**	**QI**	**P4P**
Early Elective Deliveries				
Treatment * Post	-0.22 (0.017)	-0.025* (0.015)	0.002 (0.015)	-0.032** (0.014)
N	989	1,688	1,905	1,028
Low-Risk C-Sections				
Treatment * Post	0.015 (0.011)	0.002 (0.009)	0.001 (0.008)	0.003 (0.011)
N	1,773	3,021	3,421	1,827

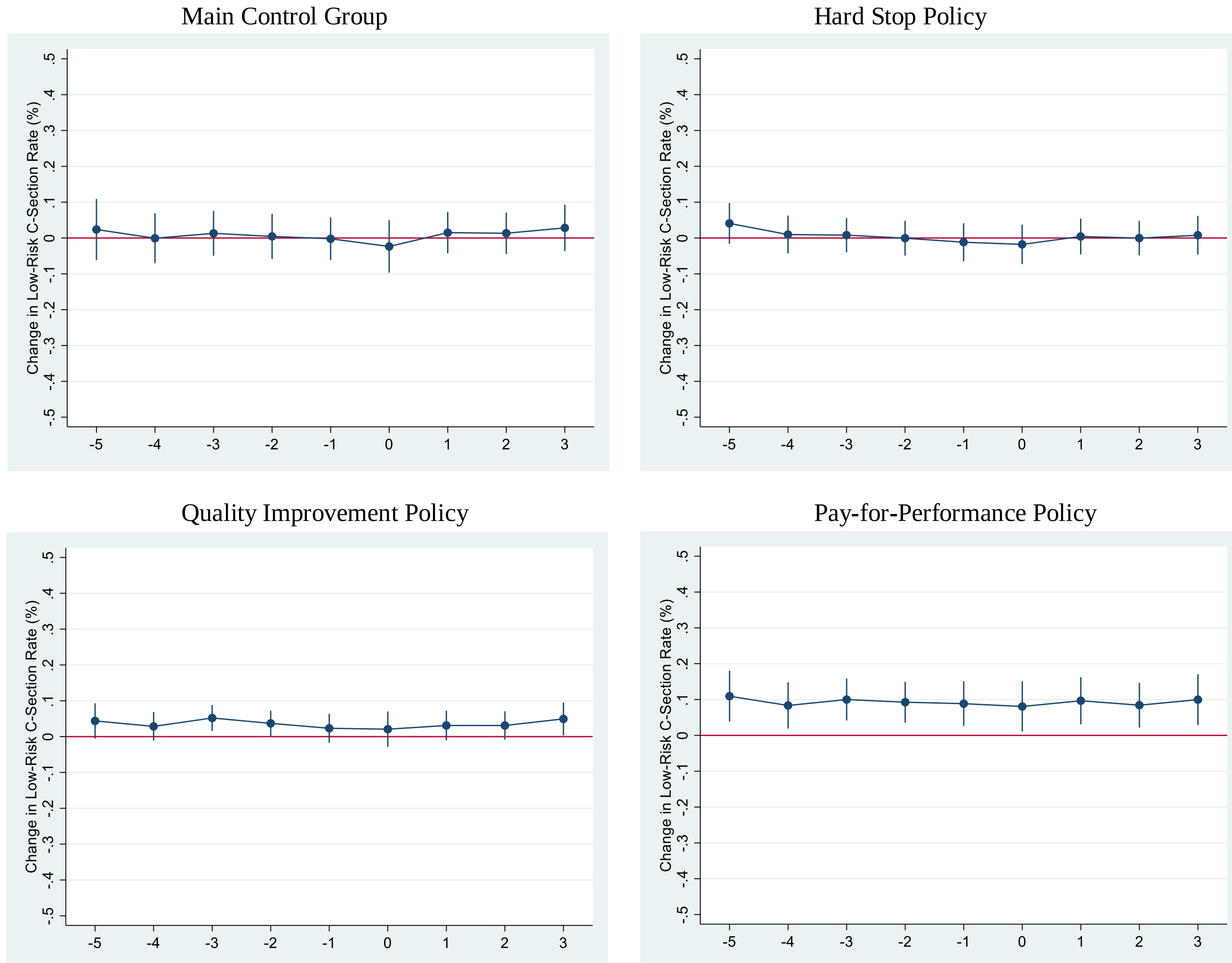
Notes: Sample estimates for EEDs from the Hospital Compare Database in 2013 and 2015-2017. Sample estimates for low-risk c-sections are from Truven MarketScan claims, using data from births between 2010-2013 and 2015-2017. Table cells include DD coefficients with standard errors in parentheses. Standard errors are clustered at the State-MSA level. Control groups are defined by policy type, including: (1) main control group with no EED policy (8 states: ID, ME, NE, NJ, RI, ND, SD, VA, WY); (2) hard stop policy group (11 states: AR, UT, DE, IA, MA, MI, MN, NC, OR, TN, OK)); (3) quality improvement collaborative (11 states: AL, AZ, CA, CT, FL, IL, KS, WV, OH, NH, and VT); and (4) Medicaid pay-for-performance group (3 states: WA, CO, WI). Covariates include all variables in Table 1, plus year and state fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Figure A1. Event Study on EEDs



