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

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Abstract

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 pre-term birth rates *increase* by 15% and 7%, respectively, while Hispanic mortality and pre-term birth rates *decrease* by 22% and 7%, 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 Codes: I11, I18, J13
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1 Introduction

Increasingly in U.S. 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.

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 (eleven percent of the U.S. 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-

[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 $0.74 * 50 = 37$ 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.

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 pre-natal 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 U.S.-born black and Hispanic mothers. In Texas, the large majority of Medicaid births are to U.S.-born black and Hispanic women, but their children have very different health outcomes. For example, children of U.S.-born black mothers have seventy percent greater mortality and low-birth-weight rates than children of U.S.-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 eighty 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 U.S.-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 pre-term birth rate gaps also increase significantly.

With respect to quality of care, we find some suggestive evidence that after MMC, black mothers, relative to Hispanics, are less likely to begin pre-natal care in their first month of pregnancy, more likely to have fewer than eight

[3]This black-Hispanic gap in health has also been widely documented in other settings. We review this evidence in Section 3.3.

pre-natal 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 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 pre-existing 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 *et al.* (2007) find that pre-natal 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 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 Almond and Currie, 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 *et al.*, 2012; Miller and Wherry, 2014; Brown *et al.*, 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 2 describes the

[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 pre-natal care and birth outcomes that relies primarily on cross-sectional variation and pre/post analyses without comparison groups (see Kaestner *et al.*, 2002 for an overview).

[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.

transition from FFS to MMC in Texas. Section 3 introduces the main data source and empirical strategy, and Section 4 presents the results. Section 5 lays out our theoretical framework. Section 6 concludes.

2 Background on Medicaid and the transition to MMC in Texas

In 1995, the Texas legislature voted to begin a staggered, state-wide 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 (Appendix Table 1 provides details). According to HHSC officials, small urban areas switched first because they tended to have well-established health-care 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]

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

[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`.

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 re-enroll 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."[9]

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: (1) 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; (2) provided at appropriate locations and at the appropriate levels of care for the treatment of clients' conditions; (3) consistent with health care practice guidelines and standards that are issued by professionally- recognized health care organizations or govern-

[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]Email correspondence with the chief actuary for HHSC (March 30, 2012).

mental agencies; (4) consistent with the diagnoses of the conditions and (5) no more intrusive or restrictive than necessary to provide a proper balance of safety, effectiveness, and efficiency."[10]

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 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].[11]

Plans thus have discretion to deny services to some enrollees while providing them to others. We discuss these discretionary services further in Section 5.2.

Finally, as we argue in Section 5, 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 dis-enroll 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.[12]

3 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

[10]Source: `www.dads.state.tx.us/providers/communications/alerts/TexasMedicaidCHIPHandout.pdf+&cd=3&hl=en&ct=clnk&gl=pl`.

[11]See `http://www.hhsc.state.tx.us/medicaid/reports/PB8/PDF/Chp-6.pdf`, p. 6-7.

[12]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.

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 U.S.-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.

3.1 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 (Appendix Table 1). 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.[13] Therefore, we 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

[13]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.

us with 2,814,681 observations.

3.2 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×conception-year-month cells and weight by cell size.[14] Our estimating equation thus takes the form:

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

for births to mothers residing in county c and conceived in year y and month m. 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×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 *et al.* (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 con-

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

dition on county linear trends. The even-numbered columns in Appendix Table 2 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 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.

3.3 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: U.S.-born blacks, U.S.-born Hispanics, foreign-born blacks, foreign-born Hispanics, and (all) married white non-Hispanics.

U.S.-born black and Hispanic mothers are slightly younger than average, and considerably younger than married non-Hispanic white mothers. Pre-natal care measures are substantially different for minorities and non-Hispanic whites. Only one-fifth of U.S.-born blacks and Hispanics receive pre-natal care in the first month of pregnancy, whereas 30 percent of married whites do. While less than four percent of married whites receive their pre-natal 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 U.S.-born black mothers have rates of low-birth-weight, pre-term delivery, and death that are, respectively, 71 percent, 41 percent, and 74 percent greater than the corresponding rates for children of U.S.-born Hispanics. The black-Hispanic gap is very similar among the foreign-born as well—slightly larger for mortality and low-birth-weight, and slightly smaller for pre-term births. These large differences, while perhaps striking, are consistent with an established medical literature. Hispanic infants in the U.S. are remarkably healthy—in fact, researchers use the term "Hispanic paradox" to describe the fact that despite socio-economic deprivation comparable to blacks, they have much better health outcomes.[15] We generally take the cost differences between blacks and Hispanics as given, though briefly review potential explanations in the footnote below.[16]

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).[17] As column (1) shows, black newborns incur charges 81 percent greater than their Hispanic counterparts, or, in absolute terms, an additional $4,218.[18] 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.[19] The differences in costs associated with the mother are also substantial, with black mothers incurring 21 percent greater costs than Hispanics (col. 2).[20]

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

[16] 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 U.S., 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 US are healthier than their counterparts who remain.

[17] 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.

[18] 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.

[19] See, e.g., Tommiska *et al.* (2003) and McCormick *et al.* (1991).

[20] Note this cost gap is not driven by differences in mothers' ages (col. 3), the only relevant individual-level covariate we have in the discharge data. Additionally, we show in Appendix Table 3 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

Figures 1(a) and 1(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 $250. We censor at the 95$^{\text{th}}$ percentile ($8,452) as otherwise the graph is extremely compressed: the difference at the 99$^{\text{th}}$ percentile is $76,341. The differences in delivery charges associated with the mother are relatively constant for all percentiles.[21]

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 record some Medicaid births as being covered by a private or "other/unknown" insurer, a possibility hypothesized by Aizer *et al.* (2007). We estimate that 30 percent of MMC births are incorrectly recorded.[22]

Appendix Table 4 shows Medicaid coverage in 2005 for our different subsets of Texas births (means are grossed up by 1.3 to adjust for under-reporting). Medicaid covered approximately 84 and 88 percent of births to U.S.-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. Appendix Table 4 suggests that Medicaid coverage for foreign-born Hispanics is less than a third of that for their U.S.-born counterparts.[23]

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of hospital for delivery may be endogenous to plan incentives.

[21] As noted, we would have ideally compared the costs of U.S.-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 was actually slightly larger when restricted to U.S.-born members of those groups.

[22] See the notes to Appendix Table 4 for this calculation.

[23] In the Appendix, we estimate an upper bound of 44 percent for the documented share

Medicaid coverage of foreign-born blacks is only forty percent of that for U.S-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.

4 Results

Table 3 presents the results from estimating equation (1) for U.S.-born black and Hispanic mothers (for ease of exposition, unless otherwise noted, "black" and "Hispanic" will refer to U.S-born black and U.S.-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.[24]

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×year×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 mothers.[25] Both effects are statistically significant.

of foreign-born Hispanic women in Texas, which is itself an upper bound on the *Medicaid-eligible* share.

[24]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.

[25]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

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% 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 2(a) shows that following MMC, there is an increase in the mortality rates of children of U.S.-born black mothers, while Figure 2(b) shows a similarly marked, but negative, shift in the mortality rates of children of U.S.-born Hispanic mothers. There is no evidence of significant pre-trends for either group.

Table 4 shows results for other birth outcomes. Again, health significantly worsens for black infants (columns 1, 3, 5): the incidence of pre-term 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 correlation with maternal well-being during pregnancy (as male fetuses are more likely to miscarry).[26] 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 pre-term 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.

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.

[26] See Fukuda *et al.*, 1998 and Catalano *et al.*, 2005.

Appendix Figures 1 and 2 show graphically the results for pre-term and male share of births, which showed the largest black-Hispanic post-MMC divergences in Table 4. As with mortality, the divergence in the pre-term share for blacks and Hispanics begins in the first year after a county switches to MMC and we see no evidence of pre-trends.[27] 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).

4.1 Robustness checks

Results for placebo groups. Table 5 shows that the improvement of outcomes for children of U.S.-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 U.S.-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 ($\beta^{U.S.\text{-}born} - \beta^{Foreign\text{-}born}$) 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 equal for mortality, share pre-term, 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 U.S-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 U.S.-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 Appendix Table 5. Addition-

[27]It does appear that the reduction in pre-term 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.

ally, Appendix Figures 3(a) and 3(b) show that mortality and pre-term birth rates of children of foreign-born Hispanic mothers evolved similarly to those of children of U.S.-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.[28]

Finally, Appendix Table 7 and Appendix Figure 4 examine another plausible placebo group—married non-Hispanic white mothers. We find marginally significant increases in mortality and pre-term birth rates for this group, although the coefficient magnitudes are far smaller than the corresponding magnitudes found for U.S.-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 *et al.*, 2007).

Additional county time-varying controls. We can add additional time-varying county controls, but at the expense of losing some sample size. Appendix Table 8 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. Appendix Table 9 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

[28]Immigration patterns affect the composition of the foreign-born population, making the assumption that trends in outcomes for children of foreign-born and U.S.-born mothers evolve similarly potentially problematic. In Texas, there was a 90% 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% 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, Appendix Table 6 demonstrates that our treatment variable is uncorrelated with either the share or the log of births to foreign-born black and Hispanic women.

the mortality gap between blacks and Hispanics is statistically significant at the 5% 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 control for county trends (see Appendix Table 2). 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 Appendix Figures 5 and 6 for mortality and pre-term 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 95$^{\text{th}}$ 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 parallel results for pre-term births in Appendix Figure 7, 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. Appendix Table 10 tests whether the incidence of maternal risk-factors changes for U.S.-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.[29] 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 re-run regressions in Appendix Tables 11 and 12 for each of the birth outcome variables using individual-level data and controlling for all plausible pre-determined 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 U.S.-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 Appendix C for more details). Given the very small decline

[29]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 pre-natal 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 pre-natal care immediately, may also pick up pre-MMC behavior.

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 Appendix Table 10, 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 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, pre-term 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 *et al.* (2007)'s estimate of the deleterious effects of MMC in California on neonatal death (a fifty 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 zero 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 ($0.85 \cdot 30$) 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.[30] We should note that during this time period, there was substantial

[30] 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 pre-term births in Pennsylvania, Missouri, and California over our sample time period. Assuming that NICU ac-

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 *et al.* (2004) find that Medicaid mothers with access to higher-quality hospitals in California experienced a 9 percent decline in neonatal mortality.[31] 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 Home Visiting Partnership (NHVP) 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 5.2, nurse and social worker visits are a service that some plans choose to provide but is not mandated by the state.

