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Early Life Health Conditions and Racial Gaps in Education

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Racial disparities in infant health conditions have persisted for decades. However, there is surprisingly limited evidence regarding the long-term consequences of these disparities. Using novel linked administrative data from Texas and the shift to Medicaid Managed Care (MMC), I show that MMC-driven declines in infant health worsened cognitive and noncognitive outcomes for Black children, while MMC-driven enhancements in infant health improved noncognitive outcomes and educational attainment for Hispanics. Effects concentrate in low-value added districts for either demographic, suggesting that the long run impacts of changes to early life health conditions are more pronounced in less effective schools for one's demographic.

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Early Life Health Conditions and Racial Gaps in Education

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Abstract

Racial disparities in infant health conditions have persisted for decades. However, there is surprisingly limited evidence regarding the long-term consequences of these disparities. Using novel linked administrative data from Texas and the shift to Medicaid Managed Care (MMC), I show that MMC-driven declines in infant health worsened cognitive and noncognitive outcomes for Black children, while MMC-driven enhancements in infant health improved noncognitive outcomes and educational attainment for Hispanics. Effects concentrate in low-value added districts for either demographic, suggesting that the long run impacts of changes to early life health conditions are more pronounced in less effective schools for one's demographic. *JEL Codes:* I14, I21, I24, I32, I38, J13, J15, J24

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1 Introduction

The link between improved early life conditions and upward mobility is well established (e.g. García et al., 2020). Early life interventions that increase access to health care, nutrition, and education are seen as a promising way to reduce gaps during adulthood, and indeed, extensive US public expenditures currently support vulnerable youth before the age of 5 (e.g. Almond, Currie, & Duque, 2018; Bailey, Hoynes, Rossin-Slater, & Walker, 2020). However, despite spending on such programs amounting to about $200 billion annually, significant inequalities throughout the lifecycle persist across high and low-income children, especially among children born into low-income Black families (Hoynes & Schanzenbach, 2018).[1]

Some of the most significant racial disparities in the United States, extensively discussed in research and policy debates, pertain to early life health conditions (e.g. Gunja et al., 2023; Kennedy-Moulton et al., 2022). Black infants experience markedly worse early life health outcomes, with an infant mortality rate nearly three times higher than that of infants from *all* other racial groups.[2] Despite the policy importance of these disparities, there is little causal evidence on how they affect the trajectories of Black children. The main difficulty in evaluating the long-run implications of these early life disparities is identifying sources of exogenous variation in health conditions at birth. Existing studies on the causal impacts of infant health mainly use twin fixed effects designs (e.g. D. Figlio, Guryan, Karbownik, & Roth, 2014) and historical events have been used to examine racial disparities in infant health and their impact on later gaps (e.g. Chay, Guryan, & Mazumder, 2009). However, twin comparisons don't fully explain how population-level racial health disparities contribute to later gaps, and while insightful it's uncertain if historical findings apply today due to medical advancements and changes in education. Additionally, assembling administrative data within the United States spanning birth to adulthood is challenging for recent cohorts (Almond et al., 2018).[3] This makes it difficult to link changes in early life health conditions to later outcomes, as well as pinpoint when deficits due to racial health disparities may arise or the potential role schools play in mitigation.

[1] Black students score roughly 1 standard deviation below white students throughout their schooling (Casselman, 2014; CEPA, 2014). Compared to white students, Black students are also less likely to graduate from college (39.9% for black students vs. 62.1% of white students). Moreover, for the past 50 years the average labor market earnings and wealth of White families has been 6 and 2 times that of Black families, respectively (Derenoncourt, Kim, Kuhn, & Schularick, 2022; Aliprantis, Carroll, & Young, 2019).

[2] Kennedy-Moulton et al. (2022) find that the infant health outcomes of Hispanics and Asians closely tracks those of White infants, and even the wealthiest Black families have worse infant health outcomes than the poorest families of *all* other racial groups.

[3] For instance, large-scale datasets, such as tax records, offer information on birthplace and long-term outcomes, but lack details from childhood. Conversely, administrative schooling datasets contain detailed information on cognitive and noncognitive development, but lack details about conditions at birth. Notable exceptions include D. Figlio et al. (2014) and Chyn et al. (2021) who match Florida and Rhode Island birth records to administrative schooling records, respectively. However, in Florida children cannot be followed into adulthood, and the sample sizes in Rhode Island are quite small.

This paper fills these gaps by examining the extent to which early life health disparities contribute to racial gaps throughout childhood into early adulthood. I advance the literature by focusing on a novel natural experiment in the Medicaid program – the primary source of healthcare coverage to low-income families in the US – which resulted in one of the most recent abrupt widening in infant health disparities to date. In particular, I leverage the county level rollout of Medicaid Managed Care (MMC) in Texas, which began in 1993. While all children were exposed to MMC, only Black infants experienced declines in their health (Kuziemko, Meckel, & Rossin-Slater, 2018). This allows me to isolate a decline in early life health conditions for Black children exclusively, and use other racial groups as comparisons. Furthermore, I construct a unique administrative dataset that links precise birth details with a comprehensive set of cognitive and noncognitive outcomes from childhood to early adulthood in Texas. In combination with the MMC rollout, I use this detailed administrative data to provide the first causal estimates of how *current* racial health disparities affect the development of Black children and the potential role of schools in mitigating these disadvantages.

The introduction of MMC in Texas provides an ideal opportunity to investigate the long-term implications of racial disparities in infant health. The shift from traditional Medicaid fee-for-service (FFS) to MMC lead to one of the most substantial and plausibly exogenous shifts in infant health racial gaps for a recent cohort of children to date. Specifically, MMC implemented cost containment measures, prompting most healthcare providers to transition from receiving payments for individual services to capitation payments, or *fixed* payments per enrollee per year. While large racial gaps in infant health translate to vastly different costs across Black and Hispanic infants, MMC reimbursement rates do not vary across racial groups. After the switch from FFS to MMC, the health gaps between Black and Hispanic infants increased by 69%, with Black infant health worsening and Hispanic infant health slightly improving (Kuziemko et al., 2018). The authors show that these results cannot be driven by compositional changes and are instead consistent with a risk selection model where under MMC providers offer better care to low-cost clients to retain them for future births and worse care to high-cost clients to deter them from future births.[4] Together with this abrupt shock to racial infant health gaps, I exploit the staggered county-level rollout of MMC between 1993 and 2006 for my identification approach. Specifically, I utilize difference-in-differences and event-study models that take advantage of the differential timing of MMC rollout across counties.

To be able to focus on the impacts of racial infant health disparities during key developmental

[4]See Section 4.1 for a more detailed discussion of this model as a plausible explanation for the widening infant health gaps.

years, I construct a novel dataset linking the Texas Birth Index with restricted access administrative schooling data from Texas. These records provide precise birth details, enabling the connection of early life health conditions to a comprehensive set of cognitive and noncognitive outcomes from childhood to early adulthood. This allows me the opportunity to take a close look at how racial infant health gaps manifest throughout childhood and interact with other school-based interventions. Notably, only a few studies have successfully linked birth records to schooling records within the US. Additionally, these existing studies typically focus on either childhood or early adult outcomes, not both. Furthermore, Texas is a large and diverse state, allowing for extensive heterogeneity analyses that will enable me to explore how the changes in infant health interact with measures of school quality, serving as an informal test of the dynamic complementarities hypothesis. This is crucial because, in addition to the limited knowledge on how infant racial health gaps affect later outcomes, even less is known about whether school investments can mitigate early life health disparities.

My results show that prenatal MMC exposure significantly reduced test scores for low-income Black students. Across grades 3 through 8 reading and math test scores declined by about 3% of a standard deviation. During high school, I find that absences, grade repetition, and suspensions increased, suggesting declines in noncognitive skills. Given the Black-White test score gap is about 0.5 of a standard deviation, this suggests that MMC increased achievement gaps by roughly 6%.[5] These changes in the achievement gap are similar to those driven by segregation, school resources, and home resources (Krueger & Whitmore, 2001; Card & Rothstein, 2007; Todd & Wolpin, 2007). In contrast, prenatal MMC exposure did not affect the short-run cognitive development of Hispanic students overall, but it improved their longer-run educational attainment and noncognitive skills. This suggests that the improved infant health among Hispanics improved skills that cannot be captured by test scores, but are nonetheless important for long-run success. Reassuringly, there is little to no effect of prenatal MMC exposure on placebo groups such as those with higher-income or likely foreign-born parents who do not qualify for Medicaid, supporting a causal interpretation. In addition, I come to similar conclusions using an alternative estimator proposed by Gardner (2021) that accounts for biases sometimes present in two-way fixed effects models.

Having identified the downstream impacts of widening racial infant health gaps, I expand the analysis to explore whether later educational investments can mitigate these early-life disadvantages. Specifically,

[5]This is calculated based on the results presented in Table 3. The Black-White math score gap is $-0.433(=-0.557-(-0.124))$, and the MMC transition (statistically significantly) decreased math scores only for Black children by -0.026. Therefore, the math achievement gaps widened by roughly $6\%(=0.026/0.433)$.

I test whether the impacts of prenatal MMC exposure differ based on measures of school district quality constructed using administrative schooling data. Consistent with prior literature, I find that the impacts of infant health are invariant to measures of *overall* school district quality (D. Figlio et al., 2014). However, looking at *overall* measures mask differences across districts with different effectiveness for Black and Hispanic children. For instance, Black children prenatally exposed to MMC experience the largest test score declines in districts that have low value-added for Black children, and while I found prenatal MMC exposure did not impact Hispanic test scores overall, there are positive test score gains in school districts with low value added for Hispanic children. This suggests that when schooling resources are targeted, they can help mitigate the effects of poor infant health on cognitive development or, conversely, mute any positive gains that improved infant health may generate. I also find sizable classroom spillovers as a result of MMC. I find that a 5% increase in Black Medicaid eligible peers decreases test scores by about 0.3-3.8% of a standard deviation. The magnitude of these effects are in line with prior literature but slightly smaller than other studies that find a 5% increase in disruptive peers and grade-repeating peers decreases test scores by 3% and 6% of a standard deviation, respectively (Carrell & Hoekstra, 2010; Abramitzky & Lavy, 2014).

The most likely explanation for the persistent impacts of prenatal MMC exposure is the widening of infant health gaps documented by Kuziemko et al. (2018). I provide evidence that my findings are not attributable to compositional changes, and through a bounding exercise, I am able to rule out that these results are mechanically driven by direct mortality effects. It is also unlikely that my results are driven by changes in childhood health or other Medicaid related changes during childhood, as a model that includes prenatal and childhood exposure separately reveals that my results are driven by the prenatal period.[6] Nonetheless, it is still possible that abrupt changes in early life health could have had downstream impacts. If, for example, parents had to take time off from work to attend more doctor visits, this could have reduced financial stability and increased stress. In addition, providers may have been less able to offer important parenting advice to Black families due to declines in their health and time pressures related to addressing more urgent health issues. Therefore, my results combine the effects of declines in early life health, along with any associated downstream impacts resulting from changes in early life health conditions.

[6]Since Medicaid eligibility for my main analysis sample was constant over this time period the changes on test scores, it is unlikely that my results can be explained by changes in health insurance eligibility. As will be discussed in more detail in Section 3 my main results focus on a high-impact sample of children growing up in families whose incomes fall below 130% of the federal poverty line at school entry. During this time period, these children would have qualified for Medicaid at birth and through age 5.

This paper extends the current literature in several important ways. First, my paper provides the first evidence, to my knowledge, on the impact of a sudden shift in health investments on the perpetuation of existing racial gaps in educational outcomes for a recent cohort of children. Early life health has been shown to have a strong influence on long-run success (e.g. Royer, 2009; Bharadwaj et al., 2018; Black et al., 2007). At the same time, Black infants have worse health than *all* other racial groups, and are 60 percent more likely to be born pre-term and twice as likely to be born at a low birth weight (of Medicine, 2002). However, despite a robust literature establishing both the importance of infant health and the persistence of racial gaps in infant health outcomes, causally identifying the extent to which racial gaps in early life health conditions contribute to gaps on later life outcomes is challenging. Most approaches used to identify the importance of infant health focus on twin comparisons, and compare the outcomes of the heavier twin to the lighter twin. However, samples of twins are quite small, and do not typically lend themselves to explorations of differences by race due to less statistical power.[7] Other studies that have relied on historical events, such as the integration of hospitals in the South, find that increased health investments for Black children during the first three years of life played an important role in reducing later racial gaps (Chay et al., 2009). While insightful, with large differences in medical and schooling contexts, it is unclear whether we should expect similar results for more recent cohorts.

Second, this paper contributes new evidence on the medium-run impacts of early life health conditions. This is critical for better understanding why changes to early life health have been shown to have such a profound impact on labor market outcomes (e.g. Royer, 2009; Black et al., 2007). While the literature on the impacts of early life health conditions on adult outcomes is robust, the literature on childhood outcomes is relatively sparse. I am only aware of a handful of studies that have used large scale administrative data to trace out the impacts of early life health conditions on a rich set of childhood outcomes (e.g. D. Figlio et al., 2014; Ballis & Page, 2020; Bharadwaj et al., 2018; Chyn et al., 2021). This project contributes new evidence on the medium-run impacts of early life health conditions using detailed administrative data from Texas that allows me to trace out the impacts of an early life health shock *both* throughout childhood and on educational attainment during early adulthood for a large sample of children. This provides a unique opportunity to better understand the pathways by which early life health conditions affect adult outcomes.

[7] D. Figlio et al. (2014) use a twin fixed effects strategy to investigate the heterogeneous impacts of lower birth weight for Black children overall. Their sample size includes approximately 7,000 Black children. My sample of Black likely Medicaid eligible children is approximately 167,000 which is 24 times the size of D. Figlio et al. (2014)'s sample affording me much more statistical power to investigate differences across gender and measures of school quality.

Finally, to my knowledge, this is the first quasi-experimental study to extend beyond the health impacts of MMC at birth, to investigate impacts on childhood and early adulthood outcomes. A number of studies document the impact of MMC on infant health and health utilization (e.g. Kuziemko et al., 2018; Aizer et al., 2007). In this paper, I examine the later effects of prenatal MMC on key schooling outcomes for a large representative sample. MMC's introduction significantly changed Medicaid, with states implementing MMC in various ways.[8] My results reveal that MMC had meaningful effects on non-health related outcomes, and can help improve cost benefit calculations that are critical for effective MMC program design, particularly in relation to how the design and delivery of Medicaid may impact racial disparities throughout life.

2 Background

Medicaid currently finances roughly half of all births in the US, at a cost of $89 billion dollars annually (Hoynes & Schanzenbach, 2018). In the early years of Medicaid, only very low-income single mothers qualifying for cash assistance (i.e. the Aid to Families with Dependent Children program) and falling 61 percent below the federal poverty line (FPL) qualified for Medicaid. However, since the 1980s several federal laws have been passed to increase income eligibility thresholds and break the link between cash assistance and eligibility. Currently, most states cover pregnant women and infants with incomes under 185 percent of the FPL. In 1993, pregnant women and children less than 5 under 185 percent of the FPL qualified for Medicaid in Texas, and children could continue to qualify up until age 19 if their family income fell below 133 of the FPL. These income eligibility thresholds have remained fairly constant since then, with only small increases. In 2022, pregnant women and infants in Texas who fall below 198 percent below the FPL, along with children aged 1-5 and 6-19 who fall below 144 and 133 percent of the FPL, respectively, qualify for Medicaid.[9] In Texas, most noncitizens do not qualify for Medicaid and did not at the time of MMC's rollout (Texas Health and Human Services Commission, 2022).

Since the early 1990s, there has been a shift from traditional Medicaid FFS to MMC. Nationally, between 1989 and 2010, the fraction of Medicaid caseloads in MMC rose from 6.8% to 70% (Currie & Fahr, 2005). Texas saw a similar increase, where the fraction of Medicaid caseloads under MMC rose from 2.9% in

[8]For instance, one way that the implementation of MMC could differ is how states dealt with higher-cost patients. For example, MMC plans in California can pass on additional costs of higher cost patients back to the state, but they can't in Texas.

[9]A related program, the Children's Health Insurance Program (CHIP) was signed into Texas law in 1997 and implemented in 1999. CHIP provides healthcare to children in families earning too much to qualify for Medicaid. In 1999, children (ages 5-19) in families earning 150% of the FPL could qualify for CHIP. In 2021, those in families earning 201% of the FPL could qualify.

1994 to 79.2% in 2012 (Texas Health and Human Services Commission, 2022). Unlike traditional FFS where healthcare providers act independently and receive payments for each service without oversight on utilization, MMC places more emphasis on primary care and encourages coordination among providers. Additionally, MMC introduces incentives to reduce healthcare utilization, employing either cost targets or capitation payments (i.e., a fixed payment per enrollee per year) to managed care organizations (MCOs) tasked with coordinating and delivering care. A-priori, the impacts of MMC on health outcomes are ambiguous. On the one hand, MMC could improve health outcomes if care is better coordinated and there are less repeated procedures. On the other hand, MMC could worsen health outcomes if capitation payments or cost targets dissuade providers from providing expensive (but needed) treatments. High-cost groups are particularly at-risk of this sort of skimping on care, as their yearly medical expenses are likely to exceed the fixed payments or cost targets MMC imposes.