Notes: Sample estimates for EEDs are from Hospital Compare in 2013; 2015-2017. Sample estimates for low-risk c-sections are from Truven MarketScan claims, using data from births between 2010-2013 and 2015-2017. Data points are adjusted coefficients for treatment*year with 95% Confidence Intervals. Estimates overlapping 0 are not significant at the p=0.05 level. Control groups are defined by policy type, including: (1) main control group with no EED policy (8 states: ID, ME, NE, NJ, RI, ND, SD, VA, WY); (2) hard stop policy group (11 states: AR, UT, DE, IA, MA, MI, MN, NC, OR, TN, OK)); (3) quality improvement collaborative (11 states: AL, AZ, CA, CT, FL, IL, KS, WV, OH, NH, and VT); and (4) Medicaid pay-for-performance group (3 states: WA, CO, WI). Covariates include all variables in Table 1, plus year and state fixed effects.

Figure A2. Event Study on Low-Risk C-Sections



Notes: Sample estimates for EEDs are from Hospital Compare in 2013; 2015-2017. Sample estimates for low-risk c-sections are from Truven MarketScan claims, using data from births between 2010-2013 and 2015-2017. Data points are adjusted coefficients for treatment*year with 95% Confidence Intervals. Estimates overlapping 0 are not significant at the p=0.05 level. Control groups are defined by policy type, including: (1) main control group with no EED policy (8 states: ID, ME, NE, NJ, RI, ND, SD, VA, WY); (2) hard stop policy group (11 states: AR, UT, DE, IA, MA, MI, MN, NC, OR, TN, OK)); (3) quality improvement collaborative (11 states: AL, AZ, CA, CT, FL, IL, KS, WV, OH, NH, and VT); and (4) Medicaid pay-for-performance group (3 states: WA, CO, WI). Covariates include all variables in Table 1, plus year and state fixed effects.

Table A9. EED Policy Timeline (2007-2017)

Intervention Group	State	Policy	Implementation Date
Treatment	Indiana	Medicaid does not cover any claim submitted for EEDs if not properly documented as medically necessary.	July 2014
	Mississippi	Medicaid denies any claim submitted for EEDs if not properly documented as medically necessary.	January 2015
	Missouri	Medicaid denies any claim submitted for EEDs if not properly documented as medically necessary.	October 2014
		Midwest Health Initiative: Developed and disseminated "Policy Toolkit to Support Reduction of EEDs"	2012
	Georgia	Medicaid denies any claim submitted for EEDs if not properly documented as medically necessary.	January 2014
Main Control	Idaho	N/A	N/A
	Maine	N/A	N/A
	Nebraska	N/A	N/A
	New Jersey	N/A	N/A
	Rhode Island	N/A	N/A
	North Dakota	N/A	N/A
	South Dakota	N/A	N/A
	Virginia	N/A	N/A
	Wyoming	N/A	N/A
Hard Stop Policy	Arkansas	ARbestHealth: Program mandating all Arkansas hospitals to pledge to prevent EEDs through a hard stop policy. Hospitals voluntarily submit EED rates to the ARbestHealth Hospital Quality Team	February 2012
	Utah	Maternal and Infant Health Program: Hospitals institute policies against EEDs	2009
	Delaware	Delaware Healthy Mother and Infant Consortium: Mandate for hospitals to adopt guidelines to reliminate 100% of EEDs by December 2013	2011
	Iowa	Iowa Hospital Engagement Network: Urged participating hospitals to pledge to reduce EEDs.	2013