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 fifty percent increase in pre-term 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 pre-term birth rates (Black *et al.*, 2016; Persson and Rossin-Slater, Forthcoming). 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,

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cess has zero effect on all non-pre-term births, then if all black births were moved to non-NICU hospitals due to MMC, we should see a total mortality increase of about $0.77 \cdot Share\,preterm = 0.77 \cdot 0.135 = 0.104$ deaths per 100 births, or an 8.7 percent increase in mortality rates.

[31] 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.

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.

4.2 Results on birth inputs

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

Table 6 shows results for indicators of pre-natal care.[32] The first two columns show that U.S.-born blacks are less likely to receive immediate (within the first month of pregnancy) pre-natal care relative to Hispanics after MMC (though results for other thresholds of pre-natal care initiation are not significant). There is no difference in the total number of pre-natal visits, though blacks are less likely than Hispanics to receive at least eight visits (columns 5 and 6).[33] However, as shown in the corresponding event-study graphs in Ap-

[32]We examine the quantity of pre-natal care as a birth input, since this information is available in our data. The evidence on the impacts of pre-natal visits on birth outcomes is mixed. Randomized studies of pre-natal care typically compare the outcomes of women who had a standard number of pre-natal 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 pre-natal care might be beneficial for higher-risk women. Observational evidence (from a sibling fixed effects design) suggests that the number of pre-natal visits increases birth weight, and the effects are concentrated in the lower part of the birth weight distribution (Abrevaya and Dahl, 2008). Finally, pre-natal 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 pre-term delivery (Currie and Schwandt, 2013), pre-natal care visits may be useful in ensuring that pregnant women receive flu vaccinations.

[33]We choose this cut-off because almost all women receive at least a handful of visits so

pendix Figures 8, 9, and 10, 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). Appendix Figures 11 and 12 present the corresponding event-study figures for maternal weight gain, which confirm the regression estimates.[34]

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 time of delivery or later, we estimate in Appendix Table 13 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 pre-term 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 pre-term births), but also large mortality declines holding the initial conditions at birth constant. This pattern is consistent with recent evidence from Chen *et al.* (2014), who show

there is little variation, whereas visits beyond this point become endogenous to gestation and mother's health.

[34]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.

that differences in conditions at birth cannot fully explain the large differences in post-neonatal mortality rates between the U.S. and European countries like Austria and Finland. In their discussion, Chen *et al.* (2014) postulate that policies that target post-neonatal health, such as nurse home visiting programs, may be important for reducing mortality among U.S. infants. Given that the value-added benefits in 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 5.2).

On the whole, it appears that the deterioration of care (as measured by the pre-natal 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 5.2.

5 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 the Appendix) 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.

5.1 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.[35] 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 pre-existing conditions.[36] 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 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

[35] See, e.g., Langwell and Hadley (1989), Physician Payment Review Commission (1997), Mello *et al.* (2003) and Batata (2004).

[36] See Baicker and Dow (2009), for example, and citations therein.

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 re-enrolls 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 (a) the state cannot perfectly observe how plans treat patients and (b) how plans treat patients determines whether they will re-enroll, 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 *et al.*, 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 95$^{\text{th}}$ 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 *et al.* (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).[37] 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.[38] 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 *et al.*, 2013), suggesting at least that in that setting that racial animus alone is insufficient to increase disparities in health outcomes between blacks and Hispanics.

5.2 Further tests of the framework

While our data allow us to show results on birth outcomes and some limited pre-natal 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

[37] See Barham *et al.* (2013), and in particular Tables 7 and 8.

[38] 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.

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 2, 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.

Appendix Table 16 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% (Driscoll Children's, which serves counties near the Mexican border) to 58.1% (Parkland Community, which serves Dallas). Examples of value-added services targeted toward pregnant mothers include free baby showers, pre-natal classes, gifts, home visits, and free transportation.[39]

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 pre-natal class for pregnant mothers-to-be ("Cadena de Madres"). The pre-natal class includes three baby showers, baby gifts, and access to a nurse and a social worker (See Appendix Figure 13).[40]

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.[41] 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 pre-natal 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). Appendix Table 17 shows that the average black-Hispanic ratio is lower among plans offering baby showers (25.6%), pre-natal or post-natal gifts (22.8%, 22.6%) or pre-natal classes (25.1%). 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.

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

[40] 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, pre-natal 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).

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

6 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, pre-term, 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 pre-natal 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 policy-makers, MMC operates similarly to both the ACA exchanges and recently proposed Medicare 'premium support' models. 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

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

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 suggests 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 U.S. 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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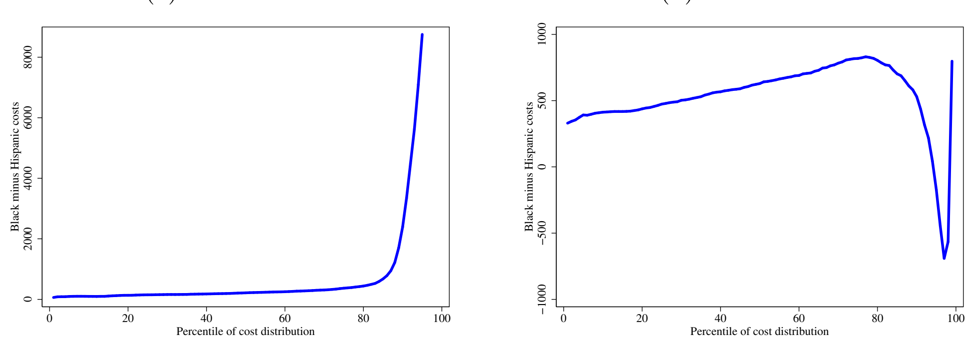
Figure 1: Distribution of hospital charge differences between Blacks and Hispanics

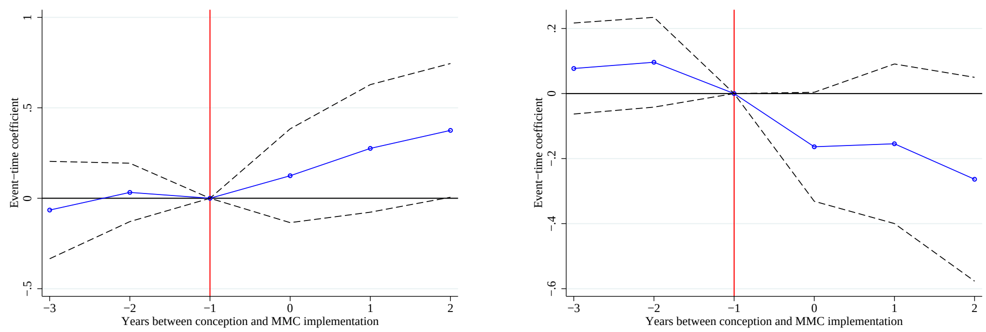
(a) Newborns (b) Deliveries



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:Q3). 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 95$^{\text{th}}$ percentile. The black-Hispanic difference for the 99$^{\text{th}}$ percentile is \$76,341.

Figure 2: Changes in mortality rates ($\times 100$) of children born to U.S.-born black and Hispanic mothers (note different scales)

(a) Blacks (b) Hispanics



Notes: These figures show the results from estimating the effects on mortality rates for black (Figure a) and Hispanic (Figure b) conceptions in the 3 years before and after MMC implementation. Specifically, we estimate the following equation:

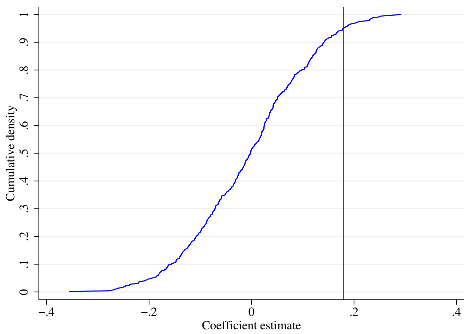
$$Y_{ymc} = \alpha + \sum_{n=-3}^{-2} \beta_n \mathbf{I}_{ymc}^{n} + \sum_{n=0}^{2} \beta_n \mathbf{I}_{ymc}^{n} + \beta_{pre} \mathbf{I}_{ymc}^{>3\,years\,pre} + \beta_{post} \mathbf{I}_{ymc}^{>3\,years\,post}$$

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

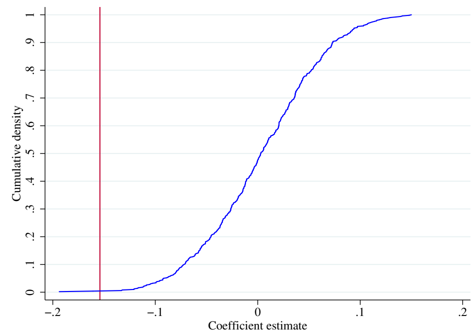
where $\mathbf{I}_{ymc}^{n}$ is an indicator variable for conceptions n years after a county c switched to MMC, meaning negative values of n indicate conceptions in years *before* MMC implementation. Time -1 denotes the year preceding MMC implementation and is the omitted category. $\mathbf{I}_{ymc}^{>3\,years\,pre}$ 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 95% confidence intervals calculated using standard errors clustered on the county level.

Figure 3: Permutation test for mortality outcome: CDFs of coefficient estimates from 500 draws of randomly assigned placebo treatment

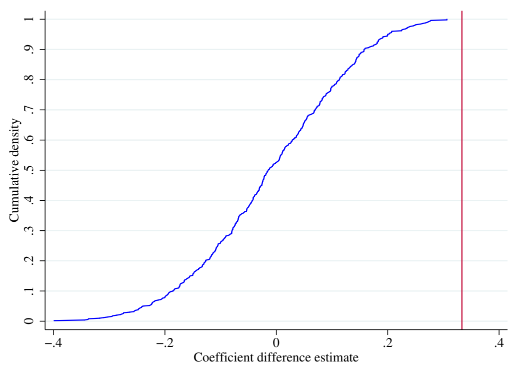
(a) Blacks



(b) Hispanics



(c) Black minus Hispanic



Notes: These figures show 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 red vertical lines show the location of the true coefficients for share pre-term for U.S.-born blacks (in 7(a)) and Hispanics (in 7(b)).

