

Better Outcomes, Lower Costs: Evidence from Competitive Bidding in WIC*

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Abstract

The Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) serves about 40 percent of U.S. infants, and infant formula is one of its largest expenses. We study the introduction of sole-source infant-formula rebate contracts awarded through competitive bidding, which most states adopted between 1987 and 1993. Using a new compilation of state participation and cost data and a stacked difference-in-differences design, we find that the rebate system's savings allowed WIC to expand and improve infant health. WIC participation increased by 14 percent, and dollars per participant did not change, suggesting that additional enrollment is the main channel for rebate money. Average birth weight rose by 3.6 grams, and the share of small-for-gestational-age births fell by 2 percent. The gains are concentrated among births to mothers with at most a high-school education and in states that cut WIC caseloads for lack of funds, with some evidence of larger gains for higher-order births, consistent with spillovers from older siblings' enrollment.

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1 Introduction

In 2019, the federal government spent nearly \$1 trillion on benefits and services for people with low income, or about 4.5 percent of GDP (Landers et al., 2021), and a central question for governments is how to get the greatest return on these resources. This question is especially relevant for programs funded by fixed appropriations, such as block grants, where insufficient funds force programs to ration and leave eligible people unserved, and for programs that deliver goods in kind, such as child care, prescription drugs, and infant formula, which tie program costs to market prices. A common strategy to contain these costs is procurement, in which private firms bid for contracts to supply goods to the government. However, we know little about how procurement affects safety net programs or whether the contracts benefit the people these programs are meant to serve.

The Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) is one such program that has historically been sensitive to costs. It is funded through fixed annual grants and provides vouchers for specific foods. When funding is insufficient to serve everyone, it uses a priority list based on need; in 1989, 28 percent of state WIC directors reported waiting lists (Food Research and Action Center, 1989). To contain costs, states began in the late 1980s to award sole-source rebate contracts for infant formula, the most expensive good in the WIC package. Through these contracts, manufacturers bid to be the exclusive supplier to a state’s WIC program in exchange for a rebate (or “discount”) they provide to the state WIC agency on each can. Infant formula rebates are central to WIC today. In fiscal year 2023, rebates totaled about \$1.6 billion and funded about 1.3 million participants a month, roughly one in five (U.S. Government Accountability Office, 2025).

We study how the staggered state rollout of the sole-source rebate contract system affected WIC participation, program finances, and infant health. We compile newly digitized state data on WIC operations, including participation, food costs, grants, and rebates, along with data from Davis (2016) on when each state adopted the sole-source system, and vital statistics data. Together, these allow us to implement a stacked difference-in-differences design that exploits variation in when states adopted the rebate system to study its effects on program dynamics and newborn health.

We find that sole-source contracts substantially expanded WIC. Caseloads rose by 5.8 participants per 1,000 age-eligible residents, or 14 percent above the pre-adoption mean. Women’s participation rose 15 percent, and children’s rose 17 percent.

This expansion occurred despite decreases in federal grants. Federal grants were allocated by a formula that protected each state’s previous grant and directed new money toward states serving a smaller share of their eligible population; a state that served more people with

the same grant therefore became a lower priority for additional funds. Following adoption, federal grants fell by 4 percent per age-eligible resident. Rebates, however, enabled additional growth; gross food spending per age-eligible resident rose by 10 percent. Gross food spending per participant, however, did not change, suggesting that the additional money went to enrolling more people rather than to changing what each participant redeemed or the value of the food package.

We next ask whether this expansion improved health. WIC serves pregnant and postpartum women, infants, and children, and the health of each of these groups may have improved with the expansion. We focus on infant birth weight, which has been shown to improve with additional access to WIC (Hoynes, Page and Stevens, 2011; Rossin-Slater, 2013; Bitler et al., 2025). Average birth weight rose by 3.6 grams, and the share of small-for-gestational-age births fell by 0.19 percentage points, or 2 percent of the baseline rate. Point estimates suggest that the incidence of low birth weight declined by 0.07 percentage points, but is not statistically significant for the general population.

We explore three types of heterogeneity. First, participation and health improved most in states whose WIC programs were most funding-strapped. In the states that cut caseloads during the 1990 funding shortfall, sole-source contracts raised participation by 18 percent and birth weight by 5.7 grams, compared with 6 percent and 2.3 grams in the other states, neither of which is significant at the 5 percent level.

Next, we use the rich demographic information in the natality data to show that births to the mothers most likely affected by WIC responded most, including mothers with a high school education or less and teenagers. Among births to mothers with a high-school education or less, birth weight rose by 6.7 grams and the share small for gestational age fell by 0.34 percentage points, while births to mothers with some college or a college degree, who are rarely income-eligible for WIC, did not change. Among births to teenage mothers, the share small for gestational age fell by 0.35 percentage points, twice the decline among older mothers. We find little evidence that effects differ by race.

We also find suggestive evidence that higher-parity births responded more strongly. Births to mothers who already had a child gained 4.3 grams, and 8.6 grams among mothers with a high-school education or less, while first births gained 1.5 grams, which is not statistically significant. Because additional enrollment was driven disproportionately by children (aged 1–4), this pattern is consistent with spillovers from an older sibling’s enrollment.

When we scale the birth-weight effect by the change in participants enrolled per birth (women, infants, and children), which accounts for the fact that pregnant women may be affected by the expansion if others in the household are newly enrolled, our estimate of the treatment-on-the-treated effect is 35 grams. This is somewhat larger than existing estimates

of 18 to 27 grams for prenatal participation (Hoynes, Page and Stevens, 2011; Rossin-Slater, 2013), although its confidence interval includes them (Section 10.1).

In a cost-benefit framework, we treat rebates as a transfer from formula manufacturers to WIC rather than as a public cost. The rebates financed about 1.3 million additional participant-years, and using estimates on how birth weight translated to subsequent hospital expenses (Almond, Chay and Lee, 2005; East et al., 2023), we estimate that hospital savings at birth alone total about \$331 million (in 2011 dollars), or about 10 cents per rebate dollar. The main potential cost outside the program falls on families who buy formula without WIC if contracts raise retail prices; the best estimates of this pass-through are small (U.S. Government Accountability Office, 2025; Oliveira et al., 2004), which we corroborate in our setting using newspaper advertisements of infant formula prices. Retail prices would have to rise by about 5 percent for this cost to equal the hospital savings (Section 10.2).

Our results contribute to the literature on how programs and recipients respond to the funding mechanisms of safety net programs. Within WIC, this work has focused on cost-containment efforts aimed at retailers and their implications for retail prices and participants' access to WIC (Meckel, 2020; Meckel, Rossin-Slater and Uniat, 2023). In other settings, research has studied how programs respond to additional resources and how those resources affect participants (Ludwig and Miller, 2007; Baicker and Staiger, 2005; Herbst and Kose, 2024). We combine these strands of the literature by considering a large cost-containment effort that was in effect a large financial shock, and we consider how the program itself responds.

We also contribute to the broader literature on the importance of in-utero conditions and targeted transfers for health and long-run outcomes (Almond and Currie, 2011; Almond, Currie and Duque, 2018; Bitler, Hines and Page, 2018), and in particular the sizable literature on WIC and in-utero conditions (Bitler and Currie, 2005; Joyce, Gibson and Colman, 2005; Joyce, Racine and Yunzal-Butler, 2008; Figlio, Hamersma and Roth, 2009; Currie and Rajani, 2015). Existing work has studied participants' direct access to WIC, through the program's rollout across counties (Hoynes, Page and Stevens, 2011; Bitler et al., 2025) or the opening of nearby clinics (Rossin-Slater, 2013); we instead study how funding for the program, and the program's own decisions about whom to serve, affect access and infant health. We also provide suggestive evidence of spillovers to younger siblings, which relates to new work showing that mothers' diet quality falls when their children lose WIC (Bitler et al., 2023).

Finally, we contribute to research on the effects of procurement systems across safety net programs, such as Medicaid (e.g., Scott Morton, 1997; Duggan and Scott Morton, 2006; Dranove, Ody and Starc, 2021) and Medicare (e.g., Duggan and Scott Morton, 2010; Lakdawalla and Yin, 2015; Ding, Duggan and Starc, 2025), and in particular WIC. To date,

work on WIC procurement of infant formula centers on the market for formula (Betson, 2009), including the size of winning rebates (An et al., 2023; Davis and Oliveira, 2015), how manufacturers bid and whether contracts raise wholesale prices (Davis, 2012), effects on retail prices outside WIC (Oliveira et al., 2004; U.S. Government Accountability Office, 2025), and the contract brand’s share of formula sold outside WIC (Oliveira, Frazao and Smallwood, 2011; Huang and Perloff, 2014; Rojas and Wei, 2019; Choi et al., 2020; Abito et al., 2026). We build on these papers and focus instead on how WIC used the savings.

The remainder of the paper is organized as follows. Section 2 provides institutional background. Section 3 describes the data, and Section 4 describes the sample and descriptive statistics. Section 5 presents the empirical strategy and tests of its assumptions. Section 6 presents the effects on participation and program finance, Section 7 the effects on infant health, Section 8 heterogeneity, and Section 9 robustness. Section 10 considers magnitudes and costs and benefits. Section 11 concludes.

2 Institutional Background

2.1 The WIC Program

The Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) is a federally funded nutrition program that provides in-kind benefits to low-income pregnant and postpartum women, infants, and children under age five. Established in 1972, and made permanent in 1974, WIC offers food packages for different age and need groups and offers nutrition education and referrals to health services. Infants receive iron-fortified infant formula, and women and children receive foods such as milk, cheese, eggs, cereal, and juice.

To qualify, applicants must have family income at or below 185 percent of the federal poverty level and be at nutritional risk (U.S. General Accounting Office, 1990). Participants receive vouchers or checks for specific foods, redeem them at authorized grocery stores, and the state reimburses the store.¹

WIC is a discretionary grant program, meaning that Congress sets an annual appropriation, and the U.S. Department of Agriculture allocates it to states as a food grant and a grant for nutrition services and administration. In fiscal year 1988, the \$1.8 billion appropriation served an average of 3.6 million participants a month, and in fiscal year 1989, \$1.9 billion served 3.9 million (Food Research and Action Center, 1989). The USDA estimated that

¹A few agencies instead used home delivery, in which contractors bring food to participants’ homes, or direct distribution, in which participants pick up food that the program buys in bulk (U.S. General Accounting Office, 1990).

7.45 million women, infants, and children were fully eligible in 1984, meaning the program reached only about half of them.

States could not spend more than their grants, and funds left at the end of the year were returned. Because caseloads are hard to control month to month, many states returned part of their grants because they were cautious not to overspend ([Food Research and Action Center, 1989](#)). Some states also supplemented their federal grants with state funds.

2.2 Rationing and the Priority System

When the grant cannot cover every eligible applicant, federal regulations require states to fill slots in order of priority ([Figure 1](#); [FNS, 2023](#)). Pregnant and breastfeeding women and infants with a documented medical risk are served first. Children with a medical risk come next, followed by women and infants whose only risk is an inadequate diet. Children whose only risk is an inadequate diet and postpartum women who are not breastfeeding are served last. In practice, this meant that despite state efforts to expand WIC caseloads, when funding ran short states would stop enrolling (or recertifying) participants in lower priority groups. In 1984, WIC served an estimated 67 percent of eligible infants but only 32 percent of eligible children ([U.S. House of Representatives, Select Committee on Hunger, 1988](#)).

Funding fell well short of need in the late 1980s. In surveys of state WIC directors in 1988 and 1989, 84 percent cited inadequate funding as a major problem in their states ([Food Research and Action Center, 1989](#)). In the 1989 survey of WIC directors in 43 states and the District of Columbia, 23 percent had cut off children above some age or stopped serving children with only dietary risk, and 28 percent reported waiting lists in their states ([Food Research and Action Center, 1989](#)).²

The pressure peaked in 1990. WIC food costs had risen 7 percent by the middle of fiscal year 1990, against a USDA forecast of less than 4 percent ([U.S. General Accounting Office, 1992](#)). In the spring, 25 states cut caseloads in the middle of the fiscal year, planning to remove 242,000 people from the program between March and September, and by June 8, 41 states were not serving all priority groups ([U.S. House of Representatives, Committee on the Budget, Task Force on Human Resources, 1990](#)). Some states removed expensive items from the food package, and others stopped enrolling lower priorities, including pregnant women at dietary risk ([U.S. General Accounting Office, 1992](#)).

²We note that this is likely a lower bound, since agencies may have had excess demand without formally operating a waiting list.

2.3 Infant Formula and Cost Containment

Against the backdrop of these fiscal pressures, WIC adopted cost containment efforts to procure infant formula. Infant formula was, and continues to be, the program’s most expensive food product. Although infants made up about one-third of WIC participants, formula accounted for nearly 40 percent of WIC food costs, and its price doubled during the 1980s (U.S. General Accounting Office, 1990). Between 1979 and 1988, formula prices rose 9.5 percent a year, which is on par with prescription drugs, which rose by 9.6 percent per year, and in contrast to fresh whole milk, which rose by 2 percent. Before cost containment, WIC paid the full retail price for formula, and participants could choose among brands.

Adoption accelerated after the fiscal year 1989 agricultural appropriations act, enacted in October 1988, which required every state agency to study formula cost containment and, where it would lower costs, to adopt it within a year (Oliveira et al., 2001; Food Research and Action Center, 1989). Some states, though, had adopted contracts before the federal mandate.

Under a sole-source contract, the state issued a request for proposals, and the manufacturer offering the highest rebate per can, or the lowest net price, became the only formula provided to WIC participants through retail stores (Food Research and Action Center, 1989; U.S. General Accounting Office, 1990).

An alternative system was the open market, in which the state negotiated a rebate with any manufacturer willing to pay one. Every company could continue to sell formula through WIC, participants chose freely among brands, and WIC staff could not encourage participants to choose brands that paid rebates. As a result, manufacturers had little incentive to offer large rebates (U.S. General Accounting Office, 1990).

Sole-source contracts proved far more valuable to states. By the end of 1989, open-market rebates averaged about \$0.95 per can, when sole-source rebates averaged \$1.37 and had peaked at \$1.52. In June 1989, the wholesale price of a 13-ounce can of concentrate was \$1.45 to \$1.58. After rebates, agencies with sole-source contracts paid \$0.36 less per can than agencies with open-market contracts, or 24 percent less (U.S. General Accounting Office, 1990). For example, California’s three-year contract, signed in 1989, was expected to save \$4.8 million a month, or \$172.8 million over the life of the contract (Food Research and Action Center, 1989). As existing open-market contracts expired, states moved to sole-source contracts to comply with a 1989 congressional mandate for competitive bidding (U.S. Congress, 1989), and every state in our sample had adopted one by fiscal year 1996 (Appendix Table A1). Today, states that deliver WIC benefits through retail stores must competitively bid their infant formula contracts, typically awarding a single supplier (U.S. Department of Agriculture, Food and Nutrition Service, 2024).

Competitive bidding was logistically feasible for WIC because the formula market was highly concentrated. In 1987, three manufacturers produced about 99 percent of the formula sold in the United States, where sales exceeded \$1.4 billion: Ross (55 percent), Mead Johnson (35 percent), and Wyeth (9 percent). This meant that a state could solicit bids from every major supplier. Ross and Mead Johnson won almost all of the rebate contracts ([U.S. General Accounting Office, 1990](#)). Winning a contract was valuable because WIC was a large share of the formula market. In 1987, WIC purchases amounted to roughly one-third of all formula sold in the United States ([U.S. General Accounting Office, 1990](#)), and today WIC participants account for over half ([Oliveira, Frazao and Smallwood, 2011](#)).

Winning a contract was valuable to manufacturers beyond WIC's own purchases. The winning brand received exclusive WIC shelf space, and stores were required to keep it in stock. One manufacturer reported that winning a state contract in the 1980s raised its share of that state's formula market by 41 percentage points ([U.S. General Accounting Office, 1990](#)), and in the 2000s the winning brand's share rose by an average of 74 percentage points ([Oliveira, Frazao and Smallwood, 2011](#)).

2.4 How Rebates Enter State Budgets

Under a sole-source contract, the state agency continues to pay retailers the sticker price of each can of the contract brand that participants redeem. However, at some predetermined time period (typically monthly or every 3 months), state agencies share how much they have spent and are reimbursed the negotiated rebate by manufacturers; this money is returned to the state's WIC budget ([Food Research and Action Center, 1989](#)). The rebate thus reduces the net cost of the infant food package to the state, while participants receive the same package and retailers are paid in the same way as before. In our sample, rebates ranged from 15 to 37 percent of a state's food costs after adoption.