Starting in 1995, Texas began a staggered county by county rollout from Medicaid FFS to MMC. Once a county transitioned, MMC participation was mandatory. The order in which the counties switched was set by the Texas Health and Human Services Commission (HHSC) and counties could not negotiate the timing of their transition. Appendix Table A.1 provides details on the timing of the transition for each county.[10] Importantly, as will be discussed in more detail in Section 4.2 earlier transitions were *not* correlated with levels or trends in pre-MMC county-level achievement. Therefore, in terms of academic outcomes (my main outcome of interest), the timing of the rollout can be considered as good as random. While most 1993 county characteristics also fail to predict the MMC rollout, counties with larger populations, higher income, and more Hispanics transitioned to MMC slightly earlier (Appendix Table A.2). This is consistent with HHSC's decision to select small urban counties first, as they already had well-established healthcare provider networks and were also small enough if unanticipated complications arose with the transition to MMC. Larger urban counties were selected next, followed by rural counties in 2012 and the rest of the state in 2015. In all specifications, I include economic and demographic controls by county $\times$ year $\times$ month of birth to account for these differences.

[10] At the time the Kuziemko et al. (2018) paper was written, the authors only had access to an incomplete MMC rollout schedule and filled in missing information through correspondence with staff at DSHS. Since then, the state has published a Medicaid reference guide (referred to as the version of the "Pinkbook") that includes the complete MMC rollout dates. I use the MMC rollout dates published in the 8th edition of the Pinkbook which differ in small ways from the dates used in Kuziemko et al. (2018). Importantly, there is very little difference if I instead use the dates used in Kuziemko et al. (2018). Appendix B provides a more detailed discussion of these differences and demonstrates that the main results are unchanged regardless of which version of the rollout dates are used.

After the transition to MMC, the most common form of managed care offered was through Health Maintenance Organizations (HMOs), organizations that receive capitation payment for each of their enrollees.[11] If costs exceed these capitation payments plans suffer a loss, which introduces strong incentives for HMOs to control costs. In some areas of the state, enhanced Primary Care Case Management (PCCM) was offered alongside HMOs. Similar to HMOs, the enhanced PCCM model places an emphasis on primary care and introduces incentives to reduce health care expenditures. While clients are assigned to a primary care provider who is responsible for coordinating their care, this model of managed care is less restrictive as providers can continue to receive FFS reimbursements. However, in Texas, the state establishes medical cost targets that match the capitation rates HMOs receive. If costs are below the HMO rates, the network that oversees the PCCM receives an incentive payment. Hence, the PCCM model still provides incentives to manage utilization and costs (Texas Health and Human Services Commission, 2001).

Utilizing data from HMO-reported information and other claims data, HHSC found that utilization and costs declined after MMC's introduction (Texas Health and Human Services Commission, 2001). Appendix Table A.3 is taken from this report and demonstrates that emergency room (ER) visits and the average length of hospital stays declined after MMC, with similar declines for the HMO and PCCM model. They also found that MMC's introduction led to significant declines in spending. HMOs generated roughly 60 million dollars in cost savings per year, while the PCCM model generated 27 million per year in cost savings. Per beneficiary per month, HMOs saved the state 10 dollars per month and the PCCM model generated 4 dollars per month in savings (Texas Health and Human Services Commission, 2001).

3 Data

I make use of administrative data for all children conceived in Texas between 1993 and 2001 and educated in Texas public schools. Specifically, I start with the Texas Birth Index (TBI) which records the universe of Texas births, including exact date and county of birth, which I use to determine prenatal MMC exposure. I approximate conception by taking each child's birthday and subtracting 9 months. This data is then linked to restricted-access administrative data from the Texas Schools Project (TSP) that follows the universe of public school students throughout childhood into adulthood, tracking key educational and economic outcomes.[12]

[11] According to Verdier and Young (2000) and Table 6.3 of the 8th Edition of "Texas Medicaid and CHIP in Perspective" 65% and 69% of births under MMC were served through HMO organizations as of 1998 and 2005, respectively.

[12] The TBI and TSP data (including the K-12, college and labor market outcomes) were merged together by the Texas Education Agency (TEA) based on: full name (including first name, middle name, and last name) and exact date of birth. Reassuringly, full

Specifically, I observe standardized exam scores, attendance, disciplinary incidences, special education (SpEd) participation yearly throughout childhood. Into adulthood, I observe high school completion, college enrollment and college completion. Although the data would also allow me to track labor market outcomes, there are an insufficient number of birth cohorts in my sample that can be tracked for at least 10 years after expected high school graduation, as shown in Appendix Table A.4. Taken together, these merged data make it possible to trace out the impacts of prenatal MMC exposure on a rich set of childhood and early adult outcomes.

While these data are rich in tracking a wide range of outcomes throughout a critical period of development, it lacks the infant health measures needed to replicate the results of Kuziemko et al. (2018) for my sample. While both papers start with children conceived in Texas between 1993 and 2001, my results focus on the 80% who are subsequently educated in Texas public schools.[13] Discrepancies in our samples arise from outmigration or children being educated outside public schools. Reassuringly, outmigration from Texas is very low; most people born in Texas remain in the state (Aisch, Gebeloff, & Quealy, 2014), and only 1.7 percent of Texas residents leave the state each year (White et al., 2016). Additionally, those enrolled in private school or who are homeschooled tend to be more advantaged, and less likely to qualify for Medicaid.

To further ensure the consistency of our samples, I make similar restrictions to those in Kuziemko et al. (2018). First, I limit my sample to children in counties that had at least two births a month and where at least 10% qualified for Medicaid. Second, I drop children in pilot counties that transitioned to MMC in 1993. Unlike the other counties, the exact date of the transition in pilot counties is ambiguous, and the transition was less abrupt. Finally, by limiting my birth cohorts to those before 2001, I only leverage the MMC rollout before 2006.[14] Counties transitioning in 2006, situated near the Louisiana border, experienced a substantial influx of Black refugees following Hurricane Katrina in September 2005, potentially altering the composition of births during this period.[15] After these restrictions I'm left with 2,222,896 observations, very similar to the 2,814,681 observations in Kuziemko et al. (2018). As will be further discussed in Appendix B, this close

name and birthdate uniquely identify 99.99% of all births in Texas between 1993 and 2001, making false matches unlikely.

[13]Importantly, Appendix Table A.8 demonstrates that the MMC rollout is uncorrelated with the fraction of children who are matched to the administrative schooling records, ruling out the possibility of differential attrition between birth and school enrollment.

[14]The schooling data only includes children who were born pre-policy in counties that transitioned to MMC in 2012 or 2015. For this reason, children born in these counties are included in the sample as controls, as they help estimate the time trends across cohorts.

[15]It is important to note, that while my sample will not be affected by compositional changes introduced by Katrina (since I only focus on births before 2001 prior to Hurricane Katrina) children living and attending schools after Katrina could have been affected by classroom spillovers. In Section 5.4 I show that my results are very similar if I drop the counties most directly affected by Katrina.

similarity in our samples help to ensure that Kuziemko et al. (2018) provides a reliable first stage estimate for this project.

Given the different impacts the MMC transition had across racial groups, I follow Kuziemko et al. (2018) and examine the impacts by race separately. In addition, I further examine the results for those who are likely Medicaid eligible by restricting the sample to children growing up in low-income families with likely US-born mothers in order to match Medicaid's eligibility requirements during this period. First, I limit the sample to those who received Free Lunch (FL) at school entry, whose family income fell below 130% of the poverty line and would have qualified for Medicaid at birth and through age 5.[16] As will be discussed in more detail in Section 4.2.1 using ACS data I show that nearly all children who qualify for FL in school receive Medicaid in Texas. Since I do not observe income at birth, an underlying assumption is that family income at school entry was similar at birth.[17] In addition, I further limit the sample to those who were unlikely to have immigrant parents, since undocumented and non-citizen parents do not (and did not at the time) qualify for Medicaid in Texas. While I do not observe parents' country of birth, I focus on students who do not enter public school as an English Language Learner (ELL) since they are less likely to have foreign-born parents. Hereafter, this sample is referred to as the likely Medicaid eligible sample.

3.1 Summary Statistics

Table 1 presents summary statistics for the population of Texas-born public school students overall and across racial groups, as well as for students who are likely Medicaid eligible across racial groups. About half of children in Texas are receiving Free or Reduced Price Lunch (FRL) and half are Hispanic. Likely Medicaid eligible children have lower achievement and educational attainment relative to the overall population.

Appendix Table A.5 presents some evidence regarding the overall representativeness of the populations of children in the TBI that matched to the public schooling records. Overall, 76% of public school students were matched to the TBI.[18] Reassuringly, the sample of children born in Texas who enroll in Texas

[16] During this period, pregnant women and children less than one qualified for Medicaid if their family incomes fall below 185% of the FPL. To continue to qualify for Medicaid through age 5 or age 18, family income must fall below 133% and 100% of the FPL, respectively. Those with family incomes falling between 130% and 185% of the FPL qualify for reduced price lunch. The results are very similar if I include those who qualify for reduced lunch in the likely Medicaid sample. However, focusing on those who qualify for FL represent a higher-impact group who likely qualified for Medicaid at birth and throughout early childhood, and were less likely to experience subsequent changes to childhood health insurance eligibility associated with the introduction of CHIP.

[17] While I am unable to observe the extent to which FL eligibility changes from birth to school entry, 86% of children receiving FL at school entry still do 5 years later. This suggests that over 5 years, FL eligibility is constant for the vast majority of students.

[18] Similarly, in Florida, 80% of those born in Florida enrolled in public school (D. Figlio et al., 2014).

public schools (shown in Column 2) is very similar to the overall Texas student population based on race, FRL status, and average achievement (shown in Column 1). The only difference is that those not born in Texas are more likely to be foreign-born and Hispanic, which is expected among this subgroup.

4 Empirical Strategy

4.1 Background

Kuziemko et al. (2018) examine the impact of MMC on infant health by estimating a difference-in-difference model that leverages the county-by-county rollout of MMC between 1993 and 2006. The basic idea behind their approach is that counties not yet transitioned to MMC (or never transitioned within the analysis period) can serve as a useful counterfactual for counties transitioned to MMC, accounting for fixed differences across counties and common time effects. Specifically, they estimate the following model:

$$Y_{iymc} = \zeta * MMC_{ymc} + \Lambda' Z_{ymc} + \alpha_c + \gamma_{ym} + \mu_c \times f(t) + \varepsilon_{iymc} \tag{1}$$

where Y_{iymc} is a measure of infant health for individual i born in year y and month m in county c. MMC_{ymc} is an indicator variable that is equal to 1 if an individual was conceived after MMC was implemented in their county of birth. α_c and γ_{ym} represent county and year $\times$ month of birth fixed effects, respectively. Z_{ymc} accounts for county $\times$ year $\times$ month controls for log population, log per capita income, log per capita transfers and unemployment.

Finally, their preferred specification includes county-specific time trends. As Column 1 of Appendix Table A.6 illustrates, their absence reveals correlations between certain county-level characteristics and the MMC rollout. However, after their inclusion shown in Column 3, except for one, all county characteristics are uncorrelated with the implementation of MMC.[19] Although their model already controls for county $\times$ year $\times$ month of birth characteristics, the inclusion of county-specific time trends help to further assuage concerns that their estimates are being driven by *unobservable* differences across counties. As will be discussed in more detail in Section 4.2, I use a slightly different method for accounting for potential differences in county-level pre-policy trajectories. Specifically, I use a de-trending procedure proposed by Goodman-Bacon

[19]Small declines in the likelihood of being Black are observed across all specifications. When accounting for pre-policy trends, the magnitude of the effect is relatively small and corresponds to a 0.4 p.p. decline in the likelihood of being Black. Reassuringly, as will be discussed in Section 4.2, despite changes in the *relative* share of Black children their underlying characteristics remained similar.

(2021).[20] Similar to when county-specific time trends are included, Column 2 demonstrates that using a de-trending procedure in most cases eliminates the association between county-level characteristics and the MMC rollout.

Using this model, Kuziemko et al. (2018) find that Black infant mortality increased by 15% and Hispanic infant mortality fell by 22% among likely Medicaid eligible infants.[21] For likely Medicaid eligible Black infants the incidence of being pre-term and low-birth weight (i.e. less than 2,500 grams) increased by 7% and 6%, respectively. For likely Medicaid eligible Hispanic infants, the incidence of pre-term births declined by 7%, without a change in the likelihood of being low birth-weight. Appendix Table A.7 presents the main results from Kuziemko et al. (2018). While their main results emphasize the widening of the Black-Hispanic infant health gaps, they do also find that infant health among White likely Medicaid eligible infants declined. However, the magnitude of the changes for White infants are much smaller than those observed for Black and Hispanic infants. This aligns with previous studies from California, which have demonstrated a tendency for infant health to deteriorate after the implementation of MMC (Aizer et al., 2007).

To explain the worsening of Black infant health outcomes and improvements for Hispanics, Kuziemko et al. (2018) propose a risk-selection model to rationalize their results. Specifically, they argue that capitation payments incentivized competing MMC plans to provide better care to low-cost clients, aiming to retain them for future births, while offering worse care to high-cost clients to discourage them from future births. As previously noted, longstanding disparities exist in healthcare costs related to the delivery of Black and Hispanic infants. Considering these differences, it's possible that MMC plans allocated resources more toward comparatively healthier patients (Hispanics) and reduced investment in the care of less healthy patients (Blacks). It is important to highlight that these incentives do not exist under a traditional FFS model, where providers are paid for *each* service they provide and do not face incentives to reduce utilization.

In support of this model, Kuziemko et al. (2018) find that Black mothers were less likely than Hispanics to receive immediate prenatal care, as suggested by self-reported birth certificate data. I also

[20]Specifically, for each county and outcome, I estimate a linear trend at the birth year $\times$ month level using children before the MMC transition. I then extrapolate this estimated trend across all birth year months and subtract the predicted value of each outcome from the observed value. This de-trended value is then used as the outcome in all models.

[21]Due to the lack of family income data in birth certificate records, Kuziemko et al. (2018) only include children with US-born mothers in their likely Medicaid eligible sample. In contrast, my sample also considers low-income children using FL eligibility data from my administrative schooling records. As shown in Appendix B, my results remain similar when I align more closely with Kuziemko et al. (2018) by only restricting my sample to children of likely US-born mothers.

find suggestive evidence supporting their model, as the test score impacts of MMC are most pronounced in areas with stronger incentives for risk selection. Specifically, declines in Black children's test scores are concentrated in areas with a higher proportion of Black mothers, where cost-cutting is most critical. Although the overall impact of prenatal MMC exposure on Hispanic test scores is mostly insignificant, Hispanics experience modest test score gains in areas with more Hispanics, where MMC plans may face stronger incentives to compete for Hispanic patients.[22] However, due to challenges in obtaining managed care claims data, I am unable to conduct a thorough test of their model, nor am I aware of any other studies that have been able to do so. While my data will not allow me to *conclusively* pinpoint *how* provider behaviors changed with detailed Medicaid claims data, the widening disparities in racial infant health are meaningful.

I extend these findings by exploring the persistence of the widening of infant health racial gaps into childhood and early adulthood. While Kuziemko et al. (2018) provide compelling evidence that the impacts of MMC on infant health outcomes are not driven by compositional changes due to differential selection into pregnancy post-MMC, a potential concern when examining outcomes several years after birth is differential attrition between birth and school entry. For instance, Kuziemko et al. (2018) demonstrate that MMC led to increased infant mortality among Black infants and reduced infant mortality among Hispanic infants. These changes in infant mortality may have affected the underlying population of Black and Hispanic children post-MMC. Additionally, MMC could have led to differential attrition outside of Texas.

To address these concerns, I examine whether the MMC rollout is correlated with the likelihood of being matched to the public schooling records. Reassuringly, Appendix Table A.8 shows that the MMC rollout is uncorrelated with the fraction of children who are matched to the schooling data, helping to rule out the possibility that my results are explained by compositional changes due to differential attrition. However, one may still worry that even statistically insignificant differential attrition could bias my estimates, particularly since changes in infant mortality are likely to affect children who are in the poorest health who are predicted to have worse childhood outcomes. It's worth noting that while the infant mortality changes are striking, the low rates of infant mortality during this period mean that the compositional impact of these changes is limited. According to my calculations, approximately 0.2% fewer Black children survived per year after MMC, while 0.14% more Hispanic children survived per year after MMC.[23] Nonetheless, I present

[22]These results are available upon request.

[23]This calculation is based off the fact that 50,000 Black children are born in Texas per year and MMC increased the infant mortality rate (IMR) from 11 to 13 deaths per 1,000 births for this group. In addition, 160,000 Hispanic children are born in Texas

a formal bounding analysis in Section 5.4. Reassuringly, the estimates remain unchanged even under extreme assumptions about selection, where changes in infant health are assumed to be driven by either the highest or lowest achievers based on standardized test performance. Furthermore, these compositional changes are likely to understate any negative impacts of the policy on Black infants and any positive impacts on Hispanic infants, assuming that those on the margin of survival have the poorest infant health and face a greater risk of poor childhood outcomes.

