A Partial Identification Approach to Misclassified Treatment Assignment in Regression Discontinuity Designs: Evidence from WIC's Effect on Breastfeeding *

Peter Anderson

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Abstract

Breastfeeding promotion and support is a core service of the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC), which serves approximately half of all U.S. infants; however, program benefits include quantity vouchers for infant formula, encouraging substitution away from breastfeeding among participating mothers. To estimate WIC's effect on breastfeeding, we formalize and apply a partial identification approach to address a fundamental challenge in regression discontinuity designs: misclassification of treatment assignment due to measurement error in the running variable. Leveraging the program's income eligibility cutoff, we apply this approach to bound the local average treatment effect (LATE) on the initiation and duration of breastfeeding. Using data from the Pregnancy Risk Assessment Monitoring System (PRAMS), we find that participation in WIC reduces the probability of initiating breastfeeding by at least 3% and the duration of breastfeeding, conditional on initiation, by at least 2 weeks. Our findings are robust to alternative specifications and more conservative identifying assumptions.

JEL Codes: C14, I18, I38

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1 Introduction

The Special Supplemental Nutrition Program for Women, Infants, and Children (WIC)—the third largest federally funded nutrition program—serves approximately half of all infants in the United States and 6.3 million participants monthly. While the program improves birth outcomes and maternal health during pregnancy (Carlson and Senauer, 2003; Bitler and Currie, 2005; Kreider et al., 2020; Figlio et al., 2009; Hoynes et al., 2011), its effects on infant feeding practices remain unclear. Mandated to promote breastfeeding and support childhood nutrition, WIC provides both breastfeeding support services and quantity vouchers for infant formula to participating mothers. The existing academic literature provides no consensus on the program's net effect on breastfeeding: studies have found no effect (Jiang et al., 2010; Topolyan and Xu, 2017), substantial decreases (Ryan and Zhou, 2006; Bullinger and Gurley-Calvez, 2016), and even increases in breastfeeding (Joyce et al., 2008; Chatterji and Brooks-Gunn, 2004). Identifying WIC's causal effect poses a methodological challenge due to the non-random nature of program participation and limitations in the available survey data.

We examine how WIC participation affects breastfeeding initiation and duration among new mothers. Understanding these effects is critical due to the well-documented health benefits associated with breastfeeding for both infants and mothers, including reduced risks of childhood obesity, Type II diabetes, and certain cancers (Dieterich et al., 2013; Belfield and Kelly, 2012).[1] Among income-eligible mothers, WIC participants are less likely to initiate breastfeeding (88% vs. 92%) and breastfeed for shorter durations (11.8 vs. 13.4 weeks) than non-participants. With WIC's broad reach, even modest changes in breastfeeding behavior may have substantial public health implications. In 2018, subsidies for infant formula accounted for 49.1% (18.2%) of the program's \$4.5 (\$2.8) billion of spending on food assistance before (after) manufacturer rebates (Kline et al., 2020). The potential for WIC's formula subsidies to discourage breastfeeding—through reducing the relative price of formula, inducing substitution away from breastfeeding—raises con-

[1] While numerous observational studies document positive associations between breastfeeding and later health outcomes, the causal interpretation of these relationships is debated. See Oster (2013) for a popular discussion of these issues.

cerns about whether the program undermines its own nutritional objectives (Rose et al., 2006; Joyce et al., 2008).

We examine this empirical question using data from the Pregnancy Risk Assessment Monitoring System (PRAMS), a Centers for Disease Control and Prevention (CDC) surveillance program of maternal attitudes and experiences before, during, and shortly after pregnancy; however, two problems have confounded previous attempts to identify WIC's causal effects on breastfeeding. First, participation in WIC is endogenous: unobserved infant feeding preferences affect a mother's decision to enroll in WIC. Mothers with stronger preferences for formula feeding may be more likely to enroll in the program precisely because it provides formula subsidies, and as a result, naïve comparisons of observed differences between participants and non-participants offer a biased measure of the program's effect. Second, few datasets contain detailed measures of infant feeding outcomes, and the existing datasets are either small, retrospective, or suffer from measurement error in key variables. Our primary data source, PRAMS, is large and collected contemporaneously with infant feeding decisions but suffers from one important limitation: household income is reported in rounded brackets, creating uncertainty about eligibility near WIC's income cutoff, set nationally at 185% of the federal poverty level (FPL).[2] This bracketed reporting complicates empirical designs, such as regression discontinuity designs (RDD), that rely on precise measurement of income and treatment assignment to leverage policy discontinuities as a source of local exogeneity, motivating the need for an alternative identification strategy.[3]

We extend the regression discontinuity framework by developing a novel partial-identification approach that accommodates mismeasurement in the running variable and misclassification in treatment assignment, enabling estimation of the local average treatment effect (LATE) of WIC participation on breastfeeding outcomes. Our approach mirrors traditional regression discontinuity designs (RDD), using WIC's

[2]Two commonly used alternatives—the National Health and Nutrition Examination Survey (NHANES) and the WIC Infant and Toddler Feeding Practices Study (ITFPS)—are similarly limited. NHANES is small and retrospective, sampling only 5,000 individuals nationwide, and collects infant feeding data only for children under age five. ITFPS, while more detailed, samples only infant WIC participants, precluding comparison across program participation

[3]Even when income is reported as a continuous value, misreporting or recall error can lead to misclassification of treatment assignment near program eligibility thresholds.

income eligibility threshold as a discontinuity in program participation. In contrast to conventional designs, our method requires neither precise knowledge of the assignment variable, continuity of potential outcomes at the threshold, nor locally exogenous treatment assignment. Instead, we impose minimal and empirically supported shape restrictions of two flexible forms: (1) sign restrictions and (2) magnitude restrictions on the slope of the conditional expectation function. Our insight is that observations far from the threshold—where treatment status is known with certainty despite bracketed income—can be used to bound treatment effects near the threshold, where classification is uncertain. By imposing restrictions on the slope of conditional expectation functions across income, we extrapolate from regions with certain assignment to the local neighborhood around the cutoff. The resulting estimates are transparent, robust, and credible, even under measurement error. This framework is well suited to applications where eligibility is measured with error or only partially observed, making it broadly applicable to the evaluation of means-tested programs using survey data.

