The Value of Improving Insurance Quality: Evidence from Long-Run Medicaid Attrition*

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Abstract

The US government increasingly provides public health insurance coverage through private firms. We examine associated welfare implications for beneficiaries, using a novel 'revealed preference' framework based on beneficiaries' program attrition rates. Focusing on the Medicaid program in New York State, we exploit quasi-random variation in beneficiary initial assignment to public versus private Medicaid, based on birth weight. We find that infants assigned to private Medicaid at birth are less likely to subsequently leave Medicaid. We show that reduced attrition reflects beneficiary responses to improved program quality, rather than alternative mechanisms such as private Medicaid plans reducing reenrollment barriers.

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1 Introduction

Government contracting of public services to private firms is highly prevalent in the United States, accounting for 10% of GDP. In particular, private provision is increasingly common in the context of public health insurance in the US. Under Medicaid, the largest government-funded means-tested health insurance program, Medicaid beneficiaries increasingly receive coverage through private managed care plans that have been contracted by the government, rather than getting it directly through the public program. In 2016, over 80% of beneficiaries received Medicaid coverage through private managed care plans, a pronounced increase from only 10% in the early 1990s (Duggan and Hayford, 2013; Centers for Medicare & Medicaid Services, 2018). Government fiscal spending on private health plans also increased substantially, from $61 billion in 2007 to $269 billion in 2016, a figure that represents almost half of total Medicaid expenditures.

Despite this increased reliance on private Medicaid Managed Care (MMC) plans, the effects of Medicaid privatization on beneficiaries are not fully understood. First, the theoretical consequences of the two key features of Medicaid privatization-competition and capitation-are ambiguous; competition among plans for beneficiaries could lead to quality improvement, at the same time that plans' cost saving incentives under capitation could lead to quality deterioration (Hart, Shleifer, and Vishny, 1997). While this is consequently is an empirical question, limited empirical work has been done on care quality or beneficiary satisfaction, with most work instead focused on Medicaid privation's effects on government health care spending and coarse health outcomes, documenting mixed effects.[1,2] The limited empirical work on this question arises from two longstanding challenges. First, private Medicaid plan data has been difficult to obtain, making private Medicaid plans a black-box for researchers. Second, administrative claims data used in past studies has limited information on care quality, outcomes, or beneficiary satisfaction.

In this paper, we overcome these past challenges through a number of inno-

[1]See, for example, Krieger et al. (1992); Levinson and Ullman (1998); Conover et al. (2001); Duggan (2004); Howell et al. (2004); Kaestner et al. (2005); Aizer et al. (2007); Herring and Adams (2011); Duggan and Hayford (2013); Harman et al. (2014); Kuziemko et al. (2018).

[2]There are exceptions. See, for example, Layton et al. (2019).

vations, to ultimately identify the effects of Medicaid privatization on beneficiary well-being. Given that cost-sharing is effectively non-existent under both private and public Medicaid, our focus notably is on non-financial dimensions of well-being.

To start, we develop a novel approach to measuring beneficiary satisfaction, using a 'revealed preference' framework that is based off beneficiaries' relative likelihood of Medicaid attrition depending on whether they are initially assigned to a private Medicaid (MMC) plan or the public fee-for-service (FFS) option. Underlying our framework is the assumption is that beneficiaries are 'voting with their feet,' with higher attrition reflecting lower degree of satisfaction (Geruso et al., 2020). Given the high rate of program attrition observed for Medicaid as a whole, this constitutes an important policy issue in its own right; for example, about 25% of all Medicaid children historically left the program in a given year despite still being eligible (of those, about 45% had obtained new private insurance, while 55% became uninsured; Sommers, 2005). Furthermore, this also constitutes an important policy issue, given low overall Medicaid take-up rate of around 70% among those eligible nationwide, which falls below 50% for certain states, and below 35% for certain high need populations (Wright et al., 2017).[3]

Second, we combine this framework with plausibly exogenous variation in public versus private Medicaid (MMC) coverage assignment in New York: automatic enrollment in the public option for infants weighing below 1,200 grams, alongside mandated private Medicaid enrollment for those infants above that threshold. Finally, we leverage unique enrollment and claims data covering all New York infants in public as well as private Medicaid (MMC), and comprehensively tracking their claims across both programs. These data are individually linked to beneficiaries' original birth weights, and longitudinally track beneficiaries' Medicaid enrollment and utilization for up to seven years.

We empirically confirm the presence of a sharp discontinuity in private versus public Medicaid assignment at birth, around the 1200 gram threshold; those born above the threshold appear 49 percentage points more likely to be in private Med-

[3]Incidentally, attrition is also a major issue in our specific sample, as only about 54% of infants who received Medicaid at birth are enrolled in Medicaid by 24 months of age in our analysis sample.

icaid in the month of birth than infants born below the threshold. The enrollment discontinuity around the threshold persists through the first few months after birth, and then attenuates subsequently, in line with actual policy rules that lift the exclusion from private Medicaid at the age of six months.

Leveraging this quasi-random variation, we proceed to examine the implications of initial assignment to private versus public Medicaid on subsequent Medicaid program participation. We find strikingly large effects: getting assigned to private instead of public Medicaid at birth increases the probability of still being in Medicaid at age 4 by 37.7 percentage points, while increasing the cumulative time in Medicaid by 12.5 months over the first 48 months.

Lower attrition rates among those initially assigned to private as opposed to public Medicaid could plausibly be beneficiary-driven, and reflect greater beneficiary satisfaction under private Medicaid (MMC), with beneficiaries 'voting with their feet.' However, lower attrition could alternatively be driven by plans, thereby not necessarily reflecting increased beneficiary satisfaction with private Medicaid (MMC). We find evidence in support of beneficiary-driven explanations, such as observably better access to care and increased use of routine care in MMC in comparison to public Medicaid. As would be expected under this mechanism, we also find more pronounced attrition effects among families with greater expected sensitivity to care quality, such as those with relatively sicker infants or with mothers historically more attentive to health (based on proxies such as those enrolling in Medicaid earlier in their pregnancies).

We then examine alternative plan-driven mechanisms, under which attrition would not necessarily be reflective of beneficiary satisfaction, and proceed to rule these out one-by-one. One potential plan-driven mechanism could be reduced administrative costs of renewing Medicaid coverage, in comparison to public Medicaid. For example, private Medicaid plans could provide added reminders for beneficiaries to renew, streamline the reenrollment process for beneficiaries, or reduce administrative burdens of reenrollment in other ways. In reducing administrative burdens or pushing to retain beneficiaries, private health plans may focus in particular on lower cost beneficiaries, who could be more profitable for the plans to cover. To test these explanations, we begin by looking at the degree of advantageous se-

lection, and whether the attrition reduction is more pronounced for beneficiaries expected to be lower cost and more profitable. Inconsistent with this explanation, we find attrition reductions are actually greater among higher cost beneficiaries, that is, those with *high* costs of health care relative to capitation payments received.

To further rule out these alternative, plan-driven explanations for our effect, we employ an additional research design. Specifically, we leverage a difference-in-differences (DD) approach using the staggered roll out of private managed care enrollment mandates, on a county-by-county basis over time. With this approach, we find no evidence of advantageous selection by plans, as we again see more pronounced attrition reductions among lower birthweight infants (1200 to 1400 grams) who are on average less profitable. At the same time, we find that private versus public Medicaid exposure has no effect on future Medicaid enrollment among infants in all other birthweight ranges. These results are also inconsistent with another related, but distinct alternative explanation: systematically lower administrative costs of Medicaid reenrollment under private plans, relative to public plans. Given we would operationally expect lower administrative costs of reenrollment to extend to all newborns in a plan, we would likewise expect to see a corresponding attrition reduction for all newborns, and not just ones with lower birthweight as we actually observe. Taken together, our results suggest that the attrition effects are beneficiary rather than plan driven, and specifically reflect beneficiaries 'voting with their feet' and preferring private to public Medicaid.

Our study builds on existing literature on Medicaid privatization's impact on enrollment, notably Currie and Fahr (2005), which looks at how privatization impacts selection into Medicaid among low-income children. By contrast, we focus on Medicaid privatization's impact on selection out of Medicaid among existing beneficiaries, in part because of the arguably closer link between program attrition and beneficiary satisfaction. Focusing on attrition out of Medicaid additionally allows us to examine potential mechanisms, which is less feasible when looking at selection into Medicaid as Currie and Fahr (2005) did.

More broadly, our paper contributes to the literature on efficiency trade-offs in privatization of public health insurance programs, particularly Medicaid and Medicare in the US. Past work on privatization of these programs has focused primarily

on its impact on health care usage or government fiscal spending, with much more limited focus around care quality or beneficiary well-being (Brown et al., 2014; Cabral et al., 2018; Curto et al., 2019; Duggan et al., 2018).[4] This gap is unfortunate, given that understanding privatization's impact on beneficiary well-being is pre-requisite to understanding its overall welfare implications, and is thereby a gap that our paper looks to fill.

More broadly, our paper is related to the literature on take-up of public social programs (Currie, 2006), and take-up of Medicaid specifically, for which take-up rates have been estimated at around 70 percent (Swartz et al., 2015; Wright et al., 2017). The drivers of this low take-up are not well understood, even though such understanding could substantially inform policy efforts around increasing program enrollment. Our findings point to program quality as an important driver of take-up, and indicate that improved program quality could ultimately increase participation in Medicaid as well as other social programs. Finally, our paper is related to the recent literature on the value of health insurance and Medicaid (Finkelstein, Hendren, and Shepard, 2019; Finkelstein, Hendren, and Luttmer, 2019). We build on this literature by showing the value of health insurance coverage to not just be a function of financial risk protection levels, but also of coverage quality broadly defined. Unlike the Finkelstein et al framework, ours allows for beneficiaries to differentially value program quality, given we find higher spending beneficiaries to ultimately be most quality sensitive. In doing so, our paper also contributes to the literature on cost-quality tradeoffs in the design of public programs.

The remainder of the paper is organized as follows. Section 2 provides background on Medicaid contracting and the exclusion policy in New York State. Section 3 describes our data. Section 4 describes our empirical strategy, while Section 5 discusses our conceptual framework. Section 6 presents our results, and Section 7 discusses potential mechanisms. Section 8 presents several robustness tests including the difference-in-differences estimation. Section 9 concludes.

[4]Several papers study a specific feature of Medicare Part D, such as consumer choice (Abaluck and Gruber, 2011, 2016; Ketcham et al., 2012, 2015; Kling et al., 2012) and consumer cost-sharing (Einav et al., 2018). Some papers examine consumer premiums (Decarolis, 2015) and prices charged to providers by private insurance relative to public Medicare (Clemens et al., 2017).

2 Background

2.1 Medicaid contracting

Government contracting of Medicaid provision to private insurance plans began in the early 1980s and has since become increasingly common, with private plans currently covering over two-thirds of Medicaid beneficiaries. As part of this contracting, state governments pay private plans flat rates (i.e., capitation) to cover certain beneficiaries and certain services for a specified period of time. In turn, private plans pay health care providers (such as doctors and hospitals) for covered medical services, in lieu of the government doing so directly.

Public and private Medicaid coverage differ not only in terms of the source of insurance coverage (government versus private entity), but also in the underlying characteristics of that coverage, specifically the health care delivery system employed. To this end, the public system employs a fee-for-service system, which gives beneficiaries maximum flexibility and does not actively coordinate nor actively restrict care. Private plans meanwhile employ a managed care delivery system, under which every beneficiary gets assigned to a primary care provider, who will then serve as a gatekeeper and a care coordinator. While managed care is characterized by relatively greater intervention as well as restriction of patient care, it also emphasizes cost and quality accountability and patient-centered care management. In addition to being universally employed among private Medicaid as well as private Medicare plans (i.e., Medicare Advantage), the managed care model is also commonly employed in commercial insurance.

The effect of privately providing Medicaid on quality of beneficiary care is theoretically ambiguous. On the one hand, privatization could lead to quality improvements through specifying quality along contractible dimensions and increased competition. On the other hand, it could lead to quality deterioration through frictions such as incomplete contracting and imperfect competition. As a result, its impact on beneficiaries represents an empirical question, motivating our focus on quality and beneficiary satisfaction effects in this paper.