4.2 The medium and long-run impacts of early life MMC exposure

I extend Kuziemko et al. (2018)'s findings to identify the longer-run impact of prenatal MMC exposure on subsequent academic achievement and educational attainment. To do so, I start with a slightly modified version of Equation 1 that looks at academic achievement, high school completion or college enrollment as outcomes. The main analysis focuses on likely Medicaid eligible children as detailed in Section 3, and eligibility is determined without information on actual take-up of Medicaid. Hence, the estimates represent intent-to-treat (ITT) effects. Specifically, I estimate the following model:

$$Y_{iymct} = \beta * MMC_{ymc} + \theta' X_i + \Lambda' Z_{ymc} + \vartheta' Z_{dym} + \alpha_c + \gamma_{ym} + \gamma_t + \varepsilon_{iymct} \tag{2}$$

where all variables are as previously defined, along with additional controls for individual, cohort, and school district-level characteristics. The individual characteristics (X) include indicators for gender and disability status. Cohort-level controls (Z) include all previous county $\times$ year $\times$ month of birth controls from Equation 1, as well as the share of children born in the same year, month, and county by race, FRL status, and ELL status, along with the total number of births. School district-level controls (ϑ) account for SpEd and ELL rates interacted with birth cohort time trends.[24] Additionally, outcome year t fixed effects γ_t are included to account for common time effects in the outcomes. Standard errors are clustered by county of birth.

I consider MMC exposure during the prenatal period, given the importance of this time period for development (Miller & Wherry, 2019). As in Equation 1, MMC_{ymc} is an indicator variable that is equal to 1 if an individual was conceived after MMC was implemented in their county of birth. However, I also

per year and MMC decreased the IMR from 7 to 5.6 deaths per 1,000 births for this group.

[24] As will be discussed in more detail in Section 5.4, there were accountability policy changes related to SpEd and ELL instruction during this period. The pressure to make instructional changes was based on the share of students served in these programs. Specifically, I incorporate baseline SpEd and ELL rates in each student's school interacted with birth cohort time trends to control for possible linear changes in instructional practices affecting schools with higher concentrations of SpEd and ELL students.

estimate a specification that models prenatal and childhood exposure separately, where MMC_{ymc} in Equation 2 is replaced with three separate variables for the share of MMC exposure: between conception and birth and between ages 0 and 5. This additional specification reveals the results are driven by the prenatal period.

As discussed in Section 4.1, trends in some county-level characteristics are correlated with the MMC rollout. My models already account for county × year × month of birth controls, however, I use a two-step de-trending procedure proposed by Goodman-Bacon (2021) to further ensure that my estimates are not influenced by differences in pre-period trajectories across counties. Specifically, for each county and outcome variable, I estimate a linear trend at the year and month of birth level using children born before the MMC transition. I then extrapolate this estimated trend across all birth year months and subtract the predicted value from each outcome from the observed value. Then, I estimate Equation 2 using the de-trended outcome variables. Sensitivity analyses include versions of the model that eliminate the control variables, include county-specific time trends, do not remove linear pre-trends, and make the same specification choices as Kuziemko et al. (2018). For Black children, the results remain very robust across these specifications.

The main parameter of interest, β, measures the impact of being conceived after MMC on student outcomes. The main identification assumptions required for a causal interpretation are that the timing of the transition to MMC is uncorrelated with factors that relate to both infant health *and* later achievement. To assess the plausibility of these assumptions, I present event-study estimates that replace MMC_{ymc} in Equation 2 with a set of indicators for 9-month intervals (or 3 quarter bins) relative to the MMC transition.[25] Specifically, I estimate the following specification:

$$\begin{aligned} Y_{iymct} = & \gamma + \sum_{n=-3}^{-2} \pi_n * I^n_{ymc} + \sum_{n=0}^{4} \tau_n * I^n_{ymc} + \beta_{pre} I^{>3\text{years pre}}_{ymc} + \beta_{post} I^{>3\text{years post}}_{ymc} + \theta' X_i + \Lambda' Z_{ymc} \\ & + \vartheta' Z_{dym} + \alpha_c + \gamma_{ym} + \gamma_t + \varepsilon_{iymct} \end{aligned} \tag{3}$$

where all variables are as previously defined, and where I^n_{ymc} is an indicator variable for conceptions n 9-month intervals (or 3 quarters) after a county switched to MMC. The omitted category is the group conceived 9 months before the MMC transition (i.e., $n = -1$), and is the last group fully conceived under FFS. Those conceived within the 9 months before MMC's implementation (i.e., $n = 0$) were partially exposed and children

[25] I look at 9-month intervals, since pregnancies typically last for 9 months.

conceived 9 months after MMC's implementation were fully exposed to MMC (i.e., all $n \geq 1$). $I_{ymc}^{>3\text{years pre}}$ and $I_{ymc}^{>3\text{years post}}$ are indicators for conceptions before and after the three-year window of MMC's introduction, respectively. This event-study approach allows me to visualize any differences in children's outcomes leading up to the MMC transition compared to children's outcomes in control counties, which either transitioned later or not at all during the analysis period, as a test of the identification assumptions. Additionally, since the transition to MMC was abrupt (i.e., after the MMC transition participation in MMC was mandatory), this event-study specification helps determine whether changes in outcomes precisely align with the MMC transition. The ability to precisely attribute changes in outcomes to the MMC transition date helps to rule out the possibility that the results are driven by long-term trends unrelated to MMC that may be influencing the trajectories of low-income Black and Hispanic children during this period.

Additional empirical tests further support the identification assumptions. First, I find that the timing of the MMC rollout is uncorrelated with levels or trends in pre-policy achievement. Figure 1 plots reading scores from 1993 and changes in achievement from 1993 to 1995 (i.e., pre-MMC transition for all counties in my sample) against the timing of the MMC transition. Both plots show a weak relationship between pre-MMC achievement and the MMC rollout. Second, to investigate whether MMC's implementation predicts compositional changes, I replace the outcome from Equation 2 with exogenous characteristics (measured in 3rd grade). These results are shown in Appendix Tables A.9 and A.10. Odd-numbered columns, lacking county $\times$ year $\times$ month of birth controls, reveal some significant correlations between MMC implementation and individual characteristics. However, even-number columns that account for these controls, reveal that most individual characteristics are uncorrelated with the MMC implementation. This is reassuring, indicating that in my preferred specification, the identification assumptions are plausible. Only the increases (decreases) in the Hispanic (White) population shown in Appendix Table A.10 remain statistically significant, but the magnitudes of these coefficients are very small and not economically meaningful.[26] Moreover, Panel C of Appendix Table A.9 demonstrates that the composition of US-born Hispanics based on observable characteristics such as English proficiency (as indicated by ELL participation), the need for SpEd services or FL eligibility remained constant over time. This provides suggestive evidence that, despite increases in the Hispanic population, their underlying characteristics were similar before and after MMC's introduction.

[26]This corresponds to a 0.3 p.p. (0.6%) increase in the likelihood of being Hispanic and a 0.4 p.p. (0.8%) decrease in the likelihood of being White. These demographic changes are consistent with the increases in immigration from Latin America during this period.

A recent literature has shown that a two-way fixed effect difference in differences approach can sometimes lead to biased estimates without never-treated units or in the presence of dynamic and heterogeneous treatment effects. In my setting, the large number of untreated counties (Appendix Table A.1) helps alleviate some of these concerns. Nonetheless, I also implement an alternative estimator proposed by Gardner (2021) that is robust to these concerns.[27] Reassuringly, I come to similar conclusions on the impacts of prenatal MMC exposure using this alternative estimator.

4.2.1 Intent-to-Treat (ITT) Interpretation

As noted above in Section 3, my measure of likely Medicaid eligibility focuses on children who likely had incomes to qualify for Medicaid (i.e. receiving FL at school entry) and were likely to be growing up in US-born families (i.e. not an ELL at school entry). This measure is computed without knowledge of Medicaid participation. Hence, my main estimates reflect ITT estimates. Medicaid participation is not available in the administrative schooling data and was not recorded on the Texas birth certificate until after 2005 (after the children in my sample were born). In addition, even if it was available, it may not accurately capture the Medicaid population. The transition to MMC may have led some providers or enrollees to incorrectly classify Medicaid births as privately insured births (Aizer et al., 2007). According to Kuziemko et al. (2018) approximately 30 percent of MMC births are inaccurately categorized as not covered by Medicaid.

To assess how close my sample gets to the sample of children who are treated, I turn to the American Community Survey (ACS). Using a similar definition for the likely Medicaid eligible sample used in this paper, I find that among Black and Hispanic likely Medicaid eligible children in Texas, 73 and 71 percent took-up Medicaid in 2010, respectively.[28] Adjusting by 1.3 to account for the underreporting of Medicaid suggests that roughly 95% of Black and 93% of Hispanic likely Medicaid eligible children received Medicaid. The high rates of Medicaid participation among my likely Medicaid eligible sample suggest that the ITT estimates presented in this paper are likely to be close to the treated on the treated (TOT) estimates.

[27] This estimator first estimates group and period effects on the sample of untreated observations. Next, treatment effects are identified by comparing treated and untreated children after removing the group and period fixed effects. The full set of control variables are used in the first and second stages. Gardner (2021) demonstrates that this method is robust to treatment effect heterogeneity under staggered adoption.

[28] Specifically, I focus on children born to US-born mothers between 1993 and 2001 whose family income qualifies for FL.

5 Results

5.1 Academic Achievement

I first examine whether prenatal MMC exposure affected academic achievement. Figures 2 and 3 present event-studies for reading and math scores for likely Medicaid eligible students. In each figure, estimates for Black, Hispanic, and White children appear in panels A, B, and C, respectively.[29] These plots reveal no evidence of a pre-trend for all groups, but an abrupt decline in reading and math test scores for Black likely Medicaid eligible children conceived after MMC's implementation. The timing of the negative impacts for Black likely Medicaid eligible children precisely coincide with MMC's introduction. Those partially exposed to MMC, who were conceived between 1 and 9 months before MMC's implementation, experienced small declines in their test scores. However, the most significant declines were for children fully exposed to MMC prenatally, who were conceived 9 months after MMC's implementation. In contrast, for Hispanic children, there is little difference in outcomes after the MMC transition. For White children, there is a small decline in test scores after the MMC transition, but these declines are not statistically significant.[30]

Difference-in-differences estimates for the likely Medicaid eligible sample are shown in Table 2. Starting with a model that does not remove pre-period trends reveals significant declines in reading and math achievement for Black children and significant improvements in reading scores for Hispanic children. After including county-specific time trends the results for Black children remain significant and negative, while the results for Hispanic children lose their positive significance, and in the case of math scores, reverse sign. Columns 3 through 5 employ the de-trending procedure outlined in Section 4.2. Initially, the model includes fixed effects for county $\times$ year $\times$ month of birth, and outcome year. I then successively add controls. In line with the event-study results, I find that having *any* prenatal MMC exposure reduced achievement among Black children but had little effect on Hispanics across most models. The estimated effects for Black children are stable across all models. Estimates for Hispanic children are also stable, but only after de-trending the outcomes. Using my preferred specification in Column 5, the estimates suggest that any prenatal MMC

[29] The final sample is not limited to children who were continuously enrolled in public schools. For outcomes measured in a specific grade, such as 3rd grade, the analysis includes only those students who were enrolled in that grade during the specified year and have available outcome data. In the pooled regressions, students are included as long as they are observed at least once within the grade span range. As shown in Appendix Table A.14 and discussed in Section 5.4, the results remain consistent even when focusing on a sample of students continuously enrolled from 3rd to 8th grade, when the main achievement outcomes are measured.

[30] A version of the event-studies for reading and math test scores where the scores are not de-trended is shown in Appendix Figures A.1 and A.2. The conclusions for Black and White children are very similar. However, for Hispanic children, there are positive impacts after MMC's introduction. Because the MMC rollout is correlated with some county-characteristics (see Appendix Table A.6 and a more detailed discussion in Section 4.1) my preferred specification removes pre-period trends.

exposure decreased reading and math scores of Black children between 3rd and 8th grade by about 3% of a standard deviation, but had no statistically significant impact on Hispanics. Similarly, Appendix Table A.11 shows that Black infants conceived after MMC experienced declines in reading test scores using the Gardner (2021) estimator that is robust to treatment effect heterogeneity under staggered adoption.

Looking at the effects on likely Medicaid children across grades reveal similar patterns. Table 3 shows that the coefficients on reading test scores for Black children are similar across grades, although less precisely estimated in some grades individually. For math, the effects are larger in earlier grades. Again, for Hispanic students there is little effect of prenatal MMC exposure on reading and math achievement across all grade levels. While White children experience modest declines in reading achievement in some grades, the pooled effect across grades is insignificant, and these changes are considerably less pronounced when compared to the substantial declines observed for Black children. This is consistent with the results of Kuziemko et al. (2018) who also found small declines in White children's infant health outcomes.

Next, I turn to a model that accounts for the share of exposure to MMC during the prenatal period and early childhood separately. These results are presented in Appendix Table A.12. This model has the advantage of more accurately capturing the intensity of treatment prenatally by using the share of prenatal exposure rather than an indicator for *any* exposure as is used in my baseline model. Additionally, this model accounts for the impacts of any possible changes during childhood that could be driven by changes in healthcare delivery after birth. Results from this model reveal more negative and precisely estimated impacts of prenatal MMC exposure for Black children. Importantly, this model also reveals that the effects are driven by MMC exposure during the prenatal period. As health care costs are very high during the prenatal period these findings are consistent with MMC having the most detrimental impact on children when the incentives to skimp on medical care are strongest. Overall, this model reveals little impact of prenatal and childhood MMC exposure on Hispanic children's achievement.

While the event-study specification helps to rule out the possibility that my results are driven by long-run trends in outcomes by attributing the changes in outcomes precisely to the MMC transition date, I can also turn to placebo groups who were unlikely to qualify for Medicaid and be directly impacted by this change. Estimates for several placebo groups are presented in Table 4. High-income children serve as a useful placebo group, given their relatively low likelihood of Medicaid eligibility. Columns 4 and 5 demonstrates that high-income children (i.e. those not receiving FRL) were largely unaffected by prenatal MMC exposure. The one exception is that higher-income Black children experience modest increases in their reading test scores (significant at the 10 percent level). The positive direction of these effects suggest, if anything, that Black students would have experienced improvements in reading achievement, which would lead me to underestimate the negative impact of MMC on student achievement.[31] Another potential placebo group are children growing up in likely immigrant families, who were also unlikely to qualify for Medicaid during this period. Overall, the impacts of prenatal MMC are not being driven by children growing up in likely immigrant families (i.e., who entered school as an ELL), although the sample sizes for Black and White children are small and the estimates are somewhat noisy. Given the fact that most immigration to Texas during this period was driven by Latin America, Hispanics with likely immigrant parents serve as a more meaningful falsification group. Encouragingly, they were not affected by prenatal MMC exposure.

5.2 Heterogeneity across School Quality

The results presented thus far have demonstrated the robust relationship between declines in one's early life health environment and later test scores for Black children. An important question is whether later educational investments are able to remediate the negative impacts of these early life health shocks. For instance, schools with more resources and more qualified teachers may be able to offset the negative early life health shocks. However, if the disadvantages associated with poor infant health are set early it's possible that better quality schools may not be able to reduce the gaps generated by poor infant health.

To explore this possibility, Table 5 compares the effects of prenatal MMC exposure across different measures of school district quality. Columns 1-2 focus on district-level average test scores, Columns 3-4 on overall district value-added, and Columns 5-8 on race-specific district-value-added. While, in general, no significant differences emerge across *overall* measures of district quality, there are significant and meaningful differences across measures of race-specific district quality. For instance, the adverse effects of prenatal MMC exposure on Black children's test scores are particularly pronounced in districts with low value-added for Black children. On the other hand, prenatal MMC exposure positively affected Hispanic children in districts with low value-added for Hispanic children improving their reading test scores by 3% of a standard deviation, but had no impact in districts with high value-added for Hispanics, a difference that is significant

[31] Another possibility which I discuss in more detail in Section 5.4 is the introduction of CHIP which expanded healthcare coverage to children growing up in higher-income families. Higher-income Black children may have benefitted from these CHIP expansions.

at the 5% level. This suggests that the benefits of early life health improvements provide an advantage to Hispanic children in districts with lower effectiveness for Hispanics, yet are undetectable in districts with higher effectiveness for Hispanics. These results, which indicate that targeted measures of district quality are what matter, are generally consistent with prior literature that finds limited evidence that the adverse effects of poor infant health are likely unable to be mitigated in schools of better *overall* quality, but can be mitigated when interventions are more targeted (D. Figlio et al., 2014; Bharadwaj et al., 2018).

5.3 Educational Attainment

I next turn to exploring whether prenatal MMC exposure impacted the likelihood of high school completion or college enrollment. For this analysis, the sample size is limited to children born in Texas between 1993 and 1997. This sample restriction is made to ensure that I can track students at least 2 years after their expected high school graduation but before 2019, as Covid-19 influenced decisions regarding educational attainment.[32] One limitation of this analysis is that these restrictions cut the sample by about half, and I cannot use the full MMC rollout. Moreover, to capture as many birth cohorts as possible I limit the focus to college enrollment within two years of expected high school graduation. While only 3% of the sample is still enrolled in high school two years after their expected high school graduation, it still could be premature to examine college enrollment outcomes, considering some students may take a gap between high school completion and college enrollment. Despite these caveats, Table 6 presents the impacts of prenatal MMC exposure on these long-run educational attainment outcomes, along with the impacts on achievement outcomes for this smaller sample.

