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Does Managed Care Widen Infant Health Disparities? Evidence from Texas Medicaid†

By Ilyana Kuziemko, Katherine Meckel, and Maya Rossin-Slater*

Medicaid programs increasingly finance competing, capitated managed care plans rather than administering fee-for-service (FFS) programs. We study how the transition from FFS to managed care affects high- and low-cost infants (blacks and Hispanics, respectively). We find that black-Hispanic disparities widen—e.g., black mortality and preterm birth rates increase by 15 percent and 7 percent, respectively, while Hispanic mortality and preterm birth rates decrease by 22 percent and 7 percent, respectively. Our results are consistent with a risk-selection model whereby capitation incentivizes competing plans to offer better (worse) care to low- (high-) cost clients to retain (avoid) them in the future. (*JEL* H75, I12, I18, I38, J13, J15)

Increasingly in US public insurance programs, the state finances and regulates competing, capitated private insurance plans but does not itself directly insure beneficiaries through a public fee-for-service (FFS) plan. Whereas Medicare debuted in 1965 as a traditional, publicly administered FFS program, today over one-fourth of participants opt to enroll in private Medicare managed care plans (and recently proposed "premium-support" reforms would significantly increase this share). The Affordable Care Act (ACA) insurance exchanges—the backbone of the 2010 reform—offer private, capitated competing insurance plans with substantial government subsidies and regulation, but no public FFS option.

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Nowhere has this trend been more pronounced than in Medicaid, the country's chief health insurance program for low-income families. In the first three decades of the program, each state administered its own Medicaid FFS program, with the movement to privatization only picking up in the early 1990s. Whereas in 1991 private Medicaid Managed Care (MMC) plans served only 11 percent of Medicaid recipients, today they serve as the primary insurer for over 74 percent of Medicaid enrollees.[1] If current trends hold, by 2020 MMC plans will serve as the primary insurer for roughly 37 million individuals (11 percent of the US population).[2]

A key goal for any insurance program is to guarantee that high-risk and high-cost patients do not face rationing by providers and insurers, who might prefer to treat low-risk, low-cost beneficiaries (Newhouse 2006). In this paper, we evaluate the switch from Medicaid FFS to MMC with respect to this criterium. Our empirical work requires comparing the outcomes of ex ante high- and low-cost Medicaid-eligible individuals before and after such a switch, relative to the difference in outcomes between high- and low-cost individuals who are ineligible. As certain aspects of cost are endogenous to the quality of care (e.g., while low birth weight is a marker of a high-cost newborn, it is potentially endogenous to the quality of prenatal care a mother receives), we require *immutable* beneficiary characteristics that are highly correlated with cost.

We make use of a natural experiment stemming from Texas' county-by-county transition from Medicaid FFS to MMC, and focus on changes in the infant health outcomes of children born to US-born black and Hispanic mothers. In Texas, the large majority of Medicaid births are to US-born black and Hispanic women, but their children have very different health outcomes. For example, children of US-born black mothers have 70 percent greater mortality and low birth weight rates than children of US-born Hispanic mothers.[3] These disparities translate into enormous differences in costs and profits—in Texas hospital discharge data, black infants have charges that are over 80 percent greater than those of Hispanics, yet MMC plans receive the same capitation payments for the two groups. We use infants of foreign-born black and Hispanic mothers, most of whom were ineligible for Medicaid during our sample period, as approximate placebo groups.

Having identified our ex ante high- and low-cost groups, we use detailed birth records data to explore how their outcomes change after the switch from FFS to MMC. Mortality rates for children born to US-born black mothers significantly *increase* (by 15 percent), while those for Hispanics significantly *decrease* (by 22 percent), causing the black-Hispanic child mortality gap to grow by 69 percent. The black-Hispanic low birth weight and preterm birth rate gaps also increase significantly.

[1] See Duggan and Hayford (2011) and http://kff.org/medicaid/state-indicator/total-medicaid-mc-enrollment/.

[2] See, https://www.cbo.gov/sites/default/files/cbofiles/attachments/43900-2015-01-ACAtables.pdf, Table B-2. CBO estimates that by 2019, Medicaid will serve as the primary insurer for 50 million Americans (this estimate reflects the 2012 Supreme Court decision limiting the ACA Medicaid expansion). As MMC currently accounts for 74 percent of all Medicaid enrollees, we estimate that $0.74 \times 50 = 37$ million will be on MMC, likely an underestimate as the MMC share of Medicaid enrollees has been steadily growing and will likely exceed 74 percent by 2020.

[3] This black-Hispanic gap in health has also been widely documented in other settings. We review this evidence in Section IIC.

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With respect to quality of care, we find some suggestive evidence that after MMC, black mothers, relative to Hispanics, are less likely to begin prenatal care in their first month of pregnancy, more likely to have fewer than eight prenatal visits in total, and less likely to gain the minimum recommended amount of weight during pregnancy. While these results are less precise than our main outcome results, they suggest that black women experience a decline in access to care and providers during their pregnancies, while Hispanics do not. We provide anecdotal evidence from plan websites that MMC plans that service primarily Hispanic clients offer more generous care than plans serving mostly black clients. Many aspects of care remain unobserved to us, however, preventing the identification of the exact mechanisms driving the outcome results.

To rationalize our empirical findings, the final part of the paper presents a simple dynamic framework (formalized in online Appendix A) that can generate predictions consistent with our results. Much of the risk-selection literature focuses on single period models, and shows that plans have incentives to devise menus of services and prices that differentially attract healthy patients to *join*. We instead consider a multi-period setting, where we argue that risk selection can be even easier for plans. Once clients have already joined, plans can easily determine who is healthier and thus lower cost and more profitable. To retain such patients, plans provide them with more attention and better care. By the same logic, they ration attention and care to higher cost patients. A state-run FFS program, on the other hand, does not create incentives for risk-selection as providers are reimbursed per procedure. Our framework can thus explain why moving health insurance from FFS to MMC can exacerbate preexisting health disparities across groups.

The model is our preferred explanation for the pattern of results we find, and we show that other models of risk selection are less consistent with our data. But we emphasize upfront that we have almost no ability to observe plan actions and the direct evidence we can provide is at best suggestive. We hope that the framework we present might be a useful starting point for future work, which (with better data) might find evidence in favor or against this type of selection.

Our paper is most directly related to the literature on the privatization of public insurance programs. Two key papers have studied the FFS-to-MMC transition in California, using the county-by-county-rollout, as we do in Texas. Duggan (2004) finds MMC increased costs in California, which he attributes to competing MMC plans' limited ability to negotiate favorable rates with providers relative to a consolidated FFS system.[4] Aizer, Currie, and Moretti (2007) find that prenatal care and birth outcomes deteriorate under MMC in California.[5] Neither paper finds evidence of risk selection in California. As we discuss later, this discrepancy between Texas and California is consistent with differences in MMC program details between

[4] Duggan and Hayford (2011) find supporting evidence that MMC increased costs relative to Medicaid FFS nationally using state panel data.

[5] There is an earlier literature on the effect of MMC on prenatal care and birth outcomes that relies primarily on cross-sectional variation and pre-/post-analyses without comparison groups (see Kaestner, Dubay, and Kenney 2002 for an overview). Some of these studies find a positive association between MMC and prenatal care usage and birth weight (Levinson and Ullman 1998), others find declines in prenatal care and no effects on birth outcomes (Moreno 1999; Conover, Rankin, and Sloan 2001), while still others find no significant effects on prenatal care or birth outcomes (Kenney, Sommers, and Dubay 2005; Kaestner, Dubay, and Kenney 2002).

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the two states. Thus, comparing our results with past work can help illustrate the trade-offs involved in MMC program design.[6]

Our paper also relates to the large literature on infant health disparities. A mounting body of evidence has traced the origins of adult well-being to fetal and early childhood health (see Currie and Almond 2011 for an overview), highlighting how early-life health disparities may perpetuate economic inequality in adulthood (Currie 2011). Additionally, several papers have documented how public safety net programs (including Medicaid) can reduce these disparities through improving the health of the most disadvantaged children (Hoynes, Schanzenbach, and Almond 2012; Miller and Wherry forthcoming; Brown, Kowalski, and Lurie 2015; Aizer and Currie 2014). Our results highlight the possibility that program *designs* that ignore insurer incentives may exacerbate the very disparities the program aimed to close.

The remainder of the paper is organized as follows. Section I describes the transition from FFS to MMC in Texas. Section II introduces the main data source and empirical strategy, and Section III presents the results. Section IV lays out our theoretical framework. Section V concludes.

I. Background on Medicaid and the Transition to MMC in Texas

In 1995, the Texas legislature voted to begin a staggered, statewide shift from traditional Medicaid FFS to Medicaid managed care. The Texas Health and Human Services Commission (HHSC) set the order in which counties would switch (online Appendix Table B1 provides details). According to HHSC officials, small urban areas switched first because they tended to have well-established healthcare provider networks, while being small enough to limit the costs related to any unforeseen transition issues. Larger urban counties switched next, and rural counties switched most recently in 2012. The percentage of the Texas Medicaid population enrolled in the managed care program (called State of Texas Access Reform, or STAR) increased from 2.9 in 1994 to 70.8 in 2009. We use this county-by-county rollout of MMC as our source of identification, and it is reassuring that the schedule was set by a central office and not negotiated by individual counties.

In Texas, as in almost all states, pregnant women and infants—the population we study—are eligible for Medicaid if their family incomes fall under 185 percent of the federal poverty line (FPL). Undocumented immigrants are not eligible for Texas Medicaid during our sample period, and, in fact, in Texas many *legal* immigrants were (and still are) ineligible.[7]

[6] In related work, Currie and Fahr (2005) use data from the National Health Interviews Survey (NHIS), and examine how state-level MMC penetration is related to individual-level Medicaid coverage and utilization of care among *children*. They find that higher MMC penetration is associated with lower Medicaid coverage and care utilization among black children with family incomes just above the poverty line (but not among those living below the poverty line). They find no statistically significant changes in coverage or utilization among Hispanic children. Given that black-Hispanic cost differences among children are far smaller than cost differences among infants, we might not expect to find large risk-selection incentives in this setting.