Contemporary accounts suggest that states often used this money to serve more people. GAO estimated that rebates saved \$33 million in fiscal year 1988, enough to support about 74,000 participants a month, and \$293 million in fiscal year 1989, enough to support about 400,000 more, and credited the savings with helping raise coverage from less than half of eligible women, infants, and children to about 60 percent ([U.S. General Accounting Office, 1990](#)). By fiscal year 1991, the USDA estimated rebates totaled about \$600 million a year, enough to serve an additional 950,000 participants each month ([U.S. Department of Agriculture, Food and Nutrition Service, 1991](#)). Various accounts from state WIC directors reflect the same narrative; Appendix Table [A2](#) provides anecdotal evidence from nine states about what WIC directors say about their funds.

Narratives of expansion are also consistent with aggregate caseload data, which show significant expansion after 1987. National WIC caseloads grew from 3.4 million participants in fiscal year 1987 to 5.9 million in fiscal year 1993, and the number of children enrolled rose from 1.7 million to 2.8 million. Figure 2 shows the same growth in our sample states. Per 100 births, the number of infants enrolled rose from 22 in fiscal year 1985 to 44 in 1993, the number of children from 38 to 67, and the number of women from 17 to 35.

While states could spend additional money to support additional participants, they were not required to do so. For example, the rebate money might have instead crowded out state funding or been used to change elements of the food package (e.g., substituting peanut butter for beans). Starting in 1988, some funds could be used for administrative costs (U.S. House of Representatives, Select Committee on Hunger, 1988; U.S. Congress, 1988).

Finally, it is important to consider how rebate savings interacted with the broader WIC funding system. States' engagement in cost-containment efforts did not increase their appropriations from Congress, and funding mechanisms actually worked in part to decrease Federal funds. Grants were allocated by a formula that protected each state's previous year's grant and directed new money toward states serving a smaller share of their eligible population (U.S. House of Representatives, Select Committee on Hunger, 1988; U.S. General Accounting Office, 1985). This meant that a state that becomes more efficient with money (can use equivalent federal funds to serve more people) becomes a lower priority for additional funds. This is because funds are distributed first so states can maintain or 'stabilize' their current caseloads, and only when funds remain are they distributed to states to facilitate 'growth'.

This dynamic is highlighted in testimonies before Congress in which the USDA suggested that rebates had eliminated the need for significantly larger federal funding for WIC (Food Research and Action Center, 1989). State directors, meanwhile, worried that the rebate money was temporary and would not compensate for potential losses from federal funding. Therefore, whether and to what extent the rebate system expanded caseloads is an empirical question.

3 Data

Our analysis relies on administrative data, including vital statistics records, state bidding data, and newly digitized WIC food costs, grant, and participation data. Below, we describe key data sources in detail and briefly outline the datasets used to construct controls or population counts.

3.1 Rebate Bid Data

This dataset includes information on each formula bid to each state, including the date of the bid and when it went into effect (if relevant), the rebate amount, the name of the formula company, the type of bid system (open-market or sole-source), and whether that bid won or not.³ We use this rich dataset to construct the rollout of cost-containment approaches across states. Specifically, we construct a dataset that identifies the year and month when a rebate contract took effect.

Many states first operated an open-market contract and later switched to sole source. We define our treatment as the date of a state’s first sole-source contract. We reconcile the contract dates in the rebate-history data against the USDA’s national list of rebate contracts in force on May 15, 1991 (U.S. Department of Agriculture, Food and Nutrition Service, 1991). Across the two sources, the month and contract type agree for all sole-source adopters with the exception of Ohio (the USDA list indicates that Ohio operated open-market contracts from October 1989 through September 1993). Appendix Table A1 lists every state’s contract dates.

3.2 WIC Program Participation Data

We use state participation data by category (women, infants, and children) from USDA state participation tables for fiscal years 1974–1997, which report average monthly participants by category for each state (Henchy, Weill and Parker, 1999). Because these data report the average monthly caseload in each fiscal year, rather than the number of unique participants or new enrollments over the year, an increase in caseload could reflect new entrants, longer program spells, or both. Unfortunately, the women’s category pools pregnant, breastfeeding, and non-breastfeeding postpartum women, and so we are unable to separately identify prenatal participation. We divide each series by the population of the relevant age group and report effects per 1,000.

3.3 WIC Program Finance Data

We measure program finances using three annual state series: federal WIC grants, food costs, and manufacturer rebates. Food costs through fiscal year 1988 come from the USDA’s *Agricultural Statistics* yearbooks. From fiscal year 1989 on, food costs and rebates come from FNS administrative data, which report each state’s total food cost and the rebates it collected. Rebates are first reported for fiscal year 1989. Federal WIC grants come from the

³We are grateful to Davis (2016), who generously shared data on the rebate system.

Census Bureau’s annual reports on federal expenditures by state, which list federal grants to state and local governments by program (U.S. Bureau of the Census, 1986–1994). Gross food cost is total food spending, including spending financed by rebates.

We express the finance measures in two ways. For per-eligible-resident measures, we divide the federal grant and gross food cost by the combined population of women aged 15–44, infants, and children aged 1–4, using SEER population estimates. For each participant, we divide the federal grant and gross food cost by the average monthly caseload to get annual dollars per participant. Intuitively, the per-resident measures capture changes in total spending and federal funding, whereas the per-participant measures capture whether the program spent more or less on each participant.

3.4 Natality Data

We use public-use Natality records from 1983–1993 and aggregate individual records to state-by-year cells, dating each birth by the month its second trimester began. We define a birth as treated if a sole-source contract was in force by the second trimester of the pregnancy. Outcomes are mean birth weight, the share below 2,500 grams, and the share small for gestational age (SGA) defined as births below the 10th percentile of birth weight for their completed week of gestation and sex. We limit all analyses to U.S. singleton births with a recorded birth weight. We consider several dimensions of heterogeneity based on birth characteristics, including first versus higher-parity birth, mothers’ education (at most a high-school education, some college, or a college degree), and mothers’ age at birth. We weight birth outcomes by the number of births in the cell or subgroup.

3.5 Infant Formula Price Data

Administrative price series and scanner data are not available for the full adoption period. We therefore digitize advertised prices for 13-ounce liquid-concentrate infant formula from local newspapers, producing a new city-by-brand panel for the late 1980s and early 1990s. This dataset is incomplete in several respects: advertised prices are often sale prices, the panel is unbalanced, and not all cities are covered. We therefore interpret the price results as suggestive.

3.6 Other Controls

We construct two pre-determined state characteristics used as controls, including the share of a fixed national sample of pregnant women who would be eligible for Medicaid under

each state’s 1986 rules (simulated eligibility), and the change in that share between 1986 and 1991. Participation measures are divided by the relevant age-eligible population from annual state-level SEER estimates (women 15–44, infants 0–1, children 1–4) or by the number of births.

4 Sample Construction and Descriptives

In this section, we describe the states in our sample, the timing of sole-source adoption, and WIC participation, program finances, and birth outcomes before adoption.

4.1 Sample Construction

Our analysis sample has 46 states. Mississippi and Vermont distributed formula directly or through home delivery and never used retail rebates, and Nevada returned to open-market contracting in 1990, so we drop its later years. We also exclude Massachusetts, New York, and Pennsylvania because of those states’ high reliance on state-supplemented funds. In fiscal year 1989 their state WIC funds equaled 29, 24, and 17 percent of their federal food spending, against at most 11 percent, and an average of about 1 percent, in every other state (Appendix Table A3). Given high rates of self-funding, it is less likely that WIC operations will be directly impacted in these states. Rebates may have also let states cut their own contributions, but this outcome is not observable.

4.2 Timing of Adoption

Sole-source adoption was concentrated in a short window (Figure 3 and Appendix Figure A1). No births in our sample states were covered by a sole-source contract before fiscal year 1988; the share was 48 percent in 1989, 63 percent in 1991, and 87 percent in 1993. Of the 46 states in our sample, 2 adopted in fiscal year 1988, 9 in 1989, 6 in 1990, 4 in 1991, 12 in 1992, and 6 in 1993. The remaining 7 had not adopted a sole-source contract by the end of fiscal year 1993. Our design compares each of these cohorts to the states that adopted later, or that had not adopted by the end of our sample.

4.3 Descriptive Statistics

Table 1 reports state characteristics in fiscal year 1986, before any state had adopted, for all 46 states and by the timing of adoption. States that adopted by fiscal year 1990 look similar to later adopters in WIC participation, spending per participant, and mean birth weight.

They were larger, had higher low birth weight rates and more births to Black mothers, and went on to have larger Medicaid expansions, which reflects the early adoption of sole-source contracts in the South. Before adoption, WIC served 40.7 participants per 1,000 age-eligible residents; 12.1 women per 1,000 women aged 15–44, 241 infants per 1,000 infants, and 101 children per 1,000 children aged 1–4. Per 100 births, states enrolled 19.8 women and 41.6 children. The average state spent \$387 per participant per year and received a federal grant of \$494 per participant. Average birth weight was 3,368 grams, 5.9 percent of births were low birth weight (less than 2,500 grams), and 9.3 percent were small for gestational age.

5 Methodology

We use a difference-in-differences approach exploiting differences in the timing of when each state adopted its first sole-source contract. We define a fiscal year as treated if a contract was in force by April, so that it covered at least half of that year’s average monthly caseload. For example, a contract that took effect in January 1990 treats fiscal year 1990 (October 1989 to September 1990), while a contract that took effect in June 1990 first treats fiscal year 1991. Births are treated according to the pregnancy rule described above. Once a state adopts, it remains treated.

The simple two-way fixed effects representation of our model is as follows:

$$(1) \quad Y_{st} = \tau [SS \times Post]_{st} + \gamma_s + \delta_t + X'_{st}\beta + \epsilon_{st},$$

where $[SS \times Post]_{st}$ indicates a state-year observation for which a sole-source contract is in effect, γ_s is a state fixed effect, δ_t is a year fixed effect, and X'_{st} is a vector of controls.

An important literature has identified that the TWFE approach does not recover a difference-in-differences estimator in the presence of treatment heterogeneity, either across treatment cohorts or over time (Goodman-Bacon, 2021; de Chaisemartin and D’Haultfoeulle, 2020). This occurs due to the “forbidden comparison,” in which previously treated states are used as comparison groups for newly treated states. This is a concern in our setting since, as we show below, treatment effects grow over time. We therefore implement a stacked difference-in-differences estimator instead (Cengiz et al., 2019). For each adoption cohort g , we build a sub-experiment containing the states that adopted in g and the states that had not yet adopted by the end of the event window, $g + 3$ (“clean controls”), using years $g - 4$ through $g + 3$. We then stack these sub-experiments and estimate:

$$(2) \quad Y_{sdt} = \sum_{k \neq -1} \tau_k \mathbf{1}[t - g_d = k] \times T_{sd} + \gamma_{ds} + \delta_{dt} + \sum_j \lambda_{jdt} X_{js} + \epsilon_{sdt},$$

where d indexes the sub-experiment, T_{sd} indicates the adopting cohort, and γ_{ds} and δ_{dt} are sub-experiment-specific state and year fixed effects. Since each cohort is only ever compared to its own set of clean controls, previously treated states are never used as a comparison group.

X_{js} are two pre-determined state characteristics, each interacted with sub-experiment-by-year effects: simulated Medicaid prenatal eligibility in 1986 and its change between 1986 and 1991. We include these controls because Medicaid eligibility for pregnant women expanded rapidly over this period, and we want to allow states that experienced larger expansions to be on different trajectories. Adding time-varying simulated eligibility instead leaves the estimates essentially unchanged (Appendix Table A4).

We report the average of the event-time effects for years 0 through 3 relative to year -1 . Note that year 3 is only observed for the 17 states that adopted by 1990, about half of the adopters. Our sample ends in fiscal year 1993, when 39 of the 46 states had adopted a sole-source contract.

Across all estimates, standard errors are clustered at the state level since the adoption of the sole-source system is a state-level decision. We also weight by the denominator of each outcome variable. For example, when the outcome is the average birth weight in a state in a given year, we weight by the number of births in that state-year.

As robustness, Appendix Tables A5 and A6 also present the Callaway–Sant’Anna estimator with and without the Medicaid controls (Callaway and Sant’Anna, 2021), as well as stacked variants without the Medicaid controls or with not-yet-treated comparison states. Our participation, finance, and birth-weight results are consistent across these estimators.

5.1 Validity of Parallel Trends

Our identifying assumption is that, absent adoption, outcomes in adopting states would have evolved as they did in states that had not yet adopted. While we cannot test this assumption directly, we can consider whether adopting and not-yet-adopting states were on similar paths before adoption. Figures 4–6 present our estimates in event-study form. Joint tests of the three leads do not reject for total participation ($p = .11$), women’s participation ($p = .26$), children per birth ($p = .20$), or birth weight ($p = .89$), and outcomes begin to move only at or after adoption. One exception is children’s enrollment per 1,000 children, which rises gently before adoption in the pooled stack. When we estimate this separately, the pre-trend comes from the states that did not cut caseloads in 1990; in the cutback states, children’s enrollment is flat before adoption and jumps after it. While these patterns do not prove that a difference-in-differences framework is appropriate, they are at least not evidence against

it.

We also check whether pre-period characteristics predict when a state adopted, by regressing the fiscal year of adoption on state characteristics measured in fiscal year 1986 and their 1984–86 changes (Appendix Table A7). Pre-period birth outcomes, participation, and their trends do not predict timing. Earlier adopters were more often in the South and had larger subsequent Medicaid expansions, which our design accounts for through the sub-experiment-by-year effects and the Medicaid controls.

The main competing explanation for improved birth outcomes is the contemporaneous expansion of Medicaid. Between 1986 and 1990, federal legislation allowed and then required states to extend Medicaid to pregnant women with incomes well above the cash-welfare thresholds, and these expansions improved birth outcomes among the newly eligible (Currie and Gruber, 1996). The expansions could also directly raise WIC enrollment, because Medicaid recipients are income-eligible for WIC. We find some evidence that simulated eligibility for Medicaid trended differently prior to adoption; however, we do not find evidence of significant changes in simulated eligibility following the adoption of sole-source contracts (Appendix Figure A2). Nonetheless, to address this concern, our regressions control for each state’s 1986 simulated eligibility and its change between 1986 and 1991, interacted with year fixed effects, so that states with larger expansions can follow different paths. Our estimates are also unchanged when we add simulated eligibility as a time-varying control (Appendix Table A4).

6 Results: Program Dynamics

We first consider how the rebate system changed the operation of the WIC program. We consider program participation and then program expenditures.

6.1 WIC Participation

Table 2 presents the effects of sole-source contracts on total, women’s, infants’, and children’s participation. We also present the estimates in event-study form in Figure 4; Appendix Table A8 reports the coefficients.

We find that sole-source contracts increased program participation. Overall enrollment in WIC increased by 5.8 people per 1,000 age-eligible population (standard error 1.5), or 14 percent of the pre-adoption mean of 40.7. While participation increased for all groups, women and children experienced the largest increases (15 and 17 percent, respectively) compared to infants (5 percent, and imprecisely estimated). The largest response being concentrated

among children is consistent with their being lower on the priority lists relative to other groups.

The effects build over time. Total participation is 3.4 per 1,000 higher in the year of adoption (8 percent), 4.0 (10 percent) in the first year after, 6.7 (16 percent) in the second, and 9.2 (23 percent) in the third. This gradual increase is consistent with states reopening enrollment to lower-priority groups and recertifying children who would otherwise have been dropped from the program.

Per birth, which is the relevant scale for infant health, women enrolled per 100 births rose by 2.1 (11 percent) and children by 6.7 (16 percent).

6.2 WIC Expenditures

Next, we consider how the expansion was financed. Table 3 and Figure 5 present the effects on two series: the federal grant and gross food spending, which includes spending financed by rebates.

Gross food spending per age-eligible resident increased by \$1.85 (10 percent). Gross food spending per participant, however, did not increase; it fell by a statistically insignificant \$8 (2 percent). This negative estimate may reflect caseloads shifting toward children, who are lower-cost participants relative to women or infants. While we do not directly observe the value of WIC food packages or redemption, a negative point estimate on gross food spending per participant is inconsistent with changes in redemption behavior or in the value of the food package.