Table 6 reveals that although the point estimates generally tend to be negative, prenatal MMC exposure did not lead to statistically significant reductions in high school completion and college enrollment within 2 years of expected high school completion for Black likely Medicaid eligible children. To assess whether these results are driven by the different samples, I also estimate the impact of between prenatal MMC exposure on test scores for this smaller sample. While the results presented so far have demonstrated a robust relationship between prenatal MMC exposure and later test scores, for this smaller sample the estimates for reading and math scores in this smaller sample are no longer statistically significant, though the magnitudes of the coefficients for reading are similar to those in Table 2. This suggests that, due to the significantly smaller sample size and the fact that I am not using the full MMC rollout, there may not be sufficient statistical power

[32] Appendix Table A.4 shows each birth cohort in the sample, and their expected year of enrollment in Kindergarten and 12th grade, as well as the number of years after expected high school graduation and 2019, the year before Covid.

to detect an effect on the educational attainment of Black children. Hence, although the negative coefficients imply a potential decline in these outcomes, it remains inconclusive what the long-term educational impact on Black children might be.

Table 6 demonstrate that among likely Medicaid eligible Hispanic children prenatal MMC exposure increased high school completion and any college enrollment by about 3 percentage points, or 4% and 8%, respectively. While the sample size for this analysis is significantly smaller than the main results, the Hispanic population in Texas is large and nearly double that of their Black counterparts. This suggests the possibility of having more statistical power to detect effects for Hispanic children in comparison to Black children. Interestingly, the impacts on math and reading test scores between 3rd and 8th grade remain statistically insignificant. These results are consistent with a large literature that indicates that despite early test-score fade out, many early childhood interventions tend to have longer-run gains (e.g. Deming, 2009; Krueger & Whitmore, 2001). White children did not experience any change in their long-run educational attainment.

5.4 Robustness

The key identification assumption in this analysis is that children born after MMC had similar counterfactual trends relative to those born before. While I have presented several pieces of evidence in support of this assumption through event-study specifications and additional checks discussed in more detail in Section 4.2, a remaining concern is that the population of children was changing in unobservable ways after MMC's introduction. This could be the case if the changes in infant mortality documented by Kuziemko et al. (2018) or outmigration introduced later compositional changes during childhood.

As previously noted, while the changes in infant mortality rates introduced by MMC are striking, the change in the number of surviving infants was relatively modest. According to my calculations, approximately 0.2% fewer Black infants survived and 0.14% more Hispanic infants survived each year. Given these small magnitudes, it is unlikely these compositional changes will influence my later estimates on educational outcomes. Nonetheless, I implement a formal bounding exercise in the spirit of D. S. Lee (2009). Specifically, for Black children, I drop 0.2% of observations in each birth cohort born after MMC with the highest or lowest test scores. Similarly, for Hispanic children, I drop 0.14% of observations in each birth cohort born after MMC with the highest and lowest test scores. Then I identify the treatment effects on this trimmed sample. The results from this exercise are shown in Appendix Table A.13. Columns 1 and 4 show the baseline estimates, Columns 2 and 5 show the results where the highest performers are dropped, and Columns 3 and 6 show the results where the lowest achievers are dropped. Importantly, the results across all three models are nearly identical for each of the outcomes and for both the Black and Hispanic likely Medicaid eligible samples. As a result, I conclude that it is unlikely that my results can be explained by differential attrition after birth driven by the impact of MMC on infant mortality.

The other critical assumption is that there were no contemporaneous shocks. For instance, if there were other policies or location-specific shocks timed with the staggered MMC rollout, I could be misattributing the impacts to MMC to these other possible changes. To address this concern, I explore other health and educational policy changes introduced during this time period. In terms of healthcare changes, to my knowledge, the only significant policy change was the introduction of CHIP in 1999 which expanded health insurance to children from relatively higher-income families (i.e., those 150% below the FPL) who did not qualify for Medicaid. My main analysis sample of likely Medicaid eligible children (who are growing up in families 130% below the poverty line), focuses on children who were unlikely to be affected by CHIP's introduction, as they would have qualified for Medicaid regardless. However, those classified in the high-income groups (i.e., growing up in families above 130% of the federal poverty line) could have experienced increased access to health insurance during this time. If anything, this would bias my estimates on high-income children upwards (as increased healthcare access will likely improve test scores).[33]

In terms of educational policy changes in Texas, to my knowledge, the only significant policy change during this time that could have impacted children's test scores was the introduction of the Performance Based Management System (PBMAS) in the 2004-05 school year which introduced school district level monitoring of SpEd and ELL students.[34] This monitoring was aimed at improving the academic and behavioral outcomes of SpEd and ELL students. However, it also placed pressure on districts to reduce overall enrollment in SpEd programs and reduce accommodations for SpEd and ELL students specifically in terms of test-taking (i.e., reducing options for modified exams).[35] Reassuringly, since the policy was introduced at one point in time, affecting all students simultaneously, the outcome year fixed effects included in my model can help control for any shift in outcomes after the introduction of PBMAS. However, while unlikely, it is possible that the

[33] This could explain why higher-income Black children experience modest increases in their reading test scores, as shown Table 4.

[34] No Child Left Behind (NCLB) was also introduced in 2003. However, since many features of NCLB mirrored those of the existing accountability system that had been in place in Texas since 1993, I do not suspect that NCLB played a large role in Texas.

[35] Specifically, the policy introduced a SpEd enrollment cap of 8.5 percent and caps that limited the over-representation of Black or Hispanic students in SpEd.

timing of the MMC rollout was correlated with counties that faced more pressure to make more instructional changes. Results in Appendix Table A.2 rule out this possibility, showing that the timing of the MMC rollout is uncorrelated with the county-level share of ELL and SpEd children. Nonetheless, to account for this policy, in my baseline model, I incorporate baseline SpEd and ELL rates in each student's school district interacted with birth cohort time trends to control for possible linear changes in instructional practices affecting schools with higher concentrations of SpEd and ELL students. Overall, with the inclusion of outcome year fixed effects and these time trends, my models are likely to fully account for these other policy pressures.

Nonetheless, it is helpful to think through the direction of the potential bias that SpEd and ELL monitoring may have introduced. While the results of SpEd monitoring on likely Medicaid eligible students is a-priori ambiguous, prior literature finds that Black students benefited from the pressure to reduce SpEd enrollment, as they tend to be over-identified for SpEd services (Ballis & Heath, 2021a). If anything, this suggests that my estimates would be biased upwards for Black children and lead me to underestimate the impacts of a decline in infant health due to MMC. On the other hand, prior literature finds that Hispanic students were harmed by the pressures to reduce SpEd enrollment as they tend to be under-identified for SpEd (Ballis & Heath, 2021b). This suggests that this policy change is likely to bias my estimates downwards and will bias me against finding a positive impact of MMC on Hispanic children's scores.

In terms of ELL monitoring, it is unlikely that my main analysis sample of likely Medicaid eligible children will be directly affected by these changes as I condition the sample to not being enrolled in ELL programs. However, those in my sample could have been indirectly harmed if their ELL peers experienced reductions in performance as a consequence of a loss of accommodations. As schools in Texas remain highly segregated by ethnicity, ranking the third most segregated state in the US, it is plausible that Hispanics would have experienced stronger negative spillovers due to this change than Black children (Mattson, 2020). This is yet another reason why the impacts for Hispanics could be attenuated.

Next, I consider the potential impact of Hurricane Katrina in 2005. Following Hurricane Katrina, Houston, the largest city in Texas, experienced a large influx of Katrina refugees, increasing the population of Houston by 3-4% (McIntosh, 2008). Estimates from the literature suggest that there were negative spillover effects on incumbent public school students in Houston as a consequence of this migration (Imberman, Kugler, & Sacerdote, 2012). To test whether my estimates may be contaminated by the influx of refugee students, in Column 2 of Appendix Table A.14 I re-estimate my models dropping all observations from Harris county (where Houston is located). Column 1 includes the baseline results for reference. For Black and Hispanic children the results are similar (although I lose precision for the math estimates for Black children).

Finally, I examine whether my results are sensitive to changes in the test-taking regime and attrition. In Column 3 of Appendix Table A.14, I drop all observations from the STAAR test regime introduced in 2011. This ensures that all students in the sample took their exam under the same testing regime (i.e., the TAKS). The point estimates are similar to the baseline estimates shown in Column 1, but given the sample is significantly smaller, the results are less precisely estimated. Next in Column 4, I focus on a continuously enrolled sample (i.e., those enrolled in Texas public schools consistently between 3rd and 8th grade). The results are nearly identical to the baseline estimates. Finally, I focus on students who are consistently observed in the test-taking data every year in Column 5. While very similar to the baseline estimates, this time the estimates are slightly larger.

5.5 Spillovers

Even though the transition to MMC should only directly impact Medicaid eligible students, the reductions in infant health experienced among Black likely Medicaid eligible children could have affected all public school students in Texas through classroom spillovers. A large literature finds that changes in peer academic and behavioral outcomes can generate sizable classroom spillovers, and as just documented above, MMC led to an abrupt decline in academic performance among likely Medicaid eligible Black children.

To estimate MMC's spillovers, I compare children within the same schools who have different exposure to Medicaid eligible peers exposed to MMC. Variation in MMC peer exposure is driven by idiosyncratic differences across grade-cohorts in Medicaid eligible peers (that is, by chance some grade cohorts within a school will have more Medicaid eligible children), as well as MMC's introduction. The control group consists of students without Medicaid eligible peers prenatally exposed to MMC, and the treatment effects vary across students in the fraction of their peers who were likely Medicaid eligible and prenatally exposed to MMC. Specifically, I estimate a slightly modified version of Equation 2 as follows:

$$Y_{iymctsg} = \delta * ShareMMC_{sgt} + \theta' X_i + \Lambda' Z_{ymc} + \vartheta' Z_{dym} + \alpha_c + \gamma_{ym} + \lambda_{sg} + \lambda_{gt} + \varepsilon_{iymctsg} \tag{4}$$

where all variables are as previously defined, except I replace the indicator for any prenatal MMC exposure with the share of peers exposed to MMC prenatally in one's school s and grade-cohort gt. My model considers peer exposure to Black likely Medicaid eligible peers, as they were the subgroup that experienced significant declines in infant health. In addition, I control for school by grade fixed effects, λ_{sg}, to control for unobservable fixed differences across schools at the grade level and grade-year (i.e., cohort), λ_{gt}, fixed effects to control for trends in long-run outcomes for all Texas-born students.

Table 7 reveals sizable classroom spillovers resulting from MMC. Column 1 examines the likely Medicaid-eligible sample, followed by subgroups who are unlikely to qualify for Medicaid. Among Black children, negative spillovers affect all subgroups, with statistical significance in all but the high-income, foreign-born mother group. For Hispanic children, significant negative spillovers are observed among those less likely to be Medicaid eligible (i.e., with likely foreign-born mothers), while spillovers are undetectable for those likely to qualify for Medicaid (i.e., with likely U.S.-born mothers). This pattern suggests that the health advantages of likely Medicaid-eligible Hispanic children may have insulated them from the adverse spillover effects linked to higher exposure to Black likely Medicaid-eligible peers. Among White children, the negative spillovers are primarily driven by high-income children with U.S.-born mothers. Specifically, I find that a 5% increase in Black Medicaid eligible peers decreases test scores by about 0.3-3.8% of a standard deviation. The magnitude of these effects are in line with prior literature but slightly smaller than other studies, which find that a 5% increase in disruptive peers and grade-repeating peers decreases test scores by 3% and 6% of a standard deviation, respectively (Carrell & Hoekstra, 2010; Abramitzky & Lavy, 2014).

6 Discussion

Thus far, I have shown that prenatal MMC exposure negatively affected Black children's test scores, but didn't significantly improve the test scores of Hispanic children across most specifications, except in schools that have low-value added for Hispanics.[36] However, Hispanic children experienced significant increases in their long-run educational attainment. As discussed in more detail above in Section 5.3, while I do not find that prenatal MMC impacted educational attainment among Black children, this could be because I am using a significantly smaller sample and may be under-powered to estimate the effects. The longer-run impacts of MMC on educational attainment for Black children is unclear, although the negative point estimates suggest

[36]It is important to note that in some models that do not account for pre-policy trends, the estimated effect of prenatal MMC exposure for Hispanic children is positive (see results in Column 1 of Table 2 and Appendix Figures A.1 and A.2).

a decline in overall college going.

The childhood exposure models presented above suggest that the results are driven by changes occurring during the prenatal period. To my knowledge, the only change during this time was the abrupt changes in infant health driven by the transition to MMC and the subsequent changes in healthcare practices motivated by the strong incentives to reduce healthcare utilization. While my data does not allow me to conclusively determine how provider behaviors changed under MMC, the widening infant health gaps are consistent with a risk selection model where Black mothers experience a decline in the quality of care or access to care throughout their pregnancies.[37] Given there were no changes in Medicaid eligibility for my main analysis sample, this helps to rule out the possibility that my results are being driven by changes in access to healthcare. However, I cannot rule out the possibility that changes in infant health may have resulted in shifts within the household, potentially leading to increased demands on time and a depletion of family resources, especially among Black families caring for a sicker child. Nonetheless, because the effects are driven by the prenatal period, and not later during childhood, it is reasonable to conclude that the key mechanism behind my results are the abrupt changes in early life health and the subsequent consequences.

The declines in test scores observed for Black children are consistent with prior studies in the literature that test the "fetal origins" hypothesis, that emphasizes that conditions before or right after birth can have significant impacts on later development and well-being. My results indicate that a 6% increase in the likelihood of being low-birth weight decreases test scores by 0.03 of a standard deviation. This is consistent with previous twins effects studies that find a 10% increase in birthweight improves test scores by 0.04-0.06 sd (Bharadwaj et al., 2018; D. Figlio et al., 2014). It is also relevant to compare the estimates to other studies that have looked at the long-run effects of changes to health investments at birth on educational outcomes.[38] For example, studies that have analyzed the impact of treatment for very low-birth weight find that more intensive hospital care increase test scores by about 0.3 of a standard deviation (Bharadwaj, Løken, & Neilson, 2013; Chyn et al., 2021). My estimates are smaller than these estimates, however, health interventions for being very low birth weight are very intensive. While I do not observe actual healthcare utilization in my data,

[37]This could be driven by MCOs recommending fewer physician visits for Black mothers. However, it could also be driven by a reduction in access to Medicaid providers, if for example, MMC led providers to become unwilling to treat Medicaid patients. My data do not allow me to investigate these possibilities, however, prior literature finds limited evidence that MMC's introduction influenced physician participation in Medicaid (Greene, Blustein, & Remler, 2005).

[38]While I do not have data that would allow me to directly test whether healthcare utilization declined for my sample, as previously discussed in more detail Appendix Table A.3 demonstrates significant reductions in utilization for the overall Medicaid population.

it is plausible that the changes to health investments driven by MMC were smaller relative to the treatment for being very low birth weight.

The longer-run improvements in educational attainment among Hispanic children is consistent with a robust literature that finds improvements in infant health can improve educational attainment and labor market outcomes. I find that prenatal MMC exposure increased high school completion and college completion by 3 percentage points or 4% and 8%, respectively. Bütikofer, Løken, and Salvanes (2019) find that increasing well-child visits increase years of schooling by 0.15 years and Chyn et al. (2021) find that additional treatment for being very low-birth weight increases college enrollment by 17.1 percentage points. My estimates are smaller than those of Chyn et al. (2021), again who focus on an intensive health intervention, but closer to those of Bütikofer et al. (2019) who focus on a more similar change in healthcare delivery.

What can explain the absence of a corresponding increase in test scores for Hispanic likely Medicaid eligible children? Although the existing literature generally finds that improvements in infant health also enhance academic achievement, it is possible that test scores may not capture all of the benefits stemming from better early life health conditions. For instance, as will be discussed below, in addition to long-run improvements in educational attainment, noncognitive outcomes among Hispanics also improved. Changes in noncognitive outcomes have been shown to have the most significant influence on educational attainment (Jackson, 2018). Finally, it is possible that the increases in infant health were not large enough to yield later impacts on academic achievement. Unlike most papers within this literature that attribute improved academic performance to changes in birth weight, it's worth noting that changes in the likelihood of being low birth-weight did not significantly change after MMC's implementation for Hispanics.

6.1 Other Outcomes

To better understand the short-run declines in test scores for Black children and the longer-run gains in educational attainment for Hispanic children, I turn to exploring whether there were changes in SpEd placements, attendance, disciplinary actions, and grade repetition.

It is possible that changes in infant health could have affected the likelihood of a student being identified with a disability in school. Overall, Panel A of Appendix Table A.15 reveals that prenatal MMC exposure did not significantly impact the likelihood of SpEd placement from 3rd to 8th grade for any racial group. However, given the pressure to reduce SpEd access especially for Black and Hispanic students during this period (see Section 5.4 for more detail), I also estimate these results allowing for a differential linear pretrend by county and race.[39] These results are presented in Panel B, and show that Black children prenatally exposed to MMC were more likely to be placed in SpEd. This is consistent with prior literature that tends to find that having poorer infant health can lead to a higher incidence of disabilities (e.g. Elder, Figlio, Imberman, & Persico, 2020).

Turning to behavioral outcomes, Appendix Table A.16 shows the impacts of prenatal MMC exposure on a summary index of noncognitive outcomes. This summary index accounts for multiple inference (Kling, Liebman, & Katz, 2007), and is computed as the equally weighted average of the z-scores of absences, grade repetition, any suspensions, and in school suspensions. Hispanic children consistently exhibit enhanced noncognitive outcomes throughout their entire primary and secondary schooling. In contrast, Black children encounter declines in noncognitive outcomes, and only in later grades.[40] In a previous paper by Jackson (2018), which estimates the impact of teacher value-added on both academic and noncognitive outcomes, it is found that noncognitive outcomes are more likely to have a positive impact on educational attainment. This aligns with the present study's findings, wherein Hispanic children experienced the most substantial changes in noncognitive outcomes and the largest changes in their later educational attainment.