[7] For example, as a result of federal welfare reform in 1996, most legal immigrants were subject to a five-year waiting period for Medicaid coverage during our sample period. While some states chose to extend Medicaid coverage to legal immigrants during the five-year waiting period, Texas did not. In addition, Texas denies federal Medicaid coverage to many legal immigrants even after the five-year period. See: http://www.nilc.org/document.html?id=159.

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Once managed care was implemented in a county, participation among Medicaid enrollees was mandatory. Enrollees always have at least three insurers in their county from which to choose. The large majority—83 percent—of pregnant women make an active choice among MMC plans, suggesting an important role for plan reputation.[8] Because (in our pre-ACA sample period) low-income women are only Medicaid-eligible when pregnant, they must actively reenroll upon a subsequent pregnancy, as the state does not default-enroll them into their previous plan. Enrollment is coordinated via a third-party vendor, so clients do not enroll directly with plans themselves.

MMC insurance providers receive a capitation payment for each enrollee based on historical Medicaid costs in the locality. For every woman who gives birth, plans receive a Delivery Supplementary Payment and a newborn premium, which are unadjusted outside of these geographical averages. As expensive births cost far more than these fixed payments, they represent a large loss to the plans. When we asked the HHSC about whether these basic capitation payments also applied to very high-cost births, we were told that plans would simply make up these losses on profits from low-cost births: "This average [capitation payment] does include the higher cost deliveries and yes, it would under-pay for those but then again it overpays for others to make up for it." (Palmer 2012)

According to the state guidelines, MMC plans are required to provide:

> *…professional, inpatient facility, and outpatient facility medical services and prescription drug/pharmacy services, as long as the services are: (i) reasonably necessary to prevent illness or medical conditions, or provide early screening, interventions, and treatments for conditions that cause suffering or pain, cause physical deformity or limitations in function, threaten to cause or worsen a handicap, cause illness or infirmity of a recipient, or endanger life; (ii) provided at appropriate locations and at the appropriate levels of care for the treatment of clients' conditions; (iii) consistent with health care practice guidelines and standards that are issued by professionally-recognized health care organizations or governmental agencies; (iv) consistent with the diagnoses of the conditions and (v) no more intrusive or restrictive than necessary to provide a proper balance of safety, effectiveness, and efficiency.*[9]

This language suggests that, even with respect to required benefits, plans appear to enjoy substantial discretion. Moreover, plans are encouraged to tailor non-mandated ("value-added") benefits for each beneficiary. As noted in Texas HHSC Medicaid documentation:

> *Value-added services are additional health care services that an MCO [managed care organization] voluntarily elects to provide to its clients at no additional cost to the state. MCOs offer value-added services to*

[8] The remaining 17 percent are default-enrolled into a randomly assigned plan. In the rare cases when a Medicaid-eligible woman shows up at the hospital to deliver without having already chosen an MMC plan, she is randomly assigned a plan to cover the cost of the delivery and care of the infant. Note that the 83 percent figure is the current level of active enrollment (we do not have default rates during our sample period). We are grateful to Stephanie Goodman at Texas HHSC for this information.

[9] Source: www.dads.state.tx.us/providers/communications/alerts/TexasMedicaidCHIPHandout.pdf+cd=3hl=enct=clnkgl=pl.

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> *attract clients to sign up with them*, *including adult dental services and diapers for newborns.* Additional services may be offered to clients on a case-by-case basis at the discretion of the MCO [*emphasis added*].[10]

Plans thus have discretion to deny services to some enrollees while providing them to others. We discuss these discretionary services further in Section IVB.

Finally, as we argue in Section IV, the mechanism by which plans may engage in risk selection is by encouraging high-cost patients to switch to competitor plans. Switching plans is easy in Texas—there is no "lock-in" period and mothers can switch plans mid-pregnancy. Moreover, plans can disenroll patients. MMC plan handbooks state that clients can be dropped for reasons including not following the doctor's advice, repeated emergency room visits, and missing appointments.[11]

II. Data and Empirical Strategy

As noted in the introduction, we examine how outcomes for high- and low-cost groups evolve after a county switches from FFS to MMC. We first describe our main data source, and then explain how we use the county-by-county rollout of MMC to identify its effects on four different subgroups—low- and high-cost "treatment" groups that are largely eligible for Medicaid, as well as low- and high-cost "placebo" groups that are largely ineligible. We then provide evidence that US-born black and Hispanic pregnant women have large expected cost differences, while both having very high Medicaid coverage rates, whereas their foreign-born counterparts have similar cost differences but are generally ineligible for Medicaid.

A. *Main Data Source*

Our main source of data is the universe of birth records from the Texas Department of State Health Services (DSHS). These data contain detailed information on the child's exact birth date, birth outcomes, medical procedures, maternal demographics and health, and the mother's county of residence and country of birth. Using recorded information on each child's birth date and estimated gestation length, we calculate an approximate conception date for each observation. We merge the birth records data to data on the timing of MMC implementation by the mother's county of residence.

Counties switched from FFS to MMC between 1993 and 2006 (online Appendix Table B1). We drop the four pilot counties that switched in 1993 as we could not determine when the pilot period ended. We also drop counties that switched into MMC in January 2006 because this time period is concurrent with the influx of black refugees following Hurricane Katrina in September 2005.[12] Therefore, we

[10] See, http://www.hhsc.state.tx.us/medicaid/reports/PB8/PDF/Chp-6.pdf, pages 6–7.

[11] See, for example, pages 6–7 of the Parkland Community plan handbook here: http://parklandhmo.com/Handbooks/parkland%20english.pdf. We were unable to ascertain how often plans drop clients.

[12] Results are very similar when we do use the longer sample period and treat the 2006 transition as we do the earlier transitions, and in fact earlier versions of the paper included them before we realized Katrina could contaminate our results. It seems prudent to exclude this transition, however, as several of the counties that switch in 2006 are close to the Louisiana border.

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limit our sample of analysis to conceptions by mothers residing in Texas between January 1993 and December 2001, allowing for roughly three years before the first MMC switch (in December 1995) and three years after the last MMC switch (in January 1999). Finally, we drop observations missing information on gestation, parity, mother's age, mother's race/ethnicity, and mother's marital status, as well as a set of county-year controls, which leaves us with 2,814,681 observations.

B. *Empirical Design*

Our empirical strategy is straight-forward: we exploit variation in the timing of the MMC rollout across counties to create an event-study design. To ease the computational burden, we generally collapse data into county $\times$ conception-year-month cells and weight by cell size.[13] Our estimating equation thus takes the form

$$(1) \qquad Y_{ymc} = \beta MMC_{ymc} + \Lambda' W_{ymc} + \mu_c + \gamma_y \times \nu_m + \mu_c \times f(t) + \epsilon_{ymc},$$

for births to mothers residing in county c and conceived in year y and month m. The variable Y_{ymc} is a birth outcome of interest, such as mortality, birth weight, or gestation length; MMC_{ymc} indicates that the conception occurred after MMC implementation in county c; W_{ymc} is a set of county $\times$ year controls interpolated to the monthly level (which we vary to probe robustness); μ_c are county fixed effects; $\gamma_y \times \nu_m$ are conception-year-month fixed effects (i.e., separate controls for September 1994, October 1994, etc.); $\mu_c \times f(t)$ are county-specific time trends (more detail below); and ϵ_{ymc} is the error term, which we cluster by county. The key coefficient is β, which measures the effect of being conceived under MMC on the outcome of interest.

To avoid imposing constraints on coefficients, we estimate equations separately for each subgroup of interest, and then test whether the β coefficients vary significantly across groups. Moreover, by examining not only how the *gaps* between subgroups change but also how each subgroup fares in an *absolute* sense under MMC versus under FFS, we can better sort through potential mechanisms.

Our preferred specification includes county linear time trends, i.e., $f(t) = t$, in part to follow Aizer, Currie, and Moretti (2007). More to the point, our identifying assumption—that the timing of county MMC implementation is uncorrelated with factors related to infant health—appears more plausible when we condition on county linear trends. The even-numbered columns in online Appendix Table B2 show that the time-varying county characteristics available to us—log population, log per capita income, log per capita transfers, and the unemployment rate—are uncorrelated with MMC implementation once we condition on county linear trends. The odd-numbered columns show that without trends, some of these variables are correlated with MMC implementation at significant levels. As we examine a nine-year period during which Texas saw significant population growth, it is not surprising that some county-level factors would be correlated with implementation

[13] This method is equivalent to estimating the corresponding individual-level regression with no individual-level controls.