Table 1: Summary statistics

	(1) All	(2) U.S. Bl.	(3) U.S. Hisp.	(4) For. Bl.	(5) For. H.	(6) Mar. Wh.
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 (death cert. matched to birth cert.)	0.00725	0.0120	0.00715	0.0135	0.00565	0.00614
	(0.0848)	(0.109)	(0.0843)	(0.116)	(0.0750)	(0.0781)
Pre-term (Gestation less than 37 weeks)	0.0923	0.135	0.0959	0.114	0.0755	0.0859
	(0.289)	(0.342)	(0.294)	(0.318)	(0.264)	(0.280)
Low-birth weight (Birthweight below 2,500 g.)	0.0724	0.127	0.0733	0.0983	0.0579	0.0599
	(0.259)	(0.333)	(0.261)	(0.298)	(0.234)	(0.237)
Birthweight below 2,500 g. or above 4,000 g.	0.159	0.172	0.142	0.192	0.148	0.174
	(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)
Pre-natal 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)
Pre-natal 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)
Pre-natal 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)
Pre-natal 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.

Table 2: Hospital charges for newborns and deliveries

	Newborn	Delivery		Newborn	
	(1)	(2)	(3)	(4)	(5)
Black	4218.3***	1485.8***	1499.7***		
	[110.0]	[16.52]	[16.51]		
Died				80508.0***	61935.7***
				[4629.4]	[1805.1]
Mean, dept. var.	5813.6	7107.5	7107.5	10085.4	6274.2
Mean, ex. group	5236.6	7002.9	7002.9	9621.5	6092.8
Pct. diff	0.806	0.212	0.214	8.367	10.17
Age cat. FE	No	No	Yes	No	No
Sample	Bl., H.	Bl., H.	Bl., H	B.	H.
Observations	816914	788637	788637	34782	148542

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:Q3). Col. (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 col. 1) and the excluded group (e.g., Hispanics, in col. 1). That is, "Pct. Diff" just divides the coefficient by the excluded-group mean. Cols. (1) through (3) include all blacks and Hispanics, col. (4) includes only blacks and col. (5) includes only Hispanics.

Table 3: Effect of MMC on mortality rates ($\times 100$) for U.S.-born black and Hispanic births

	(1) Bl.	(2) Hsp.	(3) Bl.	(4) Hsp.	(5) Bl.	(6) Hsp.
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			-3.140	-4.784**	-8.220	-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. obs (cells)	12833	20504	12833	20504	11766	16370
Indiv. obs.	296589	646053	296589	646053	190899	250154

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 re-scale 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. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < .01$

Table 4: Effect of MMC on other birth outcomes (×100) for U.S.-born black and Hispanic births

	Preterm		LBW		Abn. BW		Male	
	(1) Bl.	(2) Hsp.	(3) Bl.	(4) Hsp.	(5) Bl.	(6) Hsp.	(7) Bl.	(8) Hsp.
Conceived after MMC	0.976*** [0.336]	-0.710*** [0.198]	0.730* [0.380]	-0.00717 [0.154]	0.849** [0.360]	-0.161 [0.349]	-0.762* [0.428]	0.786*** [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. obs (cells)	12833	20504	12828	20502	12828	20502	12833	20504
Indiv. obs.	296589	646053	296584	646051	296584	646051	296589	646053

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 birthweight) denotes birthweight $< 2,500$g or $> 4,000$g; "Pre-term" denotes gestation < 37 weeks. "Male" refers to the sex of the infant.

Table 5: Effect of MMC on mortality and birth outcomes ($\times 100$) for U.S.-born vs. foreign-born Hispanic births

	Mort.		Pret.		LBW		Abn. BW		Male	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Nat.	For.	Nat.	For.	Nat.	For.	Nat.	For.	Nat.	For.
Conceived after MMC	-0.154**	-0.0392	-0.710***	0.266	-0.00717	-0.0921	-0.161	-0.150	0.00786***	-0.00171
	[0.0749]	[0.0616]	[0.198]	[0.450]	[0.154]	[0.193]	[0.349]	[0.245]	[0.00249]	[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. obs (cells)	20504	18153	20504	18153	20502	18147	20502	18147	20504	18153
Indiv. obs.	646053	617608	646053	617608	646051	617602	646051	617602	646053	617608

Notes: See notes under Table 3 for more details about the data, sample, and specifications. The odd-numbered columns present results for children of U.S.-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 birthweight) denotes birthweight $< 2,500$g or $> 4,000$g; "Pre-term" denotes gestation < 37 weeks. "Male" refers to the sex of the infant.

Table 6: Effect of MMC on pre-natal care measures (×100) for U.S.-born black and Hispanic births

	Imm. PNC		PVS		PVS> 7		Δ W > 15		Δ W > 20	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Bl.	Hsp.	Bl.	Hsp.	Bl.	Hsp.	Bl.	Hsp.	Bl.	Hsp.
Conceived after	-2.175	0.0547	-0.0730	-0.0801	-2.280**	-0.938	-1.470**	0.149	-2.122***	0.277
MMC	[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. obs (cells)	12767	20424	12617	20271	12617	20271	12192	19902	12192	19902
Indiv. obs.	296516	645966	296225	645741	296225	645741	295429	645237	295429	645237

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 pre-natal care," indicating that the mother received care within the first month of her pregnancy. "PVS" denotes the total number of pre-natal care visits. "PVS>7" denotes more than 7 visits. The remaining two outcomes refer to maternal weight gain (ΔW) in pounds.

A Model

A.1 Modeling incentives in MMC versus FFS

Consider two types of patients, healthy (H) and sick (S). Patient types are fixed over time. There are two types of costs that plans incur: those associated with preventive care θ (defined broadly; in our context it could include factors like the number of pre-natal visits and the quality of the hospital at which the mother will deliver) and those associated with outcomes $c_i(\theta)$, where c varies by patient type.[43] For simplicity, let $c_H(\theta) = c(\theta)$ and $c_S(\theta) = c(\theta) + \alpha$, with $c' < 0$ and $c'' > 0$, so the returns to care are the same across patient type (as in Glazer and McGuire (2000)).

Incentives in MMC. Under MMC, there are at least two plans from which patients can choose. Plans receive a capitation payment p regardless of patient type. Plans face a dynamic problem—how they treat a patient today determines whether she will return in the next period.[44] Let $\lambda(\theta)$ be the probability a patient choses the same plan in the next period, which is increasing concavely in the care she receives in the current period, so $\lambda' > 0$ and $\lambda'' < 0$. (In fact, in Texas, Medicaid recipients can change plans in the middle of a pregnancy, though we were unable to determine how frequently such a transition occurs.) We scale down this probability by a discount factor δ to reflect the fact that she may exit the Medicaid program (e.g., no longer meet the income test) and to ensure a finite stream of expected profits.

[43] As both these costs are direct functions of θ we could instead formulate the model in terms of a total cost function, but splitting costs in this manner aids with intuition and maps more closely to the empirical results. Note also that we do not distinguish between mothers and infants and combine costs for both (as shown earlier, empirically all variation in this sum cost is driven by costs related to the infant).

[44] In our MMC context, "returning the next period" can either mean that the mother continues using this plan for the infant's later health care needs or that she returns to this plan the next time she is pregnant (and thus eligible for Medicaid herself).

We assume that plans can quickly learn patient type after a mother enrolls. First, they might form a reasonable estimate based on basic observables such as age and race. Second, in an initial check-up, information such as BMI, blood pressure, and health history will be gained. Third, diagnostic procedures throughout the pregnancy may reveal even more detailed information. We thus assume that patient type is observable to the plan at the point they are making many of their decisions about approving care θ.

Knowing patient type, each plan solves the following dynamic maximization problems:

$$V_t^H = \max_\theta \left\{ p - \theta - c(\theta) + \delta\lambda(\theta)V_{t+1}^H \right\} \quad [Healthy]$$
$$V_t^S = \max_\theta \left\{ p - \theta - c(\theta) - \alpha + \delta\lambda(\theta)V_{t+1}^S \right\} \quad [Sick]$$

Because for all θ, $p - \theta - c(\theta)$, the flow payoff from covering type H, is greater than $p - \theta - c(\theta) - \alpha$, the flow payoffs of covering type S, it holds that $V_{t+1}^H > V_{t+1}^S$. While the first-best merely equates the marginal cost of θ (normalized to one) and its marginal benefits (i.e., $1 = -c'(\theta)$, so identical levels of care for both types), differentiating each of the above expressions with respect to θ yields the following first-order conditions for MMC plans:

$$1 = -c'(\theta) + \delta\lambda'(\theta)V_{t+1}^H \quad [Healthy]$$
$$1 = -c'(\theta) + \delta\lambda'(\theta)V_{t+1}^S \quad [Sick].$$

For healthy patients, plans equate the marginal cost of an additional unit of θ (one) against two marginal benefits: that increasing θ decreases outcome costs (i.e., $-c'(\theta)$) while increasing the probability that the plan will enjoy the expected future profit stream (i.e., $\delta\lambda'(\theta)V_{t+1}^H$). For sick patients, the incentives are the same, except that the continuation payoff $\delta\lambda'(\theta)V_{t+1}^S$ is smaller than that associated with a healthy patient, or perhaps negative. Either way, $V_{t+1}^H > V_{t+1}^S$ and $c'' > 0$ and $\lambda'' < 0$, so it must be that $\theta_H^{MMC*} > \theta_S^{MMC*}$.

Incentives under FFS. For simplicity, we model providers under FFS as being completely indifferent to outcome costs c_i; they merely send the bills back to the state. We assume that FFS providers get paid some reimbursement rate

ρ for θ, and their cost of effort (or opportunity cost) is $e(\theta)$, which is increasing convexly in θ. Thus, for each client, they provide some standard amount of care that satisfies $\rho = e'(\theta)$, and so $\theta_H^{FFS*} = \theta_S^{FFS*}$.

Predictions. The key result of the model is a divergence of health resources θ for healthy and sick groups under MMC relative to FFS. That is:

$$(\theta_H^{MMC*} - \theta_H^{FFS*}) > (\theta_S^{MMC*} - \theta_S^{FFS*}).$$

Assuming that health inputs have the expected effect on health outcomes, we predict the same divergence in outcomes after the switch from FFS to MMC—outcomes for healthy clients improve *relative* to those for sick clients.