To rule out that the expansion was driven by other sources of financing, we test whether federal grants to states responded to rebates. We find no evidence that the federal grant paid for the expansion. In fact, the grant per age-eligible resident fell by \$0.95, or 4 percent of the pre-adoption mean of \$23.1. This result likely reflects the mechanical relationship between cost containment and states' priority for funding under WIC's funding formula; as states are able to serve more of their eligible population with less money, they lose priority for additional funding. Because caseloads grew while the grant did not, the grant per participant fell by \$56 a year, or 11 percent of the pre-adoption mean of \$494.

7 Results: Birth Outcomes

This section presents our main results on birth outcomes; these include average birth weight, the incidence of low birth weight, and the share of births that are small for gestational age.

7.1 Main birth effects

Table 4 presents the main impacts of sole-source contracts on birth outcomes. We find that adopting a sole-source contract is associated with a 3.6-gram increase in average birth weight (standard error 1.0). We also observe a 0.19 percentage point decrease in the share of births small for gestational age (standard error 0.05), which represents a 2 percent decrease on a baseline of 9.3 percent. The incidence of low birth weight also falls, by 0.07 percentage points, but this estimate is not statistically significant. Together, these results are consistent with sole-source contracts improving newborn health.

7.2 Dynamic effects

Figure 6 considers the effects over time. We observe few differences in birth outcomes between adopting and not-yet-adopting states before adoption; no lead is large in magnitude or statistically significant. Following adoption, the gains build at about the same pace as participation. Birth weight is 2.1 grams higher in the year of adoption, 2.9 grams in the first year after, 4.4 in the second, and 4.9 in the third, and the share small for gestational age is 0.10, 0.18, 0.23, and 0.26 percentage points lower over the same years (Appendix Table A10).

7.3 Gestation and the composition of births

The effect appears to be a shift in birth weight at a given gestation rather than a change in gestation (Appendix Table A11). Birth weight among term births rose by 3.2 grams, and mean gestation did not change. The share of births between 2,500 and 2,999 grams fell by 0.21 percentage points, and the share at 4,000 grams or more rose by 0.11 points. We also find no significant change in the composition of births by mother’s age, race, or marital status at adoption (Appendix Table A12), which suggests that the effects are not driven by changes in who gives birth.

8 Heterogeneity

Figure 7 shows heterogeneity in participation, and Figures 8, 9, and 10 show heterogeneity in birth weight, low birth weight, and small for gestational age. Tables 5 and 6 report the estimates behind the figures, and Appendix Tables A13–A18 report them by dimension with pre-adoption means and tests of the pre-adoption coefficients.

8.1 By Financial Constraints of the State

States whose WIC programs were faring worse financially benefited the most. In the spring of 1990, rising food prices and flat appropriations forced 25 states to cut WIC caseloads in the middle of the fiscal year, and the National Association of WIC Directors reported that 41 states restricted entry by priority group ([U.S. House of Representatives, Committee on the Budget, Task Force on Human Resources, 1990](#)).

We classify as cutback states the 21 states that reported cutting caseloads by at least 2,000 participants. Of these, 20 are in our sample and 16 had adopted a sole-source contract by fiscal year 1993. Prior to adoption, cutback states look quite similar to other states. They served 42.2 participants per 1,000 age-eligible population (compared to 43.4 in other states), and average birth weight was 3,370 grams (compared to 3,359). Because for some states this split was determined after treatment, we interpret this exercise as descriptive in order to identify which states were rationing, rather than as heterogeneity by a strictly predetermined characteristic.

In the cutback states, sole-source contracts raised total participation by 18 percent, women’s participation by 20 percent, and children’s enrollment per birth by 23 percent (Figure 7; Appendix Table A13). In the other states, participation rose by 5 to 6 percent, and the increase is not significant. Women’s participation in the other states did not change.

Birth outcomes follow the same pattern (Appendix Table A14). In the cutback states, birth weight rose by 5.7 grams, and by 10.6 grams for mothers with at most a high-school education, and low birth weight fell by 0.12 percentage points. In the other states, birth weight rose by 2.3 grams, significant only at the 10 percent level. The share small for gestational age fell by similar amounts in both groups (0.17 and 0.16 points).

8.2 By Maternal Characteristics

Next, we consider which births benefited most from the expansion. Given that WIC is means-tested, we expect the effects to be concentrated among births to mothers who are likely eligible for WIC. In this sense, heterogeneity by mother’s education provides a natural placebo test of whether our results are in fact driven by changes in the WIC program. Appendix Table A15 presents results separately for mothers with at most a high-school education, mothers with some college, and mothers with a college degree. Consistent with our expectations, the gains are concentrated among the least-educated mothers. Among births to mothers with a high-school education or less, average birth weight increased by 6.7 grams, the incidence of low birth weight fell by 0.17 percentage points (2.5 percent of the baseline rate of 7.1 percent), and the share small for gestational age fell by 0.34 percentage

points (3 percent of baseline). In contrast, we find no evidence of effects among mothers with some college or mothers with a college degree, who are rarely income-eligible for WIC. The birth-weight estimates for these groups are -0.7 and -0.8 grams, respectively, and neither the low-birth-weight nor the SGA estimates are statistically significant.

We also consider heterogeneity by mother’s age (Appendix Table A16). Teen mothers, who are more likely to be eligible, appear to have gained somewhat more. Births to mothers under 20 gained 4.7 grams (standard error 2.4) compared to 3.1 grams (standard error 1.0) for older mothers, and the share small for gestational age fell by 0.35 percentage points for teen mothers compared to 0.16 for older mothers.

We find few differences by mother’s race (Appendix Table A17). Births to Black and white mothers gained 2.4 grams (standard error 3.3) and 2.7 grams (standard error 1.1), respectively. The SGA estimate for white mothers is -0.19 percentage points, while the estimate for Black mothers is close to zero but imprecisely estimated, likely because Black births are concentrated in a small number of states.

Finally, we consider whether there are any differential effects by birth parity (Appendix Table A18). This is motivated by our previous finding that children’s enrollment (aged 1–4) grew the most in response to WIC expansions. We find that higher-order births gained 4.3 grams (standard error 1.3), while first births gained only 1.5 grams (standard error 1.8), which is not statistically significant. The share small for gestational age, however, fell by a similar amount for both groups (0.17 percentage points each). This provides some evidence that higher-parity births responded more strongly than first births, consistent with a story of household spillovers whereby older siblings’ access to WIC mattered for the in-utero conditions of younger siblings.

9 Robustness

9.1 Estimators

Appendix Tables A5 and A6 compare our main estimates with other estimators and specifications. Specifically, we compare our preferred specification, the stacked DiD with Medicaid controls, with a stacked DiD without Medicaid controls, a stacked DiD that uses not-yet-treated units, and the Callaway–Sant’Anna estimator (Callaway and Sant’Anna, 2021), both with and without Medicaid controls. Across these alternative estimators, we find robust evidence for increases in participation, with effects ranging from 4.1 to 5.8 per 1,000, and robust evidence for increases in birth weight, with estimates ranging from 4.4 to 5.8 grams. We find, if anything, that other estimators produce slightly larger effects on birth weight, which

may be attributed to poorly controlling for contemporaneous Medicaid expansions.

9.2 Sample

Appendix Tables A19 and A20 show how the estimates change with the sample. When we include the states that, at baseline, significantly self-funded their WIC programs (NY, PA, and MA), we find mostly similar results. Total participation rises by 5.1 per 1,000, slightly less than in our sample, and birth weight rises by 4.3 grams, slightly more. Some participation groups, however, such as women’s participation, fall to 0.7 per 1,000 (5.5 percent of the pre-adoption mean) and are no longer statistically significant. NY and PA appear to drive this difference in women’s caseloads.

9.3 Influential States

Appendix Figure A3 drops one adopting state at a time. Total participation stays between 4.9 and 6.6 per 1,000, women’s participation between 1.4 and 2.0 per 1,000, and birth weight between 3.3 and 4.1 grams. Every participation and birth-weight estimate remains significant at the 5 percent level, except one women’s estimate ($p = .054$). The gain for births to mothers with at most a high-school education stays between 5.5 and 7.3 grams. We conclude that no single state is driving our results.

9.4 Open-Market Contracts

In this section, we ask whether the effects come from sole-source contracting itself or from any rebate contract. Thirty-one states in our sample first adopted open-market or multi-source contracts, which preserved brand choice and earned smaller rebates (U.S. General Accounting Office, 1990). We therefore compare the effect of adopting a sole-source contract with the effect of adopting an open-market contract. Because every state had some contract by fiscal year 1990, and many states switched from open-market to sole-source, we can estimate the open-market effect only in its first year of adoption (Appendix Table A21 and Appendix Figure A4). Therefore, we compare the open-market contract effect with the effect of sole-source contracting (our main estimate) and the effect of sole-source contracting measured in its first year of operation.

The measured effect of sole-source contracting is much larger than that of open-market contracts; sole-source contracts raised total participation by 5.8 per 1,000 and birth weight by 3.6 grams, compared with 2.0 per 1,000 and 1.2 grams for open-market contracts. However, when effects of sole-source contracting are measured in their first year alone, they look

somewhat more similar but are still consistently larger than those of open-market contracts (3.4 per 1,000 and 2.1 grams, both statistically significant, compared with 2.0 per 1,000 and 1.2 grams, neither significant at the 5 percent level). We conclude that there is not strong evidence that open-market contracts had large effects, and ignoring their effects would lead to underestimating the effects of sole-source contracting.

9.5 Parallel Trends

We next ask whether our estimates are sensitive to violations of parallel trends. Following Roth (2022), we calculate the linear pre-trend that our joint test of the leads would detect with 50 percent power. We then plot the event-study coefficients we would expect to observe if that trend were real, but the test failed to reject (Appendix Figure A5 and Appendix Table A22). For participation, a trend of 1.5 participants per 1,000 per year would be detected with 50 percent power and would produce an average post-adoption effect of 3.2 per 1,000, compared with our estimate of 5.8. For birth weight, a 1-gram-per-year trend would produce 2.9 of our estimated 3.6 grams. Thus, the leads are not precise enough to rule out pre-trends that explain part of our effects. However, for both outcomes, the post-adoption coefficients lie above the hypothesized trend at every horizon.

We also report robust confidence intervals following Rambachan and Roth (2023), allowing post-adoption deviations from parallel trends up to $\bar{M}$ times the largest pre-adoption deviation (Appendix Figure A6). The participation estimate remains significant up to $\bar{M} = 0.75$, and birth weight up to $\bar{M} = 0.5$. Both estimates remain just significant when we allow the linear pre-trend to continue after adoption (a smoothness restriction with $M = 0$), but not when we allow additional curvature.

10 Magnitudes and Cost-Benefit Analysis

10.1 Magnitudes

Our estimates are reduced-form effects on the full population of births in a state. In order to benchmark them against the existing literature, which typically reports the effect of WIC participation itself, we scale the birth-weight effect by the change in participation. Because we find that the gains operate through the household rather than through the pregnant woman alone, our preferred scaling divides the birth-weight effect by the change in all participants enrolled per 100 births (women, infants, and children). Participants enrolled per 100 births rose by 10.1 (standard error 2.9), which implies an effect of 35 grams per newly enrolled participant (95 percent confidence interval from 8 to 62 grams).

We interpret this with caution for two reasons. First, our participation data are average monthly caseloads, not the total number of unique participants served. Second, it is possible that birth weight is affected by channels other than new enrollment that we cannot observe (e.g., higher quality care in WIC clinics). Both scenarios suggest that the change in caseload may understate the number of households affected, meaning that the 35-gram figure we report is too large. Dividing instead by the share of births to mothers enrolled before adoption (19.8 per 100) gives a TOT of 18 grams. We estimate the true TOT is likely between 18 and 35 grams. This puts our estimate in the range of existing treatment-on-the-treated estimates, 18 grams in [Hoynes, Page and Stevens \(2011\)](#), 27 grams in [Rossin-Slater \(2013\)](#), and a lower bound of 33 grams for births to Black mothers in [Bitler et al. \(2025\)](#), though toward the higher end.

10.2 Cost-Benefit Analysis

Between fiscal years 1989 and 1993, manufacturers paid \$2.05 billion (\$3.35 billion in 2011 dollars) in rebates to the states in our sample, which the states used to expand the WIC program. Rebates are therefore a transfer from infant formula manufacturers to WIC. They are a saving to the government, but a redistribution rather than a reduction in the resources used. Below, we discuss the system’s potential costs and benefits.

One potential cost of the rebate system falls on families who buy formula outside WIC if manufacturers raise retail prices to recover the rebates. A robust literature examines the effects of retail pricing when contracts switch within a state from one manufacturer to another. Estimates find that prices of the winning contract increase by around 1.7 percent for contract products in recent contracts ([U.S. Government Accountability Office, 2025](#)), and the contract brand’s retail price rose more than other brands’ after earlier contract switches ([Oliveira et al., 2004](#)). [Davis \(2012\)](#) finds that contracts do not raise wholesale prices. We assess this question in our setting using price data on infant formula collected in newspaper advertisements. We find that the advertised price of the contract brand of 13-ounce concentrate rises by 2.5 percent relative to other brands after a state adopts, with a standard error of 2.0 percent (Appendix Table [A23](#)). Applied to the roughly \$6.7 billion (in 2011 dollars) spent on formula outside WIC in our sample states over this period, a 1.7 to 2.5 percent increase amounts to \$114 to \$167 million.⁴

On the benefit side, our estimates imply about 1.3 million additional participant-years

⁴U.S. formula sales were \$1.4 billion in 1987 and WIC bought about one-third ([U.S. General Accounting Office, 1990](#)), or about \$245 per birth; grown at the 9.5 percent annual rate of formula price increases to each year from 1989 to 1993 and converted to 2011 dollars, this is \$6.7 billion for the 11.2 million births in state-years with a sole-source contract.

(5.8 per 1,000 times 219.5 million age-eligible person-years), or about \$1.2 billion of WIC benefits at the pre-adoption grant of \$494 per participant (\$939 in 2011 dollars). Applying the 3.6-gram birth-weight effect to the 11.2 million births in treated state-years, and assuming each additional gram at birth generates \$8.29 in savings from hospital costs (in 2011 dollars) (Almond, Chay and Lee, 2005; East et al., 2023), gives savings of about \$331 million. This excludes later health and earnings gains and any benefit to the children who made up three-quarters of the new enrollment. Hospital savings at birth alone therefore amount to about 28 cents per dollar of benefits provided to the additional participants. The retail price of formula would have to rise by about 5 percent, above our estimates and those in the literature, for the cost to families outside WIC to outweigh the hospital-cost savings alone.

11 Conclusion

This paper evaluates the effects of one of WIC’s largest cost-containment efforts – infant formula rebates. We show empirically that using infant formula rebates significantly increased participation in WIC among women and children and improved health outcomes for at least one of WIC’s target populations – newborns. These results show that the infant formula procurement system is an important source of funding for WIC. These benefits are important when considering the costs of the system, such as higher retail formula prices (Oliveira et al., 2004; U.S. Government Accountability Office, 2025), and for policymakers who are considering restructuring WIC financing or the infant formula procurement system itself.

We find that the magnitudes of newborn health effects are similar, though potentially larger, relative to those in the existing literature (Hoynes, Page and Stevens, 2011; Rossin-Slater, 2013; Bitler et al., 2025). This is somewhat surprising; as a targeted program, relaxing budget constraints should have allowed those with the lowest priority to access the WIC program. The treatment effects being as large here as in other settings may suggest that the targeting schemes cannot identify groups that would benefit most from subsequent enrollment.

We also point to one suggestive new mechanism by which WIC improves in-utero conditions: sibling spillovers. Existing work has found that mothers reduce consumption in response to children losing access to WIC (Bitler et al., 2023). We find some evidence that higher-parity births responded more strongly than did first births, which would be consistent with older children’s enrollment in WIC improving younger siblings’ in-utero conditions. We encourage additional research on the spillovers of WIC within the household.

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Figures

Figure 1: The WIC priority system

Priority I

The following applicants with nutrition-related medical conditions such as anemia, underweight, overweight or pre-term birth:

- Pregnant Women
- Breastfeeding Women
- Infants

Priority II

- Infants (up to 6 months of age) whose mothers participated in WIC or could have participated and had nutrition-related medical conditions.