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Table 1—Summary Statistics

	All (1)	US black (2)	US Hispanic (3)	Foreign black (4)	Foreign Hispanic (5)	Married white (6)
Mother's age	25.76	24.12	23.79	28.72	25.93	28.05
	(6.063)	(5.949)	(5.835)	(5.902)	(5.789)	(5.555)
Child died	0.00725	0.0120	0.00715	0.0135	0.00565	0.00614
(death cert. matched to birth cert.)	(0.0848)	(0.109)	(0.0843)	(0.116)	(0.0750)	(0.0781)
Preterm	0.0923	0.135	0.0959	0.114	0.0755	0.0859
(gestation less than 37 weeks)	(0.289)	(0.342)	(0.294)	(0.318)	(0.264)	(0.280)
Low birth weight	0.0724	0.127	0.0733	0.0983	0.0579	0.0599
(birth weight below 2,500 g.)	(0.259)	(0.333)	(0.261)	(0.298)	(0.234)	(0.237)
Birth weight below 2,500 g.	0.159	0.172	0.142	0.192	0.148	0.174
or above 4,000 g.	(0.365)	(0.378)	(0.349)	(0.394)	(0.355)	(0.379)
Male	0.511	0.509	0.510	0.505	0.510	0.513
	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)
Prenatal care in first month	0.229	0.210	0.219	0.248	0.171	0.293
	(0.420)	(0.407)	(0.414)	(0.432)	(0.376)	(0.455)
Prenatal care at public clinic	0.126	0.136	0.124	0.0983	0.262	0.0398
	(0.332)	(0.343)	(0.330)	(0.298)	(0.440)	(0.195)
Prenatal care at hospital	0.172	0.248	0.134	0.292	0.294	0.0959
	(0.378)	(0.432)	(0.341)	(0.455)	(0.456)	(0.294)
Prenatal care at private doctor's office	0.677	0.601	0.745	0.581	0.362	0.851
	(0.467)	(0.490)	(0.436)	(0.493)	(0.481)	(0.356)
Observations	2,814,681	296,589	646,053	21,555	617,608	922,142

Notes: This table reports means for key variables in the Texas birth records data. The sample of analysis includes births that were conceived by mothers residing in Texas between January 1993 and December 2001.

when county trends are excluded, even if only coincidentally. Nevertheless, we show that our main results are largely robust to dropping linear trends or to including quadratic trends.

C. *Selecting High- and Low-Cost Treatment and Placebo Groups*

In Table 1, we present summary statistics for the entire sample, as well as several demographic subsets of mothers: US-born blacks, US-born Hispanics, foreign-born blacks, foreign-born Hispanics, and (all) married white non-Hispanics.

US-born black and Hispanic mothers are slightly younger than average, and considerably younger than married non-Hispanic white mothers. Prenatal care measures are substantially different for minorities and non-Hispanic whites. Only one-fifth of US-born blacks and Hispanics receive prenatal care in the first month of pregnancy, whereas 30 percent of married whites do. While less than 4 percent of married whites receive their prenatal care in public clinics, 13 and 19 percent of blacks and Hispanics do, respectively.

Differences in black-Hispanic infant health measures are substantial: children of US-born black mothers have rates of low birth weight, preterm delivery, and death that are, respectively, 71 percent, 41 percent, and 74 percent greater than the corresponding rates for children of US-born Hispanics. The black-Hispanic gap is

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Table 2—Hospital Charges for Newborns and Deliveries

	Newborn	Delivery		Newborn	
	(1)	(2)	(3)	(4)	(5)
Black	4,218.3	1,485.8	1,499.7		
	[110.0]	[16.52]	[16.51]		
Died				80,508.0	61,935.7
				[4,629.4]	[1,805.1]
Mean, dept. var.	5,813.6	7,107.5	7,107.5	10,085.4	6,274.2
Mean, ex. group	5,236.6	7,002.9	7,002.9	9,621.5	6,092.8
Percent diff.	0.806	0.212	0.214	8.367	10.17
Age cat. fixed effects	No	No	Yes	No	No
Sample	Bl., H.	Bl., H.	Bl., H	B.	H.
Observations	816,914	788,637	788,637	34,782	148,542

Notes: Regressions are based on data from public-use Texas hospital discharge data (see http://www.dshs.state.tx.us/THCIC/Hospitals/Download.shtm to download these data). All regressions include county and year fixed effects and include all Hispanic and black births from the third quarter of 2000 through 2004 (charges are suppressed before 2000:III). Column 3 includes maternal age fixed effects ($age < 20, age \in [20, 25), age \in [25, 30), age \in [30, 35)$, $age \geq 35$). All means of the dependent variable are reported, as well as the percent difference between the group denoted by the reported regression coefficient (e.g., blacks, in column 1) and the excluded group (e.g., Hispanics, in column 1). That is, "Percent diff." just divides the coefficient by the excluded-group mean. Columns 1 through 3 include all blacks and Hispanics, column 4 includes only blacks and columns 5 includes only Hispanics.

very similar among the foreign-born as well—slightly larger for mortality and low birth weight, and slightly smaller for preterm births. These large differences, while perhaps striking, are consistent with an established medical literature. Hispanic infants in the US are remarkably healthy—in fact, researchers use the term "Hispanic paradox" to describe the fact that despite socioeconomic deprivation comparable to blacks, they have much better health outcomes.[14] We generally take the cost differences between blacks and Hispanics as given, though briefly review potential explanations in the footnote below.[15]

Of course, what matters to health plans is expected cost above the capitation payment. In Table 2, we use Texas hospital discharge data over 2000–2004 to estimate differences in delivery and newborn costs by race and ethnicity (mother's place of birth is not included in these data), conditional on year and county fixed effects (as capitation payments are adjusted in this manner).[16] As column 1 shows, black newborns incur charges 81 percent greater than their Hispanic counterparts, or, in absolute

[14] See, for example, Leslie et al. (2003), Brown and Howard (2007), Alexander et al. (2003), and Dominguez (2008).

[15] The literature suggests that the Hispanic paradox is best explained by the superiority of diet and other health habits in Latin American countries relative to the United States, as these advantages appear to dissipate slightly in the second generation with assimilation (see Guendelman and Abrams 1995 as well as our Table 1). Another candidate explanation is the "healthy migrant effect"—that only the healthier members of a home country choose to migrate—though Rubalcava et al. (2008) find only weak evidence that Mexicans who move to the United States are healthier than their counterparts who remain.

[16] Unfortunately, discharge data with county identifiers are only available from the third-quarter of 1999 onward, and as such we cannot use it to compare outcomes before and after a county switched to MMC, since our last group of counties switch in January 1999.

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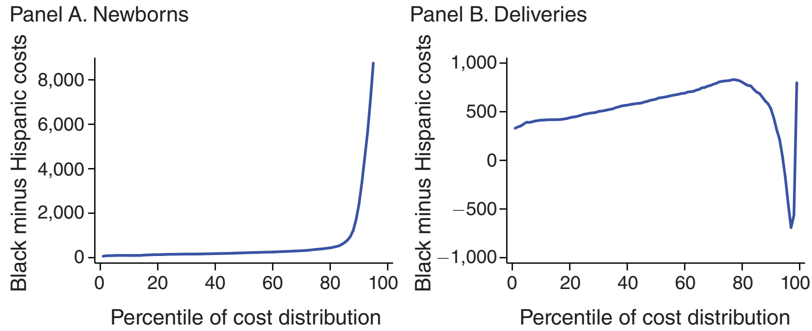

Figure 1. Distribution of Hospital Charge Differences between Blacks and Hispanics

Notes: Figures are based on data from public-use Texas hospital discharge data (see http://www.dshs.state.tx.us/THCIC/Hospitals/Download.shtm to download these data) for the third quarter of 2000 through 2004 (charges are suppressed before 2000:III). For each graph, the value of the Hispanic n^{th} percentile is subtracted from the value of the Black n^{th} percentile. Because of the extreme skewness of the newborn charges, the graph is truncated at the ninety-fifth percentile. The black-Hispanic difference for the ninety-ninth percentile is $76,341.

terms, an additional $4,218.[17] This absolute difference in initial hospital charges substantially understates the overall cost difference between black and Hispanic infants, as the elevated medical costs of at-risk births persist well beyond the first hospital stay.[18] The differences in costs associated with the mother are also substantial, with black mothers incurring 21 percent greater costs than Hispanics (column 2).[19]

In Figure 1, panels A and B show the difference in black and Hispanic births by percentile of the cost distribution. The black-Hispanic cost differences for newborns are positive at every centile, and the median difference is roughly $250. We censor at the ninety-fifth percentile ($8,452), as otherwise the graph is extremely compressed: the difference at the ninety-ninth percentile is $76,341. The differences in delivery charges associated with the mother are relatively constant for all percentiles.[20]

While we have identified ex ante high- and low-cost groups, it remains to be shown that we can separate them into treatment (i.e., Medicaid-eligible) and placebo (i.e., Medicaid-ineligible) groups. Texas only began collecting Medicaid status on the birth certificate starting in 2005, after our sample period ends. Moreover, the Medicaid variable is problematic in the context of studying MMC because privatizing Medicaid seems to have had the effect of making enrollees or providers incorrectly

[17] Hospital charge data are imperfect measures of the final cost to the insurer as plans negotiate discounts from providers. However, these discounts should not vary by demographic groups, so the comparisons in Table 2 give a good approximation of proportional cost differences.

[18] See, e.g., Tommiska, Tuominen, and Fellman (2003), and McCormick et al. (1991).

[19] Note this cost gap is not driven by differences in mothers' ages (column 3), the only relevant individual-level covariate we have in the discharge data. Additionally, we show in online Appendix Table B3 that the cost differences documented in Table 2 are robust to including hospital fixed effects. Note that differences conditional on hospital fixed effects are not directly relevant for plans, since capitation payments are not adjusted at the hospital level, and the choice of hospital for delivery may be endogenous to plan incentives.

[20] As noted, we would have ideally compared the costs of US-born blacks and Hispanics. The only study we know of that compares newborn hospital costs by race *and* place of birth is Reichman and Kenney (1998). They find that in New Jersey, the cost differences associated with black versus Mexican-origin mothers were actually slightly larger when restricted to US-born members of those groups.

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record some Medicaid births as being covered by a private or "other/unknown" insurer, a possibility hypothesized by Aizer, Currie, and Moretti (2007). We estimate that 30 percent of MMC births are incorrectly recorded.[21]

Online Appendix Table B4 shows Medicaid coverage in 2005 for our different subsets of Texas births (means are grossed up by 1.3 to adjust for underreporting). Medicaid covered approximately 84 and 88 percent of births to US-born black and Hispanic mothers in 2005, respectively; these births accounted for 56 percent of total Medicaid births.

As noted earlier, all undocumented immigrant women (and many documented immigrants as well) are excluded from Texas Medicaid during our sample period. Online Appendix Table B4 suggests that Medicaid coverage for foreign-born Hispanics is less than a third of that for their US-born counterparts.[22] Medicaid coverage of foreign-born blacks is only 40 percent of that for US-born blacks, but given the small sample size of immigrant blacks, immigrant Hispanics represent our more meaningful falsification group. As an additional check, we show results for married non-Hispanic whites, who also have relatively low Medicaid rates and thus serve as another plausible placebo group.