	Massachusetts	Massachusetts Perinatal Quality Collaborative: Urged hospitals to adopt voluntary hard stop policies.	May 2011
	Michigan	Michigan Department of Community Health: implemented hard stop policy and required all Medicaid-enrolled birthing hospitals to utilize EED evidence-based guidelines	Jan 2013
		Michigan Health and Hospital Association Keystone Obstetric Collaborative: Voluntary initiative for hospitals the prohibited elective c-sections and inductions before 39 weeks gestation.	2009
	Minnesota	Evidence-Based Childbirth Program: Law required hospitals to implement policies to minimize EEDs	January 2012
		Blended reimbursement rate for c-sections and vaginal deliveries	October 2009
	North Carolina	Pregnancy Medical Home Program: Overarching goal to improve birth outcomes and reduce costs. To qualify for participation, hospitals must adopt hard stop policy to eliminate EEDs.	March 2011
	Oregon	Oregon Perinatal Collaborative and March of Dimes 39 Weeks campaign: Urged hospitals to enact hard stop policy to eliminate EEDs	February 2012
	Tennessee	Tennessee Healthy Babies are Worth the Wait: Requested that all hospital CEOs in the state sign a pledge to adopt hard stop policies and submit data on hospital EED rate	2013
	Oklahoma	Every Week Counts Collaborative: Recruited hospitals for voluntary hard stop program to eliminate EEDs.	April 2011
Quality Improvement Collaborative	Alabama	Alabama Perinatal Excellence Collaborative: Created and disseminated guidelines for scheduling deliveries before 39 weeks gestation to hospitals in the state	January 2012
	Arizona	Arizona Perinatal Trust: Integrates voluntary certification of guideline adherence, perinatal education, and perinatal data analysis to improve maternal and neonatal outcomes and quality	January 2010
	California	The California Maternal Quality Care Collaborative: Developed and disseminated toolkit for preventing statewide EEDs	2010
		Patient Safety First: Voluntary collaborative to reduce EED rate below 5% by 2012	January 2010
		California Hospital Engagement Network: Initiative to reduce EED rate to <3% in the state	March 2012

	Connecticut	Participant of March of Dimes Perinatal Quality Improvement Initiative: Awareness campaign for obstetric providers on risks of EEDs. Tasked with integrating CMQCC Toolkit into hospitals.	2011
	Florida	Florida Perinatal Quality Collaborative: Educate providers on EED risks in collaboration with March of Dimes	June 2010
	Illinois	Illinois Perinatal Quality Collaborative: Quality improvement obstetric initiative focused on reducing EEDs	2012
		Midwest Health Initiative: Developed and disseminated "Policy Toolkit to Support Reduction of EEDs"	2012
		Midwest Business Group on Health: Collaborative between National Business Coalition on Health, Quality Quest for Health, the State of Illinois, and March of Dimes to prevent EEDs and improve maternal quality and outcomes	2011
	Kansas	Kansas Perinatal Quality Collaborative: Quality improvement initiative aimed at eliminating EEDs	September 2012
		Kansas Healthcare Collaborative and Hospital Engagement Network: Set goal to reduce EED rate to <3% by end of 2013	July 2012
	West Virginia	West Virginia Perinatal Partnership: Initiated quality improvement program to reduce EEDs. Participation consisted of 14 hospitals, representing 70% of births in the state).	2009
	Ohio	Ohio Perinatal Quality Collaborative: Initiated the 39 Weeks Delivery Charter Project, which made efforts to reduce unnecessary EEDs.	2008
	New Hampshire	Northern New England Perinatal Quality Improvement Network: A voluntary consortium of healthcare organizations committed to improving care for women and children. Offers education programs, best practice guidelines, benchmarking quality rates, and team-based approach to reducing poor outcomes.	2007
	Vermont	Northern New England Perinatal Quality Improvement Network: A voluntary consortium of healthcare organizations committed to improving care for women and children. Offers education programs, best practice guidelines, benchmarking quality rates, and team-based approach to reducing poor outcomes.	2007
Medicaid Pay-for-Performance Bonus Payment	Washington	Safety Net Assessment Act: gave hospitals a 1% increase in their Medicaid reimbursement for reducing annual EEDs	April 2010