To validate and contextualize our findings, we also implement two conventional RDD estimators as benchmarks. First, we estimate a traditional fuzzy regression discontinuity design, which uses the 185% FPL eligibility cutoff as an instrument for WIC participation. Second, we estimate a doughnut RDD, which excludes observations near the threshold in order to mitigate the influence of income misreporting. Both designs rely on precise measurement of the running variable, continuity of counterfactual outcomes, and locally exogenous treatment assignment—assumptions that may be violated in this setting due to the bracketed nature of income reporting.

Our shape-restricted partial-identification estimates provide evidence that WIC participation reduces both the initiation and duration of breastfeeding. Under weak and empirically grounded assumptions, we find that breastfeeding initiation declines by no less than 3 percentage points, and duration, conditional on initiation, falls by at least 2 weeks. For breastfeeding initiation, the estimated effect represents three-quarters of the observed difference between WIC participants and income-eligible non-participants. For duration, the estimates correspond to 15 percent of average breastfeeding length among income-eligible mothers. These bounds are robust to a range of slope restrictions and remain negative even under conservative assumptions about the rate of change in outcomes across income levels.

To assess the sensitivity of our findings, we explicitly model household income uncertainty under a uniform distribution within reported income brackets. The resulting simulation-based estimates fall within our partial-identification bounds, providing additional support for the credibility of our approach. In contrast, the conventional RDD estimates rely upon stronger identifying assumptions and exhibit considerable sensitivity to modeling choices. These designs serve as useful benchmarks, but our partial identification approach offers clear improvements over both. Leveraging minimal shape restrictions, we credibly identify the sign and magnitude of the treatment effect in the presence of income mismeasurement and treatment assignment misclassification. Despite stronger identifying assumptions, the conventional RDD estimates do not consistently identify the sign of LATE—particularly at narrower bandwidths, where local exogeneity is most plausible but precision is lowest.

Our partial-identification approach contributes to the literature on inference under imperfect data, particularly regression discontinuity designs (RDD) affected by misclassified treatment assignment and measurement error in the running variable. While recent advances address various measurement issues in regression discontinuity designs—including heaping bias, discrete running variables, and rounding error—the specific problem of misclassified treatment assignment remains distinct and underexplored (Dong, 2015; Barreca et al., 2016; Kolesár and Rothe, 2018). A key limitation of existing methods is their reliance on correctly observed treatment status, which does not hold in many applied contexts with survey data. Standard polynomial selection methods are ill-suited for settings with error-prone running variables (Gelman and Imbens, 2019; Imbens and Wager, 2019; Pei et al., 2022), and existing solutions rely upon dropping misclassified observations, modeling manipulation patterns, or leveraging auxiliary datasets (Dong, 2015; Dong and Kolesár, 2023; Gerard et al., 2020; Davezies and Le Barbanchon, 2017). In our setting, none of these solutions are viable: income is reported in broad intervals; mismeasurement is bidirectional; and there are no comparable auxiliary data with which to validate assignment and treatment.

Building on the framework of Manski and Pepper (2018), who introduced bounded-variation assumptions for difference-in-differences designs, we develop a shape-restricted partial-identification method tailored to RDDs with interval-reported running variables and misclassified treatment assignment. This approach offers a middle ground between the strong continuity and local randomization assumptions of traditional RDD and the conservatism of worst-case nonparametric bounds. Our method is specifically designed to address the challenges of fuzzy designs with mismeasurement of treatment assignment, offering applied researchers a transparent and tractable framework for bounding treatment effects when point identification is not credible.

This paper is organized as follows. Section 2 describes the institutional details of WIC eligibility and benefit structure, and introduces the PRAMS data. Section 3 formalizes the identification challenges arising from interval-reported income and presents our shape-restricted partial-identification approach, including the theoretical assumptions and supporting evidence. Section 4 details the implementation of our empirical strategy, beginning with the partial-identification method and comparing it to two conventional benchmarks: a traditional RDD and a doughnut RDD. Section 5 presents our main findings, with a particular focus on our partial-identification bounds under varying slope restrictions and a simulation-based sensitivity analysis. Section 6 concludes the paper.

2 Institutional Setting & Data

2.1 Institutional Setting

The Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) is a federal assistance program that aims to improve the health and nutrition of low-income pregnant and postpartum women, infants, and children under age five. In 2021, WIC served an average of 6.3 million participants per month, including approximately half of all infants born in the United States.[4] Eligibility

[4] WIC Program Overview (https://www.ers.usda.gov/topics/food-nutrition-assistance/wic-program/)

requires: (1) household income below 185% of the Federal Poverty Level (FPL), (2) categorical eligibility as a pregnant or postpartum woman, infant, or child under five, (3) documented nutritional risk, and (4) state residency.[5] Participants in SNAP, Medicaid, or TANF are considered adjunctively income-eligible and are exempted from WIC's standard income requirement. Although cross-program participation is substantial—in 2020, 74% of WIC participants were enrolled in Medicaid and 28% in SNAP—only 3.5% reported household incomes above the program's standard income eligibility threshold of 185% of the Federal Poverty Level (United States Department of Agriculture, Food and Nutrition Service, 2022b).