Priority III

- Children with nutrition-related medical conditions.

Priority IV

The following applicants with dietary problems (like poor diet):

- Infants
- Pregnant Women
- Breastfeeding Women

Priority V

- Children with dietary problems (like poor diet).

Priority VI

- Postpartum (non-breastfeeding) women with nutrition related-medical conditions or dietary problems.

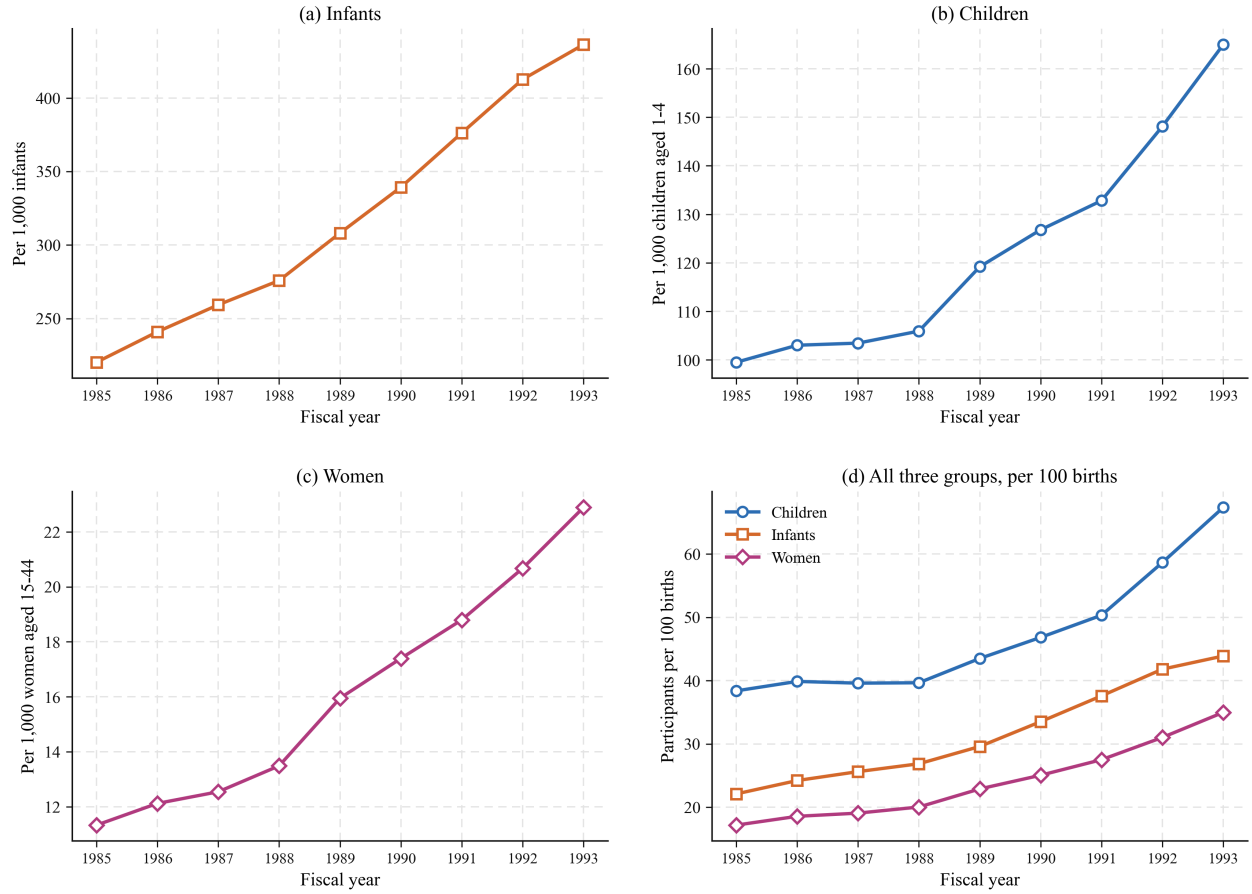
Priority VII

- Current WIC participants who without providing the WIC supplemental foods could continue to have medical and/or dietary problems.

* State agencies can decide to place homeless and migrant participants in Priorities IV-VII. * At state agency option, postpartum women may be placed in Priorities III-V. * Any priority can be subdivided into subcategories of risk, using factors such as income or age.

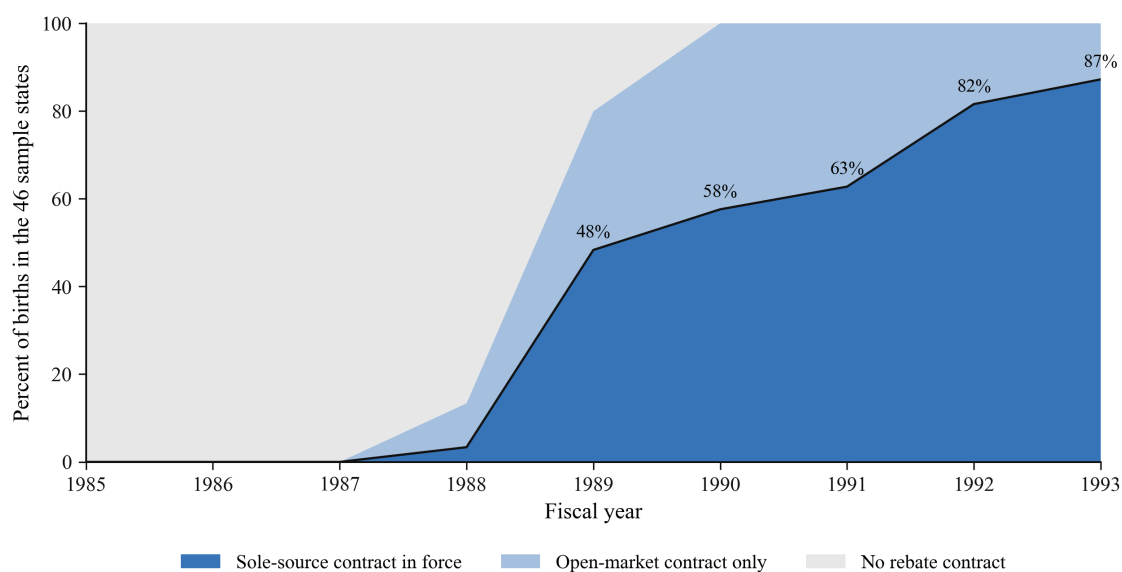
Notes: This figure summarizes the WIC priority system, obtained from the FNS website. Since 1977, federal regulations have required WIC agencies to enroll applicants in order of priority when they cannot serve all eligible applicants. The priority system has not been binding since 1997.

Figure 2: WIC participation, fiscal years 1985–1993



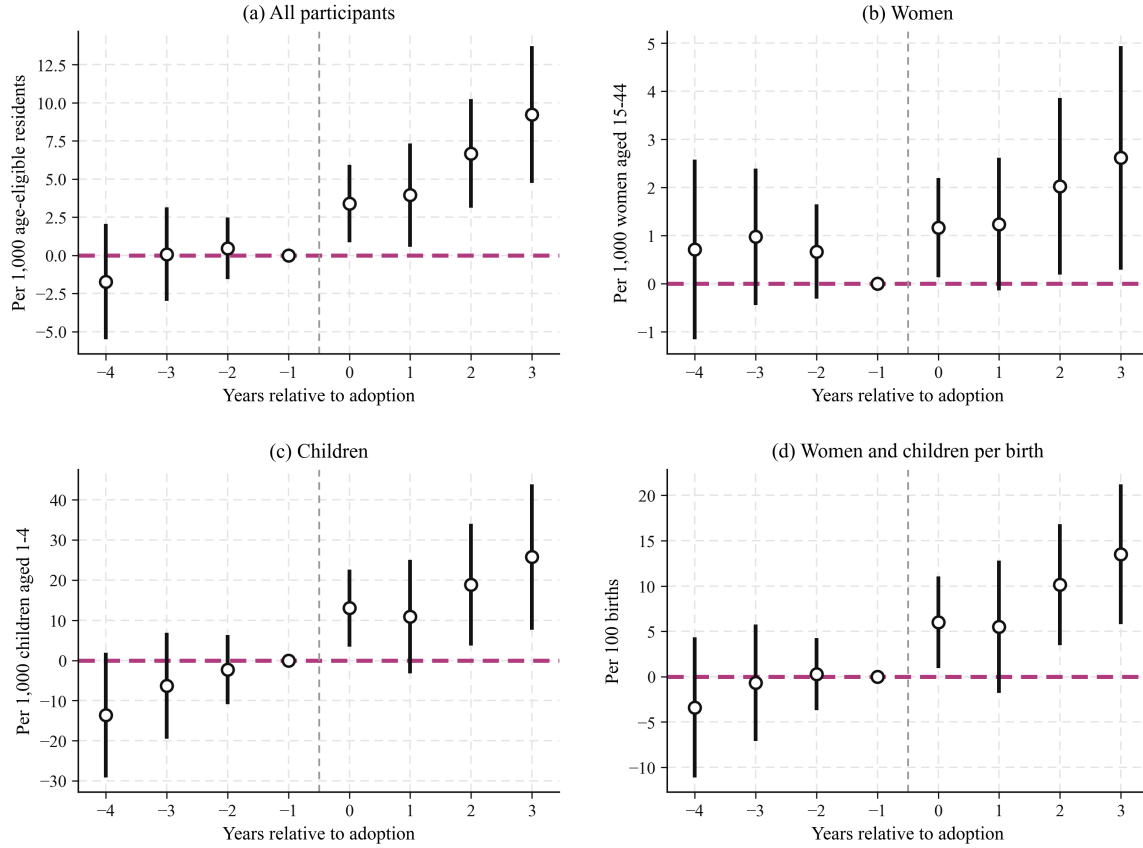
Notes: Here we plot fiscal-year average monthly WIC participants in our 46 sample states, from the USDA state participation tables. In panels (a) through (c), each category is divided by its age-eligible population from SEER (women aged 15–44, infants under one, and children aged 1–4), and in panel (d) all three categories are divided by the number of births in the fiscal year. The women’s category includes pregnant, breastfeeding, and postpartum women.

Figure 3: Share of births covered by infant-formula rebate contracts, fiscal years 1985–1993



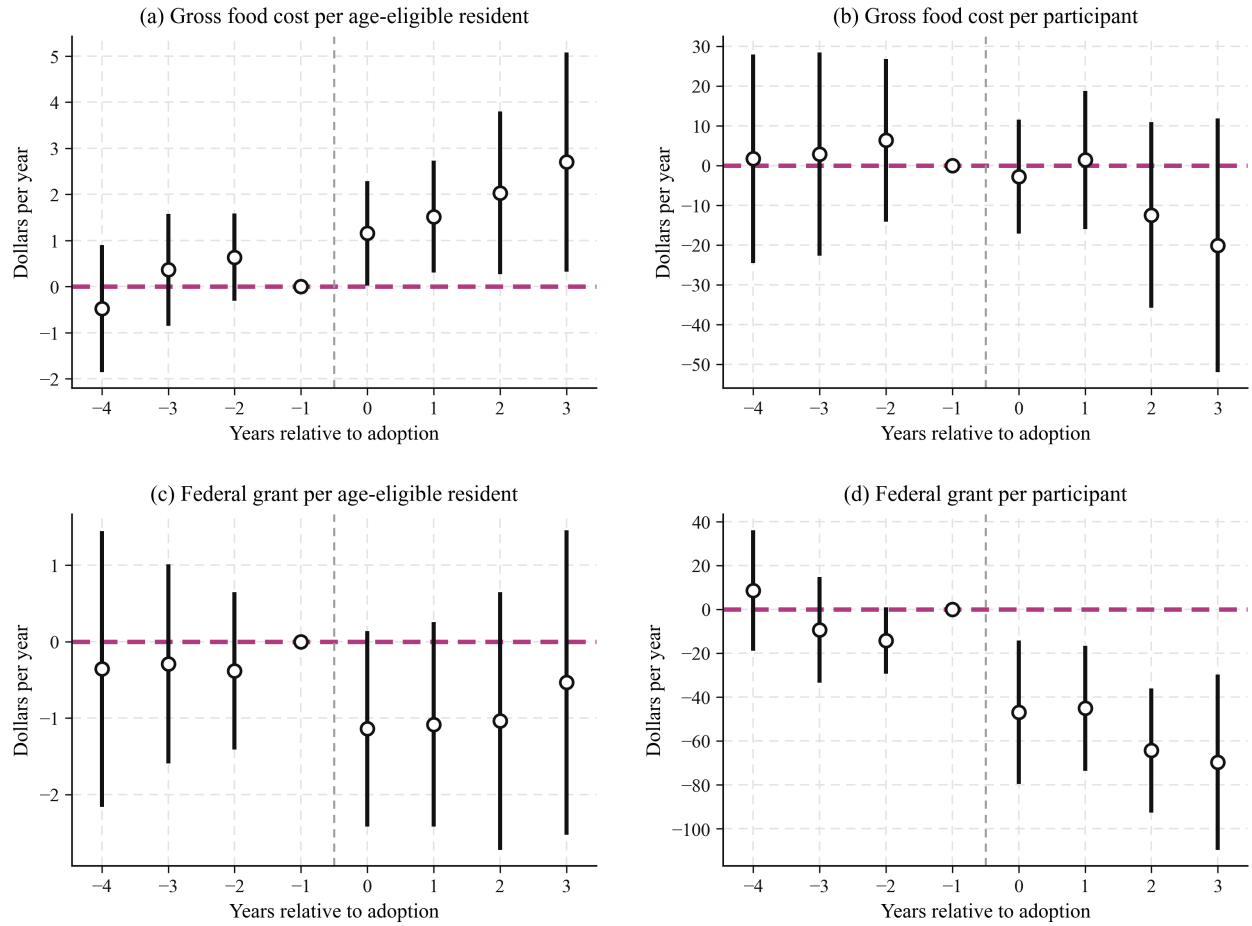
Notes: Here we plot the share of births in our 46 sample states that occurred in states with a sole-source contract in force, with only an open-market or multi-source contract, or with no rebate contract, by fiscal year. We consider a fiscal year covered if the contract was in force by April. Contract dates are from the rebate-history data shared by [Davis \(2016\)](#), which we check against [U.S. Department of Agriculture, Food and Nutrition Service \(1991\)](#).

Figure 4: WIC participation around sole-source adoption



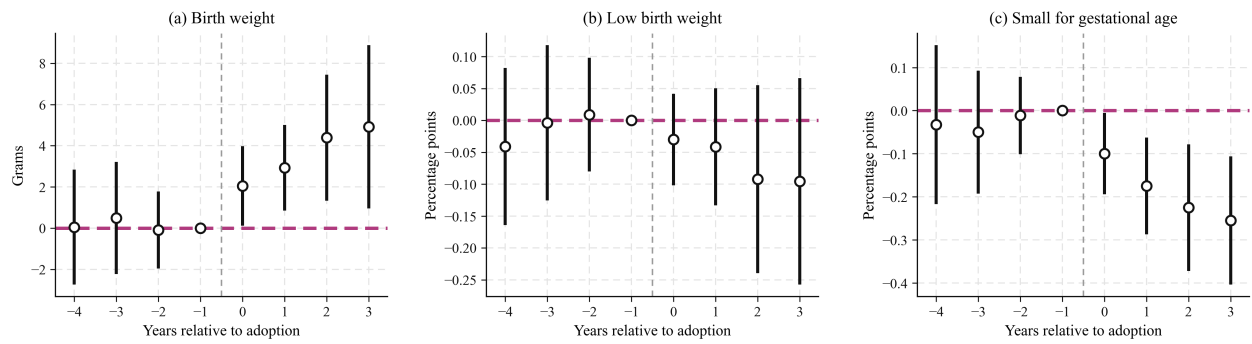
Notes: This figure presents event-study estimates from our stacked difference-in-differences specification on the 46-state sample (FY1983–1993). Each adoption cohort is compared with states that had not yet adopted by the end of its event window. All regressions include 1986 simulated Medicaid prenatal eligibility and its 1986–91 change interacted with sub-experiment-by-year fixed effects, and are weighted by the outcome’s denominator. The year before adoption is the omitted reference year (open circle), and the dashed vertical line marks adoption. Year 3 is only observed for the 17 states that adopted by 1990. Bars are 95 percent confidence intervals based on standard errors clustered at the state level. Participation is measured as the fiscal-year average monthly caseload.