7 Conclusion

In this paper, I present evidence on how early life health conditions affect racial gaps in educational outcomes. Specifically, I focus on how prenatal MMC exposure affects academic achievement, noncognitive outcomes, high school completion and post-secondary enrollment. My identification strategy is based on the county-level rollout of MMC between 1993 and 2006 in Texas. MMC introduced significant cost containment measures that led most providers to transition from receiving payments for individual services to capitation payments, or *fixed* payments per enrollee per year. This change widened infant health gaps in Texas, worsening Black children's infant health and slightly improving Hispanic children's infant health (Kuziemko et al., 2018).

I find that prenatal MMC significantly reduced test scores for low-income Black children. Across grades 3 through 8 reading and math test scores declined by about 3% of a standard deviation. Given the

[39]Specifically, I estimate a linear trend using children of the same *race* born before the MMC transition in their county, and obtain residuals. Then, I estimate Equation 2 using these de-trended outcome variables.

[40]Again, in order to allow for me to follow all birth cohorts throughout the entirety of high school the sample for the high school estimates are truncated (see Section 5.3 for more detail).

Black-White test score gap is 0.5 of a standard deviation, this suggests that MMC's introduction increased achievement gaps by about 6%. In contrast, prenatal MMC exposure didn't impact Hispanic children's test scores, but led to improvements in noncognitive outcomes and educational attainment. The long-run educational attainment gains for Hispanic children suggest that test scores may not capture all of the benefits of improved early life health conditions. I also find that MMC generated sizable spillover effects. I find that a 5% increase in Black Medicaid eligible peers decreased test scores by about 0.3-3.8% of a standard deviation.

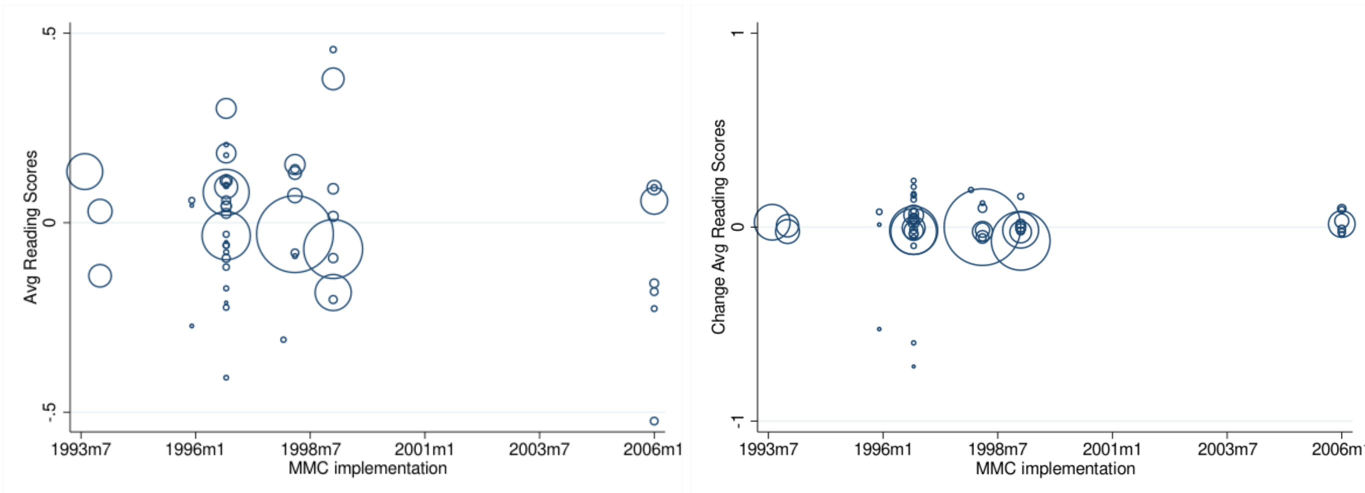
Having demonstrated the overall impact of MMC on student outcomes, I turn to exploring dynamic complementarities with various dimensions of school district quality. The adverse effects of poor infant health are most pronounced among Black children in school districts with low-value added for Black children. Despite not finding an overall improvement in Hispanic children's test scores, positive gains in test scores for low-income Hispanic children are observed in school districts with low-value added for Hispanic children. This implies that in school districts less effective for one's demographic, the adverse effects of poor early life health can be especially detrimental, and the benefits of early life health can be particularly advantageous.

This project extends the current literature in several important ways. First, to my knowledge, this is the first quasi-experimental study to look beyond the health impacts of MMC and investigate its influence on racial gaps in education. Second, this project provides new evidence on the pathways by which changes in infant health have long-lasting impacts. While there is now a large literature documenting the consequences of early life health on adulthood, less is known about the pathways that lead to these changes. Finally, using detailed administrative schooling data, I am able to focus on how a change in infant health interacts with measures of school district quality. This analysis sheds light on the potential roles of school interventions in ameliorating infant health disadvantages. I find evidence that high-value added school districts for specific demographic groups can ameliorate the negative impacts of poor early life health conditions. Understanding the specific practices or targeted interventions will be the focus of future research.

Figures/Tables

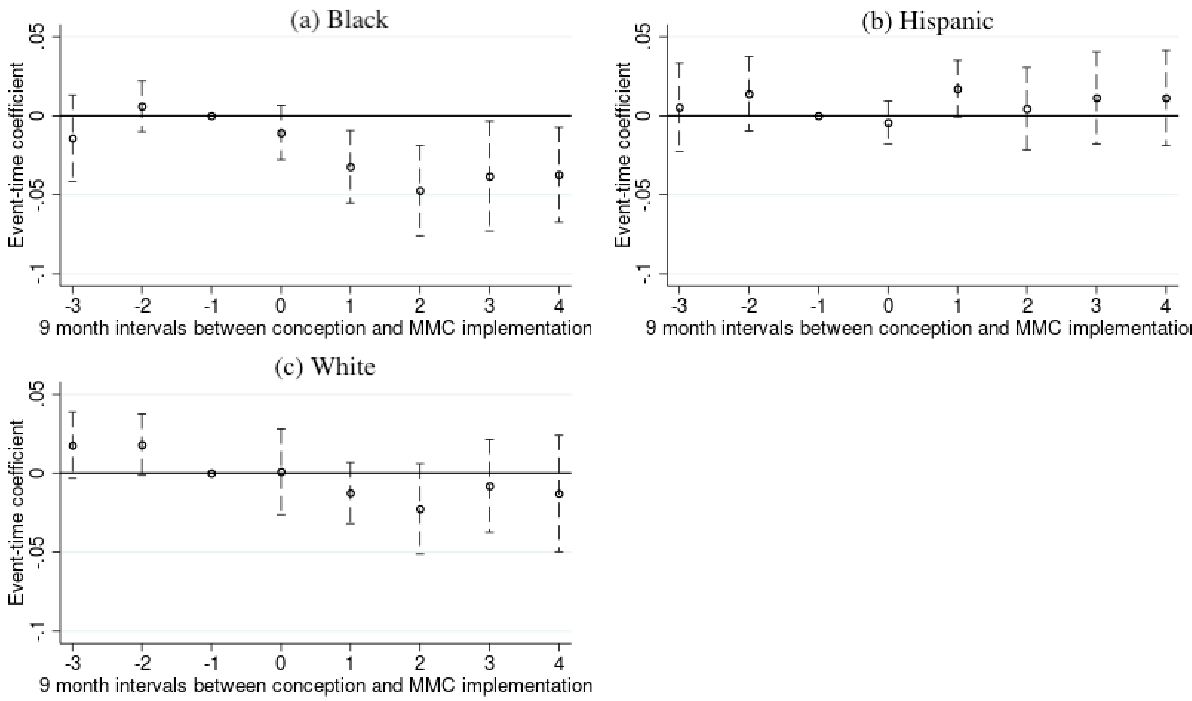
Figure 1: Relationship between Timing of MMC adoption and pre-MMC Reading Achievement Levels

(a) 1993 Reading Achievement **(b)** 1993-1995 Change in Reading Achievement



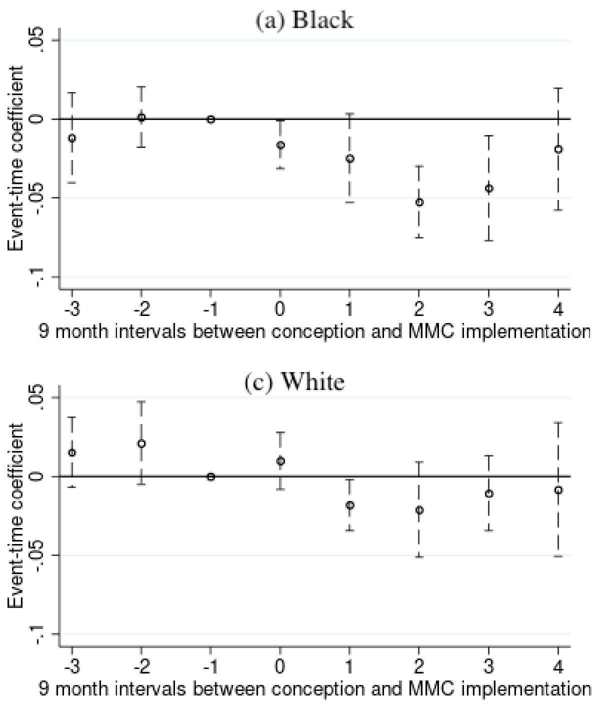
Note: The y-variable variable refers to levels or changes in Reading achievement across grades 3-8 in each county. The x-variable refers to the date that MMC was implemented.

Figure 2: The Impact of MMC at Birth on Reading Test Scores, Likely Medicaid Eligible Sample



Note: This plot shows event-study estimates and 95% confidence intervals of the impact of MMC exposure on reading test scores between 3rd and 8th grade. The estimates come from Equation 3. Negative values on the x-axis indicate conceptions before MMC's implementation. The omitted category is the group conceived 9 months before the MMC transition (i.e. at -1), and were the last group fully exposed to FFS prenataly. Those conceived within the 9 months before MMC was implemented are partially exposed (i.e. at 0) and children conceived 9 months after MMC's implementation were fully exposed to MMC. The sample for these regressions includes likely Medicaid eligible students born in Texas between 1993 and 2001. Likely Medicaid eligible children are defined as children who qualified for free-lunch (FL) and were not an English Language Learner (ELL) at school entry. The sample is restricted to children in counties that had at least two births per month and populations where at least 10% qualified for Medicaid. Pilot counties that transitioned to MMC in 1993 are also excluded. See A.1 for the list of Pilot counties. The results are separated by race, where Black children are shown in Panel A, Hispanic children are shown in Panel B, and White children are shown in Panel C. The model includes fixed effects for county of birth, year by month of birth, and the year the outcome is measured. Individual demographic controls include gender and special education status. County by year and month of birth controls include log population, log per capita income, log per capita transfers and the unemployment rate. Cohort-level controls include the share of children born in the same year, month, and county who are Hispanic, Black, White, FL, and ELL, as well as the total number of births. School level controls for policy pressures related to SpEd students and ELL students are included. Pre-period trends are estimated and removed from all observations for each county prior to the event study estimation. Standard errors are clustered by county of birth.

Figure 3: The Impact of MMC at Birth on Math Test-Score, Likely Medicaid Eligible Sample



Note: This plot shows event-study estimates and 95% confidence intervals of the impact of MMC exposure on math test scores between 3rd and 8th grade. The estimates come from Equation 3. Negative values on the x-axis indicate conceptions before MMC's implementation. The omitted category is the group conceived 9 months before the MMC transition (i.e. at -1), and were the last group fully exposed to FFS prenataly. Those conceived within the 9 months before MMC was implemented are partially exposed (i.e. at 0) and children conceived 9 months after MMC's implementation were fully exposed to MMC. The sample for these regressions include likely Medicaid eligible students born in Texas between 1993 and 2001. Likely Medicaid eligible children are defined as children who qualified for free-lunch (FL) and were not an English Language Learner (ELL) at school entry. The results are separated by race, where Black children are shown in Panel A, Hispanic children are shown in Panel B, and White children are shown in Panel C. See Figure 2 for more detail on the sample restrictions and full set of controls. Pre-period trends are estimated and removed from all observations for each county prior to the event study estimation. Standard errors are clustered by county of birth.

Table 1: Summary Statistics - Texas Born Children 1993-2001

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
					Likely Medicaid Eligible		
	Full	Black	Hispanic	White	Black	Hispanic	White
Demographics							
Female	0.513	0.511	0.509	0.518	0.510	0.509	0.514
SpEd	0.0960	0.107	0.089	0.105	0.120	0.114	0.158
FL	0.407	0.597	0.511	0.192	1	1	1
FRL	0.489	0.684	0.605	0.256	1	1	1
ELL	0.235	0.010	0.444	0.007	0	0	0
Black	0.137	1	0	0	1	0	0
Hispanic	0.495	0	1	0	0	1	0
White	0.340	0	0	1	0	0	1
Short-Run Outcomes							
Std Reading Score (G3)	-0.014	-0.285	-0.187	0.314	-0.447	-0.223	-0.037
Std Math Score (G3)	-0.018	-0.399	-0.150	0.284	-0.550	-0.265	-0.094
Long-Run Outcomes							
High School Completion	0.781	0.764	0.778	0.786	0.719	0.714	0.650
Attend Any College	0.423	0.394	0.393	0.468	0.327	0.301	0.247
Attend 4 Year College	0.171	0.164	0.138	0.206	0.120	0.086	0.068
Attend 2 Year College	0.303	0.266	0.290	0.332	0.225	0.234	0.192
Observations	2,052,560	281,454	1,015,748	697,317	166,528	254,092	131,836

Note: This table presents summary statistics on outcomes and baseline demographics which are measured in 3rd grade. The sample includes children born in Texas between 1993 and 2001 who subsequently enrolled in Texas public schools. The sample is restricted to children in counties that had at least two births per month and populations where at least 10% qualified for Medicaid. Pilot counties that transitioned to MMC in 1993 are also excluded. See A.1 for the list of Pilot counties. The first column includes all students, Columns 2-4 separate students by race, and Columns 5-7 focus on Likely Medicaid eligible students (i.e. those receiving FL at baseline and not participating in ELL) further separated by whether they are Black (Column 5), Hispanic (Column 6), or White (Column 7). All long-run outcomes are indicator variables, and measured as of 2 years after each student's expected high school graduation (assuming normal grade progression after the year a child enters Kindergarten). The sample for the long-run outcomes is limited to students conceived in Texas between 1993 and 1997 so that there is enough of a lag to observe these outcomes.