2.2 Exogenous enrollment in private Medicaid plans based on birth weight in New York State

When New York State originally introduced private Medicaid (MMC) plans, enrollment in them was voluntary, as public Medicaid was offered to beneficiaries in parallel. However, New York gradually began making enrollment in private Medicaid involuntary for certain Medicaid beneficiaries, through enrollment mandates that were introduced on a county-by-county basis between October 1997 and November 2012. One set of mandates applied only to non-disabled (non-SSI) beneficiaries, while another set applied to the SSI population and started being introduced only in the mid-2000's. Today, Medicaid beneficiaries in all 62 counties of New York are generally required to enroll in a private health plan, although certain groups continue being exempted on the basis of health or other characteristics. For example, infants with birth weights below 1,200 grams were one of the few groups who were not only excluded from involuntary enrollment requirements (mandates), but who furthermore were prohibited from enrolling in a private plan even voluntarily.[5]

The exclusion of infants under 1,200-gram birth weights from private Medicaid and requirement that they instead be under public Medicaid for the first six months of life (New York State Department of Health, 2001) yield a sharp discontinuity in private Medicaid status around the 1,200-gram birth weight threshold.[6] Children above the 1,200 birthweight threshold who get enrolled in private Medicaid, meanwhile, are automatically enrolled in their mother's plan if their mother was also in Medicaid.

[5]As part of the Medicaid Redesign Team (MRT) initiatives, since April 2012 the mandate has also included infants weighing less than 1,200 grams at birth. See `http://www.health.ny.gov/health_care/medicaid/program/update/2012/2012-02.htm#infants`

[6]New York State also excluded other subpopulations that are medically complicated and expensive to treat, such as nursing home residents and people residing in state psychiatric facilities (Sparer, 2008), from enrolling into a private health plan. This was primarily due to concerns raised by health plans and beneficiaries, given little experience of health plans with severely ill subpopulations. However, New York State has been gradually requiring even these subpopulations to enroll in private Medicaid, in hopes both of cost savings and quality improvements.

2.3 Possible effects on Medicaid attrition

All infants born to women on Medicaid are automatically eligible for Medicaid for the full year following birth. Beyond the first year of life, however, reapplication and proof of ongoing eligibility are required for infants to remain in the program. A large number of families appear either to not reapply for the program or to be unsuccessful when doing so given high observed rates of subsequent attrition of infants from Medicaid. Specifically, we find that only about 54% of infants in our sample who received Medicaid at birth are still enrolled at 24 months of age. This high rate of attrition could be driven by some combination of families no longer wanting their infants to be in Medicaid due to program dissatisfaction (even if still eligible), administrative burdens of the reapplication process, and loss of eligibility for the Medicaid program altogether.

In examining differential attrition between those initially enrolled in public versus private Medicaid by virtue of being slightly below or above the 1,200-gram threshold, we can quickly rule out differential eligibility for Medicaid as a driver.[7] As such, we are left with differential administrative burdens and differential beneficiary program satisfaction as two plausible drivers. Given that beneficiary program satisfaction is the main one of interest for this paper, we intend to decompose it from that of administrative burdens.

Turning to administrative burdens for renewing Medicaid benefits, these can be significant, and can come in the form of lengthy re-enrollment application forms and provision of evidence of eligibility such as income records. Administrative burdens could potentially differ across public Medicaid and private plans, as health plans may reduce these burdens by reminding to renew, providing assistance in the renewal process, and lowering administrative barriers. Plans would have an incentive to reduce administrative burdens in a targeted fashion, directed towards lower-cost beneficiaries, or encourage low-cost beneficiaries to remain in Medicaid

[7] One confounder at the 1,200-gram threshold is the eligibility for Supplemental Security Income (SSI), which provides Medicaid eligibility and monthly cash transfers, for infants with birth weights below the threshold. However, SSI benefits do not count towards household income when evaluating eligibility for Medicaid services. We further discuss the implications of Medicaid eligibility associated with SSI in Section 7.3.

through some other channel. This is because lower attrition among this group could mean greater profitability for plans.

3 Data

Our study relies primarily on Medicaid administrative data in the form of health care claims, individually linked to program enrollment and birth characteristics. This data enables us to track infants' health care utilization and health outcomes, as well as to identify those infants immediately above and immediately below the policy-designated birth weight threshold of interest.

The claims and enrollment data are obtained directly from the Centers for Medicare & Medicaid Services (CMS) and cover all Medicaid infants as well as their mothers, for New York State for the 2004-2010 period. The claims data covers all utilization in both private and public Medicaid, as well as the full spectrum of health care services rendered to beneficiaries, including inpatient, outpatient, emergency room, and prescription drugs. The enrollment data covers beneficiaries' demographic characteristics as well as monthly enrollment status in Medicaid, along with additional person-month level indicators for whether a beneficiary is in public versus private Medicaid. The enrollment data also contains a variable facilitating linking between different family members in Medicaid, including mothers and their infants. Finally, we make use of infant-level birth weight records, which we have obtained directly from New York State. We link these claims, enrollment, and birth weight data together at an individual-beneficiary level, through use of standardized beneficiary identifiers as well as Social Security numbers. Altogether, our data contains information on over 11 million Medicaid-enrolled child-month records from this period.

We implement two basic sample restrictions. First, we restrict to the time periods for which mandates were in effect within each specific county. This is meant to increase the sharpness of the discontinuity in private Medicaid enrollment status at the birth weight threshold, given that for those above the threshold, private Medicaid enrollment would have been mandatory rather than voluntary for these counties and time period. Second, we restrict to children who were enrolled in Medicaid at

the time of birth. This is meant to ensure exogenous assignment into private versus public Medicaid, driven by birth weight, as well as to yield a comparable sample for tracking subsequent Medicaid enrollment attrition. Our final sample consists of 9.3 million records of 330,865 unique children.[8]

Table 1 provides summary statistics for the full sample of 330,865 children in column (1) and the sub-samples of children near the 1,200-gram threshold in columns (2) and (3). In the full sample, 72.7% of children are enrolled in a private health plan under MMC. Below the 1,200-gram threshold, only 3% of infants are enrolled in MMC as opposed to 54.8% above the threshold, suggesting that the exclusion policy has a strong impact on the MMC enrollment status of these children. The average Medicaid spending during the first six months is $6,241 in the full sample, which contrasts with much higher levels of spending among low birth weight infants around the 1,200-gram threshold. The average Medicaid spending is $84,895 for infants below the threshold and $41,405 for those above the threshold.

4 Empirical strategy

We use a standard regression discontinuity (RD) design to estimate the relationship between private Medicaid (MMC) versus public Medicaid (FFS) enrollment and future Medicaid participation. We exploit the policy rule prohibiting enrollment in MMC among those with birth weights below 1,200 grams, by comparing infants with birth weights just below to those just above the 1,200-gram threshold; as a direct consequence of their birth weight, those weighing under 1,200 grams were relatively much more likely to be in FFS rather than MMC. We estimate the following regression to examine the first-stage effect of crossing the threshold on MMC participation. We then proceed to examine the reduced-form effects of crossing the birth weight threshold on several future Medicaid participation outcomes Y_i:

$$Y_i = \alpha + \beta D_i + f(X_i) + \varepsilon_i \tag{1}$$

[8]Births covered by the Children's Health Insurance Program (CHIP) are not included in our sample, as they are not tracked in the original CMS enrollment & claims data.

where i denotes a child-month record. For dependent variables, we mainly consider two measures of Medicaid participation: (1) the probability of Medicaid enrollment at a certain age; and (2) total months of Medicaid enrollment at a certain age. D_i is a binary variable with a value of one if the birth weight of a record i is greater than or equal to 1,200 grams, $I[BW_i \geq 1,200g]$. X_i denotes a running variable, which is birth weight centered at 1,200 grams, $BW_i - 1,200g$. We use a linear spline to control for differential trends across the threshold, $f(X_i) = X_i + D_i X_i$. For bandwidth selection, we employ a bandwidth selection method proposed by Calonico, Cattaneo, and Titiunik (2014), which is tailored to each individual outcome variable. Using this method, we calculate optimal bandwidths that range between 150 to 300 grams, across our different primary outcome variables. To enable comparisons across outcomes, we set the bandwidth in our main specification to be a uniform 200 grams across all outcomes. We estimate these models with Ordinary Least Squares (i.e., local linear regressions with a uniform kernel). In the tables, we report the RD estimate β with clustered standard errors at the birth weight level (Card and Lee, 2008). As a specification check, we additionally examine whether the estimates are sensitive to a range of bandwidth choices, the functional forms of $f(X_i)$, and the inclusion of control variables in Section 8.2.

The key identifying assumption inherent to our strategy is that mothers are unable to precisely manipulate the birth weight of newborns around the 1200-gram threshold (Lee and Lemieux, 2010). We validate our identification assumptions in several ways. First, visually examining the distribution of birth weights around the 1,200-gram threshold as presented in Figure 1, we observe heaping at multiples of 10 grams, 5 grams, as well as ounces. Such heaping could be problematic in the event that it is systematically associated with observable characteristics, as this would then bias our estimates (Barreca, Lindo, and Waddell, 2016). We find no evidence of non-random heaping at multiples of 10 grams, 5 grams, and ounces, as shown in Appendix Figure A.1, which plots several observable patient and hospital characteristics of patients and hospitals by heaping levels.[9] We also statistically test for birth weight manipulation right around the threshold, finding no evidence for it, given no statistically significant changes in observation count around the threshold

[9] We still conduct robustness checks to potential non-random heaping in Section 8.2.

(McCrary, 2008).

To further validate our RD design, we examine whether observable predetermined characteristics are balanced across the threshold. Since it is difficult to accurately predict birth weight prior to delivery, predetermined characteristics of infants are unlikely to change discontinuously across the threshold. As expected, predetermined characteristics do not exhibit statistically significant differences across the threshold, as shown in Columns (1)-(5) of Table 2 for baseline characteristics at birth such as child sex, race, and median income at the zip code level.[10] Column (6) shows the RD estimate on an index of MMC participation, which is calculated as predicted values from a regression of MMC participation on child sex, race indicators, and median income at the zip code level.

An additional identification assumption is that the effect of the threshold comes entirely through its effect on MMC eligibility, and not through an alternative channel; such an alternative mechanism could be theoretically possible if other program eligibility determinations are based on the same birth weight criteria, and if these eligibility determinations could themselves impact the likelihood of Medicaid participation. Consequently, one potential confounder is the use of the same birth weight threshold as a qualifier for Supplemental Security Income (SSI), which is additionally problematic due to SSI being in itself a qualifier for Medicaid enrollment. Fortunately, SSI-eligibility in practice has very limited impact on Medicaid enrollment, given that most SSI-enrollees could qualify for Medicaid by meeting other enrollment requirements (Guldi et al., 2018). Moreover, irrespective of the magnitude by which this population's SSI-eligibility actually impacts its Medicaid enrollment, it would lead to enrollment effects of the opposite sign of what we estimate. After all, we find higher attrition from Medicaid among those with birth weights below the threshold, whereas SSI-eligibility on its own would be expected to produce the opposite effect. We further discuss the role of SSI qualification in Section 7.3.

[10] We obtain median household income at the zip code level from the 2006-2010 American Community Survey (ACS).

5 Conceptual framework

We develop a simple conceptual framework to illustrate families' decision to enroll in Medicaid in the future conditional on initial coverage assignments.[11] Suppose children are randomly assigned to either public Medicaid (FFS) or private Medicaid (MMC) at birth based on their birth weight. We use $z \in \{0,1\}$ to indicate birth weight below versus above the threshold and thus the FFS versus MMC assignment. In the next period, families make enrollment decisions, $d \in \{m,p,n\}$. Families can choose to stay on Medicaid (m), leave Medicaid and enroll in private health insurance such as an employer sponsored health insurance plan (p), or leave Medicaid and become uninsured (n).