WIC provides a combination of nutrition education, referrals to public assistance programs and healthcare providers, and supplemental food packages. Food benefits include quantity vouchers—restricted to specific brands, package sizes, and other characteristics that vary by state—for designated food items, as well as cash-value vouchers for fresh produce (Table 1). For infants aged 0 to 6 months, food packages consist exclusively of infant formula, tailored to fully meet caloric needs. Between 6 and 12 months of age, packages introduce age-appropriate solid foods alongside formula. Throughout the first year of life, the quantity of infant formula provided is determined by the mother's self-reported feeding practice: *fully formula feeding*, *partially breastfeeding*, or *fully breastfeeding*. Although food packages may be adjusted if feeding practices change, such transitions are biologically constrained, and the cessation of breastfeeding is often irreversible due to declining milk production.[6]

WIC is federally mandated to promote and support breastfeeding as the preferred method of infant feeding. In line with this mandate, WIC provides educational programs and offers "anticipatory guidance, counseling, and breastfeeding educational materials" to participating pregnant mothers.[7] Postpartum, additional support includes: (1) education and counseling through group classes or individual sessions; (2) breastfeeding supplies, such as breast pumps and storage containers; and (3) peer counseling from experienced mothers (National WIC Asso-

[5] WIC Overview (https://www.fns.usda.gov/wic/about-wic)

[6] Regular milk expression is required to maintain supply; extended interruptions typically result in permanent supply reduction.

[7] `https://www.fns.usda.gov/wic/breastfeeding-priority-program`

ciation, 2019). The 2009 Food Package Revision—occurring before the period of our analysis—was designed, in part, to strengthen pecuniary incentives for breastfeeding. Following the reform, fully breastfeeding mothers became eligible for more comprehensive food benefits and extended postpartum participation—up to one year, compared to six months for mothers who do not breastfeed.

Despite evidence that the 2009 reforms increased both the initiation and exclusive duration of breastfeeding among WIC participants (Whaley et al., 2012), overall breastfeeding rates remain low (Table 2). Only 22% of WIC infants are still breastfeeding at 6 months, well below the CDC's 12-month recommendation. The high cost of formula feeding—estimated at \$1600 to \$2100 during the first year—makes infant formula the most financially valuable component of the WIC food packages (Centers for Disease Control and Prevention, 2023; United States Department of Agriculture, Food and Nutrition Service, 2022a).[8] By providing infant formula in-kind, the program reduces its relative price, thereby creating price incentives that discourage breastfeeding initiation and encourage earlier cessation. Across all levels of household income, WIC participants are less likely to initiate breastfeeding and breastfeed for shorter durations than income-eligible non-participants (Panel B of Figure 2).

The documented association between early introduction of infant formula and adverse health outcomes—including childhood obesity, maternal obesity, breast cancer, and Type II diabetes—raises concerns about the health benefits and public value of WIC's in-kind provision of infant formula (Dieterich et al., 2013; Belfield and Kelly, 2012). While WIC participation has been shown to improve health outcomes among pregnant women (Carlson and Senauer, 2003; Bitler and Currie, 2005; Kreider et al., 2020; Figlio et al., 2009; Hoynes et al., 2011), there is limited evidence of comparable health benefits for infants. More than 30% of WIC infants and children exceed the 90th percentile for standardized growth measurements—weight-for-length (ages 0–2) and BMI (ages 2–4)—and the maternal obesity rate among participants has continued to rise (Figure 1). If these outcomes are causally

[8] U.S. Surgeon General's 2011 Call to Action to Support Breastfeeding, adjusted to 2022 dollars. `https://www.hhs.gov/surgeongeneral/reports-and-publications/breastfeeding/factsheet/index.html`

linked to infant formula, the in-kind transfer may diminish the public value of program participation.

2.2 Data

This study uses data from the Pregnancy Risk Assessment and Monitoring System (PRAMS), a surveillance project conducted jointly by the Centers for Disease Control and Prevention (CDC) and participating state health departments. PRAMS collects state-specific, population-based data on maternal attitudes, behaviors, and experiences before, during, and shortly after pregnancy.[9] Mothers are surveyed 2–6 months postpartum, with annual state-level sample sizes ranging from approximately 1,000 to 3,000 respondents. The dataset links birth certificate records with survey responses collected through personalized mailing packets, use of incentives and rewards, and telephone follow-up for mail non-respondents (Shulman et al., 2018). To ensure adequate representation of key subpopulations, participating states employ stratified sampling that over-samples "mothers of infants with low-birthweight, those living in high-risk geographic areas, and racial/ethnic minority groups" (Shulman et al., 2018).

Our analysis focuses on PRAMS Phases 6–8 (2009–2022), a period selected for two key reasons: the consistent structure of the WIC infant food packages following its October 2009 revision and the systematic collection of WIC participation data. The PRAMS questionnaire includes core questions asked across all participating states (55–60% of content), standardized CDC-developed questions, and state-specific additions (Shulman et al., 2018). From an initial sample of 508,387 mothers with live births, we construct our analytical sample by excluding observations with missing values for any of four key variables: breastfeeding initiation, breastfeeding duration, WIC participation during pregnancy, and household income relative to the Federal Poverty Level (FPL). This yields a final sample of 422,537 observations.[10] We next describe sample characteristics and the construction of our

[9] PRAMS Overview(https://www.cdc.gov/prams/index.htm). State participation in PRAMS is voluntary, and as of 2023, California, Idaho, North Carolina, and Ohio are the only states not participating.

[10] Sample sizes with complete data for individual variables are: breastfeeding initiation (484,964),

key variables, including measures of household income and WIC eligibility, breastfeeding initiation and duration, and program participation status.

Sample Characteristics Table 2 summarizes the characteristics of our analytical sample by WIC participation status among income-eligible households. In addition to differences in infant feeding practices, WIC participants differ systematically from eligible non-participants along several socioeconomic dimensions. WIC mothers are more likely to have low educational attainment (24% have less than a high school degree, compared to 13% of non-participants), less likely to be married (37% versus 58%), and report lower household incomes on average ($16,672 versus $24,748). These differences highlight the challenge of selection bias in identifying causal effects and suggest that simple mean comparisons may overstate WIC's negative impact on breastfeeding, as participants disproportionately possess characteristics associated with lower breastfeeding rates.

Income PRAMS reports income in rounded brackets that vary in both width and number across survey phases. Each bracket defines upper and lower bounds for household income, with the highest income bracket left open-ended (e.g., income > $100,000). We combine these brackets with household size to calculate upper (u_i) and lower (ℓ_i) bounds on each respondent's household income as a percentage of the Federal Poverty Level (FPL), adjusted for state- and year-specific thresholds.