Table 2: The Impact of Prenatal MMC Exposure on Academic Achievement, Likely Medicaid Eligible Sample

	(1)	(2)	(3)	(4)	(5)
Black Students					
Panel A: Reading Test Scores					
Prenatal MMC	-0.034***	-0.052***	-0.025*	-0.040***	-0.038***
	(0.012)	(0.008)	(0.013)	(0.011)	(0.010)
Mean (Y)	-0.450	-0.450	-0.469	-0.469	-0.469
N	847,597	847,943	847,943	847,943	847,597
Panel B: Math Test Scores					
Prenatal MMC	-0.022**	-0.045***	-0.022	-0.029**	-0.026**
	(0.010)	(0.011)	(0.016)	(0.012)	(0.011)
Mean (Y)	-0.553	-0.553	-0.557	-0.557	-0.557
N	839,131	839,464	839,464	839,464	839,131
Hispanic Students					
Panel C: Reading Test Scores					
Prenatal MMC	0.026**	-0.004	0.001	0.003	0.011
	(0.013)	(0.012)	(0.013)	(0.011)	(0.012)
Mean (Y)	-0.260	-0.259	-0.220	-0.220	-0.220
N	1,388,187	1,389,244	1,389,244	1,389,244	1,388,187
Panel D: Math Test Scores					
Prenatal MMC	0.019	-0.020*	-0.010	-0.012	-0.001
	(0.014)	(0.011)	(0.014)	(0.011)	(0.012)
Mean (Y)	-0.300	-0.299	-0.243	-0.243	-0.243
N	1,369,010	1,370,038	1,370,038	1,370,038	1,369,010
Controls					
Individual Demos	X	X		X	X
Cohort Controls	X	X		X	X
County$\times$Year $\times$Month Controls	X	X		X	X
$f(t)\times$ ELL	X	X			X
$f(t)\times$ SpEd Rate	X	X			X
County-Specific Time Trends		X			
Outcomes De-trended	N	N	Y	Y	Y

Note: This table shows difference-in-differences estimates of the impact of prenatal MMC exposure on reading and math test scores between 3rd and 8th grade. Within each panel, each column reports estimates of β from a separate regression of Equation 2. The sample for these regressions are Black or Hispanic students conceived in Texas between 1993 and 2001 who are likely Medicaid eligible (i.e. qualified for free-lunch (FL) and not classified as an English language learner (ELL) at school entry). The sample is restricted to children in counties that had at least two births per month and populations where at least 10% qualified for Medicaid. Pilot counties that transitioned to MMC in 1993 are also excluded. See A.1 for the list of Pilot counties. All models include fixed effects for county of birth, year by month of birth, and the year the outcome is measured. Individual demographic controls include gender and special education status. County by year and month of birth controls include the log population, log per capita income, log per capita transfers and the unemployment rate. Cohort-level controls include the share of children born in the same year, month, and county who are Hispanic, Black, White, FL, and ELL, as well as the total number of births. School level controls include baseline SpEd and ELL rates in each student's school interacted with birth cohort time trends. Columns 1-2 do not implement a de-trending adjustment. Columns 3-5 de-trend the outcomes before estimation. Pre-period trends are estimated and removed from all observations for each county prior to estimation. Standard errors in parentheses are clustered at county of birth level. *$p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 3: The Impact of Prenatal MMC Exposure on Academic Achievement, Likely Medicaid Eligible Sample: Effects Across Grades

		Grade					
	Pooled	3	4	5	6	7	8
Black Students							
Reading Test Scores							
Prenatal MMC	-0.038***	-0.024	-0.028	-0.061***	-0.046***	-0.031	-0.027**
	(0.010)	(0.019)	(0.017)	(0.011)	(0.012)	(0.020)	(0.011)
Mean (Y)	-0.469	-0.463	-0.480	-0.466	-0.440	-0.470	-0.494
N	847,597	143,499	142,025	138,231	144,548	144,673	134,621
Math Test Scores							
Prenatal MMC	-0.026**	-0.015	-0.052***	-0.056***	-0.020	0.004	-0.003
	(0.011)	(0.017)	(0.012)	(0.011)	(0.016)	(0.021)	(0.026)
Mean (Y)	-0.557	-0.561	-0.563	-0.569	-0.558	-0.568	-0.519
N	839,131	142,821	142,695	139,705	144,656	145,074	124,180
Hispanic Students							
Reading Test Scores							
Prenatal MMC	0.011	0.006	0.023	0.003	0.007	0.000	0.016
	(0.012)	(0.011)	(0.015)	(0.015)	(0.016)	(0.014)	(0.014)
Mean (Y)	-0.220	-0.199	-0.217	-0.222	-0.224	-0.232	-0.227
N	1,388,187	228,116	229,993	227,303	238,448	238,480	225,847
Math Test Scores							
Prenatal MMC	-0.001	-0.013	-0.009	-0.013	0.016	-0.004	0.004
	(0.012)	(0.019)	(0.012)	(0.017)	(0.018)	(0.014)	(0.019)
Mean (Y)	-0.243	-0.225	-0.229	-0.236	-0.252	-0.266	-0.248
N	1,369,010	227,960	231,199	228,458	238,879	239,195	203,319
White Students							
Reading Test Scores							
Prenatal MMC	-0.016	-0.039***	-0.008	-0.008	-0.011	-0.014	-0.019
	(0.011)	(0.011)	(0.015)	(0.015)	(0.018)	(0.013)	(0.013)
Mean (Y)	-0.102	-0.061	-0.077	-0.086	-0.120	-0.132	-0.137
N	671,579	115,252	113,575	110,879	113,503	112,295	106,075
Math Test Scores							
Prenatal MMC	-0.016	-0.035***	-0.025*	0.012	-0.016	-0.001	-0.029*
	(0.012)	(0.013)	(0.014)	(0.020)	(0.014)	(0.014)	(0.017)
Mean (Y)	-0.124	-0.084	-0.111	-0.125	-0.139	-0.148	-0.137
N	664,287	115,124	114,243	111,584	113,758	112,691	96,887

Note: This table shows difference-in-differences estimates of the impact of prenatal MMC exposure on reading and math test scores between 3rd and 8th grade. Within each panel, each column reports estimates of β from a separate regression of Equation 2. The sample for these regressions are Black, Hispanic or White students conceived in Texas between 1993 and 2001 who were likely Medicaid eligible (i.e. qualified for free-lunch (FL) and not classified as an English language learner (ELL) at school entry). The sample is restricted to children in counties that had at least two births per month and populations where at least 10% qualified for Medicaid. Pilot counties that transitioned to MMC in 1993 are also excluded. See A.1 for the list of Pilot counties. The model includes fixed effects for county of birth, year by month of birth, and the year the outcome is measured. Individual demographic controls include gender and special education status. County by year and month of birth controls include log population, log per capita income, log per capita transfers and the unemployment rate. Cohort-level controls include the share of children born in the same year, month, and county who are Hispanic, Black, White, FL, and ELL, as well as the total number of births. School level controls include baseline SpEd and ELL rates in each student's school interacted with birth cohort time trends. Pre-period trends are estimated and removed from all observations for each county prior to estimation. Standard errors in parentheses are clustered at county of birth level. *p<0.10, ** p<0.05, *** p<0.01.

Table 4: The Impact of Prenatal MMC Exposure on Academic Achievement, Additional Subsamples

	(1)	(2)	(3)	(4)	(5)
	Full	Low-Income US Mom	Low-Income FB mom	High-Income US Mom	High-Income FB mom
Black Students					
Prenatal MMC	-0.018**	-0.038***	0.012	0.029*	0.114*
	(0.008)	(0.010)	(0.070)	(0.015)	(0.063)
Mean (Y)	-0.302	-0.469	0.012	-0.049	0.151
N	1,458,415	847,597	7,371	467,378	5,832
Hispanic Students					
Prenatal MMC	-0.007	0.011	0.007	-0.014	-0.022
	(0.008)	(0.012)	(0.006)	(0.010)	(0.016)
Mean (Y)	-0.136	-0.220	-0.343	0.139	-0.183
N	5,654,599	1,388,187	1,451,441	1,458,801	825,533
White Students					
Prenatal MMC	0.001	-0.016	-0.038	0.004	0.098
	(0.008)	(0.011)	(0.042)	(0.007)	(0.067)
Mean (Y)	0.290	-0.102	-0.213	0.403	0.208
N	3,763,642	671,579	12,201	2,829,942	14,724

Note: This table shows difference-in-differences estimates of the impact of prenatal MMC exposure on reading test scores between 3rd and 8th grade. Within each panel, each column reports estimates of β from a separate regression of Equation 2. The sample for these regressions are Black, Hispanic, or White students conceived in Texas between 1993 and 2001. The full sample is shown, along with breakdowns by whether the children were high- or low-income and whether their parents were likely foreign-born or not. High- vs. low-income children are categorized based on whether they qualified for free lunch (FL) at school entry. Children with likely foreign-born vs. likely U.S.-born parents are classified by whether they were designated as English Language Learners (ELL) at school entry. The sample is shown in the column headings. Pre-period trends are estimated and removed from all observations for each county prior to estimation. See Table 3 for more detail on the specification, sample restrictions, and full set of controls. Standard errors in parentheses are clustered at county of birth level. *p<0.10, ** p<0.05, *** p<0.01.

Table 5: The Impact of Prenatal MMC Exposure on Academic Achievement, Likely Medicaid Eligible Sample: Heterogeneity Across School Characteristics

	Average Reading Test Scores		Overall District Value-Added		Black District Value-Added		Hispanic District Value-Added	
	Low	High	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Black Students								
Prenatal MMC	-0.040***	-0.031*	-0.040***	-0.031**	-0.040***	-0.025*	-0.035***	-0.045***
	(0.011)	(0.016)	(0.011)	(0.014)	(0.012)	(0.013)	(0.012)	(0.012)
Mean (Y)	-0.510	-0.375	-0.501	-0.403	-0.525	-0.398	-0.480	-0.463
N	591,482	220,638	575,297	251,113	472,370	341,848	415,651	395,333
Hispanic Students								
Prenatal MMC	0.009	0.001	0.015**	0.004	0.017**	0.019**	0.024***	-0.004
	(0.008)	(0.011)	(0.008)	(0.011)	(0.009)	(0.009)	(0.008)	(0.011)
Mean (Y)	-0.236	-0.198	-0.255	-0.150	-0.261	-0.178	-0.248	-0.170
N	940,777	393,012	943,024	422,428	668,413	547,831	913,599	445,137
White Students								
Prenatal MMC	-0.022	-0.016	-0.016	-0.018	-0.018	-0.011	-0.008	-0.024*
	(0.014)	(0.012)	(0.011)	(0.013)	(0.013)	(0.013)	(0.012)	(0.012)
Mean (Y)	-0.144	-0.084	-0.124	-0.064	-0.111	-0.085	-0.105	-0.098
N	264,180	369,058	410,144	255,788	275,253	271,737	346,442	285,720

Note: This table shows difference-in-differences estimates of the impact of prenatal MMC exposure on reading test scores between 3rd and 8th grade. Within each panel, each column reports estimates of β from a separate regression of Equation 2. The sample for these regressions are children conceived in Texas between 1993 and 2001 who are likely Medicaid eligible. Pre-period trends are estimated and removed from all observations for each county prior to estimation. See Table 3 for more detail on the specification, sample restrictions, and full set of controls. Value-added is measured by regressing average standardized test scores on lagged test scores, indicators for a student's gender, SpEd status, ELL status, and economic disadvantage. For Black and Hispanic value-added measures only Black or Hispanic children are used. Districts are split according to the median of average test scores and value-added measures, where those below the median are labelled "Low" and those above are labelled "High". Standard errors in parentheses are clustered at county of birth level. *p<0.10, ** p<0.05, *** p<0.01.

Table 6: The Impact of Prenatal MMC Exposure on Educational Attainment, Likely Medicaid Eligible Sample

	Completed High School	College Enrolled Any	2-Year	4-Year	Reading	Math
Black Students						
Prenatal MMC	-0.004	-0.003	0.001	-0.009	-0.033	-0.010
	(0.014)	(0.013)	(0.008)	(0.008)	(0.023)	(0.029)
Mean (Y)	0.729	0.376	0.274	0.117	-0.533	-0.611
N	59,718	59,718	59,718	59,718	303,880	303,536
Hispanic Students						
Prenatal MMC	0.026**	0.029*	0.022	0.000	0.006	0.017
	(0.012)	(0.016)	(0.013)	(0.005)	(0.012)	(0.014)
Mean (Y)	0.727	0.358	0.277	0.103	-0.241	-0.265
N	111,361	111,361	111,361	111,361	587,240	587,711
White Students						
Prenatal MMC	-0.004	-0.010	-0.000	-0.002	0.015	0.002
	(0.019)	(0.015)	(0.009)	(0.009)	(0.017)	(0.019)
Mean (Y)	0.659	0.287	0.223	0.079	-0.142	-0.148
N	59,735	59,735	59,735	59,735	297,038	297,140

Note: This table shows difference-in-differences estimates of the impact of prenatal MMC exposure on educational attainment outcomes. All long-run outcomes are indicator variables, and are measured as of 2 years after each student's expected high school graduation (assuming normal grade progression after the year a child enters Kindergarten). Within each panel, each column reports estimates of β from a separate regression of Equation 2. The sample for these regressions are children conceived in Texas between 1993 and 1997 who are likely Medicaid eligible. Pre-period trends are estimated and removed from all observations for each county prior to estimation. See Table 3 for more detail on the sample, specification and full set of controls. Outcome year fixed effects are omitted, and the individual controls are measured as of 9th grade. Standard errors in parentheses are clustered at county of birth level. *p<0.10, ** p<0.05, *** p<0.01.

Table 7: Spillover Effects of MMC on Reading Test Scores

	Low-Income US Mom	Low-Income FB Mom	High-Income US Mom	High- Income FB Mom
Black Students				
MMCShareBlack	-0.065***	-0.754**	-0.091*	-0.487
	(0.012)	(0.309)	(0.049)	(0.442)
ES (Avg Student)	-0.018	-0.098	-0.016	-0.048
ES (5% increase)	{-0.003}	{-0.038}	{-0.005}	{-0.024}
Mean (Y)	-0.469	0.061	-0.048	0.189
Avg Exposure	0.274	0.130	0.174	0.099
N	845,420	5,679	464,959	4,429
Hispanic Students				
MMCShareBlack	-0.045	-0.107***	-0.108*	-0.044
	(0.038)	(0.031)	(0.059)	(0.149)
ES (Avg Student)	-0.003	-0.009	-0.004	-0.001
ES (5% increase)	{-0.002}	{-0.005}	{-0.005}	{-0.002}
Mean (Y)	-0.220	-0.343	0.140	-0.182
Avg Exposure	0.063	0.087	0.035	0.026
N	1,386,824	1,449,716	1,457,381	823,227
White Students				
MMCShareBlack	-0.066	0.058	-0.197***	-0.016
	(0.058)	(0.135)	(0.053)	(0.172)
ES (Avg Student)	-0.004	0.006	-0.009	-0.001
ES (5% increase)	{-0.003}	{0.003}	{-0.010}	{-0.001}
Mean (Y)	-0.101	-0.215	0.404	0.235
Avg Exposure	0.067	0.104	0.045	0.054
N	669,530	9,415	2,828,364	12,069

Note: This table shows difference-in-differences estimates of the spillover effects of MMC exposed Black peers on reading test scores between 3rd and 8th grade. Within each panel, each column reports estimates of δ from a separate regression of Equation 4. The sample for these regressions are students conceived in Texas between 1993 and 2001. The sample is shown in the column headings. Pre-period trends are estimated and removed from all observations for each county prior to estimation. See Table 3 for more detail on the specification, sample restrictions and set of controls. In addition to those controls, school by grade and grade by year fixed effects are additionally included. Standard errors in parentheses are clustered at county of birth level. *p<0.10, ** p<0.05, *** p<0.01.

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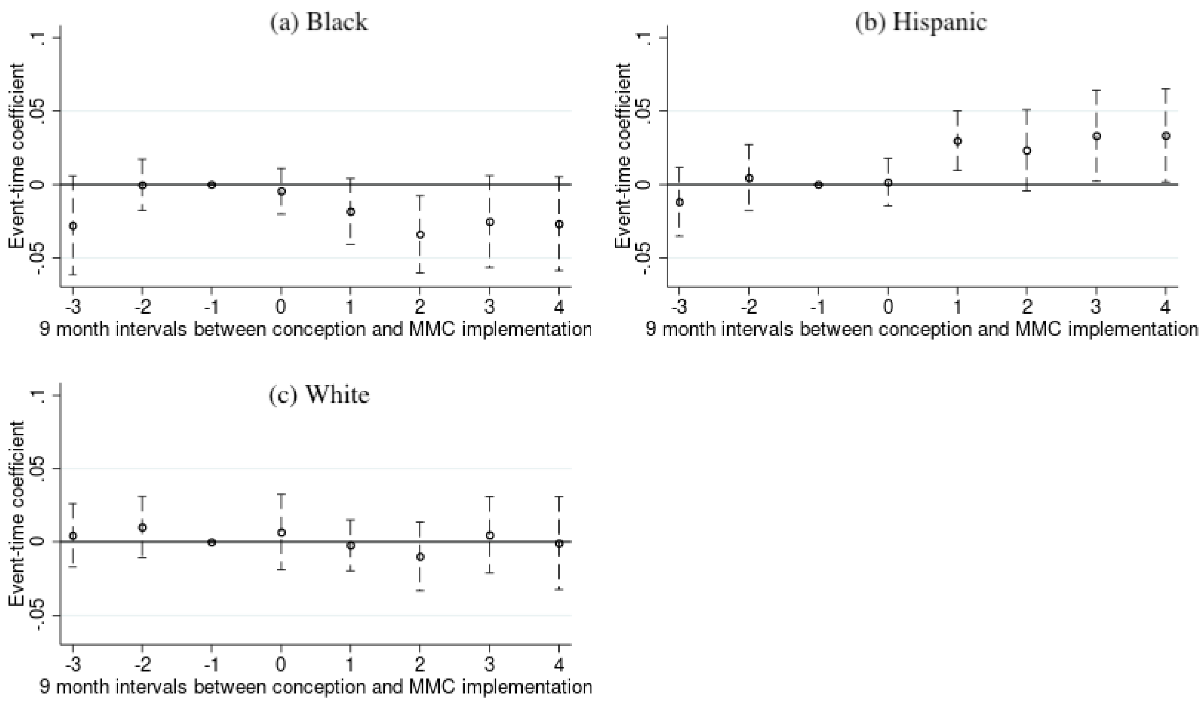
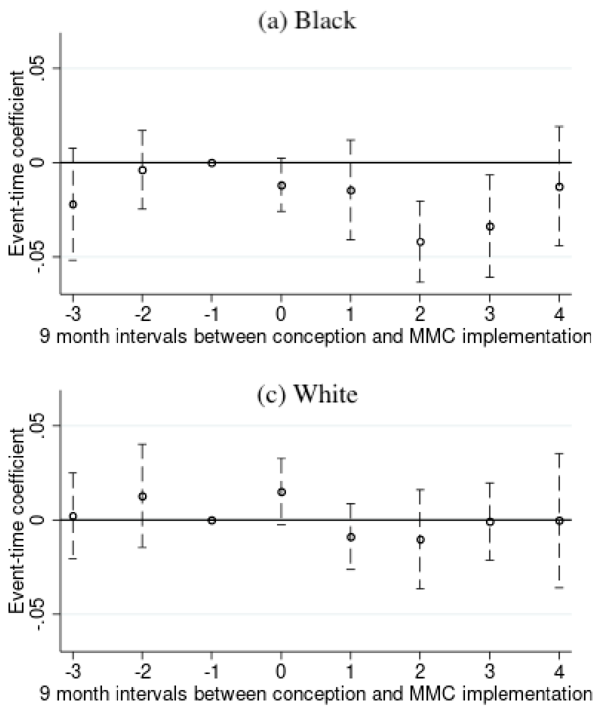
A Appendix

Figure A.1: The Impact of MMC at Birth on Reading Test Scores, Likely Medicaid Eligible



Note: This plot shows event-study estimates and 95% confidence intervals of the impact of MMC exposure on reading test scores between 3rd and 8th grade. The estimates come from Equation 3. Negative values on the x-axis indicate conceptions before MMC's implementation. The omitted category is the group conceived 9 months before the MMC transition (i.e. at -1), and were the last group fully exposed to FFS prenataly. Those conceived within the 9 months before MMC was implemented are partially exposed (i.e. at 0) and children conceived 9 months after MMC's implementation were fully exposed to MMC. The sample for these regressions includes likely Medicaid eligible students born in Texas between 1993 and 2001. Likely Medicaid eligible children are defined as children who qualified for free-lunch at school entry and were not an ELL participant at school entry. The results are separated by race, where Black children are shown in Panel A, Hispanic children are shown in Panel B, and White children are shown in Panel C. The model includes fixed effects for county of birth, year by month of birth, and the year the outcome is measured. Individual demographic controls include gender and special education status. County by year and month of birth controls include log population, log per capita income, log per capita transfers and the unemployment rate. Cohort-level controls include the share of children born in the same year, month, and county who are Hispanic, Black, White, FL, and ELL, as well as the total number of births. School level controls for policy pressures related to SpEd students and ELL students are included. Standard errors are clustered by county of birth.