III. Results

Table 3 presents the results from estimating equation (1) for US-born black and Hispanic mothers (for ease of exposition, unless otherwise noted, "black" and "Hispanic" will refer to US-born black and US-born Hispanic mothers, respectively). For this and many other tables in this section, each pair of columns presents first the estimate for blacks and then the estimate for Hispanics. Toward the bottom of the table, the "Diff/p-val" row shows in the odd-numbered columns the corresponding differences in the MMC coefficients $(\beta^{Black} - \beta^{Hispanic})$ and in the even-numbered columns the p-value associated with the test of equality across the two coefficients.[23]

Columns 1 and 2 present results from regressions that omit any time-varying county controls besides the county linear time trends. The results suggest a large and significant increase in mortality for blacks and a nearly as large and significant decline for Hispanics. We take columns 3 and 4 as our preferred specification, which adds county $\times$ year $\times$ month controls for log population, log per capita income, log per capita transfers, and unemployment (given past work that health status might vary with economic conditions), though the point estimates barely move. These results show that mortality—measured by whether a death certificate can be matched with the birth certificate—increases by 0.179 percentage points or $0.179/1.198 = 14.9$ percent among births to black mothers, while falling by 0.154 percentage points or $0.154/0.715 = 21.5$ percent among births to Hispanic

[21] See the notes to online Appendix Table B4 for this calculation.

[22] In online Appendix C.1, we estimate an upper bound of 44 percent for the documented share of foreign-born Hispanic women in Texas, which is itself an upper bound on the *Medicaid-eligible* share.

[23] We test equality using seemingly-unrelated regression in Stata, equivalent to running a single regression in which every covariate is interacted with a dummy variable for race.

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Table 3—Effect of MMC on Mortality Rates ($\times 100$) for US-Born Black and Hispanic Births

	Black (1)	Hispanic (2)	Black (3)	Hispanic (4)	Black (5)	Hispanic (6)
Conceived after MMC	0.192	−0.155	0.179	−0.154	0.269	−0.145
	[0.0701]	[0.0834]	[0.0786]	[0.0749]	[0.109]	[0.0790]
log population			−3.14	−4.784	−8.22	−5.288
			[4.685]	[2.161]	[5.930]	[3.352]
log per capita income			3.342	−1.129	6.231	−0.371
			[1.932]	[0.642]	[2.620]	[1.189]
log per capita transfers			−5.392	1.582	−7.068	0.497
			[2.750]	[1.394]	[3.137]	[2.370]
Unemployment rate			185.1	−168.5	1.348	−41.53
			[595.7]	[148.9]	[666.5]	[265.6]
Dept. var. mean	1.198	0.715	1.198	0.715	1.260	0.822
Sample	All	All	All	All	Unmar.	Unmar.
Diff/p-val	0.347	0.00208	0.333	0.00237	0.414	0.00244
Reg. observations (cells)	12,833	20,504	12,833	20,504	11,766	16,370
Individual observations	296,589	646,053	296,589	646,053	190,899	250,154

Notes: These regressions are based on Texas birth certificate data. The sample of analysis includes births that were conceived by mothers residing in Texas between January 1993 and December 2001. Units of observation are county/conception-year/conception-month cells, and all regressions are weighted by cell size. All regressions include conception year $\times$ month and county fixed effects, and county-specific linear time trends. Controls are originally at the county-year level and are interpolated to the county-month level: log population (from BEA's Regional Economic Information System (REIS)), log per capita income (REIS), log per capita transfers (REIS), and the unemployment rate (from BLS's Local Area Unemployment Statistics). Standard errors are clustered by county. The "Diff/p-val" row shows, in the odd-numbered columns, the differences in the black-Hispanic *MMC* coefficients, and the even-numbered columns present the p-value associated with the test of equality across the two coefficients. For ease of interpretation of the coefficients, we rescale the county controls as follows (before taking the log): per capita income is divided by 10,000; unemployment rate is divided by 100; population is divided by 100,000; transfers per capita are divided by 10,000.

mothers.[24] Both effects are statistically significant. This 0.333 percentage point (or $0.333(1.198 - 0.715) = 68.4$ percent) increase in the black-Hispanic mortality gap is itself highly significant ($p \approx 0.002$). Columns 5 and 6 show that this growth of the black-Hispanic mortality gap is even larger among the unmarried: for this group, the black-Hispanic mortality gap nearly doubles ($0.414(1.26 - 0.822) = 94.5$ percent).

Figure 2 shows the coefficients and corresponding 95 percent confidence intervals from two event-study regressions, normalizing the year before MMC to zero (the regressions include all controls in columns 3 and 4 of Table 3, see figure notes for further detail). Figure 1, panel A shows that following MMC there is an increase in the mortality rates of children of US-born black mothers, while Figure 1, panel B shows a similarly marked, but negative shift in the mortality rates of children of US-born Hispanic mothers. There is no evidence of significant pre-trends for either group.

[24] More precisely, our mortality measure is an indicator for whether a death certificate is matched with the birth certificate by the time we obtained our data in 2010. Thus, for births in our sample, this measure captures both infant mortality and child mortality through ages beyond the first year of life. Note that we cannot use the standard linked birth/infant-death vital statistics files as they were not collected in 1992–1994, preventing us from examining pre-period data for most of our switching counties.

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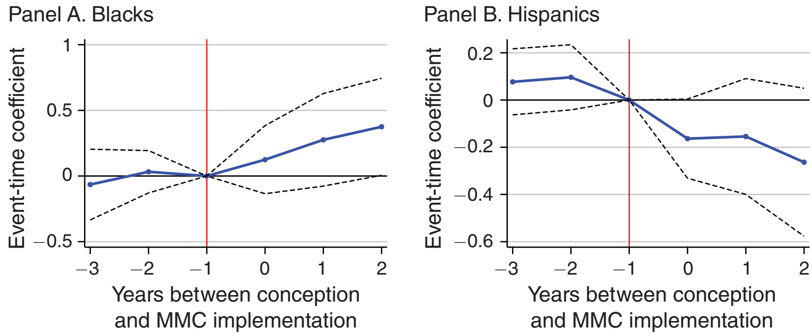

Figure 2. Changes in Mortality Rates ($\times$ 100) of Children Born to US-born Black and Hispanic Mothers (*note different scales*)

Notes: This figure shows the results from estimating the effects on mortality rates for black (panel A) and Hispanic (panel B) conceptions in the three years before and after MMC implementation. Specifically, we estimate the following equation:

$$Y_{ymc} = \alpha + \sum_{n=-3}^{-2} \beta_n \mathbf{I}_{ymc}^{n} + \sum_{n=0}^{2} \beta_n \mathbf{I}_{ymc}^{n} + \beta_{pre} \mathbf{I}_{ymc}^{>3\ years\ pre} + \beta_{post} \mathbf{I}_{ymc}^{>3\ years\ post}$$

$$+ \Lambda' W_{ymc} + \mu_c + \gamma_y \times \nu_m + \mu_c \times t + \epsilon_{ymc},$$

where $\mathbf{I}_{ymc}^{n}$ is an indicator variable for conceptions n years after a county c switched to MMC, meaning negative values of n indicate conceptions in years *before* MMC implementation. Time -1 denotes the year preceding MMC implementation and is the omitted category. $\mathbf{I}_{ymc}^{>3\ years\ pre}$ and $\mathbf{I}_{ymc}^{>3\ years\ post}$ are indicators for conceptions before and after the three-year window of MMC's introduction, respectively. The notation otherwise follows exactly from our main estimating equation (1) in the text. The figure plots the β_n coefficients along with the ninety-fifth percentile confidence intervals calculated using standard errors clustered on the county level.

Table 4—Effect of MMC on Other Birth Outcomes ($\times$ 100) for US-Born Black and Hispanic Births

	Preterm		LBW		Abn. BW		Male	
	Black	Hispanic	Black	Hispanic	Black	Hispanic	Black	Hispanic
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conceived after MMC	0.976	−0.710	0.730	−0.00717	0.849	−0.161	−0.762	0.786
	[0.336]	[0.198]	[0.380]	[0.154]	[0.360]	[0.349]	[0.428]	[0.249]
Dept. var. mean	13.51	9.593	12.72	7.334	17.25	14.21	50.95	51.04
Diff/p-val	1.687	8.95e−10	0.737	0.0206	1.010	0.0130	−1.55	0.00105
Reg. observations (cells)	12,833	20,504	12,828	20,502	12,828	20,502	12,833	20,504
Individual observations	296,589	646,053	296,584	646,051	296,584	646,051	296,589	646,053

Notes: See notes under Table 3 for more details about the data, sample, and specifications. "LBW" denotes birth weight $<$ 2,500 g; "Abn. BW" (abnormal birth weight) denotes birth weight $<$ 2,500 g or $>$ 4,000 g; "Preterm" denotes gestation $<$ 37 weeks. "Male" refers to the sex of the infant.