For our analysis, we use the midpoint of these bounds as a proxy for true FPL: $X_i = \frac{u_i+\ell_i}{2}$. Using this measure, the estimated density of household income displays substantial and abrupt variation, reflecting the coarseness of the bracketed income reporting (Panel A of Figure 2). The average bracket width, as a percentage of FPL, is approximately 33 percentage points. Importantly, for 94% of observations with an income proxy within 10 percentage points of the eligibility threshold ($|X_i - 185| \in [0, 10]$), the upper and lower bounds straddle the 185% eligibility threshold, rendering treatment assignment uncertain. The distribution of incomes within these bounds is unobserved, and this measurement error is likely to attenuate

breastfeeding duration (487,002), WIC participation (505,976), and household income (450,785). The analytical sample requires non-missing values for all four variables.

the identifying variation in WIC participation near the income eligibility threshold.

Outcomes of Interest We examine two primary measures of infant feeding: the initiation and duration of breastfeeding, conditional on initiation. Initiation, a binary indicator of whether a mother attempts to breastfeed, is recorded by both birth certificate data and the PRAMS core questionnaire throughout our sample period. As initiation is documented at the time of birth, rather than solely through retrospective self-report, the risk of misclassification is minimal and unlikely to reflect recall or social desirability bias. For observations with missing initiation but non-missing duration, we impute initiation by coding mothers who breastfed for at least one week as having initiated breastfeeding.[11]

Breastfeeding duration, defined as the number of weeks a mother breastfeeds or pumps breast milk, is recorded in the PRAMS core questionnaire. While duration is self-reported, the PRAMS survey is administered shortly after delivery, typically within two to six months postpartum, limiting the risk of recall error relative to longer retrospective surveys. We additionally construct a measure of the exclusive duration of breastfeeding, defined as the number of weeks until the introduction of liquids other than breast milk or solid foods, using responses on the timing of these introductions.[12] Measurement of duration is right-censored by the age of the infant at the time of the survey, creating heterogeneous censoring across observations.

Table 2 and Panel B of Figure 2 reveal significant differences in infant feeding practices by WIC participation among income-eligible households (Income $\leq 185\%$ FPL). WIC participants initiate breastfeeding at lower rates (88% versus 92%), and conditional on initiation, breastfeed for shorter durations (11.84 weeks versus 13.38 weeks) than eligible non-participants. Exclusive breastfeeding duration follows a similar pattern, with WIC participants introducing non-breastmilk liquids earlier (11.52 versus 12.68 weeks). These differences may reflect both selection into WIC and potential treatment effects, motivating the need for causal

[11] The one-week cutoff used for imputation is a consequence of questionnaire data, which records duration in weekly increments.

[12] As an example, for a mother who reports initiating breastfeeding and introducing a liquid other than breastmilk, e.g. infant formula, at 8 weeks and solid food at 21 weeks, we record exclusive breastfeeding for 8 weeks.

identification.

The descriptive evidence reveals a clear positive relationship between household income and breastfeeding. On average, both the initiation and duration of breastfeeding increase with income (Figures 6 and 7). This pattern persists when excluding households with indeterminate treatment assignment, those with FPL bounds straddling the income eligibility threshold. Within eligibility groups, the income-breastfeeding relationship appears approximately linear, although slopes may differ between eligible and ineligible households, suggesting heterogeneity in the income gradient across eligibility status.

WIC Participation We define treatment as participation in WIC during pregnancy, which is consistently documented in both birth certificate data and PRAMS questionnaires across all sample years. While some mothers may only participate postpartum or enroll their infants directly, data on postnatal participation is recorded in only a limited set of states.[13] This measurement limitation has two implications. First, we underestimate total WIC exposure by missing postpartum enrollment among mothers and infants. Second, our treatment variable overstates the share of prenatal participants, the period when breastfeeding promotion is most intensive. Together, these issues may lead us to underestimate the negative effects of participation in WIC on breastfeeding outcomes.

Our measure of prenatal WIC participation benefits from administrative recording at the time of birth. Participation is drawn from the birth certificate rather than retrospectively self-reported, reducing the risk of misclassification commonly associated with survey-based data. In contrast, self-reported WIC participation in the CPS-ASEC yields markedly lower participation rates at all levels of income (see Figure 5), suggesting that birth certificate-based recording in PRAMS provides a more reliable measure of take-up. While our estimates remain robust to potential under-reporting of program participation, the timing of data collection and use of birth certificate records suggest that our WIC participation variable is comparatively reliable.

[13]Between 2009 and 2022, only Illinois (Phase 6), Maine (Phases 6–8), and New Hampshire (Phase 8) collected data on postpartum and infant WIC participation.

Household income and WIC participation have a negative relationship: as household income increases, average program participation decreases. Figure 4 documents both this relationship and a distinct discontinuity at the 185% FPL eligibility threshold. The pattern and threshold discontinuity persist when varying bandwidth and excluding households with indeterminate treatment assignment—those whose income brackets straddle the eligibility threshold (Figures 4b, 4d, and 4f). Within eligibility groups, the relationship appears approximately linear, although the slope differs between eligible and ineligible households. External validation using the Current Population Survey Annual Social and Economic Supplement (CPS-ASEC) confirms this approximately linear negative gradient between household income and WIC participation (Flood et al., 2022), supporting our findings (Figure 5).

3 Identification

This section formalizes our partial-identification framework and outlines the threats to identification posed by measurement error in the running variable, an issue increasingly examined in the economic literature (Barreca et al., 2011, 2016; Dong and Kolesár, 2023). We impose theoretically and empirically grounded assumptions to identify expected treatment take-up and mean outcomes. Under these assumptions, the local average treatment effect (LATE) is partially identified.