Figure A.2: The Impact of MMC at Birth on Math Test-Score, Likely Medicaid Eligible



Note: This plot shows event-study estimates and 95% confidence intervals of the impact of MMC exposure on math test scores between 3rd and 8th grade. The estimates come from Equation 3. Negative values on the x-axis indicate conceptions before MMC's implementation. The omitted category is the group conceived 9 months before the MMC transition (i.e. at -1), and were the last group fully exposed to FFS prenataly. Those conceived within the 9 months before MMC was implemented are partially exposed (i.e. at 0) and children conceived 9 months after MMC's implementation were fully exposed to MMC. The sample for these regressions include likely Medicaid eligible students born in Texas between 1993 and 2001. Likely Medicaid eligible children are defined as children who qualified for free-lunch at school entry and were not an ELL participant at school entry. The results are separated by race, where Black children are shown in Panel A, Hispanic children are shown in Panel B, and White children are shown in Panel C. See Appendix Figure A.1 for more detail on the full set of controls. Standard errors are clustered by county of birth.

Table A.1: Texas County rollout of Managed Care

FY	Date	Counties	Total Medicaid Managed Care Enrollment	% of Medicaid Population in Managed Care
1994	Aug 1993	Travis		
	Dec 1993	Chambers Jefferson Galveston	58,243	2.90%
1996	Dec 1995	Liberty Hardin Orange	71,435	3.50%
1997	Sep 1996	Bexar Atascosa Comal Guadalupe Kendall Medina Wilson Bandera Bastrop Burnet Caldwell Hays Lee Williamson Fayette	274,694	
1997	Oct 1996	Tarrant Wise Denton Parker Hood Johnson Lubbock Crosby Floyd Garza Hale Hockley Lamb Lynn Terry Carson Deaf Smith Hutchinson Potter Randall Swisher	274,694	13.80%
1998	Mar 1998	Harris Brazoria Fort Bend Waller Montgomery	364,336	19.60%
1999	July 1999	Dallas Collin Ellis Hunt Kaufman Navarro Rockwall	425,069	23.50%
2000	Dec 1999	El Paso Hudspeth	523,832	29.00%
2007	Sep 2006	Nueces Aransas Bee Calhoun Jim Wells Kleberg Refugio San Patricio Victoria Brooks Goliad Karnes Kenedy Live Oak	1,277,400	44.97%

Note: This table provides the schedule of when each county transitioned to MMC. This information was obtained from Chapter 6 of the 8th Edition of "Texas Medicaid and CHIP in Perspective" produced by the Texas Health and Human Services Commission in January 2011 and Appendix A of the 14th Edition of the "Texas Medicaid and CHIP Reference Guide" produced by the Texas Health and Human Services Commission in 2022.

Table A.2: The Determinants of When MMC was Implemented

DV: Year MMC Implemented	
Economic Conditions (1993)	
Log Population	-28.80***
	(7.675)
Log Income	-125.7**
	(53.15)
Log Transfers	67.66*
	(37.19)
Unemployment	221.2
	(241.1)
Proportion of Births (1993)	
Hispanic	-60.31*
	(35.04)
Black	11.43
	(52.75.98)
Free-Lunch Participants	-25.97
	(37.47)
English Language Learners	54.92
	(64.72)
Special Education	-12.28
	(47.40)
Observations	184
R-squared	0.235

Note: This table shows results from a regression that regresses the year and month of MMC implementation on county-level characteristics. *p<0.10, ** p<0.05, *** p<0.01.

Table A.3: Healthcare Utilization under Traditional Fee-For Service vs. under Managed Care

Medicaid Service Area	FFS 1996	HMO 1997	HMO 1998	PCCM 1997	PCCM 1998
Panel A: ER Visits/1000 Member Months by Medicaid Service Area					
Bexar	76.1	36.1	43.2	49.3	59.9
Lubbock	96.8	44.8	47.4	60.2	70.6
Tarrant	91.7	45.9	63.2		
Travis		48.8	66.2		
Panel B: Average Length of Stay (Inpatient Hospital Days) by SDA					
Bexar	6.4	2.9	2.1	2.2	3
Lubbock	8.8	2.9	2.7	2.5	2.8
Tarrant	3.8	2.4	2.5		
Travis		3	2.5		

Note: This table reports differences in healthcare utilization under fee-for-service (FFS), Health Maintenance Organizations (HMOs), and Primary Care Case Management (PCCM). Information was obtained from Chapter 7 of "Medicaid Managed Care Review" produced by the Texas Health and Human Services Commission in 2001.

Table A.4: Birth Cohorts and Years of Expected Enrollment

Birth Year	Expected Year of Enrollment in Grade		Years after HS
	1	12	Before 2019
1993	2000/01 or 2001/02	2011/12 or 2012/13	6-7
1994	2001/02 or 2002/03	2012/13 or 2013/14	5-6
1995	2002/03 or 2003/04	2013/14 or 2014/15	4-5
1996	2003/04 or 2004/05	2014/15 or 2015/16	3-4
1997	2004/05 or 2005/06	2015/16 or 2016/17	2-3
1998	2005/06 or 2006/07	2016/17 or 2017/18	1-2
1999	2006/07 or 2007/08	2017/18 or 2018/19	0-1
2000	2007/08 or 2008/09	2018/19 or 2019/20	0
2001	2008/09 or 2009/10	2019/20 or 2020/21	0

Note: This table shows the expected year of 1st and 12th grade enrollment for each birth cohort in my sample. In addition, it shows the number of years after expected 12th grade enrollment and 2019 when long-run outcomes can be measured.

Table A.5: Representativeness of the Texas Born Sample

	Full population of TX students (1)	Population of kids Matched to Texas Birth Index (2)	Population of kids Unmatched to Texas Birth Index (3)
Black	0.139	0.143	0.128
	(0.346)	(0.350)	(0.334)
Hispanic	0.458	0.450	0.484
	(0.498)	(0.497)	(0.500)
White	0.368	0.381	0.324
	(0.482)	(0.486)	(0.468)
Female	0.511	0.514	0.501
	(0.500)	(0.500)	(0.500)
Special Education Participant	0.101	0.105	0.0887
	(0.302)	(0.307)	(0.284)
Free-Lunch	0.411	0.398	0.454
(At Baseline)	(0.492)	(0.489)	(0.498)
Free or Reduced Lunch	0.490	0.477	0.527
(At Baseline)	(0.500)	(0.499)	(0.499)
Not Free or Reduced Lunch	0.510	0.523	0.473
(At Baseline)	(0.500)	(0.499)	(0.499)
Standardized Reading Scores	0.0208	0.00596	0.0833
	(0.988)	(0.994)	(0.962)
Standardized Math Scores	0.0182	0.00107	0.0902
	(0.994)	(0.998)	(0.973)
Immigrant	0.0548	0.00430	0.213
	(0.228)	(0.0654)	(0.410)
Observations	2,932,626	2,222,896	709,730

Notes: The first column presents fractions in the population of Texas public school students enrolled in 3rd grade between 2001 and 2009. The second column presents fractions in the population of Texas public school students enrolled in 3rd grade between 2001 and 2009 who were born in Texas (i.e. matched to the Texas Birth Index). The third column presents fractions in the population of Texas public school students enrolled in 3rd grade between 2001 and 2009 who were not born in Texas (i.e. not matched to the Texas Birth Index).

Table A.6: The Impact of the MMC on County-Level Characteristics

County Level Outcome	Mean	Estimated Effect		
		(1)	(2)	(3)
<u>Economic Conditions</u>				
Log Population	-0.497	0.012	0.001	0.002
		(0.011)	(0.004)	(0.002)
Log Income	0.664	0.033***	0.007	0.000
		(0.010)	(0.006)	(0.004)
Log Transfers	-1.082	-0.013**	-0.031***	-0.002
		(0.006)	(0.008)	(0.004)
Unemployment	0.062	0.010	0.009***	0.001
		(0.006)	(0.003)	(0.005)
<u>Proportion of Births</u>				
Hispanic	0.440	0.021**	0.011	0.012
		(0.008)	(0.007)	(0.008)
White	0.429	-0.017**	-0.007	-0.005
		(0.007)	(0.004)	(0.005)
Black	0.114	-0.006*	-0.005**	-0.006**
		(0.003)	(0.003)	(0.003)
English Language Learner	0.155	0.028***	0.009	0.011
		(0.010)	(0.010)	(0.011)
Free-Lunch Participant	0.484	0.029	-0.006	0.001
		(0.020)	(0.009)	(0.010)
County-by-month (N)		10,255	10,255	10,255
Specification Choice				
De-Trend Procedure			X	
County-Specific Time Trends				X

Notes: This table shows difference-in-difference estimates of the impact of MMC on county-level economic conditions and demographics. The dependent variable is shown in the first column. Each column reports estimates from county level regressions, which regress county level exposure (i.e. MMC_{ymc}), on each of the dependent variables. Controls include county and year by month fixed effects. Data from the years 1993 and 2001 are used in these regressions. Column 1 shows the estimates without any adjustments, Column 2 removes county-specific pre-policy linear trends in the outcomes before estimation, and Column 3 includes county-specific time trends. For Column 2, pre-period trends are estimated and removed from all observations for each county prior to estimation. *p<0.10, ** p<0.05, *** p<0.01.

Table A.7: Kuziemko et al. (2018) Main Results: Effect of MMC on Birth Outcomes ($\times$ 100) for US-born Black and Hispanic Births

	Mortality		Pre-Term		LBW	
	Black	Hispanic	Black	Hispanic	Black	Hispanic
MMC	0.179	-0.154	0.976	-0.710	0.730	-0.00717
	(0.0786)	(0.0749)	(0.336)	(0.198)	(0.380)	(0.154)
Mean (Y)	1.198	0.715	13.51	9.593	12.72	7.334
N	296,589	646,053	296,589	646,053	296,584	646,051

Note: This table shows difference-in-differences estimates of the impact of MMC at birth on mortality, the likelihood of being Pre-term, and low-birth weight (LBW) denoted as birth weight $< 2,500$ g. Within each panel, each column reports estimates of ζ from a separate regression of Equation 1. These results are taken from Tables 3 and 4 of Kuziemko et al. (2018).

Table A.8: The Impact of MMC on County-Level Match Rate

County Level Outcome	Mean	Estimated Effect	
		(1)	(2)
Share Matched	0.797	0.003	0.006
		(0.006)	(0.006)
County-by-month (N)		10,255	10,255
Controls			
County$\times$Year $\times$Month Controls			X

Notes: This table shows the difference-in-difference estimates of the impact of MMC on the share of the county that was matched to the administrative schooling records. The dependent variable is computed by taking the number of children born in each county in a given year and month who were matched to the Texas Education Agency (TEA) data and dividing it by the total number of children born in that county in the same year and month. Each column reports estimates from county-level regressions, which regress county-level exposure (i.e., MMC_{ymc}) on the dependent variable. Controls include county and year-by-month fixed effects. Data from the years 1993 and 2001 are used in these regressions. Column 1 shows the estimates without any additional controls, and Column 2 includes county $\times$ year $\times$ month of birth controls. See Table 3 for more details on the control variables. *p<0.10, ** p<0.05, *** p<0.01.

Table A.9: The Impact of MMC on Baseline Student Characteristics

	Economic Disadvantaged		Female		English Language Learner		Special Education	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Full Sample								
MMC	0.013	-0.004	0.002	0.000	0.028***	0.003	0.002	-0.001
	(0.009)	(0.003)	(0.001)	(0.001)	(0.010)	(0.003)	(0.002)	(0.001)
Mean (Y)	0.605	0.605	0.513	0.513	0.235	0.235	0.096	0.096
N	2,052,560	2,051,515	2,052,560	2,051,515	2,052,560	2,051,515	2,052,560	2,051,515
Panel B: Black Students								
MMC	-0.001	-0.001	0.002	-0.004	0.003***	0.001	0.003	-0.001
	(0.005)	(0.004)	(0.005)	(0.005)	(0.001)	(0.001)	(0.003)	(0.002)
Mean (Y)	0.738	0.738	0.511	0.511	0.010	0.010	0.107	0.107
N	281,454	281,295	281,454	281,295	281,454	281,295	281,454	281,295
Panel C: Hispanic Students								
MMC	0.006	-0.001	0.001	-0.001	0.026**	0.008	0.004	-0.000
	(0.008)	(0.003)	(0.001)	(0.002)	(0.011)	(0.007)	(0.003)	(0.002)
Mean (Y)	0.803	0.803	0.509	0.509	0.444	0.444	0.089	0.089
N	1,015,748	1,015,216	1,015,748	1,015,216	1,015,748	1,015,216	1,015,748	1,015,216
Controls								
County×Year×Month		X		X		X		X
$f(t)\times$ ELL		X		X		X		X
$f(t)\times$ SpEd Rate		X		X		X		X

Notes: This table shows difference-in-difference estimates of the impact of prenatal MMC exposure on student demographics. Within each panel, each column reports estimates of β from a separate regression of Equation 2. The sample for these regressions are students conceived in Texas between 1993 and 2001. The dependent variable is shown in the column headings. Panel A includes estimates for the full sample, Panel B includes estimates for Black students, and Panel C includes estimates for Hispanic students. Odd-numbered columns include fixed effects for county of birth, year by month of birth, and the year the outcome is measured. Even-numbered columns additionally include county by year and month of birth controls. See Table 3 for more detail on the specification, sample restrictions, and full set of control variables. Standard errors in parentheses are clustered at the county of birth level. *p<0.10, ** p<0.05, *** p<0.01.

Table A.10: The Impact of MMC on Racial Composition

	Hispanic		Black		White	
	(1)	(2)	(3)	(4)	(5)	(6)
Full Sample						
MMC	0.020**	0.003**	-0.006*	-0.001	-0.016**	-0.003***
	(0.008)	(0.001)	(0.003)	(0.001)	(0.007)	(0.001)
Mean (Y)	0.495	0.495	0.137	0.137	0.340	0.340
N	2,052,560	2,051,515	2,052,560	2,051,515	2,052,560	2,051,515
Controls						
County$\times$Year $\times$Month		X		X		X
$f(t)\times$ ELL		X		X		X
$f(t)\times$ SpEd Rate		X		X		X

Notes: This table shows difference-in-difference estimates of the impact of prenatal MMC exposure on student demographics. Within each panel, each column reports estimates of β from a separate regression of Equation 2. The sample for these regressions are students conceived in Texas between 1993 and 1997. The dependent variable is shown in the column headings. Odd-numbered columns include fixed effects for county of birth, year by month of birth, and the year the outcome is measured. Even-numbered columns additionally include county by year and district level controls. See Table 3 for more detail on the specification, sample restrictions, and full set of control variables. Standard errors in parentheses are clustered at the county of birth level. *p<0.10, ** p<0.05, *** p<0.01.

Table A.11: The Impact of Prenatal MMC on Academic Achievement, Gardner (2021) estimator

	Black		Hispanic		White	
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	Gardner	Baseline	Gardner	Baseline	Gardner
Reading Test Scores						
Prenatal MMC	-0.038***	-0.040***	0.011	0.008	-0.016	-0.013
	(0.010)	(0.013)	(0.012)	(0.010)	(0.011)	(0.008)
Mean (Y)	-0.450	-0.450	-0.260	-0.260	-0.101	-0.101
N	847,597	847,597	1,388,187	1,388,187	671,579	671,579
Math Test Scores						
Prenatal MMC	-0.026**	-0.019*	-0.001	0.001	-0.016	-0.013
	(0.011)	(0.011)	(0.012)	(0.010)	(0.012)	(0.010)
Mean (Y)	-0.553	-0.553	-0.300	-0.300	-0.154	-0.154
N	839,131	839,131	1,369,010	1,369,010	664,287	664,287

Notes: This table shows estimates of the impact of prenatal MMC exposure on academic achievement. Within each panel, each odd column reports estimates of β from a separate regression of Equation 2. Within each panel, each even column reports estimates from the Gardner (2021) estimator. This estimator first estimates group and period effects on the sample of untreated observations. Next, treatment effects are identified by comparing treated and untreated children after removing the group and period fixed effects. The full set of controls are included in both the first and second stage. The sample for these regressions are students conceived in Texas between 1993 and 1997 who qualified for free-lunch (FL) and were not classified as an English language learner (ELL) at school entry. The subsample of either Black, Hispanic or White students is shown in the column headings. See Table 3 for more detail on the specification in odd columns and the control variables utilized in both odd and even columns. Standard errors in parentheses are clustered at the county of birth level. *p<0.10, ** p<0.05, *** p<0.01.