Figure 5: Program finance around sole-source adoption



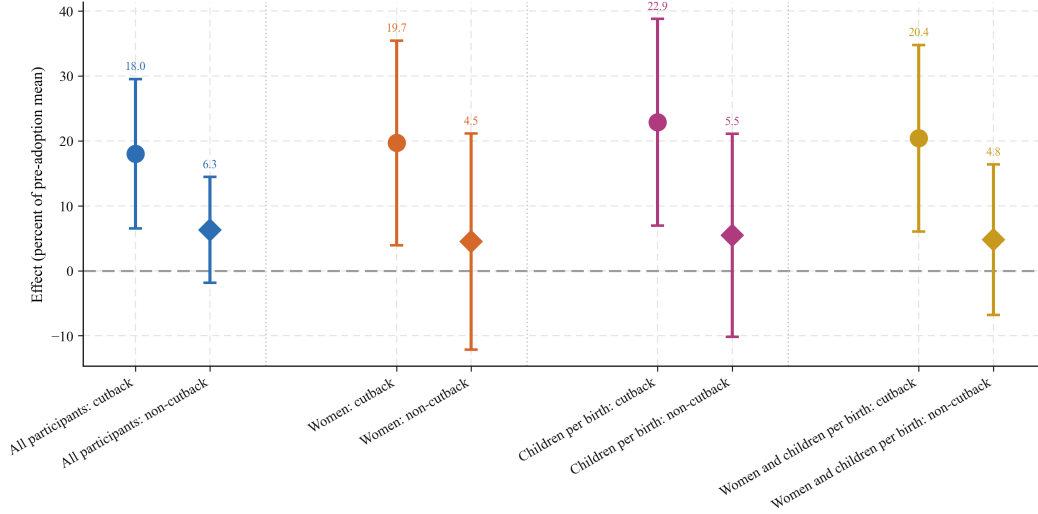
Notes: See Figure 4 for details on the specification. Gross food cost is total food spending, including spending financed by manufacturer rebates.

Figure 6: Birth outcomes around sole-source adoption



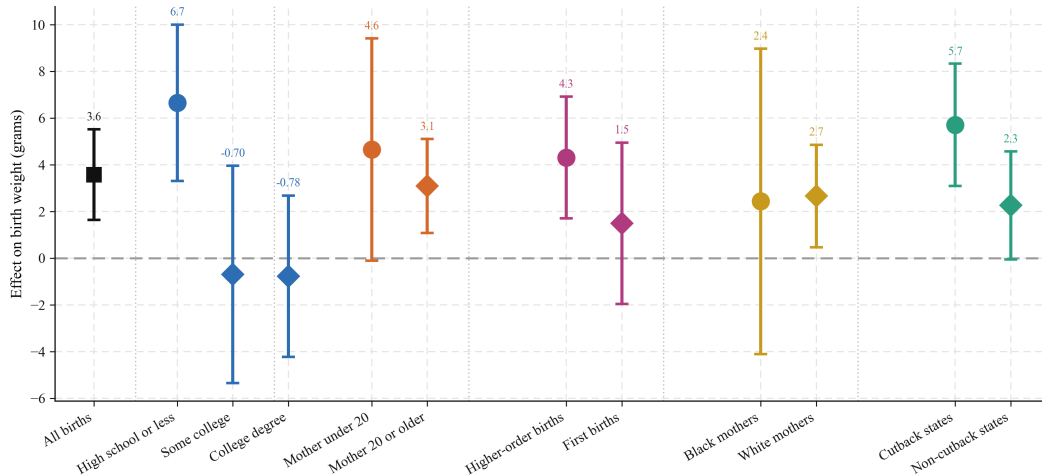
Notes: See Figure 4 for details on the specification. Birth outcomes are measured in state-by-year cells, where births are dated by the start of the second trimester, and regressions are weighted by the number of births.

Figure 7: Effects in states that cut caseloads in 1990 and in other states



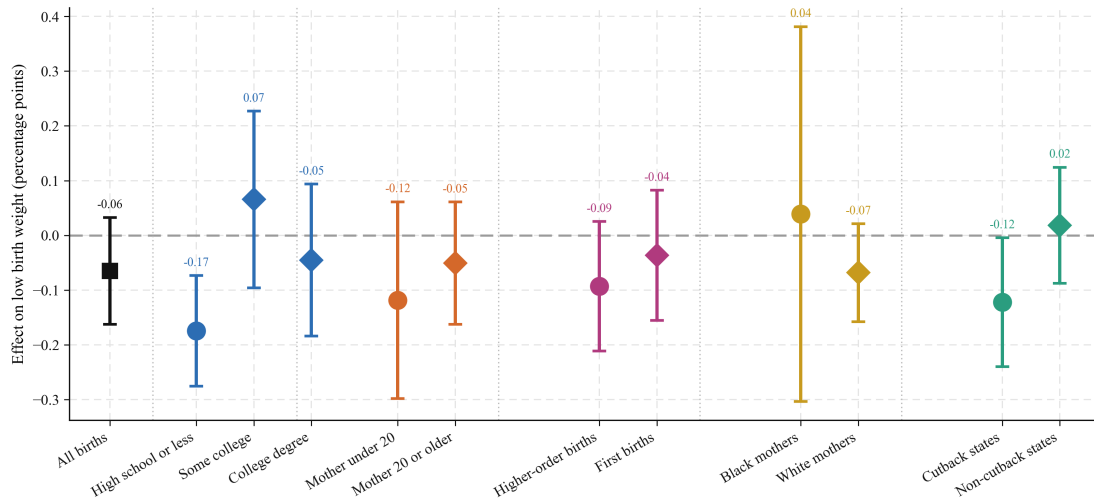
Notes: This figure presents separate stacked difference-in-differences estimates of the average effect over years 0 through 3 for the treated states in each group (keeping all comparison states), expressed as a percent of each group's pre-adoption mean. Circles are the 21 states that cut WIC caseloads by at least 2,000 participants between March and September 1990 ([U.S. House of Representatives, Committee on the Budget, Task Force on Human Resources, 1990](#)), and diamonds are all other states. Bars are 95 percent confidence intervals based on standard errors clustered at the state level. The corresponding estimates are reported in Appendix Table A13.

Figure 8: Heterogeneity in the effect on birth weight



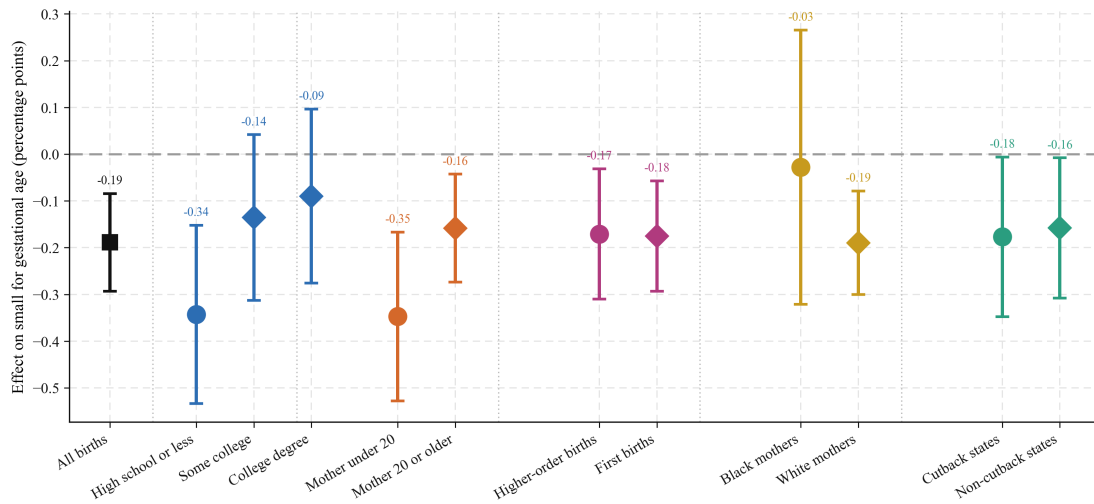
Notes: Each point in this figure is a separate stacked difference-in-differences estimate of the average effect over years 0 through 3 for the indicated group of births, with 95 percent confidence intervals based on standard errors clustered at the state level. The black square is the estimate for all births. Education estimates use the states that record mother's education on at least 85 percent of births. Cutback states are the 21 states that cut caseloads by at least 2,000 participants in 1990, and each group's estimate keeps all comparison states. The corresponding estimates are reported in Appendix Tables A13–A18.

Figure 9: Heterogeneity in the effect on low birth weight



Notes: See Figure 8 for details.

Figure 10: Heterogeneity in the effect on small for gestational age



Notes: See Figure 8 for details.

Tables

Table 1: State Characteristics Before Adoption, by Timing of Sole-Source Adoption

	All	Adopted by FY1990	Adopted FY1991–93	Not by FY1993	<i>p</i> (early vs. later)
Births (thousands)	68.4	103	43.6	61.2	0.06
WIC participants per 1,000 age-eligible residents	43.2	40.6	42.8	50.5	0.46
Women per 1,000 women aged 15–44	12.2	11.6	12.1	14.0	0.61
Infants per 1,000 infants	240	246	227	268	0.41
Children per 1,000 children aged 1–4	114	110	111	134	0.95
Federal grant per participant (\$)	508	490	510	544	0.40
Gross food cost per participant (\$)	398	388	395	431	0.66
Women enrolled per 100 births	18.8	18.6	18.2	21.3	0.80
Children enrolled per 100 births	45.7	42.0	45.7	54.7	0.43
Mean birth weight (grams)	3,374	3,361	3,393	3,343	0.13
Low birth weight (percent)	5.8	6.3	5.2	6.2	0.03
Small for gestational age (percent)	9.2	9.5	8.7	10.0	0.17
Births to Black mothers (percent)	13.2	19.8	7.7	14.8	0.02
Births to mothers under 20 (percent)	12.2	13.0	11.1	13.9	0.07
Simulated Medicaid eligibility, 1986 (share)	0.20	0.19	0.22	0.18	0.13
Change in Medicaid eligibility, 1986–91	0.10	0.13	0.09	0.10	0.03
Cut caseloads in 1990 (share of states)	0.43	0.41	0.41	0.57	0.99
States	46	17	22	7	

Notes: This table reports unweighted means across states of characteristics measured in fiscal year 1986, before any state had adopted a sole-source contract, for the 46 sample states and by the fiscal year of sole-source adoption (Ohio, whose adoption falls after our sample, is in the last group). WIC participation is from the USDA state participation tables and population denominators from SEER; birth outcomes are from the natality files; simulated Medicaid eligibility is the share of a fixed national sample of pregnant women eligible under each state’s rules. The last column is the *p*-value from a two-sided *t*-test of the difference between states adopting by FY1990 and states adopting in FY1991–93.

Table 2: Effects of Sole-Source Contracts on WIC Participation

	Per 1,000 age-eligible residents				Per 100 births		
	Total	Women	Infants	Children	Women	Children	Women + children
Effect of sole-source adoption	5.82*** (1.47)	1.76** (0.67)	11.61 (11.62)	17.14** (6.47)	2.08** (0.96)	6.71*** (2.38)	8.79*** (2.96)
Percent of pre-adoption mean	+14%	+15%	+5%	+17%	+11%	+16%	+14%
Pre-adoption mean	40.7	12.1	240.6	101.0	19.8	41.6	61.4
Joint test of leads, <i>p</i>	0.11	0.26	0.34	0.10	0.31	0.20	0.31

Notes: This table presents stacked difference-in-differences estimates on our 46-state sample (FY1983–1993). Each estimate is the average effect over years 0 through 3 relative to the year before adoption. All regressions include 1986 simulated Medicaid prenatal eligibility and its 1986–91 change interacted with sub-experiment-by-year fixed effects, and are weighted by the outcome’s denominator. Participation is the fiscal-year average monthly caseload divided by the relevant population (women aged 15–44, infants, children aged 1–4, or, for the total, the sum of the three). The pre-adoption mean is the weighted mean of the outcome before adoption. Standard errors are clustered at the state level and reported in parentheses. **p* < 0.1, ***p* < 0.05, ****p* < 0.01.

Table 3: Effects of Sole-Source Contracts on Program Finance

	\$ per age-eligible resident		\$ per participant per year	
	Federal grant	Gross food cost	Federal grant	Gross food cost
Effect of sole-source adoption	-0.95* (0.56)	1.85** (0.72)	-56.48*** (13.48)	-8.48 (8.51)
Percent of pre-adoption mean	-4%	+10%	-11%	-2%
Pre-adoption mean	23.1	18.9	494.3	415.7
Joint test of leads, p	0.89	0.26	0.09	0.75

Notes: See Table 2 for details on the specification. Gross food cost is total food spending, including spending financed by manufacturer rebates. Per-participant amounts divide by the average monthly caseload.

Table 4: Effects of Sole-Source Contracts on Birth Outcomes

	Birth weight	Low birth weight	Small for gestational age
	grams	percentage points	percentage points
Effect of sole-source adoption	3.57*** (0.99)	-0.065 (0.050)	-0.189*** (0.053)
Pre-adoption mean	3,367.6	5.88	9.31
Joint test of leads, p	0.89	0.43	0.87

Notes: See Table 2 for details on the specification. Birth outcomes are measured in state-by-year cells, where births are dated by the start of the second trimester, and regressions are weighted by the number of births.

Table 5: Heterogeneity in Participation Effects by State Finances

	Total per 1,000	Women per 1,000	Children per 100 births	Women + children per 100 births
All states	5.82*** (1.47)	1.76** (0.67)	6.71*** (2.38)	8.79*** (2.96)
Percent of pre-adoption mean	+14%	+15%	+16%	+14%
1990 cutback states	7.61*** (2.47)	2.53** (1.04)	9.67** (3.43)	12.83** (4.61)
Percent of pre-adoption mean	+18%	+20%	+23%	+20%
Other states	2.73 (1.80)	0.56 (1.06)	2.50 (3.65)	3.20 (3.95)
Percent of pre-adoption mean	+6%	+4%	+5%	+5%

Notes: See Table 2 for details on the specification. Each row is a separate stacked regression for the treated states in the indicated group, keeping all comparison states. Cutback states are the 21 states that cut WIC caseloads by at least 2,000 participants between March and September 1990 ([U.S. House of Representatives, Committee on the Budget, Task Force on Human Resources, 1990](#)). Standard errors are clustered at the state level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Heterogeneity in Birth Outcome Effects

	Birth weight	LBW	SGA
	grams	pp	pp
All births	3.57*** (0.99)	-0.065 (0.050)	-0.189*** (0.053)
<i>By state finances</i>			
1990 cutback states	5.70*** (1.34)	-0.122* (0.060)	-0.177* (0.087)
Other states	2.26* (1.18)	0.018 (0.054)	-0.158** (0.076)
<i>By mother's education</i>			
High school or less	6.65*** (1.71)	-0.174*** (0.052)	-0.343*** (0.097)
Some college	-0.70 (2.37)	0.066 (0.082)	-0.135 (0.090)
College degree	-0.78 (1.76)	-0.045 (0.071)	-0.090 (0.095)
<i>By mother's age</i>			
Under 20	4.65* (2.43)	-0.118 (0.092)	-0.347*** (0.092)
20 or older	3.09*** (1.03)	-0.051 (0.057)	-0.158** (0.059)
<i>By mother's race</i>			
Black	2.43 (3.34)	0.039 (0.175)	-0.028 (0.150)
White	2.66** (1.12)	-0.068 (0.046)	-0.190*** (0.057)
<i>By birth order</i>			
First births	1.49 (1.76)	-0.036 (0.061)	-0.175*** (0.060)
Higher-order births	4.31*** (1.33)	-0.093 (0.060)	-0.171** (0.071)

Notes: See Table 4 for details on the specification. Each row is a separate stacked regression for the indicated group of births (or, for the state panel, for the treated states in the group, keeping all comparison states). Education estimates use the states that record mother's education on at least 85 percent of births. Pre-adoption means and tests of the pre-adoption coefficients for each group are reported in Appendix Tables A13–A18. Standard errors are clustered at the state level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A Appendix Tables and Figures

Appendix tables and figures appear in the order in which they are referenced in the text.