Additionally, this model implicitly predicts that the effective price of child-bearing increases for high-cost groups, while decreasing for low-cost groups. As such, the switch to MMC can affect birth composition as the groups whose care diminishes under MMC may lower their fertility (either through lower conception rates or higher abortion rates) in response. Albanesi and Olivetti (2010) offer evidence that improved health care for pregnant women during the 1950s contributed to the Baby Boom.[45] Moreover, if the continuation probability λ is not very responsive to quality of care θ and thus mothers' inertia is high, then plans might differentially encourage birth control (which is covered under Medicaid) for high-cost mothers.

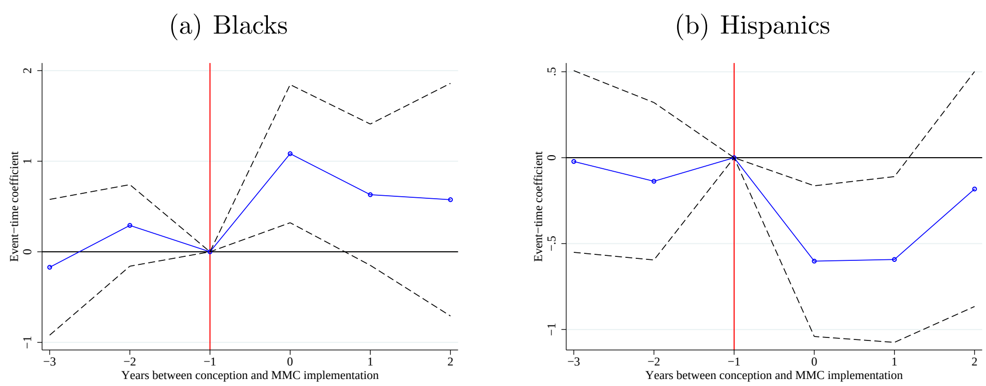
Extending the model to initial enrollment. The model abstracts from how individuals initially select their plans, and instead focuses on their decisions to continue in them. While beyond the scope of this paper, available evidence suggests that "word-of-mouth" from friends and family play a large role in plan selection, and thus λ in our model may be modified to include not only the probability a patient returns but also that she recommends the plan

[45] There is a small literature on whether Medicaid itself or similar programs that provide pre- or post-natal care are pro-natalist. As discussed by Lopoo and Raissian (2012), as Medicaid has generally provided both enhanced coverage for the costs related to child birth *as well as* access to birth control, it is hard to separate whether the enhanced coverage alone would be pro-natalist.

to others in her social network.[46] Given that race and ethnicity are excellent proxies of expected health costs in Texas, plans should aim to create a positive "word-of-mouth" among Hispanic clients (assuming that most individuals' friends are family are from their own race/ethnicity). As such, concerns about reputation may lead plans to also improve the care of individual high-cost members (e.g., high-risk Hispanic mothers) that come from low-cost groups.

B Appendix figures and tables

Appendix Figure 1: Changes in pre-term birth rates (×100) of children born to U.S.-born black and Hispanic mothers (note different scales)



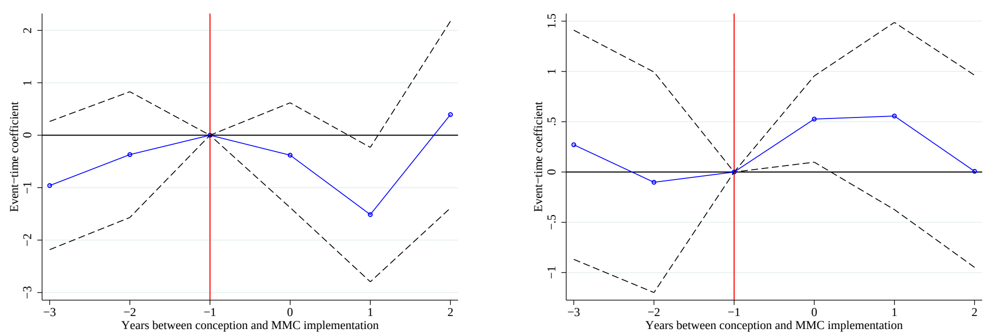
Notes: These figures show the results from estimating the effects on preterm birth rates for black (Figure a) and Hispanic (Figure b) births in the 3 years before and after MMC implementation. See the notes to Figure 2 for further details on the estimation procedure.

[46]Edgman-Levitan and Cleary (1996) document that seniors value word-of-mouth recommendations from friends and family more than they do aggregate "report card"-type ratings in choosing a managed care plan. Isaacs (1996) surveys adults of all ages and finds that family and friends' recommendations are weighed nearly the same as a *doctor's* recommendation in choosing a plan.

Appendix Figure 2: Changes in the male share of births ($\times 100$) born to U.S.-born black and Hispanic mothers (note different scales)

(a) Blacks

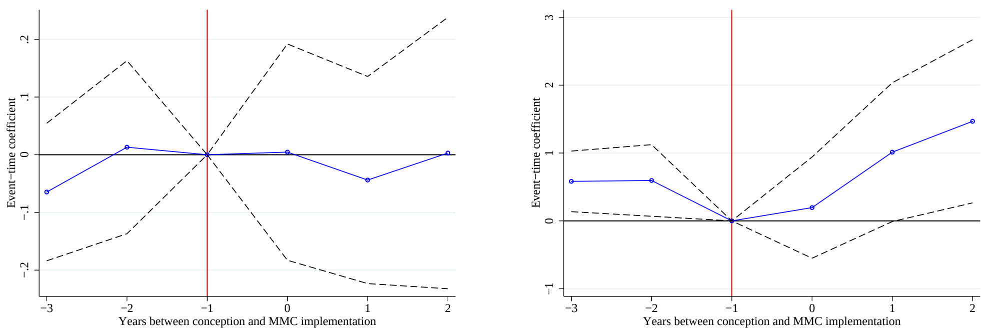
(b) Hispanics



Notes: These figures show the results from estimating the effects on share male births for black (Figure a) and Hispanic (Figure b) births in the 3 years before and after MMC implementation. See the notes to Figure 2 for further details on the estimation procedure.

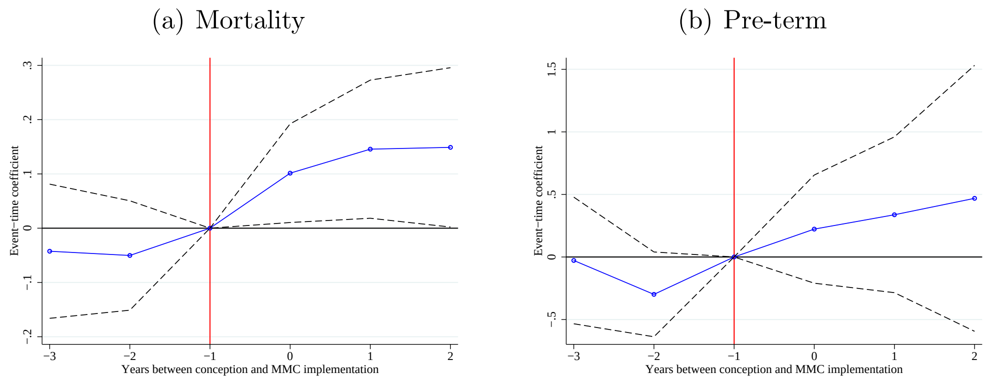
Appendix Figure 3: Changes in mortality rates and pre-term birth rates ($\times 100$) of children born to foreign-born Hispanic mothers (note different scales)

(a) Mortality (b) Pre-term



Notes: This figure shows the results from estimating the effects on the mortality rate (a) and pre-term rate (b) for children of foreign-born Hispanic mothers in the 3 years before and after MMC implementation. See the notes to Figure 2 for further details on the estimation procedure.

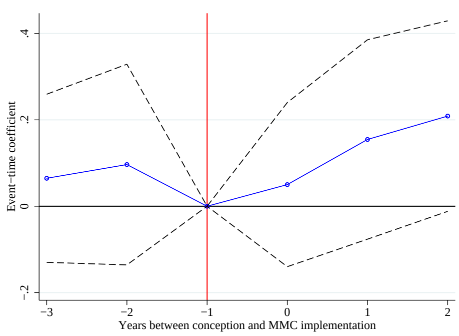
Appendix Figure 4: Changes in mortality rates and pre-term birth rates ($\times 100$) of children born to married non-Hispanic white mothers (note different scales)



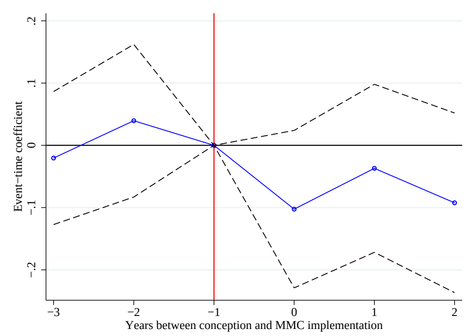
Notes: This figure shows the results from estimating the effects on the mortality rate (a) and pre-term rate (b) for children of married non-Hispanic white mothers in the 3 years before and after MMC implementation. See the notes to Figure 2 for further details on the estimation procedure.

Appendix Figure 5: Changes in mortality rates ($\times 100$) of children born to U.S.-born black and Hispanic mothers (note different scales), no county trends

(a) Blacks



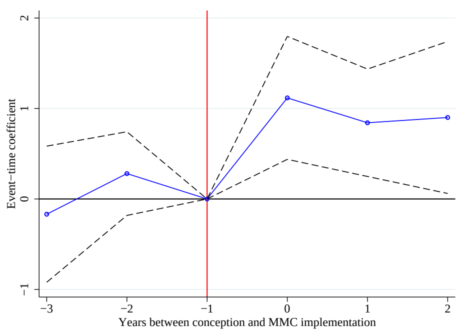
(b) Hispanics



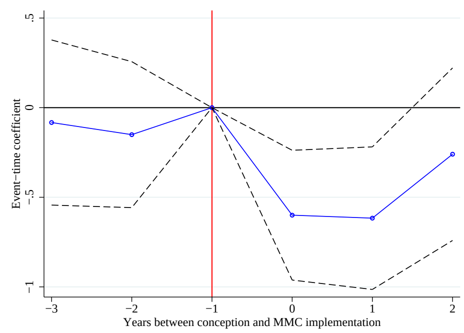
Notes: These figures show the results from estimating the effects on mortality rates for black (Figure a) and Hispanic (Figure b) births in the 3 years before and after MMC implementation. The event-study regressions underlying these graphs do *not* include county-specific trends. See the notes to Figure 2 for further details on the estimation procedure.