Table A.12: The Impact of Prenatal and Childhood MMC Exposure on Academic Achievement, Likely Medicaid Eligible Sample

		Grade					
	Pooled	3	4	5	6	7	8
Black Students							
Reading Test Scores							
Prenatal MMC	-0.045***	-0.034	-0.037*	-0.069***	-0.048***	-0.039*	-0.033*
	(0.013)	(0.020)	(0.021)	(0.013)	(0.014)	(0.021)	(0.017)
Ages 0-5 MMC	0.021	0.002	0.030	0.037	0.001	0.051	0.059
	(0.032)	(0.063)	(0.037)	(0.045)	(0.073)	(0.069)	(0.044)
Mean (Y)	-0.469	-0.463	-0.480	-0.466	-0.440	-0.470	-0.494
N	847,597	143,499	142,025	138,231	144,548	144,673	134,621
Math Test Scores							
Prenatal MMC	-0.040***	-0.032	-0.076***	-0.087***	-0.025	-0.006	-0.001
	(0.011)	(0.021)	(0.018)	(0.012)	(0.018)	(0.027)	(0.031)
Ages 0-5 MMC	0.001	-0.025	-0.051	-0.005	-0.022	0.029	-0.025
	(0.052)	(0.041)	(0.063)	(0.086)	(0.078)	(0.084)	(0.049)
Mean (Y)	-0.557	-0.561	-0.563	-0.569	-0.558	-0.568	-0.519
N	839,131	142,821	142,695	139,705	144,656	145,074	124,180
Hispanic Students							
Reading Test Scores							
Prenatal MMC	0.003	0.003	0.018	0.005	-0.008	-0.012	0.007
	(0.011)	(0.011)	(0.015)	(0.018)	(0.019)	(0.015)	(0.014)
Ages 0-5 MMC	0.038	0.098***	0.027	0.028	0.010	0.032	0.030
	(0.025)	(0.020)	(0.034)	(0.044)	(0.042)	(0.036)	(0.023)
Mean (Y)	-0.220	-0.199	-0.217	-0.222	-0.224	-0.232	-0.227
N	1,388,187	228,116	229,993	227,303	238,448	238,480	225,847
Math Test Scores							
Prenatal MMC	-0.008	-0.016	-0.013	-0.014	0.005	-0.017	0.003
	(0.012)	(0.016)	(0.014)	(0.021)	(0.020)	(0.018)	(0.024)
Ages 0-5 MMC	0.001	0.053**	-0.011	0.001	-0.039	-0.015	-0.036
	(0.029)	(0.024)	(0.040)	(0.052)	(0.040)	(0.037)	(0.039)
Mean (Y)	-0.243	-0.225	-0.229	-0.236	-0.252	-0.266	-0.248
N	1,369,010	227,960	231,199	228,458	238,879	239,195	203,319

Note: This table shows difference-in-differences estimates of the impact of MMC at birth on reading and math test scores between 3rd and 8th grade. Within each panel, each column reports estimates of β from a separate regression of a slightly modified version of Equation 2. Rather than a simple indicator for prenatal exposure, three separate treatment variables are include: Share of prenatal period exposed to MMC and share of time between ages 0-5 exposed to MMC. The sample for these regressions are Black or Hispanic students conceived in Texas between 1993 and 2001 who qualified for free-lunch (FL) and were not classified as an English language learner (ELL) at school entry. Pre-period trends are estimated and removed from all observations for each county prior to estimation. See notes to Table 3 for details about the specification, sample restrictions and the full set of controls. Standard errors in parentheses are clustered at county of birth level. *p<0.10, ** p<0.05, *** p<0.01.

Table A.13: Impacts of Prenatal MMC Exposure on Academic Achievement, Likely Medicaid Eligible: Lee Bounding Exercise

	(1)	(2)	(3)	(4)	(5)	(6)
	Black			Hispanic		
	Baseline	Bounded Above	Bounded Below	Baseline	Bounded Above	Bounded Below
Panel A: Reading Scores						
Prenatal MMC	-0.038***	-0.042***	-0.032***	0.011	0.008	0.015
	(0.010)	(0.010)	(0.010)	(0.012)	(0.012)	(0.012)
Mean (Y)	-0.469	-0.470	-0.467	-0.220	-0.221	-0.219
N	847,597	846,983	847,131	1,388,187	1,387,493	1,387,644
Panel B: Math Scores						
Prenatal MMC	-0.026**	-0.030**	-0.024**	-0.001	-0.004	0.000
	(0.011)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
Mean (Y)	-0.557	-0.542	-0.540	-0.243	-0.226	-0.224
N	839,131	815,421	815,523	1,369,010	1,332,759	1,332,866

Note: This table shows difference-in-differences estimates of the impact of MMC at birth on reading and math test scores between 3rd and 8th grade. Within each panel, each column reports estimates of β from a separate regression of Equation 2. The sample for these regressions are Black or Hispanic students conceived in Texas between 1993 and 2001 who qualified for free-lunch (FL) and were not classified as an English language learner (ELL) at school entry. See notes to Table 3 for details about the specification, sample restrictions, and the full set of controls. Columns 1 and 4 present the baseline estimates. Columns 2 and 5 show the results where the highest achievers are dropped. Columns 3 and 6 show the results where the lowest achievers are dropped. The results for Black children are shown in Columns 1-3, and for Hispanic children in Columns 4-6. For Black (Hispanic) children 0.2% (0.14%) of observations in each birth cohort born after MMC with the highest or lowest test scores are dropped. *p<0.10, ** p<0.05, *** p<0.01.

Table A.14: The Impact of Prenatal MMC Exposure on Academic Achievement, Likely Medicaid Eligible: Alternative Sample Restrictions

	(1) Baseline	(2) Houston Dropped	(3) STAAR Dropped	(4) Consistently Enrolled	(5) Consistently Tested
Black Students					
Reading Test Scores					
Prenatal MMC	-0.038***	-0.039***	-0.024*	-0.042***	-0.050***
	(0.010)	(0.015)	(0.013)	(0.011)	(0.011)
Mean (Y)	-0.469	-0.490	-0.434	-0.448	-0.351
N	847,597	582,561	565,159	772,096	615,522
Math Test Scores					
Prenatal MMC	-0.026**	-0.027	-0.011	-0.030**	-0.035***
	(0.011)	(0.021)	(0.015)	(0.012)	(0.012)
Mean (Y)	-0.557	-0.560	-0.543	-0.534	-0.475
N	839,131	576,688	564,766	764,300	573,415
Hispanic Students					
Reading Test Scores					
Prenatal MMC	0.011	0.019	0.008	0.012	0.014
	(0.012)	(0.013)	(0.011)	(0.012)	(0.012)
Mean (Y)	-0.220	-0.219	-0.184	-0.205	-0.123
N	1,388,187	1,166,074	887,510	1,280,569	1,062,045
Math Test Scores					
Prenatal MMC	-0.001	0.004	0.001	-0.001	0.004
	(0.012)	(0.013)	(0.015)	(0.012)	(0.014)
Mean (Y)	-0.243	-0.239	-0.215	-0.225	-0.191
N	1,369,010	1,150,723	888,830	1,262,599	964,199

Note: This table shows difference-in-differences estimates of the impact of MMC at birth on reading and math test scores between 3rd and 8th grade. Within each panel, each column reports estimates of β from a separate regression of Equation 2. The sample for these regressions are Black or Hispanic students conceived in Texas between 1993 and 2001 who qualified for free-lunch (FL) and were not classified as an English language learner (ELL) at school entry. Pre-period trends are estimated and removed from all observations for each county prior to estimation. See Table 3 for more detail on the sample, specification and full set of controls. Sample restrictions are shown in the column headers. Standard errors in parentheses are clustered at county of birth level. *p<0.10, ** p<0.05, *** p<0.01.

Table A.15: The Impact of Prenatal MMC Exposure on Special Education Placement, Likely Medicaid Eligible Sample

	(1)	(2)	(3)
	Black	Hispanic	White
Panel A: Detrended by County			
Prenatal MMC	0.002	0.004	0.003
	(0.002)	(0.003)	(0.003)
Mean (Y)	0.055	0.027	0.061
N	975,194	1,538,696	758,898
Panel B: Detrended by County and Race			
Prenatal MMC	0.008**	0.005	0.002
	(0.004)	(0.004)	(0.003)
Mean (Y)	0.025	0.028	0.067
N	975,194	1,538,696	758,898

Note: This table shows difference-in-differences estimates of the impact of MMC at birth on special education participation between 3rd and 8th grade. Within each panel, each column reports estimates of β from a separate regression of Equation 2. The sample for these regressions are Black, Hispanic or White students conceived in Texas between 1993 and 2001 who were likely Medicaid eligible (i.e. qualified for free-lunch (FL) and not classified as an English language learner (ELL) at school entry). Panel A presents results that remove linear pre-trends at the county level, while Panel B presents results that remove linear pre-trends at the county and race level. See notes to Table 3 for details about the specification, sample restrictions and full set of controls. *p<0.10, ** p<0.05, *** p<0.01.

Table A.16: The Impact of Prenatal MMC Exposure on Noncognitive Outcomes, Likely Medicaid Eligible Sample

	(1)	(2)	(3)
	Black	Hispanic	White
Panel A: Elementary and Middle School (Grades 3-8)			
Prenatal MMC	-0.013	-0.041**	-0.000
	(0.016)	(0.016)	(0.017)
N	975,194	1,538,696	758,898
Panel B: High School (Grades 9-12)			
Prenatal MMC	0.044**	-0.043***	0.005
	(0.018)	(0.016)	(0.014)
N	57,281	106,193	54,311

Note: This table shows difference-in-differences estimates of the impact of MMC on a summary index for noncognitive skills. As a proxy for noncognitive skills, I focus on absences, grade repetition and suspensions. Panel A focuses on outcomes between 3rd and 8th grade, while Panel B focuses on outcomes between 9th and 12th grade. Within each panel, each column reports estimates of β from a separate regression of Equation 2. The sample for regressions in Panel A are Black, Hispanic or White students conceived in Texas between 1993 and 2001 who qualified for free-lunch (FL) and were not classified as an English language learner (ELL) at school entry. The sample in Panel B is limited to conceptions between 1993 and 1997 who are likely Medicaid eligible. See notes to Table 3 for details about the specification, sample restrictions and the full set of controls. *p<0.10, ** p<0.05, *** p<0.01.

B Reconciling Kuziemko et. al (2018)

Kuziemko et al. (2018) find that prenatal MMC exposure improved Hispanic infant health while leading to declines in Black infant health. Although I lack data on infant health necessary to replicate their results in this paper, I closely follow their methodology and sample selection choices for my analysis.[1] Due to differences in our data and advancements in the difference-in-difference literature, my main specification deviates from theirs in three ways. However, these changes have little to no impact on the point estimates. Consequently, despite the minor adjustments I make to estimate the impacts of prenatal MMC on academic achievement, the estimates from Kuziemko et al. (2018) still provide a reliable first stage estimate for this project.

First, I have slightly updated the MMC rollout schedule to reflect the most recent information from DSHS. At the time Kuziemko et al. (2018) was written, the authors only had access to an incomplete MMC rollout schedule and filled in the missing information through correspondence with DSHS staff. Since then, the state has published a Medicaid reference guide (referred to as the "Pinkbook") that includes the complete MMC rollout dates. I use the MMC rollout dates from the 8th edition of the Pinkbook, which differ slightly from those used in Kuziemko et al. (2018). Importantly, the differences are minimal. Appendix Table B.1 shows that the estimates are very similar regardless of the MMC rollout schedule utilized: Column 1 uses the updated rollout dates, and Column 2 uses the old rollout dates, with no other changes to the sample or specification. Regardless of which version of the rollout dates is used, the results are nearly identical.

Second, my preferred specification includes control variables only available in administrative schooling records, such as special education status and other school district controls. Additionally, rather than using county-specific time trends as Kuziemko et al. (2018) did, I apply a de-trending procedure suggested by Goodman-Bacon (2021) to remove linear trends in the outcome variables. Recent literature has highlighted that county-specific time trends may over-control for time-varying treatment effects (e.g. Goodman-Bacon, 2021). Importantly, neither of these changes to the specification affect my point estimates.

Finally, Kuziemko et al. (2018) restrict their likely Medicaid sample to US-born mothers due to the absence of family income measures in the birth certificate records. In my study, I focus on both likely

[1] As previously noted, Kuziemko et al. (2018) utilize birth certificate data focusing on all children born in Texas between 1993 and 2001. Similarly, my project begins with the same population from the Texas Birth Index but narrows the focus to children who were subsequently enrolled in Texas public schools. Eighty percent of children born in Texas are matched to public school records, indicating a significant overlap in our samples.

US-born and low-income mothers, as they are more likely to qualify for Medicaid. Although the Texas birth certificate data used by Kuziemko et al. (2018) does not include family income, they argue that most births to Black and Hispanic mothers in Texas are to those who qualify for Medicaid. In my administrative schooling data, free-lunch (FL) eligibility matches Medicaid's income requirements during this period.

Column 3 of Appendix Table B.1 presents estimates focusing on the sample of likely US-born mothers to align with the sample restrictions made by Kuziemko et al. (2018). Compared to the baseline estimates in Column 1, these estimates are slightly smaller and less significant but quantitatively very similar. This is expected, as my baseline estimates focus on a higher impact sample – both likely US-born and low-income students. Finally, Column 4 of Appendix Table B.1 provides estimates using the same specification and sample restrictions as Kuziemko et al. (2018). That is, I exclude school-level control variables, incorporate county-specific time trends, do not employ the de-trending procedure proposed by Goodman-Bacon (2021), and concentrate on likely US-born mothers. The results and conclusions remain very similar to the baseline estimates presented in Column 1.[2]

[2]The one exception is that the point estimate for math test scores for Hispanic students is now negative and statistically significant, though the magnitude of the effect is small. This specification includes county-specific time trends, which may result in over-controlling and could explain this small but significant negative result.

Table B.1: The Impact of Prenatal MMC Exposure on Academic Achievement, Likely Medicaid Eligible: Alternative Sample and Specification Choices to match Kuziemko, Meckel, Rossin-Slater (KMR) (2018)

	(1)	(2)	(3)	(4)
	Baseline	KMR Rollout	KMR Sample	KMR Specification & KMR Sample
Black Students				
Reading Test Scores				
Prenatal MMC	-0.038***	-0.033***	-0.019**	-0.029***
	(0.010)	(0.011)	(0.008)	(0.007)
Mean (Y)	-0.469	-0.469	-0.306	-0.283
N	847,597	847,597	1,443,608	1,444,440
Math Test Scores				
Prenatal MMC	-0.026**	-0.024**	-0.014*	-0.030***
	(0.011)	(0.011)	(0.007)	(0.007)
Mean (Y)	-0.557	-0.558	-0.413	-0.402
N	839,131	839,131	1,425,918	1,426,740
Hispanic Students				
Reading Test Scores				
Prenatal MMC	0.011	0.009	-0.003	-0.010
	(0.012)	(0.011)	(0.009)	(0.008)
Mean (Y)	-0.220	-0.222	-0.029	-0.077
N	1,388,187	1,388,187	3,167,986	3,170,026
Math Test Scores				
Prenatal MMC	-0.001	-0.002	-0.006	-0.018***
	(0.012)	(0.011)	(0.008)	(0.005)
Mean (Y)	-0.243	-0.245	-0.077	-0.132
N	1,369,010	1,369,010	3,109,830	3,111,796

Note: This table shows difference-in-differences estimates of the impact of MMC at birth on reading and math test scores between 3rd and 8th grade. Within each panel, each column reports estimates of β from a separate regression of Equation 2. The sample consists of Black or Hispanic students conceived in Texas between 1993 and 2001. Columns 1-2 include students who qualified for free lunch (FL) and were not classified as English language learners (ELL) at school entry. Columns 3-4 include students who were not classified as ELL at school entry. Column 1 reports the baseline estimates, while Columns 2-4 introduce small modifications to the baseline specification or sample to align with KMR (2018). The specific changes are indicated in the column headers. Column 2 uses the same sample and specification as the baseline but employs MMC rollout dates from KMR (2018) instead of those in Appendix Table A.1. In Columns 1 -3 pre-period trends are estimated and removed from all observations for each county before estimation. In Column 4, county-specific time trends are included. Refer to Table 3 for detailed information on the full set of controls used in Columns 1-3. Column 4 employs the same controls, except for special education status, the share of children born in the same year, month, and county who are Hispanic, Black, White, FL, and ELL, the total number of births, and school-level controls, including baseline SpEd and ELL rates in each student's school, interacted with birth cohort time trends. Standard errors, shown in parentheses, are clustered at the county of birth level. *p<0.10, ** p<0.05, *** p<0.01.