Table 4 shows results for other birth outcomes. Again, health significantly worsens for black infants (columns 1, 3, 5): the incidence of preterm birth (defined as gestation less than 37 weeks), low birth weight (birth weight less than 2,500 grams), and abnormal birth weight (birth weight less than 2,500 g or more than 4,000 g) increases by 7.2, 5.7, and 4.9 percent, respectively. We also include the sex ratio as an outcome (column 7), given the growing literature documenting its positive

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Table 5—Effect of MMC on Mortality and Birth Outcomes (× 100) for US-Born versus Foreign-Born Hispanic Births

	Mortality		Preterm		LBW		Abn. BW		Male	
	National (1)	Foreign (2)	National (3)	Foreign (4)	National (5)	Foreign (6)	National (7)	Foreign (8)	National (9)	Foreign (10)
Concieved after MMC	−0.154 [0.0749]	−0.0392 [0.0616]	−0.71 [0.198]	0.266 [0.450]	−0.00717 [0.154]	−0.0921 [0.193]	−0.161 [0.349]	−0.150 [0.245]	0.00786 [0.00249]	−0.00171 [0.00412]
Dept. var. mean	0.715	0.565	9.593	7.550	7.334	5.794	14.21	14.78	0.510	0.510
Diff/p-val	−0.115	0.0952	−0.976	0.0333	0.0849	0.761	−0.0108	0.974	0.00958	0.0186
Reg. N (cells)	20,504	18,153	20,504	18,153	20,502	18,147	20,502	18,147	20,504	18,153
Indiv. N	646,053	617,608	646,053	617,608	646,051	617,602	646,051	617,602	646,053	617,608

Notes: See notes under Table 3 for more details about the data, sample, and specifications. The odd-numbered columns present results for children of US-born Hispanic mothers (same as in the main results in Tables 3 and 4), while the even-numbered columns present results for children of foreign-born Hispanic mothers. "LBW" denotes birth weight < 2,500 g; "Abn. BW" (abnormal birth weight) denotes birth weight < 2,500 g or > 4,000 g; "Preterm" denotes gestation < 37 weeks. "Male" refers to the sex of the infant.

correlation with maternal well-being during pregnancy (as male fetuses are more likely to miscarry).[25] The male share of births falls for black mothers by 1.5 percent.

The even-numbered columns showing the Hispanic results tell a very different story. While results for birth weight are not significant, the preterm birth share falls by 7.4 percent and the male share increases by 1.5 percent. For all outcome variables in the table, the black-Hispanic gaps move in the direction of increasing health disparities after MMC, and are significant at the 5 percent level.

Online Appendix Figures B1 and B2 show graphically the results for preterm and male share of births, which showed the largest black-Hispanic post-MMC divergences in Table 4. As with mortality, the divergence in the preterm share for blacks and Hispanics begins in the first year after a county switches to MMC and we see no evidence of pre-trends.[26] The increase in the Hispanic male share also takes place at the time of MMC's introduction (the corresponding effect for blacks is noisier, reflecting the marginal significance of the coefficient in Table 4).

A. *Robustness Checks*

Results for Placebo Groups.— Table 5 shows that the improvement of outcomes for children of US-born Hispanic mothers does not extend to children of foreign-born mothers. The odd-numbered columns repeat the coefficients we have already presented for the outcomes of children of US-born Hispanic mothers in Tables 3 and 4. The even-numbered columns now present the results for the children of foreign-born mothers. Here, the "Diff/p-val" row shows, in the odd-numbered columns, the corresponding differences in the *MMC* coefficients $(\beta^{US\text{-}born} - \beta^{Foreign\text{-}born})$ and in the even-numbered columns the p-value associated with the test of equality across the two coefficients. We can reject that the β coefficients are

[25] See Fukuda et al. 1998 and Catalano et al. 2005.

[26] It does appear that the reduction in preterm births for Hispanics is relatively short-term; the coefficient is close to zero three years post-implementation. We do not look beyond three years graphically as the sample would be unbalanced and thus different observations would identify different coefficients.

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equal for mortality, share preterm, and share male. The large number of foreign-born Hispanic mothers ($N > 600{,}000$) should give us the power to distinguish even small effects from zero and thus serves as a powerful placebo test. We conclude that the improved outcomes of children born to US-born Hispanic mothers do not appear to be driven by unobserved trends affecting all Hispanics in Texas.

Similarly, the rise in poor birth outcomes to US-born black mothers does not extend to their foreign-born counterparts, but given the small sample size of foreign-born black mothers in Texas this placebo test is admittedly not as powerful and thus we relegate this analysis to online Appendix Table B5. Additionally, online Appendix Figure B3 shows that mortality and preterm birth rates of children of foreign-born Hispanic mothers evolved similarly to those of children of US-born Hispanic mothers prior to MMC (i.e., we do not observe any pre-trends for either group), giving us some reassurance that they serve as useful counterfactuals for each other.[27]

Finally, online Appendix Table B7 and Appendix Figure B4 examine another plausible placebo group—married non-Hispanic white mothers. We find marginally significant increases in mortality and preterm birth rates for this group, although the coefficient magnitudes are far smaller than the corresponding magnitudes found for US-born blacks and Hispanics. While we do not observe the mechanisms behind this result, we note that it is broadly consistent with prior evidence that average birth outcomes deteriorate under MMC in California (Aizer, Currie, and Moretti 2007).

Additional County Time-Varying Controls.—We can add additional time-varying county controls, but at the expense of losing some sample size. Online Appendix Table B8 shows that (on this smaller sample) our main black and Hispanic mortality results are not sensitive to the inclusion of: the employment to population rate, the log annual firm profits, log total number of establishments, and the death rate for individuals aged 65+.

Varying County Trend Controls.—Online Appendix Table B9 tests the robustness of our main mortality result to the inclusion of different types of county trends. Columns 1 and 2 show specifications without any trends; columns 3 and 4 replicate our main specification with county linear trends; columns 5 and 6 include county quadratic trends. Across all specifications, the increase in the mortality gap between blacks and Hispanics is statistically significant at the 5 percent level. When we omit county trends, the results are somewhat weaker (and while the individual black and Hispanic coefficients are the same sign as with linear trends, they are smaller and no longer significant on their own), but this is perhaps not surprising given our earlier finding that the MMC rollout may be correlated with other county factors when we do not

[27] Immigration patterns affect the composition of the foreign-born population, making the assumption that trends in outcomes for children of foreign-born and US-born mothers evolve similarly potentially problematic. In Texas, there was a 90 percent increase in the foreign-born population over the 1990–2000 period (see: http://www.migrationpolicy.org/data/state-profiles/state/demographics/TX), and about a 130 percent increase in the estimated undocumented Hispanic population over the same time period (see: http://www.pewhispanic.org/2014/12/11/unauthorized-trends/). However, these increases have been fairly linear during our sample time frame, and we show that they are largely uncorrelated with the timing of the MMC rollout. Specifically, online Appendix Table B6 demonstrates that our treatment variable is uncorrelated with either the share or the log of births to foreign-born black and Hispanic women.

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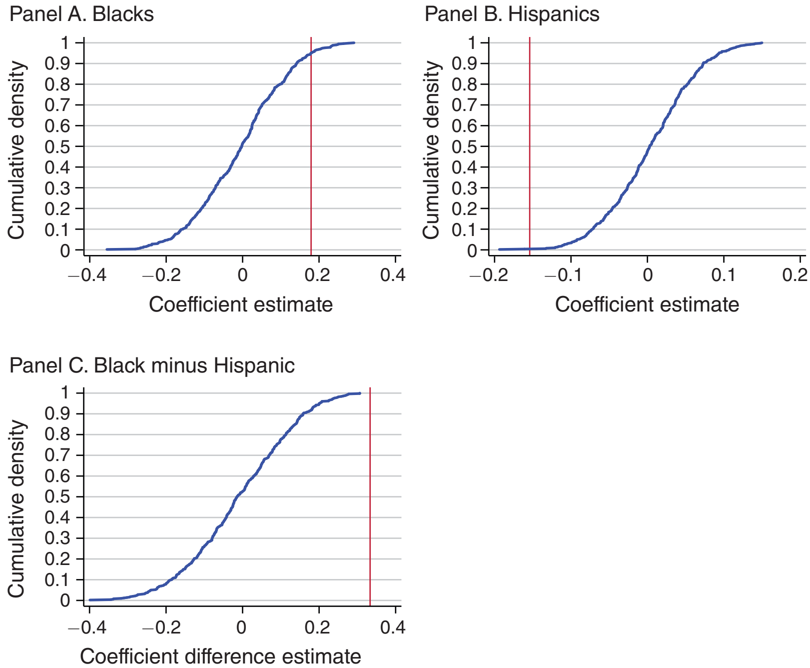

Figure 3. Permutation Test for Mortality Outcome: CDFs of Coefficient Estimates from 500 Draws of Randomly Assigned Placebo Treatment

Notes: This figure shows the cumulative density functions that come from a permutation test in which, in every iteration, each "switcher county" is randomly assigned an implementation date (we exclude the six months before and after the true implementation) instead of the true implementation date. The graphs show the cdfs generated by 500 draws. The vertical lines show the location of the true coefficients for mortality for US-born blacks (in panel A), Hispanics (in panel B), and the black-Hispanic difference (panel C).

control for county trends (see online Appendix Table B2). Nevertheless, the shape (if not most of the magnitudes) of the event-study figures for our main outcomes are largely unchanged when we omit county trends (see online Appendix Figures B5 and B6 for mortality and preterm births, respectively). Including quadratic trends slightly increases the magnitude of our main result, relative to linear county trends.

Permutation Tests.—How likely is it that we would find the results we report in Table 3 if instead of using the actual MMC reform dates for our counties, we used *randomly assigned* reform dates? We perform a simulation in which, for a given iteration, each of our "switcher counties" is randomly assigned an implementation date (we exclude the six months before and after the true implementation as these will be mechanically close to the actual estimate). We then estimate our baseline regression model and record β^{Black} and $\beta^{Hispanic}$. We present the cumulative density functions for each statistic generated by 500 draws in Figure 3.

For mortality, the true black coefficient falls at roughly the ninety-fifth percentile, while the Hispanic coefficient is below the first percentile. The black-Hispanic difference we observe is larger than any produced by the 500 iterations. We show

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parallel results for preterm births in online Appendix Figure B7, which, if anything, shows our result is even more unlikely under random assignment of reform dates.