Appendix Figure 6: Changes in pre-term birth rates ($\times 100$) of children born to U.S.-born black and Hispanic mothers (note different scales), no county trends

(a) Blacks



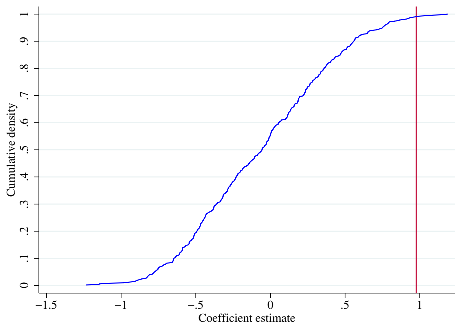
(b) Hispanics



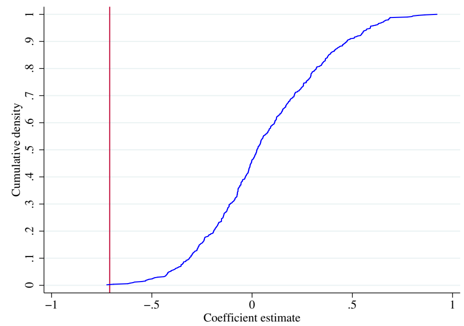
Notes: These figures show the results from estimating the effects on preterm birth rates for black (Figure a) and Hispanic (Figure b) births in the 3 years before and after MMC implementation. The event-study regressions underlying these graphs do *not* include county-specific trends. See the notes to Figure 2 for further details on the estimation procedure.

Appendix Figure 7: Permutation test: Cumulative density functions of coefficient estimates for share pre-term from 500 random draws of placebo treatment assignment

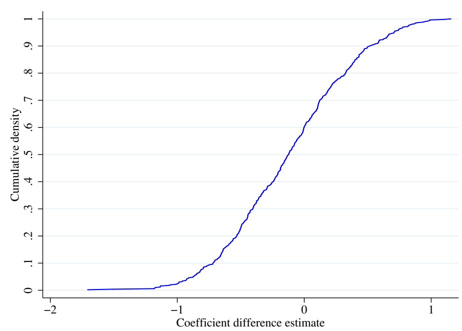
(a) Blacks



(b) Hispanics



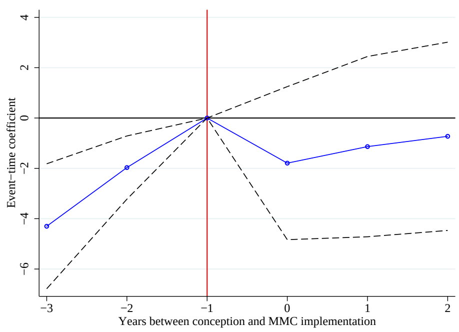
(c) Hispanics



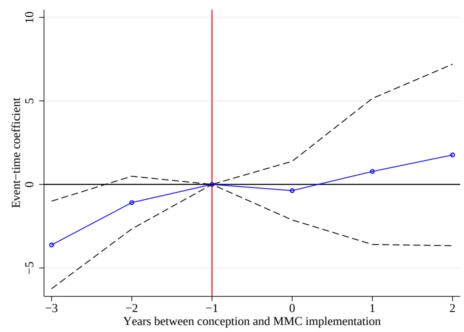
Notes: These figures show the cumulative density functions that come from a permutation test in which, in every iteration, each "switcher county" is randomly assigned an implementation date (we exclude the six months before and after the true implementation) instead of the true implementation date. The graphs show the cdfs generated by 500 draws. The red vertical lines show the location of the true coefficients for share pre-term for U.S.-born blacks (in 7(a)) and Hispanics (in 7(b)).

Appendix Figure 8: Changes in the share of mothers initiating pre-natal care in the 1st month of pregnancy (×100) among U.S.-born black and Hispanic mothers (note different scales)

(a) Blacks



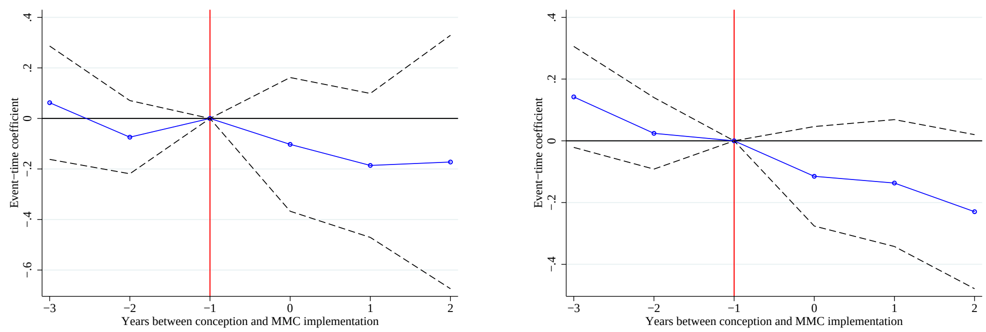
(b) Hispanics



Notes: These figures show the results from estimating the effects on the share of mothers initiating pre-natal care in the first month of pregnancy for black (Figure a) and Hispanic (Figure b) births in the 3 years before and after MMC implementation. See the notes to Figure 2 for further details on the estimation procedure.

Appendix Figure 9: Changes in the average number of pre-natal care visits among U.S.-born black and Hispanic mothers (note different scales)

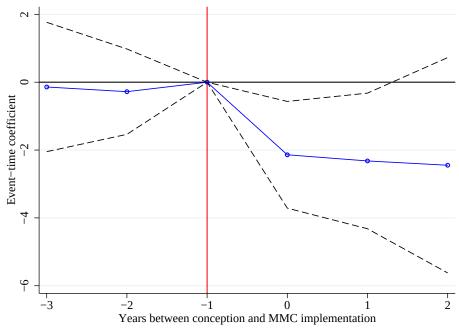
(a) Blacks (b) Hispanics



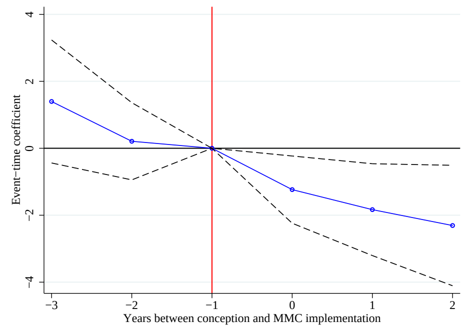
Notes: These figures show the results from estimating the effects on the average number of pre-natal care visits for black (Figure a) and Hispanic (Figure b) births in the 3 years before and after MMC implementation. See the notes to Figure 2 for further details on the estimation procedure.

Appendix Figure 10: Changes in the share of mothers receiving more than 7 pre-natal care visits (×100) among U.S.-born black and Hispanic mothers (note different scales)

(a) Blacks



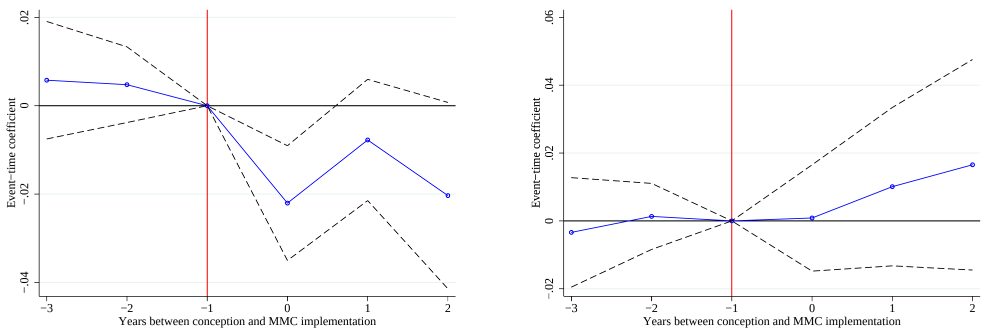
(b) Hispanics



Notes: These figures show the results from estimating the effects on the share of mothers receiving more than 7 pre-natal care visits for black (Figure a) and Hispanic (Figure b) births in the 3 years before and after MMC implementation. See the notes to Figure 2 for further details on the estimation procedure.

Appendix Figure 11: Changes in the share of mothers gaining at least 15 pounds during pregnancy (×100) among U.S.-born black and Hispanic mothers (note different scales)

(a) Blacks (b) Hispanics

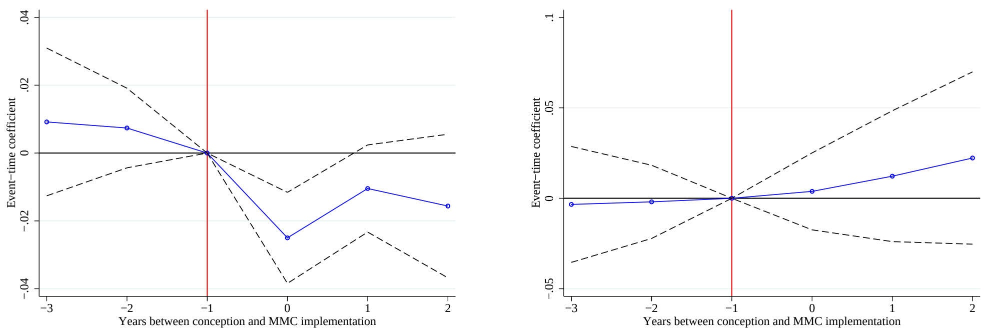


Notes: These figures show the results from estimating the effects on the share of mothers who gained at least 15 pounds during pregnancy for black (Figure a) and Hispanic (Figure b) births in the 3 years before and after MMC implementation. See the notes to Figure 2 for further details on the estimation procedure.