		Blended reimbursement rate for uncomplicated c-sections and vaginal deliveries	April 2009
		Washington State Perinatal Collaborative: Encouraged hospitals to sign pledge to reduce EEDs	November 2010
	Wisconsin	Obstetric Medical Home: Pays $1,000 bonus for each Medicaid patient that attends at least ten prenatal visits and a postpartum visit within 60 days of birth. Additional $1,000 bonus per positive birth outcome.	Piloted 2011 to 2013 in some counties. Enacted across entire state January 2014.
		Blended reimbursement rate for c-sections and vaginal deliveries.	January 2010
	Colorado	Partnership for Patients: A quality improvement program, led by the Colorado Hospital Association, aimed at reducing statewide EED rates.	April 11
		Hospital Quality Incentive Payment Program: Offers volume-adjusted payments based on Medicaid discharges and quality achievement on EED performance.	2011
Excluded	Louisiana	Commercial and Medicaid insurers do not reimburse any claim submitted for EEDs if not properly documented as medically necessary. **Excluded due to multi-insurer effort; would not measure direct spillover.**	September 2014
	Maryland	Maryland Perinatal System Standards: Maryland Department of Health and Mental Hygiene develop and disseminate voluntary standards and hospitals participate in hard stop policy to eliminate EEDs **Excluded due to Hospital Global Budget; potential contamination of policy effect.**	July 2012
	Montana	Medicaid denies any claim submitted for EEDs if not properly documented as medically necessary. **No Montana hospitals reported EED rates.**	October 2014
	New Mexico	Medicaid denies any claim submitted for EEDs if not properly documented as medically necessary. **Excluded due to Medicaid for maternity care based on one global budget; potential contamination of policy effect.**	January 2014
		Medicaid offers blended reimbursement rate for c-sections and vaginal deliveries	April 2011

	Nevada	Medicaid denies any claim submitted for EEDs if not properly documented as medically necessary. **Excluded due to lack of availability of pre-policy data (no pre-policy data available prior to 2013).**	June 2012
	New York	Medicaid reduced payments for EEDs by 10% unless documented as medically indicated **Excluded due to lack of availability of pre-policy data (no pre-policy data available prior to 2013).**	July 2013
		Medicaid Redesign Team Reforms: quality improvement collaborative aimed at lowering statewide Medicaid spending. One initiative directed towards EEDs.	January 2011
	South Carolina	Medicaid and BlueCross BlueShield deny payment for non-medically necessary EEDs.	January 2013
		South Carolina Birth Outcomes Initiative: Encouraged hospitals to adopt hard stop policy pledge to reduce EEDs. **Excluded due to lack of availability of pre-policy data (no pre-policy data available prior to 2013).**	March 2011
	Texas	Texas House Bill 1983 required all hospitals to implement practices to reduce EEDs	September 2011
		Medicaid denies any claim submitted for EEDs if not properly documented as medically necessary. **Excluded due to lack of availability of pre-policy data (no pre-policy data available prior to 2013).**	June 2011
	Kentucky	Medicaid EEDs require prior authorization. **Excluded due to potential for policy contamination; policy implementation occurs after 2015.**	September 2017
	Pennsylvania	Pennsylvania Hospital Engagement Networks Obstetric Adverse Events Collaborative: Used peer comparisons and quality reporting of EEDs to discourage their provision. **Excluded due to pre-policy trends not being parallel in quality improvement comparison group; suggests unobservable differences in PA could drive policy adoption and effects.**	May 2013