Each family i has utility $U_i(d,z)$, which is a function of the initial Medicaid assignment z and the future enrollment decision d. We assume that d and z can affect utility through two channels: quality and cost of health care, i.e., $U_i(d,z) \equiv U(q(d,z),c(d,z))$. Utility increases in quality and decreases in costs ($U'_q > 0$ and $U'_c < 0$). Under this setting, each family makes the enrollment decision that maximizes their utility.

$$D_i(z) = argmax_{d \in \{m,p,n\}} U_i(d,z)$$

Based on this simple framework, MMC can reduce Medicaid attrition if $U_i(m,1) > U_i(m,0)$. Utility from enrolling in Medicaid can be higher following the initial MMC assignment if families experience higher quality of care or lower costs under MMC than under FFS. Since Medicaid in New York essentially provides free care for beneficiaries under 21 years old under both MMC and FFS, we assume that the relevant cost is the potentially substantial administrative cost of enrolling in Medicaid, such as verifying income and filling out complicated application documents.

In Section 6, we examine future Medicaid participation decisions following initial Medicaid coverage assignments based on birth weight. In Section 7, we explore the potential mechanisms driving our main enrollment effect.

[11] We use a framework similar to that of Kline and Walters (2016).

6 Main results

6.1 MMC enrollment conditional on Medicaid participation

We begin by documenting a sharp discontinuity in private (MMC) versus public (FFS) Medicaid enrollment status at birth around the 1200-gram birth weight threshold, conditional on overall Medicaid enrollment. In doing so, we employ administrative micro-data tracking all Medicaid births in New York over the 2004-2010 period, along with birth weight and infants' post-birth health care utilization under Medicaid. We focus on the subset of infants with birth weights around the 1200-gram threshold.

We provide visual confirmation of the sharp discontinuity in MMC enrollment status around the threshold. Figure 2 plots the mean probability of MMC enrollment within each 20-gram bin (dots), conditional on Medicaid participation, along with fitted regression lines (solid lines) and associated 95% confidence intervals (dashed lines). Consistent with policy rules, panel (a) shows a sharp increase in the share of MMC enrollment by roughly 50 percentage points at the birth weight threshold in the month of birth.

At the same time, we find that the discontinuity in MMC enrollment status at the threshold does not persist as infants age, being effectively absent among those 24 months in age, as shown in panel (b) of Figure 2. To more directly examine how this MMC enrollment discontinuity attenuates the further we look from the time of birth, we separately estimate the RD model for each discrete monthly age value.

Figure 3 (a) plots the point estimates from all the age-specific RD models, for the 0 to 72 month age range. The discontinuity in the share of Medicaid recipients in private plans around the threshold becomes less pronounced at six months of age. This result is consistent with policy rules, given that the birth weight-based exclusion from MMC only directly applies to those six months and younger. Nonetheless, the discontinuity in private plan enrollment persists through the first year, at which point we observe another reduction in the magnitude of the discontinuity. A statistically significant discontinuity persists at the threshold through around 24 months of age. As a companion to these figures, we present the underlying re-

gression point estimates in Panel A of Table 3. Our sample may differ in composition across different age points not only as a result of inclusion being directly conditional on Medicaid enrollment, but also as a result of our data extending only through 2010, meaning that it will have less longitudinal coverage for more recent births.[12]

In addition to looking at the point-in-time probability of a Medicaid recipient being in a private MMC plan, we also estimate the effect on cumulative months of enrollment from time of birth. Figure 3 (b) and panel B of Table 3 summarize these estimates. Over the first 2 years of life, children born above the 1,200-gram threshold experience 7.8 more cumulative months of MMC enrollment than children born below the 1,200-gram threshold. The cumulative difference in FFS versus MMC exposure widens even further with age, as those above the threshold accumulate 11.4 more months of MMC enrollment by age 5.

6.2 Overall Medicaid participation

Building on our previous results, which look at MMC enrollment conditional on Medicaid participation, we proceed to our main study question around Medicaid participation overall. Specifically, we are interested in how Medicaid participation over the long-run is influenced by whether an infant is exogenously assigned to FFS versus MMC at birth.

We find that those exogenously assigned to MMC rather than FFS at birth, as a consequence of birth weight, have higher levels of subsequent Medicaid participation. For these analyses, we employ beneficiary-month level data on Medicaid enrollment status for infants in New York, linked to their original birth weights. In doing so, we focus on infants enrolled in Medicaid at birth, with birth weights around the 1,200-gram cutoff. We also implement our other standard sample restriction, limiting to individuals in counties where MMC enrollment requirements were already in effect.

In Figure 4, we provide graphical evidence of an effect of initial FFS versus MMC assignment on long-run Medicaid participation, with this effect emerging a

[12] We find that our results are robust to a balanced panel (Section 8.2).

year following birth and becoming more pronounced over time. Specifically, the figure plots the RD point estimates for Medicaid enrollment status as of different ages, with panel (a) showing no discontinuity in the probability of Medicaid enrollment around the threshold over the first 12 months following birth.[13] However, we find that rates of Medicaid enrollment start to diverge at 12 months of age, becoming statistically significant at 24 months of age. At that age, children with birth weights slightly above the threshold have a 16.6 percentage point greater enrollment rate than children below it, where around 42% of children below the threshold are still in Medicaid 24 months from birth (panel A of Table 4). In terms of the cumulative effect on Medicaid participation, infants above the threshold are enrolled in Medicaid for 6.1 more total months over the 48 months following birth, as shown in panel B of Table 4.

To supplement these figures, we also compile RD graphics for four different age points, shown in Appendix Figure A.3. These RD figures indicate that Medicaid participation status diverges right at the threshold, highlighting that the initial assignment into FFS versus MMC based on birth weight in fact drives the differences in future Medicaid participation.

Assuming that the exclusion restrictions hold (i.e., birth weight affects future Medicaid participation only through differential assignment to MMC at birth), we can employ the 1,200-gram birth weight threshold as an instrument for MMC assignment at birth. In turn, we can then look at the effect of MMC enrollment on future Medicaid participation, with use of this instrument. Estimating a two stage least squares (2SLS) model, we find that MMC enrollment in the month of birth increases Medicaid enrollment rates by 33 percentage points at 24 months of age, among the subset of infants around the threshold that whose births were originally covered by Medicaid (Table 5). In these analyses, we instrument for MMC enrollment at birth based on whether a newborn exceeds the birth weight threshold.[14] The

[13]This is likely because Medicaid provides children with 12 months of continuous coverage in New York, meaning that children are guaranteed coverage over that time frame, and will only lose it if they actively disenroll. Consistent with this, Appendix Figure A.2 shows a sharp decrease in Medicaid participation right at 12 months following birth.

[14]We use MMC coverage at birth as the endogenous variable instead of the cumulative months of MMC coverage, given that cumulative exposure is systematically correlated with future Medicaid participation.

first stage F statistic decreases as we push the time period further from birth, but it is generally greater than the rule-of-thumb value of 10, suggesting that our instrument satisfies the instrument relevance condition. Similarly, we find that MMC exposure in the birth month increases Medicaid participation by a cumulative 12.5 months over the first 48 months.

7 Potential mechanisms

7.1 Supporting evidence for beneficiary-driven mechanism

We stipulate that lower attrition from Medicaid under private plans could arise through a number of mechanisms, including increased beneficiary satisfaction as well as unrelated plan-driven factors.

Focusing first on the mechanism of primary interest, increased beneficiary satisfaction, we find several pieces of evidence consistent with this mechanism. As part of this, we examine whether quality of care appears better under MMC versus FFS, at least to the extent this can be measured; this would be consistent with the mechanism of increased beneficiary satisfaction, albeit in a suggestive way. In addition, we investigate for heterogeneous effects of MMC assignment on subsequent program attrition, based on proxies of mothers' relative conscientiousness; under a beneficiary satisfaction driven mechanism, we expect children of more conscientious mothers to have subsequent Medicaid attrition that is relatively more responsive to initial FFS versus MMC assignment. We construct this proxy based on the amount of time that mothers spent under Medicaid over the year before their infant's birth; this proxy presumes that prenatal enrollment in Medicaid reflects increased desire for prenatal care, and therefore greater conscientiousness.

7.1.1 Differences in early-life health care

First, we look for observable indicators of superior quality under MMC as opposed to FFS, which would be consistent with the mechanism of increased beneficiary satisfaction. To examine differences in health care delivered across FFS and MMC, we focus in on the initial six months following birth, as the explicit exclusion

of low birth weight infants from MMC only extends through this time span.[15] Restricting to this time period also allows us to isolate program effects of MMC from possible selection effects, given the absence of differential attrition across the two programs during this time span (partly by virtue of 12 month continuous eligibility rules).

First, we find evidence of greater use of preventive care under MMC as opposed to FFS. We construct individual-level measures of health care utilization aggregated across the first six months and find that those exogenously assigned to MMC rather than FFS have 0.6 more claims (54% increase when evaluated at the sample mean below the threshold) and $8 higher spending (or 43% when evaluated at the sample mean below the threshold) on preventive care, as shown in columns (1) and (2) of Table 6.[16] We argue that this effect comes through program differences between MMC and FFS, and not through differences in group composition stemming from differential Medicaid attrition, given that these groups' overall Medicaid attrition rates are in fact statistically identical throughout the first 12 months of life.

Second, we find evidence of increases in office visits under MMC as opposed to FFS, driven specifically by the increased number of visits to primary care physicians (PCP's). As shown in columns (3)-(7) of Table 6, we find that infants with birth weights just above the 1,200-gram threshold have 0.7 more total office visits relative to those just below (30% increase when evaluated against the sample mean below the threshold) over the first six months of life. Decomposing office visits into specific subcategories, we find 1.2 more visits to primary care physician (PCP)[17] and 0.5 fewer visits to specialists. We find that this increase in office visits translates into 16 more minutes in the office and $25 higher spending on office care during the first six months.

[15] Alternatively, we exclude the first three months of records in calculating the observable quality measures as many of the low birth weight infants in our sample stay hospitalized for an extended period (on average 36 days). This restriction ensures that most of the infants are discharged from the initial hospitalization and are able to receive office care and preventive care, upon which our two forms of quality measures are dependent. We find that our results are robust to this restriction.

[16] The spending variable here is based on the amount paid for medical services, which would be paid to physicians directly by the government under FFS, while being paid by plans to physicians under MMC.

[17] PCP is defined as health care provider whose specialty is internal medicine or family practice or other general practitioner.

Figure 5 shows corresponding RD figures, showing corresponding graphical evidence of discontinuous changes in these quality measures across the threshold. We find similar sized effects when looking at office visits with "routine care" designations, suggesting that increased office visits are reflective of increased preventive care rather than worsening health.[18] Altogether, our results suggest that children under MMC receive more preventive care in the office setting, which could conceivably lead to more favorable parent perceptions of the Medicaid program.

Finally, we find higher spending above the threshold on relatively low-cost services, such as services provided in home, outpatient, and lab settings. By contrast, we generally find no difference in relatively high-cost services, such as services provided in inpatient and emergency room settings, across the threshold (Appendix Table B.1). These results are consistent with greater use of certain care under MMC coming through improved access to routine care, and not through increased need for care under MMC.

7.1.2 Heterogeneity by mother's MMC enrollment prior to childbirth

To further validate our proposed mechanism for the observed attrition effects, as being beneficiary driven and reflective of underlying beneficiary satisfaction, we examine heterogeneity in effects based on maternal characteristics. Specifically, we look for heterogeneity based on whether mothers had below versus above-median exposure to Medicaid during the year before birth. We argue that mothers' previous Medicaid exposure can proxy for their attentiveness/conscientiousness, particularly for getting recommended prenatal care.

For this analysis, we take advantage of a unique feature of our data — the ability to link infants and their mothers and thereby link together their claims and enrollment records. We find that the differential Medicaid attrition patterns — higher Medicaid participation above the threshold — are stronger for children whose mother had above-median exposure to Medicaid during pregnancy (Table 7 and Figure 6).[19]

[18] Here, routine care diagnoses are specifically defined based on whether the primary diagnosis is either V700 or V20.2, which are the standard codes for routine care.

[19] Our results are similar when examining heterogeneity based on mothers' previous enrollment in MMC, rather than overall Medicaid enrollment.

These results appear consistent with a beneficiary-driven mechanism for Medicaid attrition, under which lower attrition from MMC reflects greater beneficiary satisfaction. Specifically, under this beneficiary-driven mechanism, we would expect mothers with greater apparent conscientiousness or attentiveness to be most responsive to program quality. In line with this, we find that mothers with greater attentiveness to health (as proxied by time in Medicaid during pregnancy) to also be the ones whose infants' attrition from Medicaid is most sensitive to initial MMC versus FFS assignment.