3.1 Identification Conundrum

We are interested in identifying the causal effect of WIC participation ($T \in \{0, 1\}$) on breastfeeding outcomes (Y). Following the potential outcomes framework, we observe treatment status $T = T_Z$, where $Z \in \{0, 1\}$ denotes treatment assignment. The observed outcome is given by

$$Y = Y_0 + T_Z(Y_1 - Y_0)$$

where Y_0 and Y_1 are the potential outcomes for the treated and untreated, respectively. Much of the existing academic literature focuses on estimating the average treatment effect (ATE), defined as ATE $= \mathbb{E}[Y_1 - Y_0|V]$, conditional on covariates V. However, even after conditioning on observables, unobserved heterogeneity in infant feeding preferences may influence enrollment in WIC, introducing selection bias and rendering ATE estimates non-causal.

The income eligibility threshold for WIC, set nationally at 185% of FPL, provides a natural source of exogenous variation in program eligibility, offering a solution to the problem of selection endogeneity. Let $X^\star$ denote a household's true income relative to the threshold, centered so that the cutoff occurs at zero. Eligibility for WIC is then defined as

$$Z = \mathbb{1}[X^\star \leq 0]$$

Following Dong and Kolesár (2023), we define the conditional probability of treatment under assignment $z \in \{0, 1\}$ as $p_z^\star(x) := \mathbb{E}[T_z|X^\star = x]$ and the observed treatment probability as

$$p^\star(x) := \mathbb{E}[T|X^\star = x] = \mathbb{1}[x > 0]p_0^\star(x) + \mathbb{1}[x \leq 0]p_1^\star(x)$$

Similarly, let $g_t^\star(x) := \mathbb{E}[Y_t|X^\star = x]$ denote the conditional mean of the potential outcomes, where $t \in \{0, 1\}$ denotes treatment, and define the conditional mean of the observed outcomes as

$$g^\star(x) := \mathbb{E}[Y|X^\star = x] = g_0^\star(x) + p^\star(x)[g_1^\star(x) - g_0^\star(x)]$$

Under the canonical assumptions of the fuzzy regression discontinuity design,

(i) Monotonicity: $\mathbb{P}(T_1 \geq T_0|X^\star = 0) = 1, \quad$ with $\mathbb{P}(T_1 > T_0|X^\star = 0) > 0$

(ii) Continuity: $p_z^\star(x)$ and $g_t^\star(x)$ are continuous at $x = 0$ for $z, t \in \{0, 1\}$

the local average treatment effect (LATE) is identified as:

$$\tau = \frac{\lim_{x \uparrow 0} g^\star(x) - \lim_{x \downarrow 0} g^\star(x)}{\lim_{x \uparrow 0} p^\star(x) - \lim_{x \downarrow 0} p^\star(x)}$$

PRAMS reports household income in categorical brackets rather than exact dollar amounts, precluding precise calculation of each household's income as a percentage of the Federal Poverty Level (FPL). This interval-based reporting induces measurement error in the running variable, as true household income is only known to fall within the reported bracket. As a result, we observe a noisy proxy $X = X^\star - e$, where e denotes measurement error. In our context, we define this proxy as the midpoint of each household's reported FPL interval. Regardless of the particular estimate $X_i \in [\ell_i, u_i]$, the width of these brackets creates uncertainty about treatment assignment—that is, true eligibility status—for 32,928 observations near the eligibility threshold. Unlike the setting analyzed by Dong and Kolesár (2023), measurement error in our case not only affects the precision with which we observe the running variable but also misclassifies treatment assignment.

When using the noisy measure X in place of true income $X^\star$, we observe assignment

$$\tilde{Z} = \mathbb{1}[X \leq 0]$$

rather than true assignment

$$Z = \mathbb{1}[X^\star \leq 0]$$

for observations where X and $X^\star$ lie on opposite sides of the threshold. Under the potential outcomes framework, the observed conditional means based on X are given by:

$$\tilde{p}(x) := \mathbb{E}[T \mid X = x] = \int_{-\infty}^{\infty} p^\star(x^\star) f_{X^\star|X}(x^\star|x) dx^\star$$
$$\tilde{g}(x) := \mathbb{E}[Y \mid X = x] = \int_{-\infty}^{\infty} g^\star(x^\star) f_{X^\star|X}(x^\star|x) dx^\star$$

The corresponding estimator using the noisy running variable is:

$$\tilde{\tau} = \frac{\lim_{x\uparrow 0} \tilde{g}(x) - \lim_{x\downarrow 0} \tilde{g}(x)}{\lim_{x\uparrow 0} \tilde{p}(x) - \lim_{x\downarrow 0} \tilde{p}(x)}$$

However, in this context, the conventional regression discontinuity estimator $\tilde{\tau}$ does not generally recover the true LATE. The discontinuities in $\tilde{g}(x)$ and $\tilde{p}(x)$ at $X = 0$ reflect an unobserved, weighted average of income-eligible and ineligible households. The resulting bias depends both on the degree of misclassification and on differences in potential outcomes at the threshold. Without knowledge of these misclassification probabilities, which depend on the distribution of measurement error e near the threshold, neither the direction nor magnitude of bias is identified. Thus, standard RDD estimators do not yield valid causal estimates under these conditions. This identification challenge motivates our partial-identification approach, which establishes bounds on the treatment effect without requiring assumptions on e.

3.2 Partial-Identification

Given measurement error in treatment assignment, we establish bounds on the treatment effect rather than pursuing a point estimate. Our approach leverages observations with unambiguous treatment assignment—households whose reported income intervals, as a percentage of the Federal Poverty Level (FPL), lie entirely above or below the eligibility threshold—to bound effects at the threshold, where assignment is uncertain.