Changes in Selection.—Our results are consistent with blacks receiving lower quality care relative to Hispanics after MMC, but also with negative changes in selection into birth for black versus Hispanic infants. Online Appendix Table B10 tests whether the incidence of maternal risk factors changes for US-born blacks versus Hispanics after MMC. Columns 1 and 2 show that after MMC, the share of mothers of advanced maternal age (35 and older) declines; columns 3 and 4 show both groups are less likely to have diabetes or hypertension (though for neither group is the effect significant); columns 5 and 6 show blacks are less likely and Hispanics are more likely to smoke (though neither result is significant on its own). Of the three outcomes, only one (smoking) shows a statistically significant black-Hispanic divergence, in the direction of blacks being relatively *positively* selected after MMC.[28] This result suggests that, if anything, the effect of MMC on the divergence of birth outcomes in Tables 3 and 4 is actually understated. In the final two columns, we show the results when "predicted mortality"—generated using a large set of pre-determined characteristics and their interactions—serves as the outcome. Again, relative to Hispanics, blacks appear more positively selected post-MMC.

Indeed, when we rerun regressions in online Appendix Tables B11 and B12 for each of the birth outcome variables using individual-level data and controlling for all plausible predetermined covariates on the birth certificate (see the table notes), the results are essentially unchanged.

Finally, although the evidence points in favor of limited potentially positive selection of black mothers (relative to Hispanic mothers) on observable characteristics post-MMC, it is of course conceivable that there is some negative selection on unobservables. In supplementary analysis, we found that the switch from FFS to MMC led to a small and insignificant decrease in births to US-born black women, driven by those who are unmarried (results available upon request). We thus do a back-of-the-envelope calculation to estimate how much healthier than the pre-period baseline the "missing" black infants would have to be for our effect on mortality to be fully explained by compositional changes (see online Appendix C.2 for more details). Given the very small decline in black births post-MMC alongside the sizable increase in infant mortality, the mortality rates of "missing" black infants would have to be substantially *negative* to explain our mortality results.

In sum, given how stable the coefficients are with and without controls, the results on selection in online Appendix Table B10, and our back-of-the-envelope calculation on the mortality rates of "missing" black births, the evidence would seem to suggest that differential selection into pregnancy post-MMC is unlikely to explain our

[28] It is also plausible that maternal smoking rates are affected by MMC plans' actions rather than through selection. In particular, it may be that plans are effective at reducing smoking among black pregnant women but not Hispanic pregnant women. However, we do not have any direct evidence on this point, and it is inconsistent with our finding that other aspects of prenatal care seem to deteriorate for blacks relative to Hispanics (see below). Moreover, the wording of the question about smoking on the birth certificate refers to "any tobacco use during pregnancy," which, for women who do not initiate prenatal care immediately, may also pick up pre-MMC behavior.

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results. While we cannot rule out selection on unobservables, the evidence points to treatment effects of MMC rather than selection as the most plausible explanation.

Plausibility of Magnitudes.—The relative effects we find for blacks and Hispanics—especially for mortality, preterm birth, and the sex ratio—are large, but not out of step with past research on the effects of health care on infant outcomes. Perhaps most relevant, Aizer, Currie, and Moretti (2007)'s estimate of the deleterious effects of MMC in California on neonatal death (a 50 percent increase) is larger than the mortality increases we find for infants born to black mothers (14.9 percent). In their seminal paper on the effects of Medicaid coverage on infant health, Currie and Gruber (1996) find that if a state were to change from 0 to 100 percent Medicaid eligible, there would be a 30 percent reduction in infant mortality. Given that about 85 percent of the black and Hispanic mothers in our sample are on Medicaid, the comparable 85 percent change in Medicaid treatment status would lead to a 26 (0.85×30) percent change in infant mortality, nearly twice the effect we find.

It is also useful to compare our magnitudes to estimates on the effects of specific medical interventions at the hospital of birth. For example, based on studies on the effect of being born in a hospital with a NICU, we estimate that a lack of NICU availability in hospitals where black women give birth could explain about 60 percent of the increase in black children's mortality that we find.[29] We should note that during this time period, there was substantial variation in NICU access in Texas hospitals (even in most urban, populous counties). Thus, even within the same county, a plan's decision about where a patient would deliver could determine whether a birth would have access to this technology.

Other markers of hospital quality matter as well—Aizer, Lleras-Muney, and Stabile (2004) find that Medicaid mothers with access to higher quality hospitals in California experienced a 9 percent decline in neonatal mortality.[30] Access to a hospital with electronic medical records (EMR) technology leads to about a 32 percent decline in neonatal mortality rates, even when conditioning on NICU availability (Miller and Tucker 2011).

Outside of the hospital setting, other interventions have proved effective at improving infant and child health, especially for at-risk populations (Currie and Rossin-Slater 2015). A two-decade follow-up of the Nurse Family Partnership (NFP) randomized control trial conducted in the 1990s shows that child mortality through age 20 is reduced by 1.6 percentage points (in fact, a 100 percent reduction) as a result of the treatment (Olds et al. 2014). As we discuss in Section IVB, nurse and social worker visits are a service that some plans choose to provide but is not mandated by the state.

[29] Specifically, Lorch et al. (2012) find that being born in a hospital with NICU capability leads to an average reduction of 0.77 deaths per 100 preterm births in Pennsylvania, Missouri, and California over our sample time period. Assuming that NICU access has zero effect on all non-preterm births, then if all black births were moved to non-NICU hospitals due to MMC, we should see a total mortality increase of about $0.77 \times \textit{Share preterm} = 0.77 \times 0.135 = 0.104$ deaths per 100 births, or an 8.7 percent increase in mortality rates.

[30] They proxy hospital quality by whether higher SES women give birth in a given hospital. After payment reform, more Medicaid mothers gave birth in the same hospitals as high-SES women did.

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Table 6—Effect of MMC on Prenatal Care Measures (× 100) for US-Born Black and Hispanic Births

	Imm. PNC		PVS		PVS > 7		ΔW > 15		ΔW > 20	
	Bl. (1)	Hsp. (2)	Bl. (3)	Hsp. (4)	Bl. (5)	Hsp. (6)	Bl. (7)	Hsp. (8)	Bl. (9)	Hsp. (10)
Conceived after MMC	−2.175	0.0547	−0.0730	−0.0801	−2.280	−0.938	−1.470	0.149	−2.122	0.277
	[1.893]	[0.925]	[0.0851]	[0.0739]	[0.923]	[0.726]	[0.701]	[0.768]	[0.686]	[1.210]
Dept. var. mean	21.01	21.88	10.45	10.87	79.42	83.02	86.45	87.11	74.48	74.72
Diff/*p*-val	−2.229	0.0611	0.00706	0.929	−1.341	0.0331	−1.619	0.0399	−2.398	0.0777
Reg. *N* (cells)	12,767	20,424	12,617	20,271	12,617	20,271	12,192	19,902	12,192	19,902
Indiv. *N*	296,516	645,966	296,225	645,741	296,225	645,741	295,429	645,237	295,429	645,237

Notes: See notes under Table 3 for more details about the data, sample, and specifications. Note that the key explanatory variable of interest is an indicator for being born after (rather than conceived after) MMC. "Imm. PNC" denotes "immediate prenatal care," indicating that the mother received care within the first month of her pregnancy. "PVS" denotes the total number of prenatal care visits. "PVS > 7" denotes more than seven visits. The remaining two outcomes refer to maternal weight gain (ΔW) in pounds.

Finally, recent work suggests that maternal stress during pregnancy has important effects on birth outcomes. For example, Lauderdale (2006) finds that women with "Arabic-sounding" names exhibited a 50 percent increase in preterm births after September 11, 2001. Other research shows that in utero exposure to maternal stress due to the death of a family member has small but significant impacts on low birth weight and preterm birth rates (Black, Devereux, and Salvanes 2016; Persson and Rossin-Slater 2018). Fukuda et al. 1998 and Catalano et al. 2005 find that the Kobe earthquake in Japan and the unemployment rate, respectively, increases in the male neonate death rate, resulting in changes to the sex ratio, which are slightly larger than the effects we find in Table 4. While it is of course impossible to objectively compare the stress associated with (our hypothesized) decrease in care over the course of a pregnancy with the events (many one-time, acute episodes) examined in these studies, this line of research highlights that stress may be an additional mechanism that contributes to the effects we find.

In sum, as MMC could presumably affect infant health through all of these margins, our effect sizes appear well within the bounds suggested by past policy and medical interventions.

B. *Results on Birth Inputs*

The birth certificate data also provide information on prenatal care, though as Reichman and Schwartz-Soicher (2007) document, prenatal care information on birth certificates (relying on mothers' recall) is less accurate than birth outcomes data.

Table 6 shows results for indicators of prenatal care.[31] The first two columns show that US-born blacks are less likely to receive immediate (within the

[31] We examine the quantity of prenatal care as a birth input, since this information is available in our data. The evidence on the impacts of prenatal visits on birth outcomes is mixed. Randomized studies of prenatal care typically compare the outcomes of women who had a standard number of prenatal care visits with those of women who had a reduced schedule of visits, finding little impact of additional visits on birth outcomes (Sikorski et al. 1996; Fiscella 1995). However, these trials are conducted on relatively small numbers of low-risk women, and thus cannot address the question of whether prenatal care might be beneficial for higher risk women. Observational evidence

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first month of pregnancy) prenatal care relative to Hispanics after MMC (though results for other thresholds of prenatal care initiation are not significant). There is no difference in the total number of prenatal visits, though blacks are less likely than Hispanics to receive at least eight visits (columns 5 and 6).[32] However, as shown in the corresponding event-study graphs in online Appendix Figures B8, B9, and B10, the results are fairly imprecise and thus should be interpreted with some caution. Table 6 also shows that, after MMC, black women are more likely than Hispanics to gain insufficient weight during pregnancy, which increases the probability of an infant being small for gestation age and of infant death (Park et al. 2011, Tenovuo 1988, Giapros et al. 2012). Online Appendix Figures B11 and B12 present the corresponding event-study figures for maternal weight gain, which confirm the regression estimates.[33]

Unfortunately, the Texas birth certificate does not provide hospital identifiers or any other information on hospital characteristics (e.g., whether the hospital has a neonatal intensive care unit) nor, by construction, can it record inputs a plan might provide once the mother and infant return home. To get some understanding of whether the large mortality effects we find are likely due to pre-birth inputs or inputs delivered at time of delivery or later, we estimate in online Appendix Table B13 the results for mortality using individual-level data, where we flexibly control for gestation, birth weight, and their interactions (see table notes). Our idea is that mortality effects conditional on these flexible controls are likely attributable to care given at the time of delivery or later. For Hispanics, these controls decrease the MMC coefficient by only 10 percent. For blacks, roughly 53 percent of the mortality effect is explained, and in fact the MMC coefficient is no longer significant ($p = 0.11$), suggesting that much of the mortality effect is explained by the rise in low birth weight and preterm births.