7.2 Ruling out supply-side factors

After finding the observed attrition effects to be consistent with beneficiary-driven actions, we then proceed to examine and potentially rule out plan-driven explanations. Doing so, we offer additional evidence that lower attrition under MMC reflects increased beneficiary satisfaction, rather than alternative mechanisms.

In theory, private plans could make it easier for their enrollees to file necessary paperwork and meet administrative requirements for retaining Medicaid coverage, at least relative to the public FFS system. In particular, we examine whether attrition from plans is positively correlated with underlying costliness, as this could be reflective of plan-driven attrition in the form of risk selection. As New York State's Medicaid risk-adjustment system was solely demographics-based and condition-based for most of our sample period, it would be in plans' financial interests to get rid of sicker members, as these members would be unprofitable (given that payments for these members' coverage would not be adjusted to reflect their sickness).[20] One concrete way that plans could do so, for example, would be to make it easier for healthier beneficiaries to stay enrolled in Medicaid than sicker ones (for example, by targeting enrollment support resources towards those beneficiaries). Given that the FFS program does not face the same incentive as MMC plans to 'cream-skim' beneficiaries, any Medicaid attrition differences between the two

[20] Appendix Figure A.4 highlights private plans' incentive to "cream-skim." While average capitation payments to MMC plans are relatively flat across the birth weight distribution, there is large variation in beneficiaries' costs to plans across this same distribution. This suggests that health plans can financially benefit from retaining relatively healthy infants or driving out relatively sick ones.

could potentially reflect this 'cream-skimming' mechanism rather than the alternative beneficiary-driven one.

To probe this alternative explanation, we first examine whether the costliness of children is smooth at the birth weight threshold. We then proceed by looking at whether higher-cost groups are differentially more likely to attrit than lower-cost groups, even conditional on being on one side of the threshold versus the other. We proxy for a child's expected future costliness based on their medical costs in terms of payments to providers, over the first six months following birth.[21]

One caveat here is that our primary cost measures are only inclusive of costs for non-institutional providers. Specifically, we add up non-institutional provider costs in various places of service such as office, home, inpatient hospital, outpatient hospital, emergency room, and lab. In constructing these cost measures, we impute costs for MMC, partly because these costs are not tracked directly in our claims data, and partly to ensure that this measure is standardized across populations to only reflect medical utilization differences and not unit price differences.[22] To ensure that our results are not sensitive to the particular cost proxy definition, we construct secondary cost proxies based on utilization (hospital length of stay in the month of birth) within an institutional setting.

First, we find that our cost proxy is smooth across the birth weight threshold, although as expected it is inversely correlated with birth weight, as shown in Appendix Figure A.5. This suggests that there are no underlying differences in costliness or health across the threshold, which further validates our identification strategy.

Second, we find that those with higher-costs are more likely to stay in Medicaid than those with lower costs, conditional on receiving MMC coverage at birth. Table 8 summarizes the comparison between high-cost and low-cost children relative to the median total non-institutional provider costs. We find that the differential

[21]We do not examine this heterogeneity using measures of fiscal Medicaid spending, given that these measures are mechanically dependent on MMC status; total Medicaid spending measures payments to providers under FFS, while it reflects capitated payments paid to plans under MMC. Given that this measure mechanically changes depending on whether someone is in FFS versus MMC, we do not focus on total Medicaid spending.

[22]We limit our cost measure just to non-institutional settings, given that this imputation is only possible for non-institutional providers and not institutional ones such as hospitals.

attrition is stronger in the subgroup with above-median costs (i.e., high cost children above the threshold are more likely to stay in Medicaid), which is inconsistent with plans trying to keep healthier children in their plans. Appendix Figure A.6 (a) shows the corresponding figure.

Similarly, we find that the reduced attrition effects are stronger for infants with above-median length of stay in the hospital at birth (Appendix Table B.2 and Appendix Figure A.6 (b)), showing that this pattern holds across a range of different beneficiary costliness proxies.

7.3 Alternative hypotheses

Differential mortality. Another possible explanation for our results is that initial assignment to MMC versus FFS impacts Medicaid attrition through differential mortality between the two groups. To explore this possibility, we consider mortality as an outcome. Appendix Table B.3 shows that point estimates are positive but statistically insignificant, and if anything would run counter to our main results given the sign. Consequently, we can rule differential mortality out as a potential mechanism.

Supplemental Security Income. We furthermore consider how the concurrent use of the 1200 gram birthweight threshold as a basis for SSI eligibility might bias our results, and specifically whether it might be driving some of the differential Medicaid attrition across the birthweight threshold rather than the coinciding differences in FFS versus MMC eligibility. Conceptually, we would expect SSI eligibility to increase Medicaid participation, given that SSI enrollment typically confers Medicaid eligibility. As such, we would expect concurrent SSI eligibility to increase Medicaid participation below the threshold on its own, when we are actually seeing decreased Medicaid participation below the threshold under FFS assignment. Consequently, rather than driving our main estimated effect, concurrent SSI eligibility may in fact be attenuating it.

To further rule this out as an issue, we examine Medicaid participation rates across the threshold over the first six months of life among births initially covered by Medicaid, as it is during this period that SSI benefits are awarded to children

with presumptive eligibility based on birthweight. We find null effects on Medicaid participation, suggesting that SSI eligibility does not have a measurable impact on Medicaid participation. Appendix Figure A.7 provides a closer look at the distribution around the threshold at month 0 and 6. If anything, the figures show that there are more children enrolled in Medicaid above the threshold. This suggests that SSI has no significant effect on Medicaid participation, consistent with prior literature (Duggan and Kearney, 2007; Guldi et al., 2018; Lee, 2020).

In addition, we directly examine differences in the specified basis or reason for Medicaid eligibility around the birthweight threshold, and specifically whether SSI-based eligibility becomes more common below the threshold; this basis is directly specified and listed in our data. Only about 2.7% of infants in our estimation window are eligible for Medicaid through SSI in the first year, and the difference in the probability having SSI as basis for eligibility is generally insignificant across the threshold (Appendix Figure A.8), further alleviating this issue as a concern.

Though we have ruled out SSI eligibility as a direct driver of Medicaid participation differences around the threshold, it could still theoretically have an indirect effect. For example, SSI eligibility could lead to additional cash payments for those below the threshold, which could in turn impact future Medicaid participation (Ko et al., 2020). To address any potential concerns surrounding this, we exploit an alternative source of variation in Section 8.1.

8 Robustness Checks

In this section, we conduct various robustness checks to further validate our main result of increased Medicaid attrition under FFS than MMC, as well as further validate the beneficiary-driven mechanism that we have postulated for this.

8.1 Alternative source of variation: Difference-in-differences estimation

In this section, we test the robustness of our results to an alternative source of variation that is not confounded by SSI eligibility or other potential changes at the

1,200-gram threshold. Specifically, we estimate a difference-in-differences (DD) model using the rollout of a MMC enrollment requirement across New York counties. The MMC enrollment requirement serves as an effective instrument for actual MMC enrollment. Focusing on children whom we observe at birth, we define "treatment" counties as counties that implemented the mandate during our study period, 2004-2010, and "control" counties as those that did not adopt the mandate during the study period. Control counties thus include counties that adopted the mandate prior to or after the study period. In the DD analysis, we drop New York City because New York City may behave substantially differently from other counties and thus act as a poor control. Appendix Figure A.9 shows the map of treatment and control counties.[23]

We estimate basic DD models controlling for year and county fixed effects, and also controlling for whether the time period falls after the mandate implementation. We cluster our standard errors at the birth county level.[24] One advantage of the DD model is that we can look at children across the full range of birth weights, as well as separately focusing in on specific birthweight ranges. We thus estimate the effect of mandate on Medicaid participation and health care utilization separately for infants right above the threshold (with birth weights between 1,200 and 1,400 grams) and for infants with higher birth weights (birth weight above 1,400 grams) in a DD framework.

To start, Table 9 shows that the MMC mandate has a significant impact on MMC enrollment at birth for both infants right above the threshold and for infants with higher birth weights. Meanwhile, columns (1) and (2) of Table 10 show that the MMC mandate did in fact increase future Medicaid enrollment for infants right above threshold, consistent with our RD model. We similarly find that the mandate led to an increase in preventive care spending during the first six months for this group of infants. However, we find that the MMC mandate had no or *negative* (but

[23] In an alternative specification, we drop counties that are not contiguous to our treatment counties, and find similar results.

[24] Appendix Figure A.10 shows the event study estimates on (a) total months of Medicaid enrollment at 24 months and (b) total preventive care claims during the first six months using the full sample of infants. We find no evidence of pre-trends in these outcomes. While the MMC mandate does not significantly increase future Medicaid participation, we find that the MMC mandate significantly increases preventive care claims even in the full sample of infants.

insignificant) effect on future Medicaid participation for infants whose birth weight is greater than 1,400 grams (column (3)-(4) of Table 10). At the same time, we still find an increase in preventive care among these infants.

Figure 7 summarizes DD estimates for two of our main outcomes, total months of Medicaid enrollment at 24 months and preventive care claims during the first six months, by birth weight. We find that the increase in future Medicaid participation is concentrated among infants with birth weights between 1,200-1,399 grams, suggesting that our RD results are robust to a different source of variation using the county-level mandates. However, we find no effect on future Medicaid participation among infants with higher birth weights. We find similar increases in preventive care claims both for infants immediately above the threshold and also for infants with higher birth weights.

While this result suggests reduced attrition effects under MMC may not apply to average infants, and instead may only persist for lower birthweight infants, it does shed light on the potential mechanisms driving the reduced Medicaid attrition. For example, this result is inconsistent with a cream-skimming mechanism, given that under this mechanism we would expect reduced attrition among higher rather than lower birthweight infants.[25]

Further, this result is inconsistent with another alternative explanation for reduced attrition: reduced reenrollment barriers under MMC. After all, presuming MMC plans reduce reenrollment barriers for beneficiaries relative to FFS (for example, by providing help filling out reenrollment applications), we would expect the reduction to be uniform across all plan members. In turn, we would expect reduced attrition across a broad range of birth weights, inconsistent with our actual finding of reduced attrition only for lower weight births.

Our result, however, is consistent with a beneficiary-driven mechanism, reflecting beneficiary satisfaction with program quality. After all, our result is driven lower birthweight and thereby sicker infants, whose parents are likely to be more sensitive to quality of care. These parents are thereby likely to be more responsive

[25]The capitation payments received by health plans are not risk-adjusted by birthweight or other health-specific characteristics, meaning that lower birthweight infants are less profitable to plans on account of their higher average spending.

to program quality, in terms of enrollment decisions, than parents of healthier and higher birthweight infants.

8.2 Sensitivity to regression specifications

Sensitivity to the choice of bandwidth, degree of polynomial, and inclusion of controls. We examine whether our main estimates are sensitive to the choice of bandwidth, degree of polynomials, and inclusion of control variables. We repeat the RD estimations for the main outcomes, varying bandwidths from 100 grams to 300 grams in 50-gram increments. In addition to a linear polynomial, we use quadratic polynomials to control for trends in birth weight, separately below and above the threshold. Finally, we test whether our estimates are sensitive to the inclusion of several control variables, such as a series of indicators for child's sex and race as well as year fixed effects, month fixed effects, and birth county fixed effects.

We find that our estimates are highly robust to different choices of regression specifications, as we show in Appendix Figure A.11 for two of our main outcomes: probability of Medicaid participation at 24 months and the total preventive care claims during the first six months.

Robustness to heaping. Although we find no evidence of non-random heaping (Appendix Figure A.1), we conduct several robustness checks: (1) dropping observations at 1,200 grams, a method known as donut RD (Barreca et al., 2011); (2) dropping heaps; and (3) using only heaps. Appendix Table B.4 shows how the estimates change as we restrict the sample in various ways for two of our main outcomes: the probability of Medicaid participation at 24 months (our preferred estimate: 0.166 with standard error 0.068) and the total preventive care claims during the six months (our preferred estimate: 0.581 with standard error 0.246). Although the estimates are imprecise when we drop all heaps, our point estimates are similar across different restrictions, and they are not statistically different from our main estimates.