We focus on bounding the local average treatment effect (LATE), defined in terms of the potential outcomes Y_0 and Y_1. As described above, let $g^\star(x) := \mathbb{E}[Y \mid X^\star = x]$ denote the conditional mean of outcome Y, and let $p^\star(x) := \mathbb{E}[T \mid X^\star = x]$ denote the observed treatment probability. Our partial-identification strategy imposes three assumptions:

Assumption 1 (Positive First Stage)**.** Income eligibility increases treatment take-up at the threshold:

$$\tau_T := \lim_{x \uparrow 0} p^\star(x) - \lim_{x \downarrow 0} p^\star(x) > 0$$

Assumption 2 (Monotonicity in Income)**.** For any $x^-, x^+ \in \mathbb{R}$ with $x^- \leq x^+$ such that $0 \notin [x^-, x^+]$:

$$\begin{aligned} &\text{(i)} \quad p^\star(x^-) \geq p^\star(x^+) \\ &\text{(ii)} \quad g^\star(x^-) \leq g^\star(x^+) \end{aligned}$$

Assumption 3 (Bounded Rate of Change)**.** Let $\mathcal{R}_1 = [-\delta_1, 0]$ and $\mathcal{R}_0 = (0, \delta_0]$ denote neighborhoods on the eligible and ineligible sides of the threshold, respectively. For $z \in \{0, 1\}$ and all $x \in \mathcal{R}_z$:

$$\text{(i)} \quad \left| \frac{dp^\star(x)}{dx} \right| \leq \lambda_z$$

$$\text{(ii)} \quad \left| \frac{dg^\star(x)}{dx} \right| \leq \kappa_z$$

where $\kappa_z, \lambda_z > 0$ are assignment-specific bounds and $\delta_z > 0$ defines the size of the neighborhood.

These assumptions are grounded in economic theory and supported by empirical regularities in the data. Assumption 1, the first-stage assumption, reflects that removing a constraint on program participation, in this case moving from income ineligibility to eligibility, should weakly increase participation rates. Empirically, Figure 4 shows a discontinuous increase in WIC participation at the eligibility threshold, with consistently higher participation rates observed among income-eligible households. This pattern is consistent with administrative data and prior analyses: WIC Participant Characteristics Reports and survey-based analyses document that program take-up is concentrated below the 185% FPL threshold, with participation rates declining steadily as income rises (United States Department of Agriculture, Food and Nutrition Service, 2022b; Hoynes and Schanzenbach, 2015).

Assumption 2 imposes monotonicity in income for both treatment take-up and outcomes. As income rises, program benefits represent a decreasing share of household resources, decreasing the value of participation. Figure 4 shows decreasing rates of participation as income increases and a discontinuous decline at the eligibility threshold. With respect to infant feeding practices, mothers in higher-income households typically have greater access to paid leave, flexible work arrangements, professional lactation support, and familial resources that serve to facilitate breastfeeding. Existing research documents strong socioeconomic gradients in breastfeeding behaviors, with higher-income and higher-education mothers initiating and sustaining breastfeeding at higher rates (American College of Obstetricians and Gynecologists, 2021; Diaz et al., 2023). Figures 6 and 7 show clear upward trends in both initiation and duration of breastfeeding as household income increases, both across the income distribution and within assignment groups.

Assumption 3 places limits on the rate at which these conditional expectations may change with household income. This reflects that changes in participation and breastfeeding behaviors are driven by structural and socioeconomic forces—program eligibility, workplace policies, and access to information—that evolve smoothly with income. These determinants, such as paid leave, flexible work arrangements, health literacy, and social support, are closely tied to income and accumulate gradually across the income distribution (American College of Obstetricians and Gynecologists, 2021; Diaz et al., 2023). Figures 4, 6, and 7 display smooth relationships without abrupt changes or irregularities, and within-group patterns appear approximately linear. Figure 8 provides further empirical support, showing that race, access to paid leave, return-to-work timing, and maternal education—-key determinants of both WIC participation and breastfeeding behaviors—evolve smoothly with household income. The gradual evolution of these characteristics across income levels provides empirical support for the bounded-slope restriction in local neighborhoods.

Construction of Bounds

Under the maintained assumptions of a positive first stage, monotonicity in income, and bounded rates of change in the conditional expectation functions, we derive bounds on the discontinuities in treatment take-up (τ_T) and outcomes (τ_Y) at the eligibility threshold. These bounds rely on observations with unambiguous treatment assignment and partially identify the local average treatment effect (LATE). Define the left and right limits at the threshold as:

$$g^\star(0^-) := \lim_{x \uparrow 0} g^\star(x), \quad g^\star(0^+) := \lim_{x \downarrow 0} g^\star(x)$$
$$p^\star(0^-) := \lim_{x \uparrow 0} p^\star(x), \quad p^\star(0^+) := \lim_{x \downarrow 0} p^\star(x)$$

so that $\tau_Y = g^\star(0^-) - g^\star(0^+)$ and $\tau_T = p^\star(0^-) - p^\star(0^+)$.

Household income is observed in intervals $[\ell, u]$. For households whose brackets lie entirely below the threshold ($u \leq 0$), treatment assignment is known with certainty: $Z = 1$. Likewise, for households with brackets entirely above the threshold ($\ell > 0$), treatment assignment is likewise certain: $Z = 0$. A household's true income $X^\star$ is unobserved but bounded: $X^\star \in [\ell, u]$.

Let x^- denote any point in an eligible bracket (where $u < 0$), and x^+ denote any point in an ineligible bracket (where $\ell > 0$). Under our shape restrictions, observations at x^- provide bounds for the left limits $g^\star(0^-)$ and $p^\star(0^-)$, and observations at x^+ provide bounds for the right limits $g^\star(0^+)$ and $p^\star(0^+)$.