While these results are merely suggestive, they point to the possibility that Hispanics not only experienced an increase in healthy pregnancies post-MMC (as reflected in the large decline in Hispanic preterm births), but also large mortality declines holding the initial conditions at birth constant. This pattern is consistent with recent evidence from Chen, Oster, and Williams (2014), who show that differences in conditions at birth cannot fully explain the large differences in post-neonatal mortality rates between the United States and European countries like Austria and Finland. In their discussion, Chen, Oster, and Williams (2014) postulate that policies that target post-neonatal health, such as nurse home visiting programs, may be important for reducing mortality among US infants. Given that the value-added benefits in

(from a sibling fixed effects design) suggests that the number of prenatal visits increases birth weight, and the effects are concentrated in the lower part of the birth weight distribution (Abrevaya and Dahl 2008). Finally, prenatal care may be beneficial in providing mothers-to-be with medical services that are not only limited to pregnant women. For instance, given evidence that exposure to the influenza virus can lead to preterm delivery (Currie and Schwandt 2013), prenatal care visits may be useful in ensuring that pregnant women receive flu vaccinations.

[32] We choose this cut-off because almost all women receive at least a handful of visits so there is little variation, whereas visits beyond this point become endogenous to gestation and mother's health.

[33] Recommended weight gain depends on pre-pregnancy BMI. According to the CDC, 57 and 64 percent of Hispanic and black women are overweight, respectively. The Institute for Medicine recommends weight gain during pregnancy of 15–25 pounds when women begin pregnancy overweight. As such, we choose cut-offs of 15 and 20 pounds in our regression analysis, as higher cut-offs would unlikely affect fetal health for this population.

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MMC include home visits, it is plausible that our mortality effects could be in part explained by differentially generous provision of these benefits to Hispanic mothers (a possibility for which we provide some qualitative support in Section IVB).

On the whole, it appears that the deterioration of care (as measured by the prenatal inputs recorded on birth certificates) for blacks relative to Hispanics is likely too small to fully explain the large increases in outcome disparities documented in Tables 3 and 4. Moreover, while we saw large absolute improvements for Hispanic outcomes (as opposed to merely improvements relative to blacks' decline) after MMC, we see little evidence for *absolute* improvements for Hispanic inputs. Given that we find no evidence that selection changed, this pattern of results suggests a role for margins of care that the birth certificate data do not record. We provide some qualitative evidence linking plan benefits to the demographic composition of the area it serves in Section IVB.

IV. Do MMC Plan Incentives Explain the Rise in Health Disparities?

Our empirical results presented above consistently document that the switch from FFS to MMC led to an increase in health disparities between black and Hispanic pregnant women and infants. Below we sketch a framework of risk-selection that can generate these results (a more formal treatment is relegated to online Appendix A) and show that more traditional risk-selection models fail to predict some aspects of the patterns we find.

We emphasize upfront that our data cannot provide a very exacting test of our model. Nonetheless, we include this discussion as it may serve as a useful starting point for future researchers, who (with better data) could be in a better position to support or refute the framework.

A. *Modeling Incentives in MMC versus FFS*

The existing literature on risk selection in insurance markets is rich, but it is not directly applicable to selection in the MMC versus FFS setting. For example, in Medicare, private Medicare Advantage (MA) plans compete alongside a state-run FFS program, and researchers have examined whether MA plans are able to avoid high-cost individuals by directing them to the FFS plan.[34] However, this type of risk selection is irrelevant for MMC plans, as they compete only against each other and there is no FFS option. Similarly, many papers have found that in the so-called "wild west" of the pre-ACA-regulations non-group insurance market, private plans risk-selected by simply denying coverage to high-cost enrollees, charging them higher premiums, or carving out coverage of preexisting conditions.[35] But state MMC regulations mandating guarantee issue, community rating (at a universal premium of $0), and a comprehensive set of guaranteed benefits prohibit such blunt risk-selection techniques. To the best of our knowledge, there is no model of

[34] See, e.g., Langwell and Hadley (1989), Mello et al. (2003), and Batata (2004).
[35] See Baicker and Dow (2009), for example, and citations therein.

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risk selection that fully captures MMC's institutional characteristics and we attempt to sketch out such a framework below.

While profit-maximing plans have an incentive to "cream-skim" however possible to attract the lowest cost patients, they must also decide how to treat patients (both low- and high-cost) who have already joined. In both the MMC and ACA settings, plans are banned from refusing to cover a given patient. However, we assume that the state cannot perfectly observe how plans treat patients (what benefits they approve, how long they wait for a specialist, etc.) and whether they exercise discretion in treating some patients better than others.

Consider two patients, healthy (H) and sick (S), with expected costs below and above the capitation payment, respectively. The probability that a patient returns to the same plan in the following period (e.g., in our context of Medicaid births, that a mother chooses the same plan for her child's subsequent care or for her next pregnancy) increases with the level of care. If we assume that higher quality care is more expensive, then, all else equal, a plan spending more money on a patient increases the chance she reenrolls in the future.

Consequently, plans have an incentive to retain the healthy, low-cost, profitable patient H, and thus provide her with greater levels of care. By contrast, plans balance two competing incentives in treating the sick, high-cost, unprofitable patient S — although reducing the level of care may worsen her outcomes and increase costs in the current period, it will also encourage her to switch to a competitor plan in the next period. Important to the model is the fact that patients always have a choice between at least two plans (which is the case in MMC and in the ACA exchanges).

We assume, as in Newhouse (1996), that such incentives do not exist in FFS. As providers are reimbursed *per service*, the fact that higher cost patients require more services does not create a reason to ration their care or to avoid them. So, the switch from FFS to MMC would imply a shift of health resources toward the *already healthy*, thus increasing pre-existing health disparities between groups.

Unlike many models of adverse selection, plans in our framework need not be able to predict the costs of enrollees ex ante or to devise a menu of services that encourage the healthy to self-select (though they may engage in such tactics as well). Instead, they can learn about patient costs and profitability ex post and adjust the quality of care based on whether they wish to retain the patient in the future.

Can Other Models Rationalize Our Results?—The classic models of insurance and risk selection (e.g., Rothschild and Stiglitz 1976, Glazer and McGuire 2000) are single-period. Plans attempt to design menus of prices and benefits that separate the healthy and the sick. Relative to a world where these incentives do not exist (e.g., FFS), these models will typically involve *lower levels of benefits* for both the sick *and* the healthy when premium prices are not allowed to vary (as in our MMC setting, when premiums are set to zero for all). In essence, plans cannot provide better benefits to differentially attract healthy types, as the sick types would in fact find these benefits even more enticing. Static models of risk selection have trouble delivering a result where healthy types enjoy *improved* care or coverage. The dynamic feature of our model allows plans to exercise much more flexibility. They can see exactly who is high- and low-cost, and so long as (i) the state cannot perfectly

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observe how plans treat patients and (ii) how plans treat patients determines whether they will reenroll, they will have an incentive to give more care to the healthy.

Related, consider a model where privatization over-incentivizes cost-cutting relative to other social objectives, as in Hart, Shleifer, and Vishny (1997). Such a model would again have trouble explaining the improvement in outcomes for the low-cost group that we have documented empirically.

Second, consider a model in which the managed care model of care (e.g., narrow networks, physician gatekeepers) is simply better (worse) at treating low-risk (high-risk) clients, separate from any financial incentives arising under capitation. Given how tightly linked the managed care model and capitation are in the public health insurance landscape, separately identifying the effects of the *model of care* from the *incentives induced by capitation* is difficult (and impossible in Texas, as all plans are fully capitated).

We thus turn to California MMC as a setting that offers some separation. In California, plans are allowed to "carve out" costs above a given percentile and pass them back to the state. As we showed in Figure 1, differences in costs between blacks and Hispanics below the ninety-fifth percentile are relatively small, and thus these carve-outs blunt much of the risk-selection incentives we have argued exist in Texas. Consequently, the California MMC transition allows us to examine a setting where patients switch from FFS to the managed care model but plans do not have strong financial incentives to avoid sick patients. If the managed care model alone were responsible for exacerbating black-Hispanic health gaps, we should see similar patterns in California.

While Aizer, Currie, and Moretti (2007) did not examine results by race and ethnicity in California, recent follow-up work has, and finds no difference in the effect of MMC on health outcomes for blacks versus Hispanics (both tend to deteriorate).[36] In short, no evidence of risk selection emerges in a setting that switched from FFS to the managed care model of care, but that did not subject plans to high-powered financial incentives. Of course, there are many other differences between California and Texas that might contribute to disparate result patterns. Nevertheless, this evidence is consistent with financial incentives being relevant.

Finally, it is difficult to completely eliminate an alternative model of institutional racism on the part of plans or the providers with whom they contract.[37] While our model emphasizes the role of financial incentives, its predictions are observationally equivalent to prejudice or animus against the high-cost group. To the extent competition limits the effects of discrimination (Becker 1957), then the presence of at least three competing plans per county would, at least in theory, argue against this alternative explanation. Further, as noted above, in California's MMC program, financial incentives are blunted and black and Hispanic care outcomes are similar (Barham, Gertler, and Raube 2013), suggesting at least that in that setting racial animus alone is insufficient to increase disparities in health outcomes between blacks and Hispanics.