Robustness to a balanced panel. Given that we track children over time and the sample changes as we look at older ages, we examine a subgroup of newborns whom we fully observe for the first 48 months of life, and re-estimate main effects

for this balanced panel. As shown in Appendix Figure A.12, we find that our results are robust to this restriction. Corresponding regression estimates are shown in Appendix Table B.5.

9 Discussion and Conclusion

We find that getting exogenously assigned to private (MMC) over public Medicaid (FFS), by virtue of birth weight, leads to 12.5 more months of (or 46% more of) cumulative Medicaid enrollment over the first four years of life. We find evidence of this effect being beneficiary-driven, and reflecting greater beneficiary satisfaction under MMC than FFS. For example, we find observable improvements in access to care, as well as more pronounced enrollment effects among more beneficiaries with greater expected quality sensitivity. Meanwhile, we rule out alternative explanations, such as MMC plans either engaging in risk selection or reducing administrative costs of reenrollment broadly. Our estimates are large but comparable to existing estimates on effects of alternative factors impacting public insurance enrollment. To this end, Finkelstein, Hendren, and Shepard (2019) find that insurance take-up falls about 25% for each $40 increase in monthly enrollee premiums in the context of Massachusetts' subsidized insurance exchange. Wright et al. (2017) find that enhanced outreach efforts increased Medicaid enrollment by 10-50% in Oregon.

Our findings suggest that beneficiaries' valuation of Medicaid is a function of underlying program quality, not just financial risk protection. To this end, we find high cost beneficiaries, that is those who would get most financial protection from Medicaid, to also be the ones placing greatest value on program quality. Meanwhile, we find beneficiaries' decisions to enroll in Medicaid versus alternatives to implicitly reflect the relative valuation of these different options. Ours builds on an existing framework proposed by Finkelstein et al. (2019) on the value of Medicaid, which presumes that higher cost beneficiaries will always value Medicaid more, based on greater value placed on financial protection. Our framework, meanwhile, introduces the possibility of differential valuation of non-financial program quality across beneficiaries that could be inversely related to the value they place on fi-

nancial protection. As such, those who could have the greatest relative valuation of Medicaid under the Finkelstein framework might have the lowest relative one under ours.

What do we learn about the specific dollar value of insurance quality in Medicaid? Following our conceptual framework, children leaving Medicaid may obtain private insurance coverage or become uninsured. According the Kaiser Commission on Medicaid and the Uninsured (Coughlin et al., 2014), uninsured individuals paid about 20% and insured individuals paid about 12% of their total medical expenses out-of-pocket in 2013. This suggests that at least some families are willing to leave Medicaid and pay at least 12% of their medical expenditures for improved quality. For an average 1 to 2-year-old FFS enrollee with birth weights near the 1,200-gram threshold, this translates into \$868 a year (an average Medicaid spending \$7,230 $\times$ 0.12).[26] Note that this is an underestimate because families incur additional costs for private insurance in the form of premium payments, whose magnitude varies depending on geographic region and employer (and the extent to which the employer subsidizes premiums).

Our findings and resulting conceptual framework are generalizable beyond Medicaid to a broad range of public social programs, both in modeling how beneficiaries value these programs, as well as the factors driving beneficiaries to enroll in public programs over private alternatives. One major limitation of our study is that we are unable to assign a specific dollar value to how much beneficiaries value MMC over FFS or the value they place on quality specifically. Future research can better quantify how much beneficiaries value program quality in Medicaid and beyond, which can then inform how policymakers think through cost-coverage tradeoffs in program design.

[26]While this provides a lower bound of how much individuals value the quality of the outside option over FFS, we are unable to identify how much individuals value quality of MMC over quality of FFS. Our findings suggest that for marginal families attriting under FFS but not under MMC, the quality differential between MMC and the outside option is less than the financial cost of the outside option, but we are unable pin down exactly how much less.

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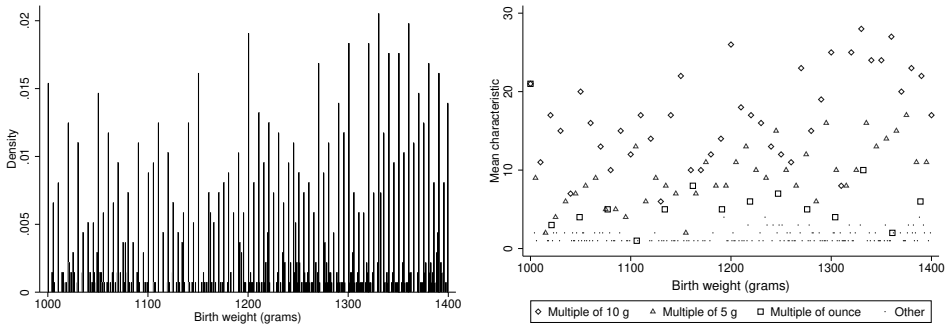
10 Figures

Figure 1: Distribution of birth weight

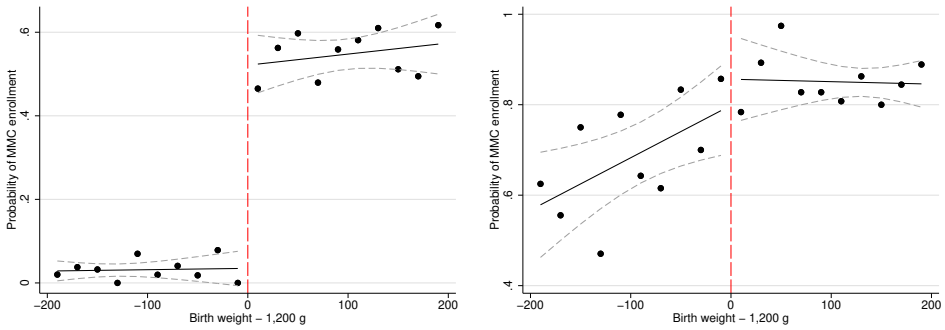
(a) At the gram level (b) By points of heaping



Notes: Panel (a) plots the frequency of birth weight at each gram. Panel (b) plots the frequency by multiples of 10-gram, multiples of 5-gram (but not of 10-gram), and multiples of ounce.

Figure 2: Probability of MMC enrollment

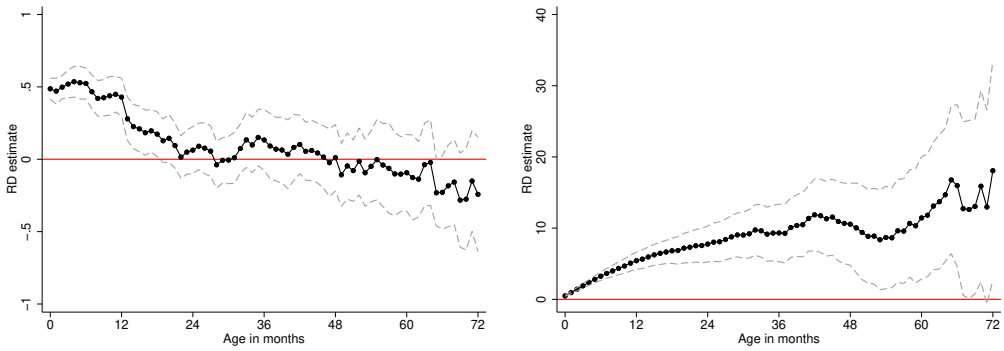
(a) At birth (b) At 24 months



Notes: Each figure plots mean probability of MMC participation at each 20-gram bin (dots) along with regression fitted lines (solid lines) and the 95% confidence intervals below and above the threshold.

Figure 3: MMC participation by age in months

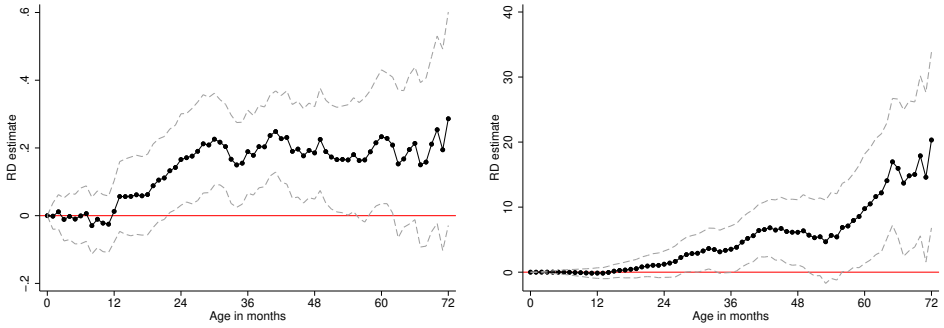
(a) Probability of MMC enrollment (b) Total number of MMC months



Notes: Each dot is an RD estimate from a separate regression by age in month. The dashed line indicates the 95% confidence intervals for each estimate.

Figure 4: Medicaid participation by age in months

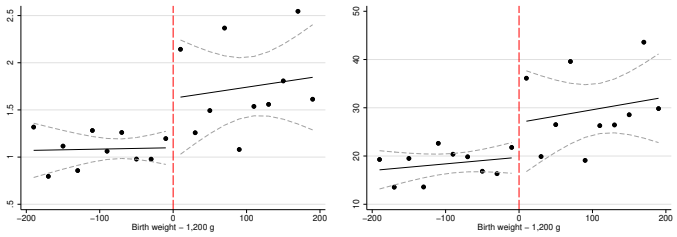
(a) Probability of Medicaid enrollment (b) Total number of Medicaid months



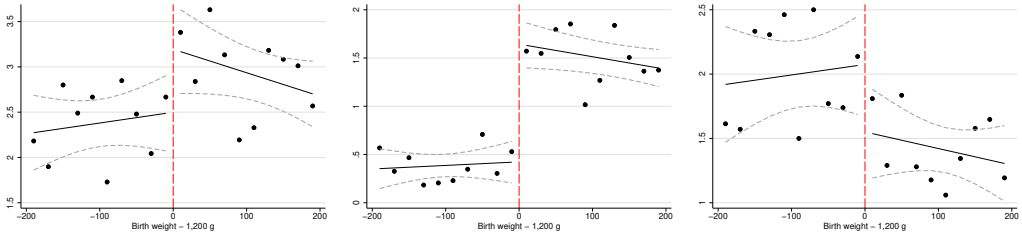
Notes: Each dot is an RD estimate from a separate regression by age in month. The dashed line indicates the 95% confidence intervals for each estimate.

Figure 5: Preventive care and office care during the first six months

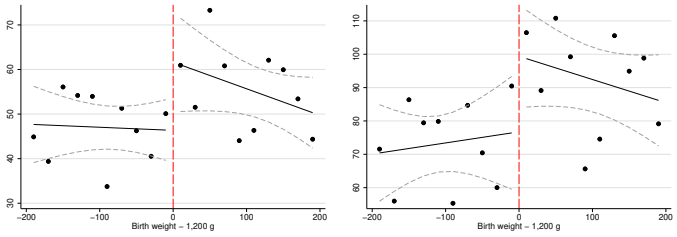
(a) Preventive care claims (b) Preventive care spending



(c) Office visits (d) PCP visits (e) Specialist visits

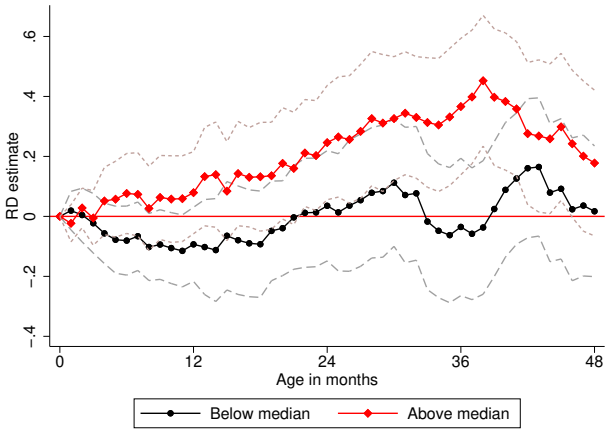


(f) Minutes in office (g) Office spending



Notes: Each figure plots mean values at each 20-gram bin (dots) along with regression fitted lines (solid lines) and the 95% confidence intervals below and above the threshold.