Appendix Figure 12: Changes in the share of mothers gaining at least 20 pounds during pregnancy ($\times 100$) among U.S.-born black and Hispanic mothers (note different scales)

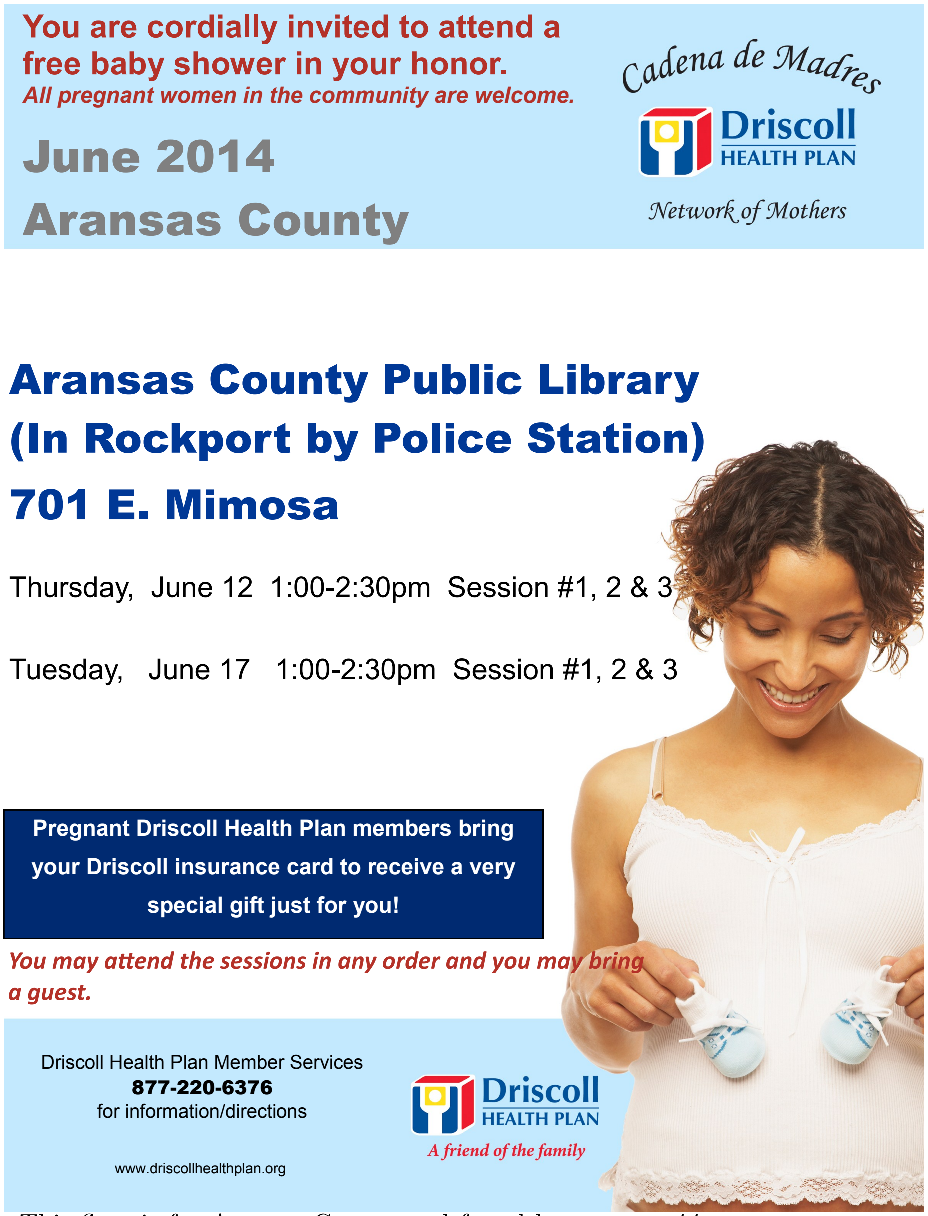
(a) Blacks

(b) Hispanics



Notes: These figures show the results from estimating the effects on the share of mothers who gained at least 20 pounds during pregnancy for black (Figure a) and Hispanic (Figure b) births in the 3 years before and after MMC implementation. See the notes to Figure 2 for further details on the estimation procedure.

Appendix Figure 13: Driscoll's "Cadena De Madres" Flyer



Notes: This flyer is for Aransas County and found here: `http://www.dchpkids.com/pdf/AransasInvite.pdf`. All flyers and other information on Cadena de Madres is found here: `http://www.dchpkids.com/services/?location=cadena_de_madres`. Note that the class is open to all pregnant women, but it is only free for those in Driscoll's MMC plan (called "Driscoll Health Plan"). Driscoll Health Plan members also receive a special gift for attending, as advertised.

Appendix Table 1: Roll Out Schedule for Texas MMC

Date	Counties
Aug 1993	Travis
Dec 1993	Chambers Jefferson Galveston
Dec 1995	Liberty, Hardin, Orange
Sep 1996	Burnet Williamson Lee Bastrop Fayette Caldwell
	Hays Lubbock Terry Lynn Garza Crosby Hockley Llano Hale
	Floyd Swisher Randall Deaf Smith Potter Hutshinson Carson
	Bexar Atascosa Wilson Guadalupe Comal Kendall Bandera
	Medina Tarrant Hood Parker Wise Denton Johnson
Dec 1997	Houston
Mar 1998	Harris Galveston Brazoria Matagorda Wharton Fort Bend
	Austin Waller Montgomery
Jan 1999	Dallas Ellis Navarro Kaufman Rockwall Hunt Collin El Paso
	Hudspeth
Jan 2006	Nueces Kenedy Brooks Kleberg Jim Wells San Patricio
	Live Oak Aransas Refugio Bee Goliad Victoria Karnes Calhoun

Notes: This information was obtained from Chapter 6 of the report available here: `www.hhsc.state.tx.us/medicaid/reports/PB8/PinkBookTOC.html`

Appendix Table 2: Is the MMC rollout correlated with underlying county trends?

	Log Pop.		Log Inc./Cap.		Log Transf./Cap.		Unemp. Rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
After MMC	0.0256*	0.00201	0.0300***	-0.00428	-0.0119**	-0.00215	0.00946*	0.000278
	[0.0133]	[0.00206]	[0.00883]	[0.00394]	[0.00524]	[0.00334]	[0.00560]	[0.00367]
Mean, dept. var	1.538	1.538	0.829	0.829	-1.287	-1.287	0.0613	0.0613
Reg. obs. (cells)	26021	26021	26021	26021	26021	26021	26021	26021
Underlying births	2814681	2814681	2814681	2814681	2814681	2814681	2814681	2814681
Time Trends	No	Yes	No	Yes	No	Yes	No	Yes

Notes: See notes to Table 3 for details about the data, sample and specification. Regressions include county and year×month fixed effects. County linear time trends are included only in even columns. We use county-year data on per capita income, per capita transfers and population from the Regional Economic Information System (REIS), and unemployment data from the Local Area Unemployment Statistics (LAUS) of the Bureau of Labor Statistics. We interpolate to create monthly measures to avoid sharp jumps at the end of calendar years.

Appendix Table 3: Hospital charges for newborns and deliveries (including hospital fixed effects)

	Newborn	Delivery		Newborn	
	(1)	(2)	(3)	(4)	(5)
Black	3026.1***	866.7***	882.0***		
	[117.3]	[15.72]	[15.72]		
Died				75399.9***	56471.4***
				[4598.4]	[1795.0]
Mean, dept. var.	5813.6	7107.5	7107.5	10085.4	6274.2
Mean, ex. group	5236.6	7002.9	7002.9	9621.5	6092.8
Pct. diff	0.578	0.124	0.126	7.837	9.269
Age cat. FE	No	No	Yes	No	No
Sample	Bl., H.	Bl., H.	Bl., H	B.	H.
Observations	816914	788637	788637	34782	148542

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:Q3). Col. (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 col. 1) and the excluded group (e.g., Hispanics, in col. 1). That is, "Pct. Diff" just divides the coefficient by the excluded-group mean. Cols. (1) through (3) include all blacks and Hispanics, col. (4) includes only blacks and col. (5) includes only Hispanics.

Appendix Table 4: Estimated Medicaid share of births in 2005

	(1) All	(2) U.S. Bl.	(3) U.S. Hisp	(4) For. Bl.	(5) For. Hsp.	(6) Wh.
Medicaid share	0.539	0.836	0.877	0.338	0.271	0.437
Medicaid share, married	0.360	0.471	0.692	0.245	0.265	0.269
Observations	273,471	26,615	69,146	2,647	64,610	100,526

Notes: Texas does not record Medicaid status on birth certificates until 2005. As we discuss in Section 3, these numbers appear substantially under-reported, likely due to women or providers who are on privatized Medicaid mistakenly reporting that the birth is covered by a private or "other" instead of Medicaid. For example, comparing conceptions from 2004-2005 to those in 2007-2008 in the counties that switched to MMC in 2006, the reported Medicaid share falls from 64.7 percent to 49.9 percent (it did not fall in other counties). This drop suggests that the true Medicaid share is roughly 1.3 times (64.7/49.9) the reported share in the post-period. Similarly, in 2005, the official count of Medicaid births from the Texas DHHS is 1.3 times the count in the birth certificate data. See `http://www.hhsc.state.tx.us/medicaid/reports/PB8/PDF/Chp-4.pdf`, p. 4-15. The official count indicates that 54 percent of births are covered by Medicaid, whereas our birth certificate data indicate 41 percent. We thus "gross up" the Medicaid share by 1.3 in this table

Appendix Table 5: Effect of MMC on mortality and birth outcomes ($\times$100) for U.S.-born vs. foreign-born black births

	Mort.		Pret.		LBW		Abn. BW		Male	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Nat.	For.	Nat.	For.	Nat.	For.	Nat.	For.	Nat.	For.
Conceived after MMC	0.179**	0.200	0.976***	-0.643	0.730*	-0.500	0.849**	0.679	-0.00762*	0.0152
	[0.0786]	[0.347]	[0.336]	[0.778]	[0.380]	[0.728]	[0.360]	[1.441]	[0.00428]	[0.0107]
Dept. var mean	1.198	1.355	13.51	11.38	12.72	9.831	17.25	19.20	0.509	0.505
Diff/p-val	-0.0203	0.894	1.619	0.000000521	1.231	0.00178	0.171	0.777	-0.0228	0.0000901
Reg. obs (cells)	12833	2387	12833	2387	12828	2381	12828	2381	12833	2387
Indiv. obs.	296589	21555	296589	21555	296584	21549	296584	21549	296589	21555

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

Appendix Table 6: Correlation between MMC and foreign-born black and Hispanic birth rates

	Foreign-Born Black		Foreign-Born Hisp.	
	(1) Share of births	(2) Log births	(3) Share of births	(4) Log births
Conceived after MMC	-0.000639* [0.000326]	-0.0559 [0.0334]	-0.00291 [0.00222]	-0.0313 [0.0240]
Mean, dept. var.	0.00766	3.826	0.219	6.030
Reg. obs. (cells)	26021	648	26021	6156
Indiv. obs.	2814681	18712	2814681	585721

Notes: See notes to Table 3 for details about the data, sample and specification. When logs are used in columns (2) and (4), counties are restricted to those with at least one birth to black foreign-born (in column 2) and to Hispanic foreign-born (in column 4) women (to avoid taking the log of zero and to have a consistent sample of counties. Columns (1) and (3) are weighted by the total number of births in each county/year/month, while columns (2) and (4) are weighted by the number of foreign-born black (foreign-born Hispanic) births in each county/year/month.