For certainly eligible households ($u < 0$), Assumptions 2 and 3 imply:

$$p^\star(0^-) \in [p^\star(x^-) - \lambda_1 \cdot |x^-|, \quad p^\star(x^-)]$$
$$g^\star(0^-) \in [g^\star(x^-), \quad g^\star(x^-) + \kappa_1 \cdot |x^-|]$$

Similarly, for certainly ineligible households ($\ell > 0$), we obtain:

$$p^\star(0^+) \in [p^\star(x^+), \quad p^\star(x^+) + \lambda_0 \cdot x^+]$$
$$g^\star(0^+) \in [g^\star(x^+) - \kappa_0 \cdot x^+, \quad g^\star(x^+)]$$

We obtain bounds on discontinuities τ_T and τ_Y by considering the extreme cases:

$$\tau_T \in \left[p^\star(x^-) - p^\star(x^+) - \lambda_0 \cdot x^+ - \lambda_1 \cdot |x^-|, \quad p^\star(x^-) - p^\star(x^+)\right]$$
$$\tau_Y \in \left[g^\star(x^-) - g^\star(x^+), \quad g^\star(x^-) - g^\star(x^+) + \kappa_1 \cdot |x^-| + \kappa_0 \cdot x^+\right]$$

These bounds on the treatment and outcome discontinuities partially identify the local average treatment effect (LATE). For the first stage, we obtain bounds $[\tau_T^{LB}, \tau_T^{UB}]$ on the discontinuity in treatment take-up, which is constrained to be positive by Assumption 1. For the outcome, we similarly obtain bounds $[\tau_Y^{LB}, \tau_Y^{UB}]$ on the discontinuity in the outcome—either the initiation or duration of breastfeeding. The ratio of these bounds yields an interval for LATE:

$$\tau = \begin{cases} \left[\dfrac{\tau_Y^{LB}}{\tau_T^{UB}}, \dfrac{\tau_Y^{UB}}{\tau_T^{LB}}\right] & \text{if } \tau_Y^{LB} \geq 0 \\ \left[\dfrac{\tau_Y^{LB}}{\tau_T^{LB}}, \dfrac{\tau_Y^{UB}}{\tau_T^{UB}}\right] & \text{if } \tau_Y^{UB} < 0 \\ \left[\dfrac{\tau_Y^{LB}}{\tau_T^{LB}}, \dfrac{\tau_Y^{UB}}{\tau_T^{LB}}\right] & \text{if } \tau_Y^{LB} < 0 < \tau_Y^{UB} \end{cases}$$

The applicable case depends on the sign of the outcome bounds. When $\tau_Y^{LB} \geq 0$, both numerator bounds are positive, and dividing the lower bound by the largest denominator (τ_T^{UB}) and the upper bound by the smallest denominator (τ_T^{LB}) yields the narrowest valid interval. When $\tau_Y^{UB} < 0$, both numerator bounds are negative, and the relationship is reversed. When the outcome bounds straddle zero, the treatment effect is unsigned, and we conservatively divide by the smallest denominator (τ_T^{LB}) in both expressions to ensure a valid identification region.

Our approach departs from the traditional regression discontinuity design (RDD) assumption of exogeneity, which requires that households cannot precisely manipulate their income to fall just below the eligibility threshold. Instead, we exclude observations near the threshold, where manipulation is most likely, and restrict attention to households with unambiguous treatment assignment. Households capable of sorting to just below the threshold in response to incentives are thus omitted from our identification strategy. By imposing shape restrictions on the conditional expectation functions (CEF), we extrapolate from manipulation-free regions to bound treatment effects at the threshold. In so doing, we trade the local randomization assumption of the standard RDD for smoothness and monotonicity restrictions on the CEFs. As long as the first stage is positive, the local average treatment effect (LATE) is well-defined, and the resulting bounds remain valid—even in the presence of sorting around the eligibility threshold. Unlike conventional RDD approaches, our method does not require dense support or local randomization near the eligibility threshold. Identification is achieved through shape restrictions rather than localized comparisons, and our bounding procedure avoids reliance on narrow bandwidths and remains valid even with sparsely populated neighborhoods around the cutoff.

4 Empirical Strategy

This section outlines our empirical strategy for estimating the causal effect of WIC participation on breastfeeding outcomes. We begin with our primary partial-identification approach, which bounds the local average treatment effect (LATE) under flexible, empirically supported assumptions. To benchmark these results, we implement two conventional designs: (1) a traditional fuzzy regression discontinuity design (RDD) and (2) a doughnut RDD that excludes observations near the eligibility threshold. Both conventional approaches rely on stronger identification assumptions, including local exogeneity and precise classification of treatment assignment. All approaches use a common income proxy, $X_i = (\ell_i + u_i)/2$, representing the midpoint of each household's reported FPL interval.

4.1 Partial-Identification RDD

To implement our partial-identification approach, we construct bounds using households with unambiguous treatment assignment—those whose income brackets lie entirely above or below the eligibility threshold. We exclude observations near the threshold, where measurement error creates uncertain eligibility, and restrict the estimation sample to households with known treatment assignment. We then extrapolate from these manipulation-free regions to bound treatment effects at the threshold.

4.1.1 Bin Construction and Reference Sets

Our identification strategy relies on households with certain treatment assignment in order to bound conditional expectation functions $g^\star(x)$ and $p^\star(x)$ at the eligibility threshold. For each bandwidth h, we partition observations into bins of width h, defined over the observed income proxy X_i.

We then identify reference sets: the nearest bins on either side of the threshold for which treatment assignment is unambiguous for the average household. On the left (eligible) side of the threshold, we select the first bin for which the mean upper bound of reported income falls below $-h$:

$$\frac{1}{n_b} \sum_{i \in b} u_i < -h$$

On the right (ineligible) side, we select the first bin for which the mean lower bound of income exceeds h:

$$\frac{1}{n_b} \sum_{i \in b} \ell_i > h$$

These conditions ensure that the average income bracket in each reference bin lies outside the neighborhood $[-h, h]$ surrounding the threshold. We denote these reference bins as b_h^- and b_h^+, requiring each to contain at least 100 observations. Under Assumption 2, the mean outcomes in b_h^- and b_h^+ provide one-sided bounds on the conditional expectations over the regions $[-h, 0]$ and $(0, h]$ adjacent to the threshold, corresponding respectively to marginally income-eligible and -ineligible households.

4.1.2 Estimation Strategy

Having established one-sided bounds on the conditional expectation functions from the reference bins on either side of the threshold, we construct complementary bounds by combining observed bin means with constraints on the allowable rate of change in outcomes with respect to income. Specifically, we bound the functions $g^\star(x) = \mathbb{E}[Y \mid X^\star = x]$ and $p^\star(x) = \mathbb{E}[T \mid X^\star = x]$ separately for each side of the income eligibility threshold. Together with Assumption 2, these constraints yield upper and lower bounds on the conditional expectations over the regions $[-h, 0]$ and $(0, h]$ adjacent to the threshold.