Figure 6: Probability of Medicaid enrollment by maternal exposure to Medicaid in the year prior to childbirth

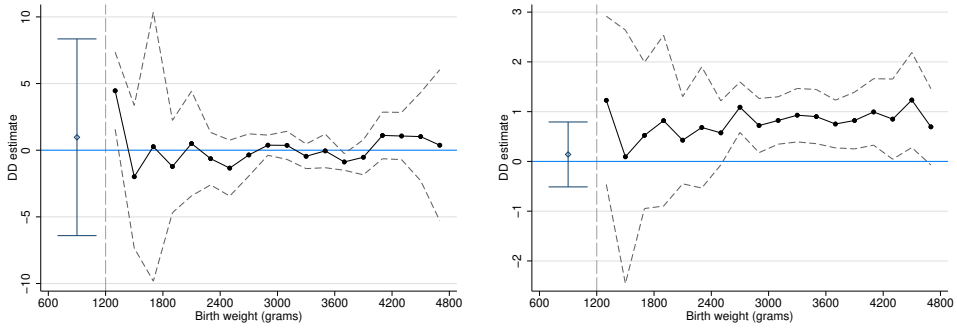


Notes: Each dot is an RD estimate from a separate regression by age in month. The dashed line indicates the 95% confidence intervals for each estimate.

Figure 7: DD estimates by birth weight

(a) Total months of Medicaid enrollment at 24 months

(b) Preventive care claims during the first 6 mo.



Notes: Each figure plots a DD estimate (dot) for each birth weight group along with the 95% confidence intervals (dashed lines). Panel (a) shows the estimates for the probability of Medicaid participation at 24 months of age, and panel (b) show the estimates for preventive care claims during the first six months.

11 Tables

Table 1: Summary statistics of child-month records

	(1) Full sample	(2) Birth weight $\in$ [1000, 1200)	(3) Birth weight $\in$ [1200, 1400]
Birth weight (gram)	3,276	1,097	1,305
Boy	0.512	0.506	0.505
White	0.349	0.241	0.235
Black	0.233	0.312	0.365
Hispanic	0.234	0.222	0.235
Zip-code level median income	46,586	47,134	46,173
In the month of birth			
MMC enrollment	0.727	0.030	0.548
Length of stay	3.99	39.25	33.56
During the first six months			
Non-institutional provider costs	397	4,250	2,854
Total Medicaid spending	6,241	84,895	41,405
Observations	330865	528	834

Notes: Non-institutional provider costs indicate the total costs for non-institutional providers in various places of service such as office, home, inpatient hospital, outpatient hospital, emergency room, and lab. We impute these non-institutional provider costs for MMC.

Table 2: Balanced of predetermined chatacteristics

	(1) Male	(2) White	(3) Black	(4) Hispanic	(5) Median income	(6) Index†
Above 1,200 g	-0.067	-0.031	0.066	-0.014	3569.824	-0.001
	(0.052)	(0.048)	(0.052)	(0.045)	(2621.676)	(0.002)
Observations	1362	1362	1362	1362	1202	1202
Mean below cutoff	0.506	0.241	0.312	0.222	47134.169	0.891
Mean above cutoff	0.505	0.235	0.365	0.235	46173.320	0.889

Notes: In addition to the indicator for birth weight$\geq$1,200 g, each regression includes a linear spline of birth weight. Robust standard errors clustered at the birth weight level are reported in parentheses. "Index" is predicted MMC participation based on welfare participation, child's gender, race, and zip code level median income. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table 3: MMC participation by age in months

	0 m	6 m	12 m	24 m	36 m	48 m	60 m
Panel A: MMC participation conditional on Medicaid enrollment							
Above 1,200 g	0.487***	0.524***	0.429***	0.063	0.133	0.011	-0.093
	(0.036)	(0.054)	(0.066)	(0.081)	(0.105)	(0.115)	(0.132)
Observations	1362	1103	942	527	364	244	164
Mean below cutoff	0.030	0.214	0.372	0.677	0.702	0.712	0.764
Mean above cutoff	0.548	0.776	0.830	0.855	0.844	0.801	0.826
	0-3 m	0-6 m	0-12 m	0-24 m	0-36 m	0-48 m	0-60 m
Panel B: Total months of MMC participation							
Above 1,200 g	1.897***	3.252***	5.427***	7.762***	9.322***	10.556***	11.435***
	(0.155)	(0.295)	(0.601)	(1.261)	(2.015)	(2.881)	(4.296)
Observations	1331	1263	1164	974	757	551	354
Mean below cutoff	0.225	0.706	2.284	5.925	9.588	12.796	17.745
Mean above cutoff	2.328	4.356	8.541	15.431	21.450	26.908	33.639

Notes: In addition to the indicator for birth weight≥1,200 g, each regression includes a linear spline of birth weight. Robust standard errors clustered at the birth weight level are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table 4: Medicaid enrollment by age in months

	3 m	6 m	12 m	24 m	36 m	48 m	60 m
Panel A. Probability of Medicaid enrollment							
Above 1,200 g	-0.011	-0.001	0.013	0.166**	0.189***	0.185***	0.233**
	(0.032)	(0.041)	(0.045)	(0.068)	(0.061)	(0.068)	(0.099)
Observations	1331	1263	1164	974	757	551	354
Mean below cutoff	0.905	0.829	0.747	0.418	0.379	0.324	0.369
Mean above cutoff	0.944	0.900	0.847	0.617	0.548	0.525	0.532
Panel B. Total months of Medicaid enrollment							
Above 1,200 g	0.007	-0.004	-0.138	1.228	3.530*	6.135**	9.760**
	(0.070)	(0.171)	(0.406)	(1.011)	(1.792)	(2.501)	(4.207)
Observations	1331	1263	1164	974	757	551	354
Mean below cutoff	3.802	6.375	11.126	17.178	22.581	26.893	32.792
Mean above cutoff	3.887	6.622	11.804	19.760	27.219	34.052	42.356

Notes: In addition to the indicator for birth weight≥1,200 g, each regression includes a linear spline of birth weight. Robust standard errors clustered at the birth weight level are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table 5: Medicaid enrollment by age in months, 2SLS estimates

	3 m	6 m	12 m	24 m	36 m	48 m	60 m
Panel A. Probability of Medicaid enrollment							
MMC at birth	-0.023	-0.001	0.027	0.334**	0.392***	0.377***	0.516**
	(0.066)	(0.086)	(0.094)	(0.138)	(0.133)	(0.143)	(0.239)
Observations	1331	1263	1164	974	757	551	354
Mean below cutoff	0.905	0.829	0.747	0.418	0.379	0.324	0.369
Mean above cutoff	0.944	0.900	0.847	0.617	0.548	0.525	0.532
F statistic	175.354	166.532	163.009	164.793	95.223	66.568	32.720
Panel B. Total months of Medicaid enrollment							
MMC at birth	0.014	-0.008	-0.290	2.470	7.316*	12.468**	21.595**
	(0.144)	(0.357)	(0.853)	(2.038)	(3.798)	(5.211)	(10.227)
Observations	1331	1263	1164	974	757	551	354
Mean below cutoff	3.802	6.375	11.126	17.178	22.581	26.893	32.792
Mean above cutoff	3.887	6.622	11.804	19.760	27.219	34.052	42.356
F statistic	175.354	166.532	163.009	164.793	95.223	66.568	32.720

Notes: In addition to the months of MMC indicator, each regression includes a linear spline of birth weight. Robust standard errors clustered at the birth weight level are reported in parentheses. The endogenous variable, months of MMC, is instrumented with the in the indicator for birth weight≥1,200 g. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table 6: Preventive care and office visits during the first six months of age

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Preventive care		Office care				
	Claims	Spending	Office visits	PCP visits	Specialist visits	Minutes in office	Spending
Above 1,200 g	0.581**	7.912*	0.715**	1.226***	-0.512**	15.795**	24.616**
	(0.246)	(4.212)	(0.329)	(0.196)	(0.253)	(6.990)	(11.562)
Observations	1263	1263	1263	1263	1263	1263	1263
Mean below cutoff	1.079	18.317	2.388	0.392	1.996	47.188	73.722
Mean above cutoff	1.766	30.211	2.964	1.516	1.448	56.079	93.102

Notes: In addition to the indicator for birth weight≥1,200 g, each regression includes a linear spline of birth weight. Robust standard errors clustered at the birth weight level are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table 7: Probability of Medicaid enrollment by maternal exposure to Medicaid in the year prior to childbirth

	3 m	6 m	12 m	24 m	36 m	48 m	60 m
Panel A. Mother's below-median exposure to Medicaid prior to childbirth							
Above 1,200 g	-0.023	-0.081	-0.093	0.035	-0.035	0.017	-0.054
	(0.050)	(0.058)	(0.062)	(0.092)	(0.115)	(0.109)	(0.184)
Observations	595	557	513	418	301	182	85
Mean below cutoff	0.889	0.816	0.741	0.333	0.363	0.278	0.333
Mean above cutoff	0.930	0.872	0.833	0.549	0.473	0.473	0.490
Panel B. Mother's above-median exposure to Medicaid prior to childbirth							
Above 1,200 g	-0.005	0.077	0.079	0.246**	0.366***	0.178	0.279
	(0.046)	(0.067)	(0.068)	(0.095)	(0.114)	(0.122)	(0.176)
Observations	571	541	486	394	295	209	110
Mean below cutoff	0.901	0.820	0.720	0.468	0.380	0.337	0.370
Mean above cutoff	0.945	0.910	0.833	0.650	0.580	0.553	0.594

Notes: In addition to the indicator for birth weight$\geq$1,200 g, each regression includes a linear spline of birth weight. Robust standard errors clustered at the birth weight level are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table 8: Probability of Medicaid enrollment by child's costliness

	3 m	6 m	12 m	24 m	36 m	48 m	60 m
Panel A. Below-median total non-institutional provider costs							
Above 1,200 g	-0.052	-0.055	-0.085	-0.021	0.138	0.170	0.111
	(0.055)	(0.060)	(0.065)	(0.099)	(0.102)	(0.113)	(0.153)
Observations	573	556	534	484	400	311	214
Mean below cutoff	0.869	0.813	0.774	0.424	0.345	0.341	0.422
Mean above cutoff	0.939	0.905	0.869	0.614	0.552	0.529	0.493
Panel B. Above-median total non-institutional provider costs							
Above 1,200 g	0.017	0.031	0.064	0.298***	0.248**	0.194	0.313**
	(0.040)	(0.054)	(0.066)	(0.094)	(0.102)	(0.117)	(0.141)
Observations	758	707	630	490	357	240	140
Mean below cutoff	0.921	0.837	0.734	0.414	0.398	0.314	0.329
Mean above cutoff	0.948	0.895	0.823	0.622	0.542	0.515	0.636

Notes: In addition to the indicator for birth weight$\geq$1,200 g, each regression includes a linear spline of birth weight. Robust standard errors clustered at the birth weight level are reported in parentheses. Total non-institutional provider costs are the sum of costs for non-institutional providers in various places of service such as office, home, inpatient hospital, outpatient hospital, emergency room, and lab. We impute costs for MMC, partly because these costs are not tracked directly in our claims data, and partly to ensure that this measure is standardized across populations to only reflect medical utilization differences and not unit price differences; this imputation is only possible for non-institutional providers and not institutional ones (such as hospitals). * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table 9: DD: Probability of MMC enrollment at birth

	Birth weight $\in$ [1200, 1400]	Birth weight $>$ 1400
MMC mandate	0.914***	0.547***
	(0.087)	(0.007)
Observations	323	123655
Mean	0.455	0.527

Notes: In addition to the indicator for MMC mandate, each regression includes county fixed effects and year fixed effects. Robust standard errors clustered at the birth county level are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table 10: DD estimation by birth weight