Appendix Table 7: Effect of MMC on birth outcomes ($\times 100$) for married white mothers

	(1) Mort.	(2) Pret	(3) LBW	(4) ABW	(5) Male	(6) Older
Conceived after MMC	0.0779* [0.0408]	0.367* [0.190]	0.0438 [0.120]	-0.0215 [0.179]	-0.234 [0.209]	0.109 [0.193]
Mean, dept. var	0.614	8.589	5.991	17.44	51.27	11.45
Reg. obs. (cells)	23898	23898	23894	23894	23898	23898
Underlying	922142	922142	922138	922138	922142	922142

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 birthweight) denotes birthweight $< 2,500$g or $> 4,000$g; "Pre-term" denotes gestation < 37 weeks. "Older" denotes the share of mothers 35 and above. "Male" refers to the sex of the infant.

Appendix Table 8: Effect of MMC on mortality rates ($\times$100), additional county$\times$year controls

	(1) Bl.	(2) Hsp.	(3) Bl.	(4) Hsp.	(5) Bl.	(6) Hsp.
Conceived after MMC	0.188***	-0.155*	0.164**	-0.162**	0.267**	-0.149*
	[0.0696]	[0.0832]	[0.0833]	[0.0708]	[0.120]	[0.0822]
Log Population			-3.560	-4.918**	-7.725	-5.216
			[5.041]	[2.298]	[6.626]	[3.625]
Log Income Per-Capita			3.710*	-1.459**	6.867***	-0.634
			[1.954]	[0.650]	[2.619]	[1.184]
Log Transfer Per Capita			-5.718**	1.603	-7.398**	0.187
			[2.834]	[1.448]	[3.178]	[2.545]
Unemployment Rate			3.034	-2.566*	1.870	-0.895
			[6.458]	[1.466]	[7.346]	[2.549]
Emp to Pop Rate			381.6	-317.3	611.4	-79.73
			[568.9]	[306.4]	[816.3]	[491.5]
Log Annual Cnty Profits			-0.640	0.182	-0.839	-0.128
			[1.031]	[0.512]	[1.452]	[0.808]
Log Total Cnty Estab's			2.330	0.851	0.972	-0.882
			[2.146]	[0.995]	[2.635]	[2.072]
Death Rate 65+			-6.037	3.651	-3.516	1.504
			[16.02]	[3.237]	[22.79]	[4.823]
Dept. var mean	1.197	0.714	1.197	0.714	1.260	0.821
Sample	All	All	All	All	Unmar.	Unmar.
Diff/p-val	0.343	0.00224	0.326	0.00183	0.416	0.00533
Reg. obs (cells)	12784	19665	12784	19665	11733	15920
Indiv. obs.	296537	644534	296537	644534	190863	249557

Notes: See notes under Table 3 for more details about the data, sample, and specifications. The additional controls include: the employment-to-population ratio, log annual firm profits, log total number of establishments, and the elderly death rate. The data on firm profits and the number of establishments comes from the County Business Patterns (CBP), while the data on the elderly death rate is from the National Center for Health Statistics (NCHS). For ease of interpretation of the coefficients, we re-scale 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; employment per population is divided by 100; annual profits are divided by 10,000; and establishments are divided by 100. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < .01$

Appendix Table 9: Effect of MMC on mortality rates (×100), different time trends

	(1) Bl.	(2) Hsp.	(3) Bl.	(4) Hsp.	(5) Bl.	(6) Hsp.
Conceived After MMC	0.0683	-0.0755	0.179**	-0.154**	0.183**	-0.146**
	[0.0739]	[0.0486]	[0.0786]	[0.0749]	[0.0804]	[0.0634]
Log Population	-1.036	-0.116	-3.140	-4.784**	-5.850	-6.058**
	[0.707]	[0.407]	[4.685]	[2.161]	[6.177]	[2.916]
Log Income Per-Capita	-0.996	-0.700	3.342*	-1.129*	3.584*	-1.342**
	[0.911]	[0.474]	[1.932]	[0.642]	[2.012]	[0.641]
Log Transfer Per Capita	-3.093**	-1.480**	-5.392*	1.582	-7.866**	1.116
	[1.297]	[0.663]	[2.750]	[1.394]	[3.329]	[1.786]
Unemployment Rate	0.791	1.219	1.851	-1.685	8.411	-0.739
	[5.142]	[0.868]	[5.957]	[1.489]	[8.267]	[1.409]
Dept. var mean	1.198	0.715	1.198	0.715	1.198	0.715
Sample	All	All	All	All	All	All
Diff/p-val	0.144	0.0371	0.333	0.00237	0.328	0.000977
Reg. obs (cells)	12833	20504	12833	20504	12833	20504
Indiv. obs.	296589	646053	296589	646053	296589	646053
County Trends	None	None	Linear	Linear	Quad.	Quad.

Notes: See notes under Table 3 for more details about the data, sample, and specifications. Columns (1) and (2) do not include time trends; columns (3) and (4) include county linear time trends (our baseline specification); columns (5) and (6) include county quadratic time trends. $^{*}p < 0.1, ^{**}p < 0.05, ^{***}p < .01$

Appendix Table 10: Changes in risk-factors (×100) after MMC for U.S.-born black and Hispanic mothers

	Older		Diab/Hyper		Smokes		Pred. Mort.	
	(1) Bl.	(2) Hsp.	(3) Bl.	(4) Hsp.	(5) Bl.	(6) Hsp.	(7) Bl.	(8) Hsp
Conceived after MMC	-0.460**	-0.298**	-0.0840	-0.237	-0.286	0.278	-0.00899***	-0.000697
	[0.179]	[0.140]	[0.193]	[0.217]	[0.303]	[0.202]	[0.00283]	[0.00135]
Dept. var mean	5.659	4.699	3.469	3.164	6.284	3.483	1.182	0.708
Diff/p-val	-0.162	0.421	0.153	0.530	-0.564	0.0287	-0.00829	0.00631
Reg. obs (cells)	12832	20504	12833	20504	12808	20489	12801	20471
Indiv. obs.	296588	646053	296589	646053	296563	646037	296556	646019

Notes: See notes under Table 3 for more details about the data, sample, and specifications. "Older" denotes the share of mothers age 35 and above. "Diab/Hyper." denotes the share of mothers with diabetes or hypertension. "Smokes" denotes the share of mothers who reported smoking during pregnancy. "Predicted Mortality" is the proportion of infant mortality predicted by the following pre-existing risk variables: maternal age, maternal education, and infant parity indicators, as well as all interactions between the maternal education and maternal age indicators and all interactions between infant parity and maternal age indicators.

Appendix Table 11: Effect of MMC on U.S.-born black birth outcomes (×100) after controlling for covariates

	(1) Mort.	(2) Pret.	(3) LBW	(4) ABW	(5) Male
Conceived after MMC	0.185**	1.004***	0.817**	0.924***	-0.759*
	[0.0773]	[0.327]	[0.376]	[0.350]	[0.418]
Dept. var mean	1.198	13.51	12.72	17.25	50.95
Indiv. obs.	296589	296589	296279	296279	296589

Notes: These regressions are based on individual-level Texas birth records data. The sample of analysis includes births that were conceived by mothers residing in Texas between January 1993 and December 2001. All regressions include the typical controls in the cell-aggregated regressions (county and year×month fixed effects, county linear time trends, and controls for log population, log per capita income, log per capita transfers, and the unemployment rate) as well as the following individual-level controls: indicators for married and first-parity child, age (in four-year bins) fixed effects, and educational attainment fixed effects (no high school education, high school education, some college and college graduate). Standard errors are clustered by county. "LBW" denotes birth weight $< 2,500$g; "ABW" (abnormal birthweight) denotes birthweight $< 2,500$g or $> 4,000$g; "Pret." (pre-term) denotes gestation < 37 weeks. "Male" refers to the sex of the infant. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < .01$

Appendix Table 12: Effect of MMC on U.S.-born Hispanic birth outcomes (×100) after controlling for covariates

	(1) Mort.	(2) Pret.	(3) LBW	(4) ABW	(5) Male
Conceived after MMC	-0.151**	-0.679***	0.0127	-0.129	0.782***
	[0.0744]	[0.190]	[0.156]	[0.349]	[0.244]
Dept. var mean	0.715	9.593	7.334	14.21	51.04
Indiv. obs.	646053	646053	645778	645778	646053

Notes: These regressions are based on individual-level Texas birth records data. The sample of analysis includes births that were conceived by mothers residing in Texas between January 1993 and December 2001. All regressions include the typical controls in the cell-aggregated regressions (county and year×month fixed effects, county linear time trends, and controls for log population, log per capita income, log per capita transfers, and the unemployment rate) as well as the following individual-level controls: indicators for married and first-parity child, age (in four-year bins) fixed effects, and educational attainment fixed effects (no high school education, high school education, some college and college graduate). Standard errors are clustered by county. "LBW" denotes birth weight $< 2,500$g; "ABW" (abnormal birthweight) denotes birthweight $< 2,500$g or $> 4,000$g; "Pret." (pre-term) denotes gestation < 37 weeks. "Male" refers to the sex of the infant. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < .01$

Appendix Table 13: Effect of MMC on mortality (×100) for U.S.-born black and Hispanic births, controlling for pre-term and low-birth-weight

	Baseline		Control Pret/LBW	
	(1) Bl.	(2) Hsp.	(3) Bl.	(4) Hsp.
Conceived after MMC	0.179**	-0.154**	0.0952	-0.139**
	[0.0770]	[0.0738]	[0.0603]	[0.0560]
Dept. var mean	1.198	0.715	1.139	0.697
Indiv. obs.	296589	646053	296279	645778

Notes: These regressions are based on individual-level Texas birth records data. The sample of analysis includes births that were conceived by mothers residing in Texas between January 1993 and December 2001. All regressions include the typical controls in the cell-aggregated regressions (county and year×month fixed effects, county linear time trends, and controls for log population, log per capita income, log per capita transfers, and the unemployment rate). Columns (3) and (4) also include a full set of dummy variables for birth weight in 250 gram bins and weeks of gestation, and the interactions between them. $^{*}p < 0.1, ^{**}p < 0.05, ^{***}p < .01$

Appendix Table 14: Changes in black share of elem. school enrollment after MMC

	(1) Share black	(2) Log bl. enrollment	(3) Log bl. enroll (w 0s)
After MMC	-0.000576	0.00399	0.00606
	[0.00125]	[0.0274]	[0.0272]
Mean, dept. var	0.148	8.199	8.161
Number county-year cells	1913	1773	1913

Notes: These data come from the National Center of Education Statistics' Public Elementary and Secondary School Universe Survey. The sample of analysis includes school enrollment data from all Texas counties except for the four pilot counties over 1993-1994 to 2000-2001. We define a school as elementary if its highest grade is 6th or below. We sum enrollment by race to county-year cells. All regressions are weighted by total enrollment in each cell. In the "Log bl. enroll (w 0s)" specifications, cells with 0 values are recoded to 1. All regressions include county and year fixed effects, county-specific linear time trends, and controls at the county-year level (unemp, log income per capita, log population per county, log transfers per capita). Standard errors are clustered by county.