Table A1: Sole-Source Contract Timing by State

State	First contract	First contract FY	Sole-source FY	1990 cutback	Education sample
Alabama	Sole source	1989	1989		Yes
Alaska	Open market	1990	1993	Yes	Yes
Arizona	Open market	1990	1996 ^d	Yes	Yes
Arkansas	Open market	1989	1992		Yes
California	Sole source	1989	1989	Yes	
Colorado	Open market	1989	1994 ^d	Yes	Yes
Connecticut	Open market	1990	1991	Yes	
Delaware	Sole source	1990	1990		Yes
District of Columbia	Sole source	1990	1990		Yes
Florida	Open market	1988	1989	Yes	Yes
Georgia	Sole source	1989	1989	Yes	Yes
Hawaii	Open market	1990	1992		Yes
Idaho	Open market	1990	1993		Yes
Illinois	Open market	1989	1992		Yes
Indiana	Sole source	1989	1989		Yes
Iowa	Open market	1989	1993	Yes	Yes
Kansas	Open market	1989	1992		Yes
Kentucky	Open market	1989	1991	Yes	Yes
Louisiana	Open market	1989	1994 ^d		Yes
Maine	Open market	1990	1992		Yes
Maryland	Sole source	1990	1990		Yes
Massachusetts ^a	Sole source	1990	1990		Yes
Michigan	Open market	1988	1990		Yes
Minnesota	Sole source	1989	1989	Yes	Yes
Mississippi ^c	Direct distribution	—	—	—	—
Missouri	Open market	1989	1992	Yes	Yes
Montana	Open market	1990	1991		Yes
Nebraska	Open market	1990	1992		Yes
Nevada ^b	Sole source	1989	1989		Yes
New Hampshire	Open market	1990	1992	Yes	Yes
New Jersey	Sole source	1989	1989		Yes
New Mexico	Open market	1989	1992		Yes
New York ^a	Sole source	1989	1989	Yes	
North Carolina	Open market	1989	1992	Yes	Yes
North Dakota	Open market	1989	1992		Yes
Ohio	Open market	1990	1994 ^d	Yes	Yes
Oklahoma	Sole source	1990	1990	Yes	Yes
Oregon	Sole source	1988	1988	Yes	Yes
Pennsylvania ^a	Sole source	1989	1989		Yes
Rhode Island	Sole source	1990	1990		Yes
South Carolina	Open market	1989	1994 ^d		Yes
South Dakota	Open market	1989	1994 ^d		Yes
Tennessee	Sole source	1988	1988		Yes
Texas	Sole source	1989	1989	Yes	
Utah	Open market	1990	1993	Yes	Yes
Vermont ^c	Direct distribution	—	—	—	—

State	First contract	First contract FY	Sole-source FY	1990 cutback	Education sample
Virginia	Open market	1989	1992		Yes
Washington	Open market	1990	1993		
West Virginia	Open market	1990	1994 ^d	Yes	Yes
Wisconsin	Open market	1989	1991	Yes	Yes
Wyoming	Open market	1988	1993		Yes

Notes: First contract refers to the type of a state’s first infant-formula rebate contract. Fiscal years run from October to September, and a fiscal year is treated if the contract was in force by April. Cutback states are those that cut WIC caseloads by at least 2,000 participants between March and September 1990 (U.S. House of Representatives, Committee on the Budget, Task Force on Human Resources, 1990). The education sample consists of the states that record mother’s education on at least 85 percent of births in every year. ^aExcluded from the main sample because of heavy state funding (Table A3). ^bNevada returned to open-market contracting in 1990, and we drop its later years. ^cMississippi and Vermont distributed formula directly and are excluded. ^dThe state adopted sole source after FY1993 and serves as a comparison state throughout our sample.

Table A2: State Accounts of What Rebate Savings Bought, 1987–1990

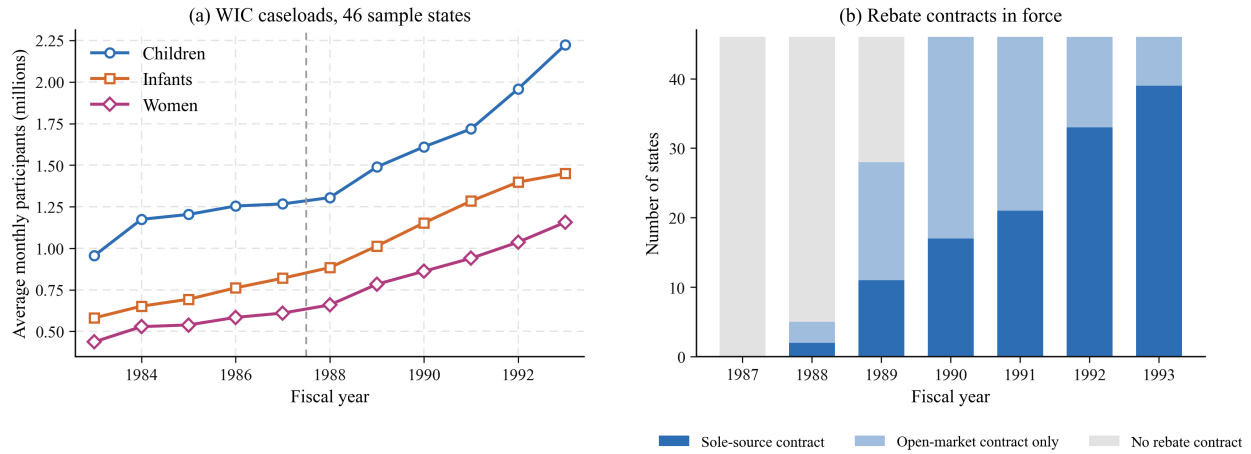
State	Contract	Reported effect
Tennessee	Sole source, Wyeth; bid accepted Aug. 1986, in force June 1987	Participation rose by 12,000 (16 percent) from 1987 to 1988 (U.S. Senate, Committee on Agriculture, Nutrition, and Forestry, Subcommittee on Nutrition and Investigations, 1990)
Texas	Sole source, May 1988	Caseload rose by 80,000 (Bilheimer, 1990)
Alabama	Sole source, Ross, \$1.17, June 1988	Income limit raised from 150 to 185 percent of poverty (U.S. House of Representatives, Select Committee on Hunger, 1988)
Minnesota	Sole source, Ross, \$1.15, Oct. 1988	Caseload rose from about 56,000 to 70,000; \$700,000 state grant to enroll participants before rebates arrived (Bilheimer, 1990; U.S. House of Representatives, Select Committee on Hunger, 1988)
Missouri	Open market, July 1988	\$8.3 million expected in FY1989, enough for 14,243 more participants a month; caseload from 63,000 to 78,000 (U.S. Senate, Committee on Agriculture, Nutrition, and Forestry, Subcommittee on Nutrition and Investigations, 1990)
California	Sole source, Ross, Jan. 1989	110,000 participants added by mid-1990 (U.S. House of Representatives, Committee on the Budget, Task Force on Human Resources, 1990)
New York	Rebate contract, 1989, \$1.50	\$42 million in rebates; 50,000 more participants a month (U.S. House of Representatives, Committee on the Budget, Task Force on Human Resources, 1990)
South Carolina	Rebate contract, Apr. 1989	Expected to serve all priority groups (Bilheimer, 1990)
Washington	Rebates expected, 1989	Caseload expected to rise from about 50,000 to 66,000 a month (U.S. House of Representatives, Committee on Education and Labor, Subcommittee on Elementary, Secondary, and Vocational Education, 1989)

Notes: This table collects accounts of what rebate savings bought, as given by state officials, advocates, and members of Congress in hearings and reports at the time. We note that several of these are projections rather than realized changes. Rebates are per 13-ounce can of milk-based concentrate at the time the contract was signed.

Table A3: State Self-Funding of WIC, FY1989

State	Sole-source FY	State WIC funds (\$M)	Federal food spending (\$M)	State funds as % of food
MA	1990	8.1	27.9	29
NY	1989	35.8	146.2	24
PA	1989	13.0	74.3	17
DC	1990	0.6	5.5	11
IL	1992	6.0	67.7	9
WI	1991	2.2	28.7	8
AK	1993	0.2	4.1	6
TX	1989	5.0	136.5	4
MN, NH, WA, MI, IN		0.1–0.7		<3

Notes: State supplemental WIC appropriations are from Table 5 of FRAC (1989), as compiled by the Center on Budget and Policy Priorities. We omit contingency funds (NJ, IA, RI, AZ, AK; \$0.04–0.65 million). Federal food spending is from the USDA cost data.

Figure A1: WIC caseloads and rebate contracts, 46 sample states

Notes: Panel (a) plots fiscal-year average monthly WIC participants by category in our 46 sample states, from the USDA state participation tables; the shaded region marks the adoption period. Panel (b) plots the number of sample states with a sole-source contract, with only an open-market or multi-source contract, or with no rebate contract in force in each fiscal year, where a fiscal year is treated if the contract was in force by April.

Table A4: Controlling for Time-Varying Medicaid Eligibility

	Total, per 1,000	Women, per 1,000	Women + children, per 100 births	Birth weight (g)	Low birth weight (pp)	SGA (pp)	Birth weight, HS or less (g)
Main	5.82*** (1.47)	1.76** (0.67)	8.79*** (2.96)	3.57*** (0.99)	-0.065 (0.050)	-0.189*** (0.053)	6.65*** (1.71)
Adding time-varying simulated eligibility	5.83*** (1.41)	1.77*** (0.63)	8.80*** (2.85)	3.56*** (0.97)	-0.064 (0.049)	-0.187*** (0.053)	6.77*** (1.75)

Notes: This table presents stacked difference-in-differences estimates on our 46-state sample; see Table 2 for details on the specification. Each estimate is the average effect over years 0 through 3 relative to the year before adoption. Standard errors are clustered at the state level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The second row adds each state's time-varying simulated Medicaid prenatal eligibility as a control.

Table A5: Robustness to the Estimator: Participation

	Total	Women	Women + children /100 births
Stacked (main)	5.82*** (1.47)	1.76** (0.67)	8.79*** (2.96)
Stacked, no Medicaid controls	4.57*** (1.28)	1.33** (0.52)	6.38** (2.94)
Stacked, not-yet-treated comparison states	5.37*** (1.23)	1.64*** (0.60)	7.97*** (2.07)
Stacked, not-yet-treated comparison states, no Medicaid controls	4.49*** (1.24)	1.68*** (0.52)	5.83* (2.95)
Callaway–Sant’Anna, Medicaid controls	4.06** (1.69)	1.79** (0.82)	4.06 (2.85)
Callaway–Sant’Anna, no Medicaid controls	5.78*** (1.54)	2.18*** (0.73)	6.99** (3.28)

Notes: This table compares estimators on our 46-state sample (FY1983–1993). All stacked rows use the event window from year -4 to year 3 and average the effects over years 0–3. The first row is our main specification (Section 5), which compares each adoption cohort with states that do not adopt during its event window and controls for 1986 simulated Medicaid eligibility and its 1986–91 change. The second row drops the Medicaid controls. The third and fourth rows also use states that adopt during the event window as comparisons until the year before they adopt, with and without the Medicaid controls. The Callaway–Sant’Anna estimates use not-yet-treated states as controls and average over all post-adoption periods. Standard errors are clustered at the state level and reported in parentheses. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

Table A6: Robustness to the Estimator: Birth Outcomes

	Birth weight	Low birth weight	SGA	Birth weight, HS or less
Stacked (main)	3.57*** (0.99)	-0.065 (0.050)	-0.189*** (0.053)	6.65*** (1.71)
Stacked, no Medicaid controls	4.99*** (1.45)	-0.122** (0.057)	-0.146** (0.062)	6.52*** (2.05)
Stacked, not-yet-treated comparison states	4.40*** (1.13)	-0.097* (0.051)	-0.145** (0.056)	6.90*** (2.17)
Stacked, not-yet-treated comparison states, no Medicaid controls	5.43*** (1.51)	-0.142** (0.060)	-0.143** (0.059)	6.54*** (2.10)
Callaway–Sant’Anna, Medicaid controls	4.40** (1.99)	-0.104 (0.094)	-0.167 (0.136)	5.08* (2.76)
Callaway–Sant’Anna, no Medicaid controls	5.79*** (1.72)	-0.161** (0.072)	-0.154** (0.069)	6.34*** (2.10)

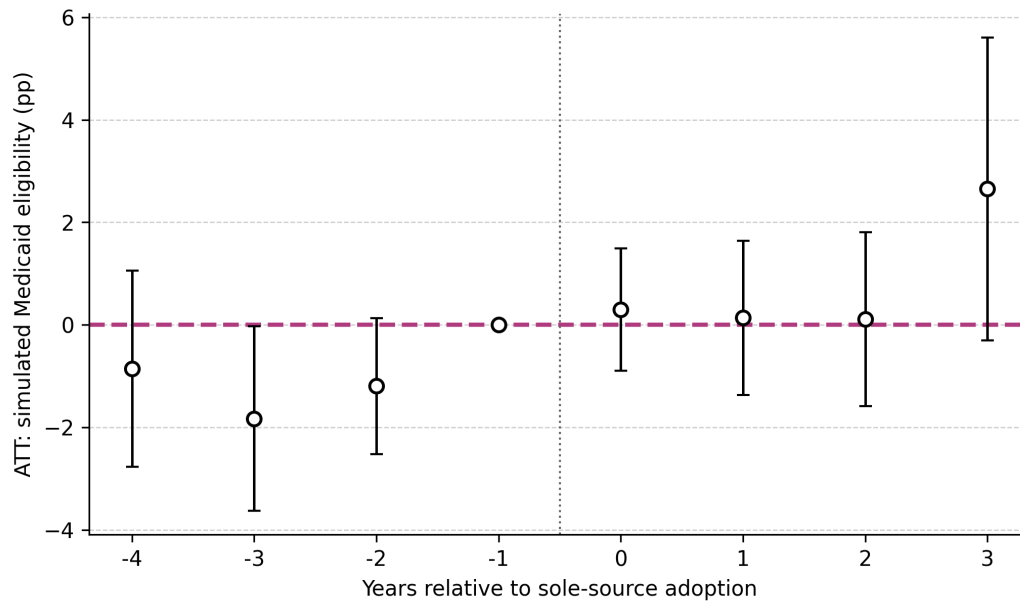
Notes: See Appendix Table A5 for details.

Table A7: Pre-Period State Characteristics and the Timing of Sole-Source Adoption

	Fiscal year of adoption		Adopted by FY1990	
	One at a time	Jointly	One at a time	Jointly
WIC participants per 1,000 (FY1986)	0.45 (0.36)	1.08*** (0.39)	-0.08 (0.07)	-0.21** (0.09)
Federal grant per participant (FY1986)	0.48* (0.25)	3.06* (1.83)	-0.07 (0.05)	-0.37 (0.46)
Food cost per participant (FY1986)	0.42 (0.27)	-2.43 (2.03)	-0.06 (0.05)	0.31 (0.48)
Change in participants per 1,000, FY1984–86	0.43** (0.22)	0.30 (0.54)	-0.12** (0.06)	-0.09 (0.14)
Change in grant per participant, FY1985–86	0.35 (0.31)	0.48 (0.46)	-0.08 (0.07)	-0.07 (0.13)
Change in food cost per participant, FY1984–86	0.21 (0.29)	0.32 (0.57)	-0.05 (0.06)	-0.10 (0.16)
Simulated Medicaid eligibility, 1986	0.13 (0.30)	0.50 (0.61)	-0.07 (0.07)	-0.08 (0.16)
Change in Medicaid eligibility, 1986–91	-0.58** (0.24)	-0.10 (0.51)	0.15** (0.06)	0.04 (0.15)
Share of births to teen mothers, 1986	-0.09 (0.32)	-0.51 (0.69)	0.09 (0.07)	0.11 (0.19)
Share of births to Black mothers, 1986	-0.42 (0.30)	-1.76 (1.49)	0.18*** (0.06)	0.48 (0.30)
Mean birth weight, 1986	-0.07 (0.32)	-0.01 (1.55)	-0.07 (0.07)	-0.03 (0.30)
Low birth weight rate, 1986	-0.21 (0.27)	2.43 (2.39)	0.14*** (0.05)	-0.49 (0.48)
Change in mean birth weight, 1984–86	-0.30 (0.30)	-0.68 (0.44)	0.01 (0.06)	0.10 (0.10)
Change in low birth weight rate, 1984–86	0.03 (0.37)	-0.68 (0.45)	0.04 (0.07)	0.23* (0.12)
Cut caseloads in 1990	0.03 (0.65)	0.83 (0.81)	-0.01 (0.15)	-0.12 (0.19)
Midwest	0.41 (0.67)	2.89** (1.45)	-0.14 (0.15)	-0.48 (0.44)
South	-0.83 (0.68)	1.59 (1.61)	0.32** (0.15)	-0.23 (0.46)
West	0.98 (0.69)	1.82 (1.37)	-0.26* (0.14)	-0.28 (0.35)
Joint test of all regressors, p		0.01		0.01
R^2		0.50		0.47
States	45	45	45	45

Notes: This table regresses the fiscal year of a state's sole-source adoption (states that had not adopted by FY1993 are coded 1995) and an indicator for adoption by FY1990 on state characteristics measured in fiscal year 1986, before any state had adopted, and on their changes over fiscal years 1984–86 (1985–86 for the grant). The one-at-a-time columns report bivariate regressions and the jointly columns a single regression with all regressors. Continuous regressors are standardized, so coefficients are per one standard deviation; region indicators are relative to the Northeast. Heteroskedasticity-robust standard errors are in parentheses. One state lacks 1986 natality cells and is omitted. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure A2: Simulated Medicaid eligibility around sole-source adoption



Notes: This figure plots event-study coefficients from our main stacked specification, without controls, with each state's simulated Medicaid prenatal eligibility (in percentage points) as the outcome. Estimates are relative to the year before adoption, weighted by births, with 95 percent confidence intervals based on standard errors clustered at the state level. The average effect over years 0 through 3 is 0.8 percentage points (standard error 0.9), on a pre-adoption mean of 22.6 percent.