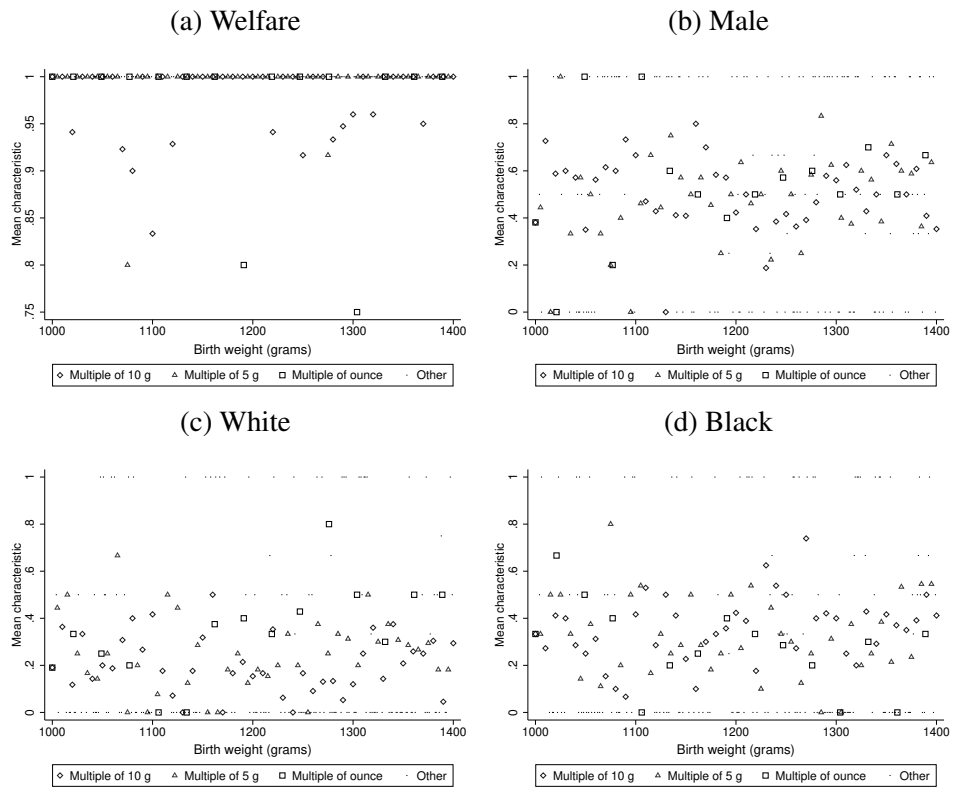
	(1)	(2)	(3)	(4)
	Birth weight $\in$ [1200, 1400]		Birth weight $>$ 1400	
	Probability of Medicaid enrollment	Total number of Medicaid enrollment months	Probability of Medicaid enrollment	Total number of Medicaid enrollment months
Panel A. Medicaid participation measured at 24 months				
MMC mandate	0.178	4.458***	0.008	-0.154
	(0.203)	(1.453)	(0.018)	(0.273)
Observations	309	309	119410	119410
Mean	0.647	19.845	0.638	20.153
	Preventive care claims	Preventive care spending	Preventive care claims	Preventive care spending
Panel B. Quality measures during the first six months				
MMC mandate	1.201	21.720*	0.838***	12.289***
	(0.854)	(11.626)	(0.241)	(3.917)
Observations	334	334	129202	129202
Mean	2.129	34.378	2.527	42.081

Notes: Each cell shows an estimate from a different DD model. Each DD model includes year fixed effects, county fixed effects, as well as an indicator for periods after the MMC mandate. Robust standard errors clustered at the birth county. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Appendix

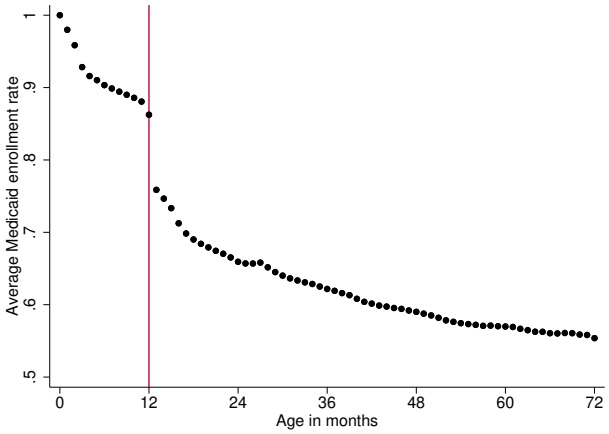
Appendix A. Appendix Figures

Figure A.1: Heaping and characteristics



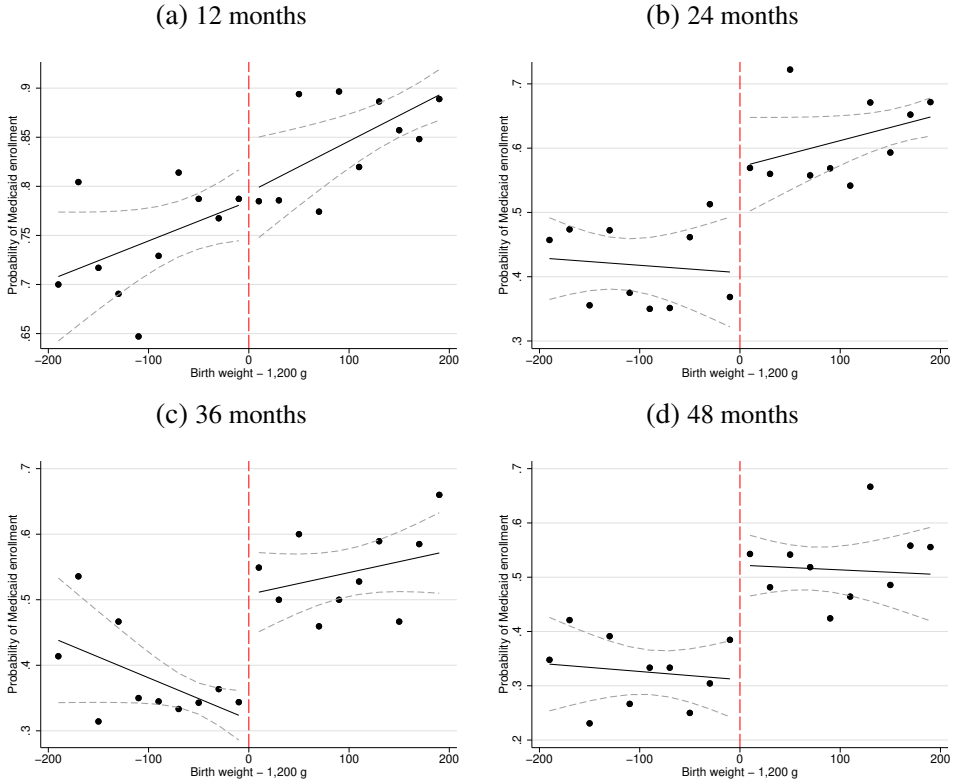
Notes: Each panel plots the mean characteristic separately by rounding numbers.

Figure A.2: Average Medicaid enrollment rate by age in months



Notes: The figure plots the mean Medicaid enrollment rate by age in months.

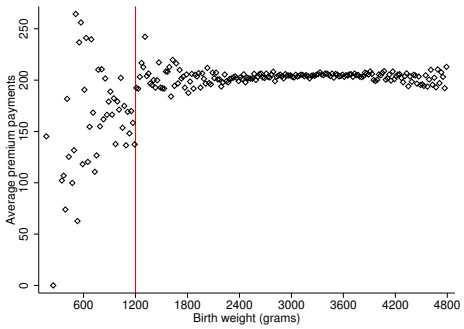
Figure A.3: Probability of Medicaid enrollment by age in months



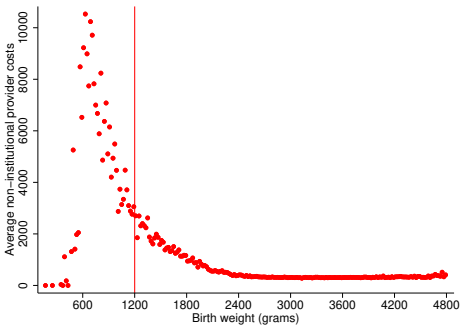
Notes: Each figure plots mean probability of Medicaid enrollment at different ages at each 20-gram bin (dots) along with regression fitted lines (solid lines) and the 95% confidence intervals below and above the threshold.

Figure A.4: Average premium and total non-institutional provider costs by birth weight, conditional on MMC enrollment

(a) Average premium payments

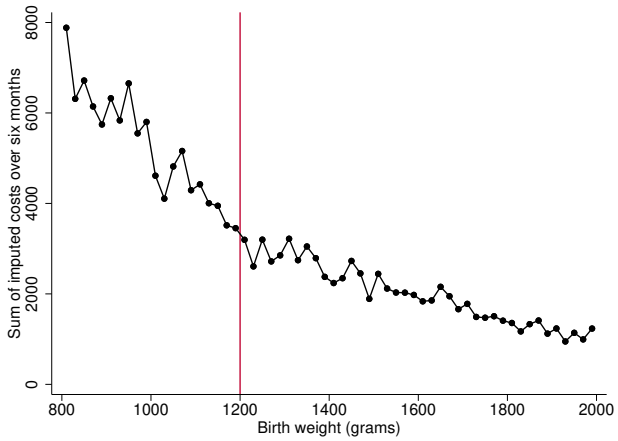


(b) Total non-institutional provider costs during the first six months



Notes: We focus on children under MMC. Total non-institutional provider costs indicate the sum of costs for non-institutional providers in various places of service such as office, home, inpatient hospital, outpatient hospital, emergency room, and lab during the first six months. We impute costs for MMC, partly because these costs are not tracked directly in our claims data, and partly to ensure that this measure is standardized across populations to only reflect medical utilization differences and not unit price differences; this imputation is only possible for non-institutional providers and not institutional ones (such as hospitals).

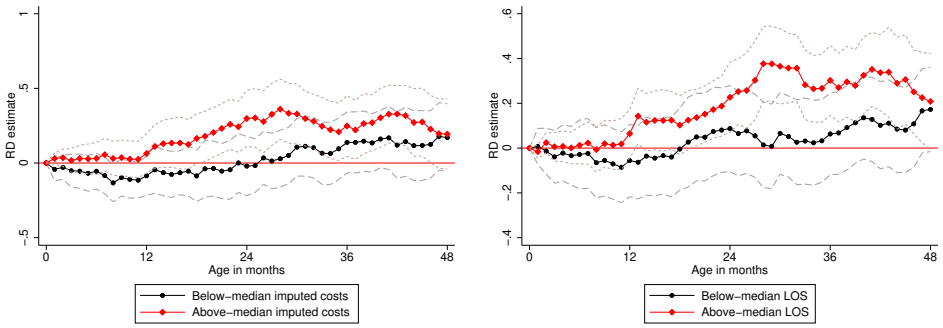
Figure A.5: Total non-institutional provider costs during the first six months by birth weight



Notes: The figure shows the mean values at each 20-gram bin. We include the full sample of infants and examine whether our proxy of costliness/sickness is smooth across the threshold. Our proxy–total non-institutional provider costs–is measured as the sum of costs for non-institutional providers in office, home, inpatient hospital, outpatient hospital, emergency room, and lab during the first six months. We impute costs for MMC, partly because these costs are not tracked directly in our claims data, and partly to ensure that this measure is standardized across populations to only reflect medical utilization differences and not unit price differences; this imputation is only possible for non-institutional providers and not institutional ones (such as hospitals).

Figure A.6: Probability of Medicaid enrollment by costliness

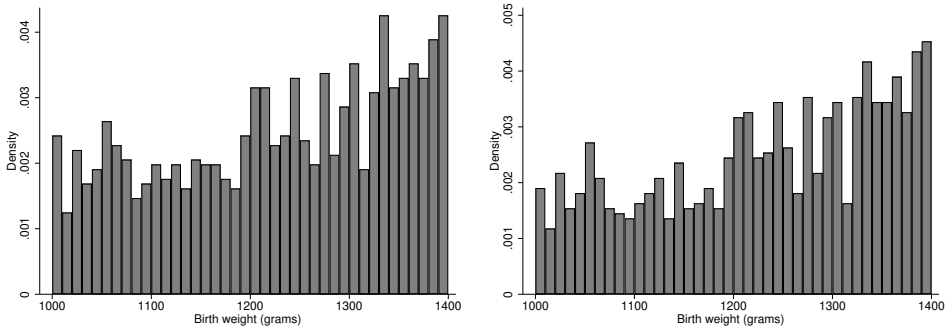
(a) By imputed non-institutional costs (b) By length of stay at birth



Notes: Each dot is an RD estimate from a separate regression by age in month. The dashed line indicates the 95% confidence intervals for each estimate.