[36] See Barham, Gertler, and Raube (2013), in particular Tables 7 and 8.
[37] See the the Institute of Medicine's landmark 2002 publication on racial disparities for a review of ways in which African Americans receive lower quality care even conditional on rich sets of observables.

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B. *Further Tests of the Framework*

While our data allow us to show results on birth outcomes and some limited prenatal inputs that are consistent with the model, more work is necessary to test the model. As such, we lay out some avenues for future research (with better data, in different settings) that might provide stronger evidence to support or refute the framework we propose.

A major limitation is that we do not observe plan actions. In general, managed care plans are not required to report their activities (e.g., claims data) to the payor (in our case, the state), so it is not surprising such data is hard to find. But researchers that are able to access data from either an MMC plan or a plan in the ACA exchanges could make greater headway in testing the predictions of our model. In an ideal setting, one would compare spending on black and Hispanic mothers under FFS (in principle, such data are available as states needed to reimburse providers) to spending by an MMC plan after a state switches from FFS to MMC. Our model would predict that, relative to FFS, MMC plans spend less on blacks and more on Hispanics.

Another major limitation we face is that the framework depends crucially on there being at least two periods, but we only have cross-sectional data and cannot follow mothers over time (nor do we know which plans they choose). Researchers with access to longitudinal data might examine the following predictions of our framework. First, patient satisfaction should correlate with a higher probability of choosing the same plan in the future. Second, higher cost patients should switch plans more often than lower cost patients (as they receive worse care and should have lower satisfaction).

While we would love to have quantitative data on plan activities, we close by offering some qualitative evidence on services that plans advertise. This information serves to show that plans seem aware of the financial incentives they face with regard to black and Hispanic births, but does not serve as definitive proof of any particular mechanism underlying risk-selection.

As documented in Section I, MMC plans in Texas are given discretion over to whom they offer so-called "value-added services" (and of course discretion along the many aspects of care unobserved by the state). Individual-level access and use of such services is not recorded, so we take a second-best approach and ask whether plans that operate in areas with more Hispanic clients appear to advertise more generous services.

Online Appendix Table B14 provides plan-level data on both the demographics of the areas they serve as well as the services they advertise on their websites. Many plans (e.g., Amerigroup) operate across the state, and thus their demographics reflect the state's average. Others are more local and thus provide greater variation. The black/Hispanic population ratio varies from 1.2 percent (Driscoll Children's, which serves counties near the Mexican border) to 58.1 percent (Parkland Community, which serves Dallas). Examples of value-added services targeted toward pregnant mothers include free baby showers, prenatal classes, gifts, home visits, and free transportation.[38]

[38] Value-added services are paid for by the plan, not the Medicaid program.

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By way of example, consider the services advertised by Driscoll to those advertised by Parkland. Driscoll offers MMC clients free eyeglasses, cell phone minutes, transportation to appointments, dental care, gift cards, and a bilingual prenatal class for pregnant mothers-to-be ("Cadena de Madres"). The prenatal class includes three baby showers, baby gifts, and access to a nurse and a social worker (see online Appendix Figure B13).[39]

While Driscoll prominently advertises its "extras," Parkland's website does not even list its value-added services; a list of these services is only found in the member handbook.[40] Parkland does not offer any of the extra services offered by Driscoll. For pregnant women, the only value-added service is a gift for completing a prenatal education class, but Parkland does not host its own course, instead offering to subsidize outside classes.

While the aggregate nature of these plan-level data is limiting, we perform the basic exercise of comparing the average black/Hispanic ratio in areas covered by generous plans relative to the average black/Hispanic ratio in the state (28.1 percent). Online Appendix Table B15 shows that the average black-Hispanic ratio is lower among plans offering baby showers (25.6 percent), prenatal or post-natal gifts (22.8 percent, 22.6 percent), or prenatal classes (25.1 percent). That is, plans serving areas with relatively more Hispanic clients appear to offer more discretionary services. As these services are provided free of charge to the state, it appears that some of the surplus plans gain from enrollees whose costs are well below the average expected cost may get passed back to Hispanic mothers in this fashion.

Back-of-the-Envelope Calculation.—In our model, plans face a trade-off between how well they treat a sick patient today (low-quality care can result in large costs in the current period, e.g., an extended stay in a NICU) and the probability she (and her negative expected profits) returns in a future period. Recall the first-order condition for sick patients: $1 = -c'(\theta) + \delta\lambda'(\theta)\, V_{t+1}^{S}$. For a given value of $\delta\lambda'(\theta)\, V_{t+1}^{S}$, if costs are very sensitive to increases in care (i.e., $-c'(\theta) \ << \ 0$), then plans will have little incentive to skimp on care for the sick. In this case, risk selection in our dynamic setting is no longer a first-order problem because current-period costs act as a strong deterrent.

To gauge whether current-period costs are prohibitive, we return to the Texas hospital discharge data. Controlling for county and year fixed effects, the average cost difference between a newborn who dies and a newborn who survives is \$48,770.36 (see column 6 in Table 2). From column 3 of Table 3, we find that black mortality increases by roughly 0.00179 percentage points after MMC. As such, this increase in mortality represents a $\$48{,}770.36 \cdot 0.00179 \ = \ \$87.30$ increase in costs per infant. This calculation almost surely overstates the cost of mortality, as we

[39] Source: http://www.dchpkids.com/star/services.php and http://www.dchpkids.com/services/location=cadena de-madres. It is worth noting that in addition to *where* services are offered, the *types* of services offered can be used to select low-cost Medicaid eligible individuals; for example, prenatal classes in Spanish would only appeal to Hispanic clients, and car seats, a frequently offered post-natal gift, would only appeal to the higher income clients who have cars (We thank Anna Aizer for making the latter point).

[40] Source: http://parklandhmo.com/healthrst%20page.html and http://parklandhmo.com/Handbooks/parkland%20english.pdf, value-added services are listed on page 9.

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are comparing the costs of infants who die to the cost of *all* infants who survive, whereas most infants on the margin, where skimping determines survival, are probably very high-cost even if they survive.[41]

This \$87.30 represents the cost of skimping to plans, whereas the benefits are not only the current-period savings from the reduced care itself, but also the higher discounted future profits from potentially avoiding the costly patient in the future, $\delta\lambda'(\theta)\, V_{t+1}^{S}$. Lacking longitudinal data, we have no direct measure of $\lambda'(\theta)$, but in equilibrium it could be very high. Assuming all plans skimp on care to the sick, one plan deviating from this strategy could quickly attract all sick patients (while receiving the same per capita capitation payment). Finally, presumably plans devote some of the skimped resources toward the healthy group, reducing costs associated with mortality for them. In sum, \$87.50 in higher expected costs via increased mortality risk per black birth would not appear to be a deterrent for risk selection.

V. Conclusion

We examine the experiences of black and Hispanic pregnant women and infants—two groups that have observably large differences in average healthcare costs and who are disproportionally covered by public health insurance—in a setting where the government finances and regulates competing capitated private insurance plans, but does not itself administer a FFS plan. We focus on the transition from FFS Medicaid to Medicaid managed care in Texas to measure the causal effects of MMC on care provision and health outcomes among black and Hispanic births. Our results show that the black-Hispanic mortality, preterm, and low birth weight birth gaps increase by 69, 45, and 12 percent, respectively, after a county switches from FFS to MMC. Moreover, after MMC, observed prenatal care inputs generally fall for blacks relative to Hispanics.

We argue that our empirical findings are consistent with a simple dynamic model of risk selection in settings where private, capitated plans compete against each other in a highly regulated environment. While we attempt to provide suggestive evidence that supports the mechanisms we have in mind in this framework, we relegate more conclusive tests of the model to future researchers who may have access to better data than we do.

We believe that our results provide compelling evidence that outcomes diverge for high- and low-cost groups under MMC, but welfare implications are complicated. Given the larger number of Hispanics than blacks in Texas, average birth outcomes do not decline. However, if society wishes to shrink health disparities, then MMC may be inferior to FFS as it appears to transfer health resources *away* from the sick to the healthy.

While not often noted by policymakers, MMC operates similarly to both the ACA exchanges and recently proposed Medicare "premium support" models.

[41] It is possible that plans may consider the threat of litigation as another cost associated with newborn death. Existing research shows that poor patients (who are predominantly on Medicaid) are less likely to sue doctors relative to higher income patients, despite the fact that the poor are more likely to suffer from medical negligence and/or malpractice (Studdert et al. 2000, McClellan et al. 2012). As such, we do not believe that this is likely to be a prohibitive cost for the plans, but do not have any direct evidence on this point.

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Like MMC beneficiaries, clients in these settings have a choice between highly regulated, capitated, competing insurance plans, but no option to join a state-run FFS program.[42] Of course, key differences exist (e.g., ACA clients can pay more for more comprehensive plans, and premium-support clients can similarly 'top-up' the premium to purchase more expensive plans) but the basic logic of private plans competing against one another in a multi-period setting may prove useful in studying risk-selection in these settings as well.

In our framework, an inefficiency arises because plans want clients with costs above the capitation payment to switch to a competitor and thus reduce their care below the socially optimal level. This externality problem would not exist with a monopolistic insurer (though other problems associated with a monopoly would likely arise). Our results suggest that competition may undermine the underlying policy goal of capitation in insurance when viewed in a dynamic setting—being the residual claimant on costs above or below the capitation payment ideally leads plans to internalize patients' future costs, but they may instead attempt to pass on these costs to their competitors. With Medicaid Managed Care, the ACA exchanges, Medicare Part D, and recent calls to transform Medicare into a private premium-support program, the United States appears to be moving toward providing public health insurance through a model of competing, capitated private insurance plans, making future work in this area of growing importance.

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[42] Medicare premium support models have differed on the question of whether FFS would still exist as an option.

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