Appendix Table 15: Determinants of contraception usage among U.S.-born black and Hispanic women

	(1)	(2)	(3)	(4)
Black	0.0791^{***}	0.0562		
	[0.0248]	[0.0370]		
Mom at least 35			0.0791^{**}	0.0791^{**}
			[0.0311]	[0.0311]
Mean, dept. var.	0.797	0.834	0.834	0.834
Regions	All	South only	All	South only
Observations	1316	651	651	651

Notes: Based on the 1995 National Survey of Family Growth. The sample is limited to U.S.-born black and Hispanic women under 185 percent of the federal poverty line. We exclude women who report being pregnant at the time of the survey or actively trying to get pregnant. The outcome variable is a summary term calculated by the NSFG coded as one if the respondent used birth control in the past twelve months, and zero otherwise.

Appendix Table 16: MMC Plans, Racial Composition, and Extra Services

Plan Name	Service Areas	Black Population, 2010 Census	Hispanic Population, 2010 Census	Black/Hispanic	Link to Value Added Services
Aetna	Bexar and Tarrant	480,774	1,795,499	0.2678	www.aetnabetterhealth.com/texas/members/medicaid/value-adds
Amerigroup	Bexar, Dallas, Harris, Jefferson, Lubbock, Tarrant	2,333,160	5,333,867	0.4374	www.myamerigroup.com/English/Documents/TXTX_Benefits_Overview_STAR_ENG.pdf
Blue Cross Blue Shield	Travis	133,827	555,274	0.2410	www.bcbstx.com/medicaid/star.html
Christus	Nueces	14,388	185,335	0.0776	www.christushealthplan.org/members/medicaid/value-adds
Community First	Bexar	140,382	1,136,611	0.1235	www.cfhp.com/Members/STAR/Medicaid-Mbr-Hndbk-0114.pdf
Community Health Choice	Harris, Jefferson	1,164,920	2,200,235	0.5295	www.chchealth.org/GetFile.aspx?FileId=171
Cook Children's	Tarrant	340,392	658,888	0.5166	www.cookchp.org/English/members/STAR-members/Pages/Programs-and-Benefits.aspx
Driscoll	Nueces, Hidalgo	19,987	1,633,050	0.0122	www.dchpkids.com/star/services.php
El Paso Premier	El Paso	27,419	670,604	0.0409	www.epfirst.com/PremierPlan.html
First Care	Lubbock	43,287	228,564	0.1894	www.firstcare.com/STAR_Medicaid
Molina	Dallas, El Paso, Harris, Hidalgo, Jefferson	1,842,117	5,428,123	0.3394	www.molinahealthcare.com/members/tx/en-US/mem/medicaid/star/PDF/TX_STAR_Member_Handbook.pdf
Parkland	Dallas	644,179	1,109,569	0.5806	www.parklandhmo.org/Handbooks/parkland%20english.pdf
Sendero	Travis	133,827	555,274	0.2410	www.senderohealth.com/en/members/value-adds
Seton	Travis	133,827	555,274	0.2410	www.setonhealthplan.com/members/chip/STAR_handbook-12-13%20edit.pdf
Superior Health	Bexar, El Paso, Hidalgo, Lubbock, Nueces, Travis	364,902	4,224,103	0.0864	www.superiorhealthplan.com/for-members/benefits-information/extra-benefits/
Texas Children	Harris, Jefferson	1,164,920	2,200,235	0.5295	www.texaschildrenshealthplan.org/for-members/star/
United Health	Harris, Hidalgo, Jefferson	1,170,519	3,647,950	0.3209	http://www.uhccommunityplan.com/content/dam/communityplan/plandocuments/handbook/en/TX-star-handbook.pdf

Notes: 2010 population counts by race and county are from the 2010 Census: `http://www.dshs.state.tx.us/chs/popdat/st2010c.shtm`. Counties served by each plan are listed here: `http://www.hhsc.state.tx.us/medicaid/managed-care/mmc/STAR-map.pdf`. Counties are grouped into service areas as follows: Bexar Service Area= Atascosa, Bandera, Bexar, Comal, and Guadalupe; Dallas SA = Collin, Dallas, Ellis, Hurt, Kaufman, Navarro, and Rockwall; El Paso SA = El Paso and Hudspeth; Harris SA = Austin, Brazoria, Fort Bend, Galveston, Harris, Matagorda, Montgomery, Waller and Wharton; Hidalgo SA = Cameron, Duval, Hidalgo, Jim Hogg, Maverick, McMullen, Starr, Webb, Willacy and Zapata; Jefferson SA = Chambers, Hardin, Jasper, Jefferson, Liberty, Newton, Orange, Polk, San Jacinto, Tyler, Walker; Lubbock SA = Carson, Crosby, Deaf Smith, Floyd, Garza, Hale, Hockley, Hutchinson, Lamb, Lubbock, Lynn, Potter, Randall, Swisher, Terry; Nueces SA = Aransas, Bee, Brooks, Calhoun, Goliad, Jim Wells, Karnes, Kenedy, Kleberg, Live Oak, Nueces, Refugio, San Patricio, Victoria; Tarrant SA = Denton, Hood, Johnson, Parker, Tarrant, Wise; Travis SA = Bastrop, Burnet, Caldwell, Fayette, Hays, Lee, Travis, Williamson.

Appendix Table 17: Black/Hispanic Ratio by Services Offered

Benefit	Plans offering Benefit	Average Bl/Hisp
Baby shower or other social event	Community First, Community Health Choice, Driscoll, Firstcare, Texas Children's, Superior, United	0.2559
Prenatal gifts	Aetna, BCBS, Christus, Driscoll, El Paso, FirstCare, Parkland, Texas Children's, Sendero, Seton, Superior	0.2279
Postnatal gifts	BCBS, Community First, Community Health, Driscoll, El Paso, FirstCare, Molina, Sendero, United	0.2264
Hosts prenatal classes	Amerigroup, Community First, Community Health, Driscoll, El Paso, Superior, Texas Children's	0.2513
In-home visits for high-risk members	BCBS, Christus, Seton, Texas Children's, United	0.2820

See notes to the previous table.

C Back-of-the-envelope calculations

C.1 Estimating the undocumented share of foreign-born Hispanic mothers in Texas

We calculate this share for the year 2000. According to the U.S. Census, there were $20,851,820$ residents in Texas in 2000.[47] According to the Pew Hispanic Center, there were 1.1 million undocumented immigrants in Texas in 2000.[48] Also according to Pew, 76 percent of undocumented immigrants nationwide are Hispanic, which is a vast underestimate for Texas, given its position on the U.S.-Mexican border.[49] As such, a lower bound for the number of undocumented Hispanics in Texas is $0.76 * 1,100,000 = 836,000$.

Using the 2000 IPUMS, we calculate that foreign-born Hispanics (regardless of their immigration status, which the Census does not record) account for 9.77 percent of the Texas population, or $0.0977 * 20,851,820 = 2,037,222$ people.

Finally, Pew notes that undocumented immigrants are 34 percent more likely to have children (the relevant group for our regression analysis) than are documented immigrants.[50] We thus gross up the estimated number of undocumented Hispanics in the first paragraph by 1.3.

Our final calculation of the share of Hispanic foreign-born mothers who are undocumented is thus $(1.3 * 836,000) \div 2,037,222 = 53.3$ percent. Again, because we assume that the Hispanic share of undocumented immigrants in Texas is equal to the national share, this calculation is a lower bound.

C.2 Back-of-the-envelope calculation on selection

Suppose that, despite the evidence in Appendix Table 10, the "missing" black infants would have been very healthy (perhaps on unobservable margins), which would bias us toward finding deleterious effects for black infants post MMC. We can calculate how much healthier than the pre-period baseline they would have to be for our effect to be fully explained by compositional changes.

In supplementary analyses, we estimated the effects of MMC on births by black women. Our (insignificant) estimate suggests that roughly 2.5 percent of black births may "disappear" post MMC. Call this value α. From Table

[47] See `http://www.census.gov/population/www/cen2000/maps/files/tab02.pdf`.

[48] See `http://www.pewhispanic.org/2011/02/01/appendix-a-additional-figures-and-tables/`.

[49] See `http://www.pewhispanic.org/2009/04/14/a-portrait-of-unauthorized-immigrants-in-the-united-states/`.

[50] See `http://www.pewhispanic.org/2009/04/14/a-portrait-of-unauthorized-immigrants-in-the-united-states/`.

3, we find that, among the 0.975 percent of births that "remain" observable post-MMC, the increase in mortality is 0.00179 percentage points. Call this value β^{obs}. How large a decrease from baseline would the missing 2.5 percent have had to exhibit for our effect to be completely explained by selection (call this value β^{unobs})?

$$(1-\alpha)\cdot\beta^{obs}+\alpha\cdot\beta^{unobs}=0\Rightarrow$$
$$\beta^{unobs}=\frac{-(1-\alpha)\cdot\beta^{obs}}{\alpha}\approx\frac{-0.975\cdot 0.00179}{0.025}\approx-.069.$$

But mean black mortality is only 0.012 (see Table 1), so in order for the "missing" births to be fully explaining our results, they would have had to exhibited a *negative* mortality rate, which is of course impossible.