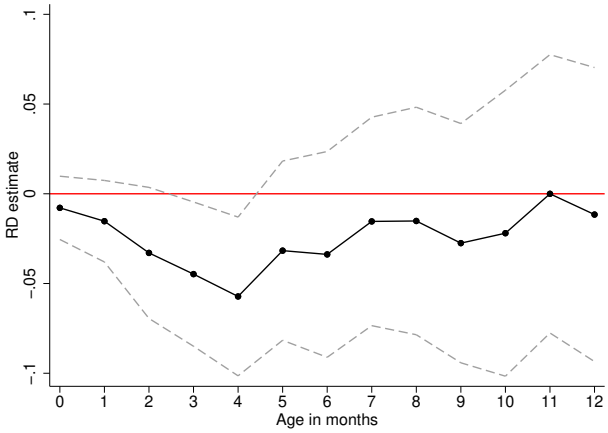
Figure A.7: Histogram of child-month records

(a) Month 0 (b) Month 6



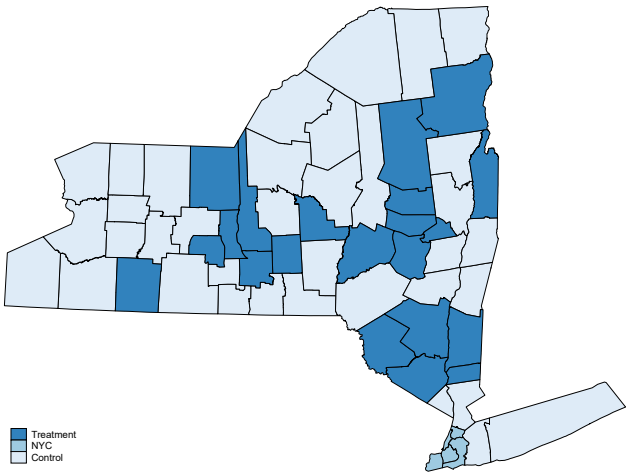
Notes: The figure shows the histograms of the child-month records in 10-gram bins at two different points in time.

Figure A.8: The effect of exceeding the threshold on having SSI as basis for eligibility



Notes: Each dot is an RD estimate from a separate regression by age in month. The dashed line indicates the 95% confidence intervals for each estimate.

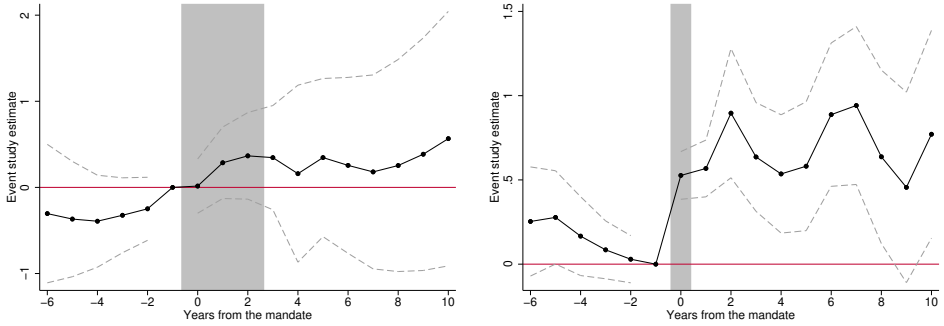
Figure A.9: Map of "treatment" and "control" counties in New York State



Notes: The figure shows the counties that adopted the mandate during our study period prior to 2010 ('Treatment') and counties that did not adopt the mandate during our study period in light blue ('Control'). We exclude New York City from the difference-in-differences analysis.

Figure A.10: Event study estimates in the full sample of infants

(a) Total months of Medicaid enrollment at 24 months (b) Preventive care claims during the first 6 mo.

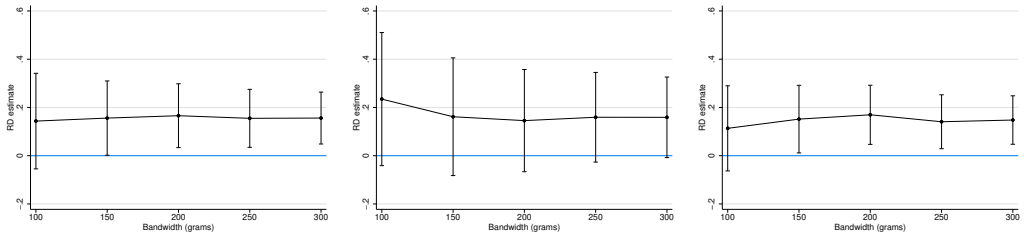


Notes: The figure plots the event study estimates for two outcomes. The shaded area indicates 24 months after the mandate in panel (a) and 6 months after the mandate in panel (b).

Figure A.11: Sensitivity to regression specifications

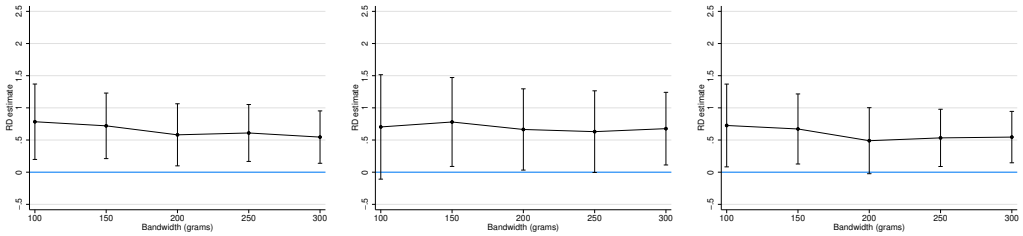
Outcome: Probability of Medicaid participation at 24 months of age

(a) Linear, without controls (b) Quadratic, without controls (c) Linear, with controls



Outcome: Preventive care claims at 6 months of age

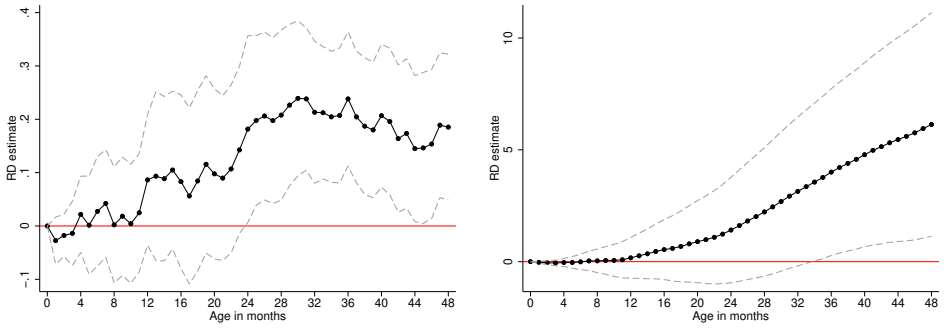
(d) Linear, without controls (e) Quadratic, without controls (f) Linear, with controls



Notes: Each bar represents an RD estimate (dot) from a separate regression along with the 95% confidence intervals. Panels (a)-(c) show the estimates for the probability of Medicaid participation at 24 months of age, and panels (d)-(f) show the estimates for preventive care claims at six months of age. Linear/quadratic indicates the degree of polynomials in each regression. Controls include a series of indicators for child's gender and race as well as year fixed effects, month fixed effects, and birth county fixed effects.

Figure A.12: Probability of Medicaid participation, balanced panel

(a) Probability of Medicaid participation (b) Total number of Medicaid months



Notes: Each dot is an RD estimate from a separate regression by age in month. The dashed line indicates the 95% confidence intervals for each estimate.

Appendix B. Appendix Tables

Table B.1: Other types of health care utilization during the first six months

	(1) Home	(2) Outpatient	(3) Lab	(4) Inpatient admissions	(5) Surgery admissions	(6) No surgery admissions	(7) ER counts
Above 1,200 g	73.350***	13.992**	2.007**	-0.043	-0.048	0.005	-1.213
	(25.161)	(6.852)	(0.868)	(0.106)	(0.054)	(0.100)	(0.875)
Observations	1263	1263	1263	1263	1263	1263	1263
Mean below cutoff	34.999	13.041	0.811	1.225	0.290	0.935	3.062
Mean above cutoff	94.013	17.816	1.118	1.188	0.246	0.941	2.341

Notes: In addition to the indicator for birth weight≥1,200 g, each regression includes a linear spline of birth weight. Robust standard errors clustered at the birth weight level are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.2: Probability of Medicaid enrollment by child's length of stay at birth

	3 m	6 m	12 m	24 m	36 m	48 m	60 m
Panel A. Below-median hospital length of stay at birth							
Above 1,200 g	-0.039	-0.028	-0.056	0.087	0.063	0.173*	0.316**
	(0.059)	(0.077)	(0.081)	(0.096)	(0.091)	(0.094)	(0.126)
Observations	632	609	578	486	375	287	196
Mean below cutoff	0.863	0.764	0.703	0.400	0.361	0.296	0.329
Mean above cutoff	0.923	0.877	0.824	0.601	0.553	0.545	0.571
Panel B. Above-median hospital length of stay at birth							
Above 1,200 g	0.006	0.013	0.064	0.227***	0.302***	0.208*	0.132
	(0.032)	(0.041)	(0.054)	(0.079)	(0.078)	(0.107)	(0.163)
Observations	699	654	586	488	382	264	158
Mean below cutoff	0.933	0.874	0.779	0.431	0.391	0.346	0.405
Mean above cutoff	0.966	0.927	0.875	0.636	0.542	0.496	0.468

Notes: In addition to the indicator for birth weight≥1,200 g, each regression includes a linear spline of birth weight. Robust standard errors clustered at the birth weight level are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.3: Probability of death by age in months

	3 m	6 m	12 m	24 m	36 m	48 m	60 m
Above 1,200 g	0.004	0.006	0.011	0.018	0.021	0.027	0.020
	(0.012)	(0.013)	(0.015)	(0.016)	(0.019)	(0.024)	(0.031)
Observations	1331	1263	1164	974	757	551	354
Mean below cutoff	0.016	0.019	0.025	0.027	0.027	0.013	0.013
Mean above cutoff	0.006	0.009	0.011	0.013	0.015	0.009	0.010

Notes: In addition to the indicator for birth weight≥1,200 g, each regression includes a linear spline of birth weight. Robust standard errors clustered at the birth weight level are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.4: Robustness to non-random heaping

	(1)	(2)	(3)
	Dropping 1,200 g	Dropping heaps	Using heaps only
Panel A. Probability of Medicaid enrollment at 24 months			
Above 1,200 g	0.153**	0.154	0.174**
	(0.069)	(0.149)	(0.074)
Observations	956	163	811
Mean below cutoff	0.418	0.429	0.416
Mean above cutoff	0.615	0.542	0.633
Panel B. Preventive care claims at six months			
Above 1,200 g	0.479*	0.556	0.597**
	(0.253)	(0.534)	(0.276)
Observations	1237	207	1056
Mean below cutoff	1.079	1.111	1.074
Mean above cutoff	1.746	1.459	1.830

Notes: "Heaps" are defined as multiples of at multiples of 10 grams, 5 grams, and ounce. In addition to the indicator for birth weight≥1,200 g, each regression includes a linear spline of birth weight. Robust standard errors clustered at the birth weight level are reported in parentheses. Each column imposes a different restriction to test for robustness to non-random heaping in the data. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.5: Medicaid enrollment by age in months, balanced panel

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Probability of Medicaid enrollment				Total months of Medicaid enrollment			
	12 m	24 m	36 m	48 m	12 m	24 m	36 m	48 m
Above 1,200 g	0.086	0.182**	0.238***	0.185***	0.170	1.415	4.004**	6.135**
	(0.061)	(0.088)	(0.063)	(0.068)	(0.455)	(1.179)	(1.864)	(2.501)
Observations	551	551	551	551	551	551	551	551
Mean below cutoff	0.813	0.427	0.360	0.324	11.849	18.102	22.698	26.893
Mean above cutoff	0.890	0.632	0.561	0.525	12.288	20.387	27.515	34.052

Notes: In addition to the indicator for birth weight$\geq$1,200 g, each regression includes a linear spline of birth weight. Robust standard errors clustered at the birth weight level are reported in parentheses. * Significant at 10%, ** significant at 5%, *** significant at 1%.