Table A8: Event-Study Coefficients: Participation

Event year	Total	Women	Children	Women + children /100 births	Adopting states
-4	-1.71 (1.93)	0.71 (0.95)	-13.61* (7.93)	-3.39 (3.94)	39
-3	0.08 (1.56)	0.98 (0.72)	-6.31 (6.72)	-0.69 (3.28)	39
-2	0.47 (1.03)	0.67 (0.50)	-2.27 (4.38)	0.29 (2.04)	39
-1	0 (ref.)	0 (ref.)	0 (ref.)	0 (ref.)	
0	3.39*** (1.29)	1.16** (0.53)	13.01*** (4.89)	5.99** (2.58)	39
1	3.95** (1.72)	1.24* (0.70)	10.91 (7.21)	5.52 (3.72)	33
2	6.68*** (1.82)	2.02** (0.94)	18.88** (7.73)	10.14*** (3.41)	21
3+	9.24*** (2.29)	2.62** (1.19)	25.74*** (9.26)	13.52*** (3.93)	17
Joint test of leads, p	0.11	0.26	0.10	0.31	

Notes: This table reports the coefficients on the event-time indicators from the stacked regressions plotted in Figure 4. The year before adoption is the omitted year, and year 3 includes all later years. The adopting states row counts the treated states observed in each event year. Standard errors are clustered at the state level and reported in parentheses, and stars are based on normal approximations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Women's Participation Under Alternative Denominators

	Women per 1,000 women 15-44	Women per 100 births	Women per 100 pregnant or postpartum
Effect of sole-source adoption	1.76** (0.67)	2.08** (0.96)	1.27** (0.57)
Percent of pre-adoption mean	+15%	+11%	+11%
Joint test of leads, p	0.26	0.31	0.27

Notes: This table presents stacked difference-in-differences estimates on our 46-state sample; see Table 2 for details on the specification. Each estimate is the average effect over years 0 through 3 relative to the year before adoption. Standard errors are clustered at the state level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The natality-based pool counts, for each month of the fiscal year, the births in the following nine months plus the births in the preceding twelve months, averaged over the year.

Table A10: Event-Study Coefficients: Birth Outcomes

Event year	Birth weight	Low birth weight	Small for gestational age	Adopting states
-4	0.05 (1.42)	-0.041 (0.063)	-0.033 (0.094)	39
-3	0.49 (1.39)	-0.004 (0.062)	-0.050 (0.073)	39
-2	-0.09 (0.95)	0.009 (0.045)	-0.011 (0.046)	39
-1	0 (ref.)	0 (ref.)	0 (ref.)	
0	2.05** (0.98)	-0.030 (0.037)	-0.100** (0.048)	39
1	2.93*** (1.06)	-0.042 (0.047)	-0.175*** (0.057)	33
2	4.39*** (1.56)	-0.092 (0.075)	-0.225*** (0.075)	21
3+	4.92** (2.02)	-0.096 (0.083)	-0.255*** (0.076)	17
Joint test of leads, p	0.89	0.43	0.87	

Notes: See Appendix Table A8 for details. Coefficients are from the regressions plotted in Figure 6.

Table A11: Birth-Weight Distribution and Gestation

	Birth weight all births, g	Birth weight term births, g	Gestation weeks	Term 37+ weeks, %	VLBW <1,500 g, %	LBW <2,500 g, %	2,500–2,999 g %	4,000 g+ %
Effect of sole-source adoption	3.66*** (1.00)	3.23*** (1.08)	0.003 (0.009)	0.14 (0.08)	0.001 (0.011)	-0.067 (0.049)	-0.207*** (0.044)	0.108** (0.049)
Pre-adoption mean	3,370.77	3,442.86	39.35	90.77	0.90	5.78	15.70	11.25
Joint test of leads, p	0.85	0.39	0.32	0.31	0.11	0.46	0.47	1.00

Notes: This table presents stacked difference-in-differences estimates on our 46-state sample; see Table 2 for details on the specification. Each estimate is the average effect over years 0 through 3 relative to the year before adoption. Standard errors are clustered at the state level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Term births are those with at least 37 completed weeks of gestation.

Table A12: Composition of Births

	Mother under 20 %	Mother 35+ %	Mother's age years	Black mother %	Married mother %
Effect of sole-source adoption	-0.194 (0.152)	0.033 (0.128)	0.022 (0.042)	-0.244 (0.191)	1.059 (0.835)
Pre-adoption mean	13.25	7.13	25.91	15.41	75.77
Joint test of leads, p	0.09	0.87	0.03	0.41	0.17

Notes: See Appendix Table A11 for details.

Table A13: Participation in the States That Cut Caseloads in 1990

	Non-cutback states				1990 cutback states			
	Total per 1,000	Women per 1,000	Children per 100 births	W + C per 100 births	Total per 1,000	Women per 1,000	Children per 100 births	W + C per 100 births
Effect of sole-source adoption	2.73 (1.80)	0.56 (1.06)	2.50 (3.65)	3.20 (3.95)	7.61*** (2.47)	2.53** (1.04)	9.67** (3.43)	12.83** (4.61)
Percent of pre-adoption mean	+6%	+4%	+5%	+5%	+18%	+20%	+23%	+20%
Pre-adoption mean	43.36	12.48	45.77	66.76	42.21	12.88	42.25	62.87
Joint test of leads, p	0.28	0.09	0.28	0.60	0.43	0.63	0.21	0.36

Notes: This table presents stacked difference-in-differences estimates on our 46-state sample; see Table 2 for details on the specification. Each estimate is the average effect over years 0 through 3 relative to the year before adoption. Standard errors are clustered at the state level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. We estimate the effects separately for the treated states in each group, keeping all comparison states. Cutback states are the 21 states that cut WIC caseloads by at least 2,000 participants between March and September 1990 ([U.S. House of Representatives, Committee on the Budget, Task Force on Human Resources, 1990](#)).

Table A14: Birth Outcomes in the States That Cut Caseloads in 1990

	Non-cutback states			1990 cutback states		
	Birth weight	LBW	SGA	Birth weight	LBW	SGA
	grams	pp	pp	grams	pp	pp
All births	2.26* (1.18)	0.018 (0.054)	-0.158** (0.076)	5.70*** (1.34)	-0.122* (0.060)	-0.177* (0.087)
Mother high school or less	1.53 (1.17)	-0.047 (0.055)	-0.198* (0.110)	10.59*** (3.69)	-0.238** (0.104)	-0.448*** (0.153)
Pre-adoption mean	3,358.8	6.10	9.57	3,370.2	5.76	9.24
Joint test of leads, p	0.53	0.60	0.33	0.96	0.42	0.85

Notes: See Appendix Table A13 for details.

Table A15: Birth Outcomes by Mother's Education

	College degree			Some college			High school or less		
	Birth weight	LBW	SGA	Birth weight	LBW	SGA	Birth weight	LBW	SGA
	grams	pp	pp	grams	pp	pp	grams	pp	pp
Effect of sole-source adoption	-0.78 (1.76)	-0.045 (0.071)	-0.090 (0.095)	-0.70 (2.37)	0.066 (0.082)	-0.135 (0.090)	6.65*** (1.71)	-0.174*** (0.052)	-0.343*** (0.097)
Pre-adoption mean	3,472.4	3.64	5.77	3,420.1	4.79	7.62	3,313.0	7.08	11.16
Joint test of leads, p	0.83	0.30	0.22	0.06	0.08	0.93	0.44	1.00	0.68

Notes: This table presents stacked difference-in-differences estimates on our 46-state sample; see Table 2 for details on the specification. Each estimate is the average effect over years 0 through 3 relative to the year before adoption. Standard errors are clustered at the state level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The education sample consists of the states that record mother's education on at least 85 percent of births in every year. High school or less is at most 12 years of schooling, some college is 13 to 15 years, and college degree is 16 or more years.

Table A16: Birth Outcomes by Mother's Age

	Mother 20 or older			Mother under 20		
	Birth weight	LBW	SGA	Birth weight	LBW	SGA
	grams	pp	pp	grams	pp	pp
Effect of sole-source adoption	3.09*** (1.03)	-0.051 (0.057)	-0.158** (0.059)	4.65* (2.43)	-0.118 (0.092)	-0.347*** (0.092)
Pre-adoption mean	3,389.1	5.46	8.76	3,226.6	8.60	12.94
Joint test of leads, p	0.87	0.32	0.92	0.58	0.93	0.86

Notes: This table presents stacked difference-in-differences estimates on our 46-state sample; see Table 2 for details on the specification. Each estimate is the average effect over years 0 through 3 relative to the year before adoption. Standard errors are clustered at the state level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. We estimate the effects separately for births to mothers under 20 and to mothers aged 20 or older.

Table A17: Birth Outcomes by Mother's Race

	White mothers			Black mothers		
	Birth weight	LBW	SGA	Birth weight	LBW	SGA
	grams	pp	pp	grams	pp	pp
Effect of sole-source adoption	2.66** (1.12)	-0.068 (0.046)	-0.190*** (0.057)	2.43 (3.34)	0.039 (0.175)	-0.028 (0.150)
Pre-adoption mean	3,417.6	4.77	8.05	3,126.2	11.70	15.51
Joint test of leads, p	0.47	0.99	0.86	0.27	0.17	0.59

Notes: This table presents stacked difference-in-differences estimates on our 46-state sample; see Table 2 for details on the specification. Each estimate is the average effect over years 0 through 3 relative to the year before adoption. Standard errors are clustered at the state level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. We estimate the effects separately for births to Black and to white mothers.

Table A18: Birth Outcomes by Birth Order

	First births			Higher-order births		
	Birth weight	LBW	SGA	Birth weight	LBW	SGA
	grams	pp	pp	grams	pp	pp
All mothers	1.49 (1.76)	-0.036 (0.061)	-0.175*** (0.060)	4.31*** (1.33)	-0.093 (0.060)	-0.171** (0.071)
Mother high school or less	4.25 (2.94)	-0.077 (0.096)	-0.377*** (0.096)	8.57*** (2.28)	-0.256*** (0.067)	-0.331*** (0.118)
Pre-adoption mean	3,320.2	6.37	10.70	3,393.0	5.60	8.57
Joint test of leads, p	0.92	0.45	0.73	0.44	0.91	0.51

Notes: This table presents stacked difference-in-differences estimates on our 46-state sample; see Table 2 for details on the specification. Each estimate is the average effect over years 0 through 3 relative to the year before adoption. Standard errors are clustered at the state level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. We estimate the effects separately for first births and for births to mothers who already had a child. The high-school-or-less rows use the education sample.

Table A19: Robustness to the Sample: Participation

	Total	Women	Children /100 births
49 retail states	5.09*** (1.38)	0.65 (0.90)	7.95*** (2.37)
Drop PA, NY	5.72*** (1.46)	1.73** (0.67)	6.59*** (2.36)
Drop MA, NY, PA (main sample)	5.82*** (1.47)	1.76** (0.67)	6.71*** (2.38)

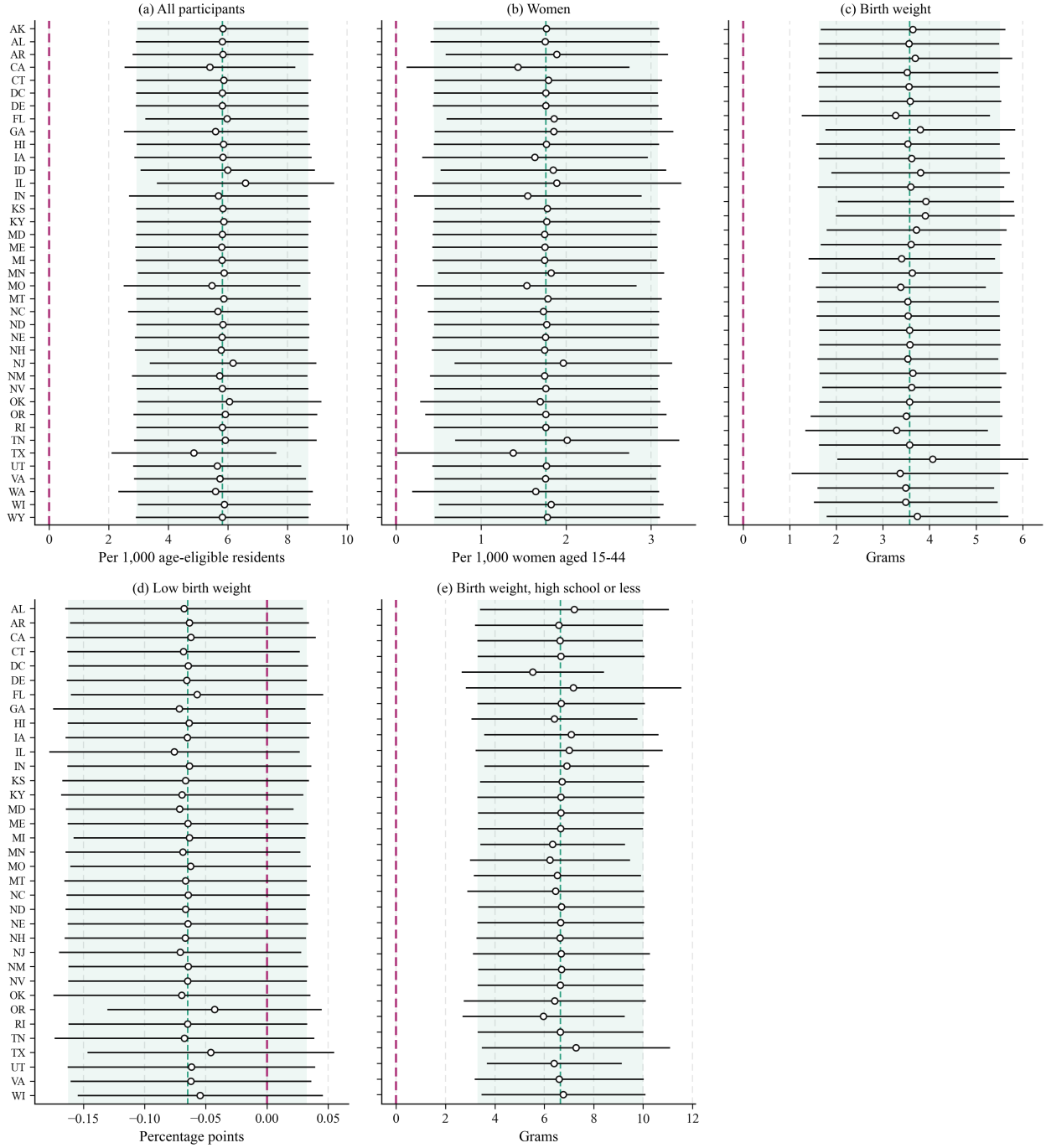
Notes: This table presents stacked difference-in-differences estimates on our 46-state sample; see Table 2 for details on the specification. Each estimate is the average effect over years 0 through 3 relative to the year before adoption. Standard errors are clustered at the state level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The 49 retail states include all states except Mississippi and Vermont.

Table A20: Robustness to the Sample: Birth Outcomes

	Birth weight	Low birth weight	Birth weight, HS or less
49 retail states	4.33*** (1.01)	-0.048 (0.044)	7.25*** (1.58)
Drop PA, NY	3.57*** (0.98)	-0.065 (0.049)	6.55*** (1.70)
Drop MA, NY, PA (main sample)	3.57*** (0.99)	-0.065 (0.050)	6.65*** (1.71)

Notes: See Appendix Table A19 for details.

Figure A3: Dropping one adopting state at a time



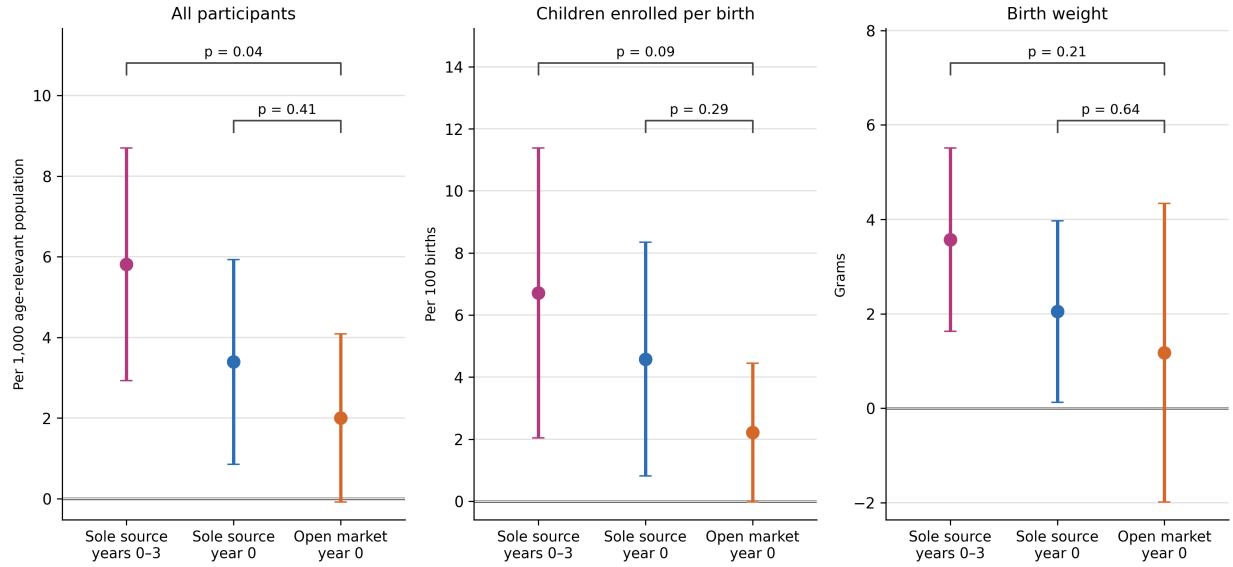
Notes: Each point re-estimates our main specification dropping the indicated state, with its 95 percent confidence interval. The dashed line and shaded band show the main estimate and its 95 percent confidence interval.

Table A21: Sole-Source and Open-Market Contracts

	Total	Women	Children /100 births	Birth weight
<i>Sole-source adoption</i>				
Year of adoption	3.39*** (1.29)	1.16** (0.53)	4.58** (1.92)	2.05** (0.98)
Mean of years 0–3 (main estimate)	5.82*** (1.47)	1.76*** (0.67)	6.71*** (2.38)	3.57*** (0.99)
<i>Open-market adoption</i>				
Year of adoption	2.01* (1.06)	0.62 (0.46)	2.22* (1.14)	1.18 (1.61)

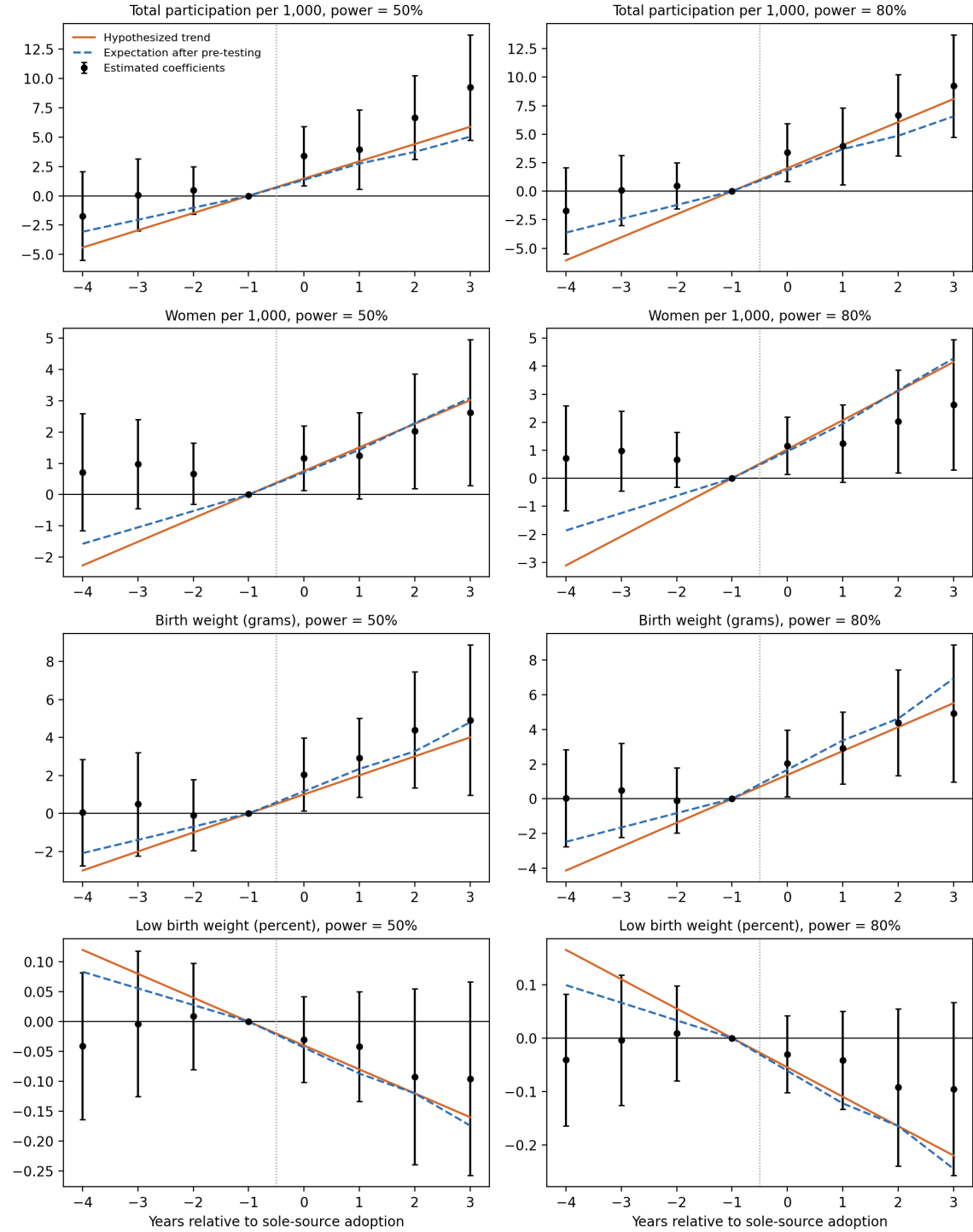
Notes: This table reports the coefficient for the year of adoption (relative to the year before adoption) from separate stacked regressions, and, for sole-source adoption, our main estimate (the mean of years 0–3). Sole-source adoption is our main specification, in which a state is treated from its first sole-source contract whether or not it operated an open-market contract before. Open-market adoption compares the states whose first contract was open market or multi-source and that adopted by FY1989 with states that had no contract yet, dropping years after any later sole-source contract. We report only the year of adoption for open-market contracts because it is the only horizon at which the open-market effect can be estimated on more than a handful of states. Standard errors are clustered at the state level and reported in parentheses, and stars are based on normal approximations.

Figure A4: Sole-source versus open-market adoption



Notes: Each point is the coefficient for the year of adoption (relative to the year before adoption) from a separate stacked regression, with 95 percent confidence intervals based on standard errors clustered at the state level. Sole source is our main specification; the first point is our main estimate, the mean of years 0–3. Open market compares the states whose first contract was open market or multi-source with states that had no contract yet. We show only the year of adoption for open-market contracts because it is the only horizon at which the open-market effect can be estimated on more than a handful of states. The brackets report p -values from tests of the difference between each sole-source estimate and the open-market estimate, treating the estimates from the separate regressions as independent. The horizontal line marks zero. The estimates are reported in Appendix Table A21.

Figure A5: Event-study coefficients and the pre-trends that the leads could detect



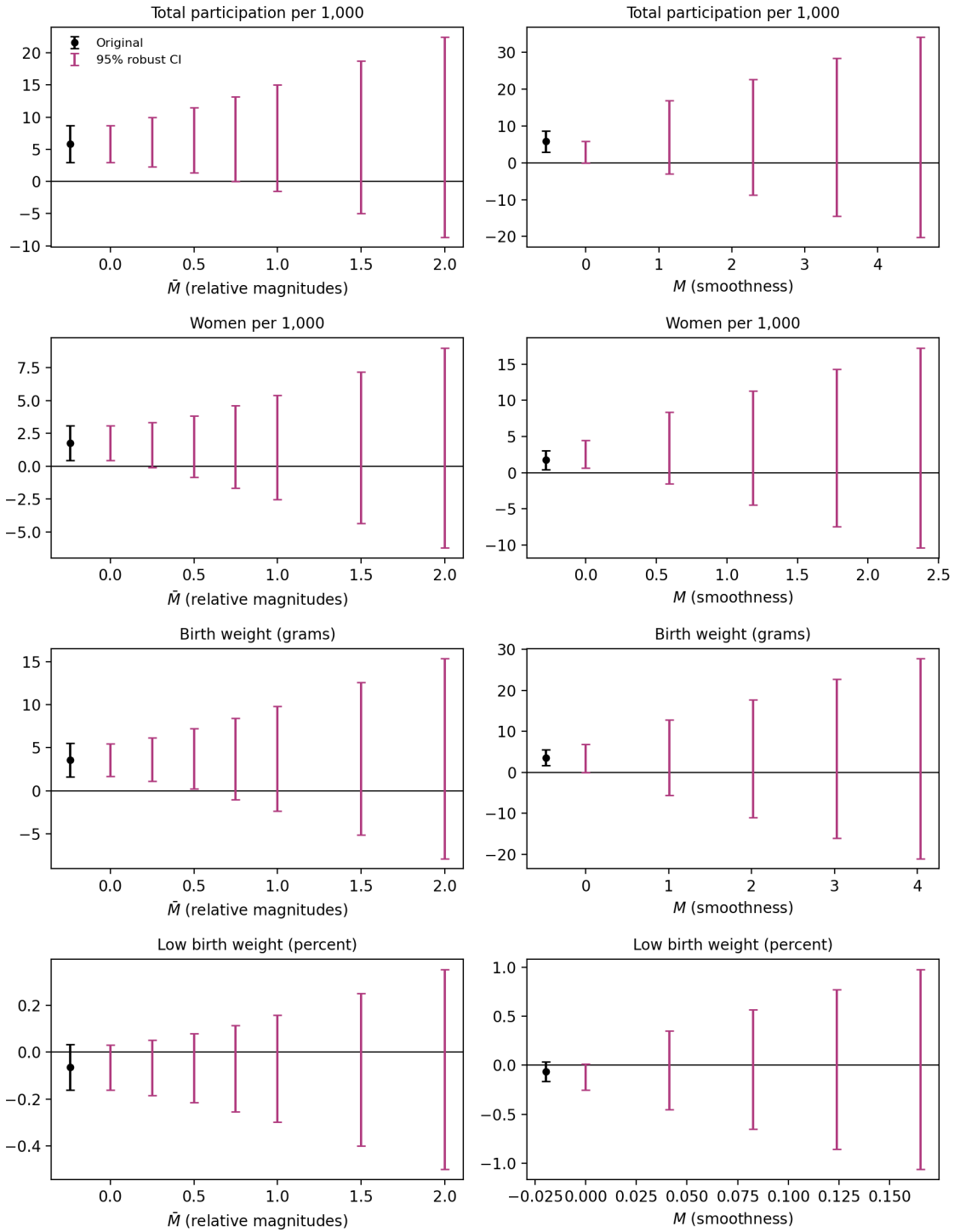
Notes: Each panel shows the event-study coefficients from the main stacked specification with 95 percent confidence intervals, together with the linear violation of parallel trends that a joint test of the three leads would detect with 50 percent (left) or 80 percent (right) power, following Roth (2022). The solid line is the hypothesized trend and the dashed line is the expected pattern of coefficients, conditional on the joint test not rejecting, if that trend were the truth. The expectation is computed by simulation from the estimated coefficients and their covariance matrix.

Table A22: Sensitivity to Violations of Parallel Trends

	Total participation	Women	Birth weight	Low birth weight
Estimated effect (mean of years 0–3)	5.82	1.76	3.57	-0.06
Joint test of leads, p -value	0.11	0.26	0.89	0.43
Slope detectable with 50% power	1.47	0.75	1.00	-0.04
Expected effect after pre-testing	3.22	1.87	2.89	-0.11
Slope detectable with 80% power	2.02	1.04	1.38	-0.05
Expected effect after pre-testing	4.23	2.58	4.15	-0.15
Breakdown $\bar{M}$ (relative magnitudes)	1.00	0.25	0.75	0.00
Breakdown M (smoothness)	0.00	0.59	1.01	0.00

Notes: The first two rows report the main stacked estimate (mean of years 0–3) and the p -value from a joint test of the three leads. Following Roth (2022), the next rows report the slope of a linear violation of parallel trends (per year, zero in the year before adoption, in the direction of the estimated effect) that the joint test would detect with 50 and 80 percent power, and the average post-adoption effect we would expect to estimate, conditional on not rejecting, if that violation were the truth. The last rows report the breakdown values from Rambachan and Roth (2023), that is, the smallest value in the grid at which the 95 percent robust confidence interval for the mean of years 0–3 includes zero. Under relative magnitudes, post-adoption deviations from parallel trends may be up to $\bar{M}$ times the largest pre-adoption deviation; under smoothness, the second differences of the deviation may be up to M in the units of the outcome, with $M = 0$ a linear extrapolation of the pre-trend. The grids are $\bar{M} \in \{0, 0.25, 0.5, 0.75, 1, 1.5, 2\}$ and M from zero to twice the standard error of the year-3 coefficient in five steps.

Figure A6: Robust confidence intervals under violations of parallel trends



Notes: Each panel shows the main stacked estimate (mean of years 0–3) with its 95 percent confidence interval, and the 95 percent robust confidence intervals of [Rambachan and Roth \(2023\)](#) for increasing violations of parallel trends. In the left column, post-adoption deviations from parallel trends may be up to $\bar{M}$ times the largest pre-adoption deviation. In the right column, the second differences of the deviation may be up to M in the units of the outcome, where $M = 0$ allows a linear extrapolation of the pre-trend. The horizontal line marks zero.

Table A23: Sole-Source Contracts and Retail Formula Prices

	Effect on price (percent)
Contract brand relative to other brands (triple difference)	2.5 (2.0)
Contract brand (difference in differences)	0.1 (6.4)
Other brands (difference in differences)	-4.5 (4.2)

Notes: This table reports the effect of a state's sole-source adoption on the log of the advertised real price per ounce of 13-ounce liquid-concentrate formula, in percent, from grocery-store newspaper advertisements in 43 states over fiscal years 1985–1993. The first row compares the contract manufacturer's brand with other brands in the same store before and after adoption, relative to states that had not yet adopted (a triple difference); the second and third rows report the two underlying differences in differences. Regressions are at the advertisement level with store-by-product and manufacturer-by-year fixed effects, and are weighted by the state's infant population. Standard errors are clustered at the state level and reported in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.