For each outcome, we fit separate models for certainly eligible ($X_i^\star \leq c$) and certainly ineligible ($X_i^\star > c$) households. We use estimation windows extending 100 percentage points on each side of the eligibility threshold, ensuring an adequate sample size while maintaining local validity.

For binary outcomes (WIC participation and breastfeeding initiation), we estimate logistic regression models:

$$\text{logit}(p_i) = h_z(X_i - c) + \mathbf{Z}_i'\boldsymbol{\beta}_z + \epsilon_i \quad \text{for } z \in \{0, 1\} \tag{1}$$

where $h_z(\cdot) = \sum_{k=1}^{p} \gamma_{z,k}(X_i - c)^k$ is a polynomial function capturing the relationship between household income and the outcome, and $\mathbf{Z}_i$ is a vector of maternal controls.

For breastfeeding duration, we implement a Tobit model that accounts for right-censoring:

$$y_i^* = h_z(X_i - c) + \mathbf{Z}_i'\boldsymbol{\beta}_z + \epsilon_i \tag{2}$$

$$y_i = \min(y_i^*, A_i) \tag{3}$$

$$\epsilon_i \sim N(0, \sigma^2) \tag{4}$$

where y_i^* is latent duration, y_i is observed duration, and A_i is the infant's age at interview. As before, $h_z(\cdot)$ is a polynomial function of the relationship with household income. We restrict estimation to mothers who have initiated breastfeeding, isolating the intensive margin effect on duration.

To operationalize Assumption 3, we separately estimate the rate of change in the conditional expectation functions with respect to income, $\frac{\partial h_z}{\partial X}$, over the intervals $[-100, 0]$ for the income-eligible ($z = 1$) and $[0, 100]$ for the ineligible ($z = 0$). We then scale these estimates by multipliers in $\{1, 1.5, 2.0\}$ to form plug-in estimators for λ_z and κ_z, which bound the rate of change in the participation and outcome functions. This approach assumes that average slope estimates across these broader regions provide a conservative upper bound on the rate of change between each reference bin and the threshold. We use these bounds to extrapolate from the observed means in each bin to the threshold, allowing deviations of up to $\lambda_z \cdot |x^{-/+}|$ for participation and $\kappa_z \cdot |x^{-/+}|$ for breastfeeding outcomes, where x^- and x^+ denote the midpoints of the reference bins for $z = 1$ and $z = 0$, respectively.

4.1.3 LATE Bounds and Implementation Details

We construct bounds on the discontinuities in treatment take-up (τ_T) and outcomes (τ_Y) by combining the upper and lower bounds on conditional expectations at the threshold, derived from the monotonicity and shape restrictions. Figure 9 provides a stylized visualization of this procedure.

Consistent with Assumption 1, we impose the requirement that $\tau_T^{LB} > 0$. When the data yield $\tau_T^{LB} \leq 0$, we do not report LATE bounds, as the first stage is too weak for credible identification. For specifications that satisfy the first-stage bounds, we construct LATE bounds as:

$$\hat{\tau} = \begin{cases} \left[\frac{\hat{\tau}_Y^{LB}}{\hat{\tau}_T^{UB}}, \frac{\hat{\tau}_Y^{UB}}{\hat{\tau}_T^{LB}}\right] & \text{if } \hat{\tau}_Y^{LB} \geq 0 \\ \left[\frac{\hat{\tau}_Y^{LB}}{\hat{\tau}_T^{LB}}, \frac{\hat{\tau}_Y^{UB}}{\hat{\tau}_T^{UB}}\right] & \text{if } \hat{\tau}_Y^{UB} < 0 \\ \left[\frac{\hat{\tau}_Y^{LB}}{\hat{\tau}_T^{LB}}, \frac{\hat{\tau}_Y^{UB}}{\hat{\tau}_T^{LB}}\right] & \text{if } \hat{\tau}_Y^{LB} < 0 < \hat{\tau}_Y^{UB} \end{cases} \tag{5}$$

The appropriate structure is determined by the outcome bounds, which govern whether the LATE bounds span only positive values, only negative values, or both.

We implement this procedure across multiple specifications, varying both the bandwidth h and polynomial order p. For each specification, we re-estimate the reference bins, refit the participation and outcome models, and recompute the slope-based extrapolation limits using multipliers $\in \{1, 1.5, 2.0\}$. This allows us to examine the sensitivity of our bounds to different modeling and bandwidth choices, paralleling the specification search common in conventional regression discontinuity designs.

To quantify sampling uncertainty, we employ a bootstrap procedure with 1,000 replications. For each bootstrap sample, we re-run the complete analysis from bin selection through LATE bound construction. We report the 2.5th and 97.5th percentiles of the bootstrap distribution as 95% confidence intervals for our bounds. This approach accounts for the complex dependence structure in our multi-step estimation procedure.

4.2 Traditional and Doughnut Regression Discontinuity Designs

As a benchmark, we implement two conventional regression discontinuity designs (RDD): a standard fuzzy RDD and a doughnut RDD that excludes observations with ambiguous treatment assignment due to income bracketing. Both approaches exploit the discontinuity in WIC eligibility at 185% of the Federal Poverty Level (FPL) and use a common income proxy, $X_i = (\ell_i + u_i)/2$, the midpoint of each household's reported income bracket.

The standard fuzzy RDD estimates the local average treatment effect (LATE) using households near the income eligibility threshold, where program participation shifts discontinuously at the locally arbitrary cutoff. We instrument for endogenous participation in WIC T_i using income eligibility $Z_i = \mathbb{1}[X_i \leq c]$, where $c = 185$ denotes the eligibility threshold in FPL percentage points. Specifically, we estimate:

$$Y_i = \alpha + \tau T_i + f^+(X_i - c) + Z_i \cdot f^-(X_i - c) + \epsilon_i \tag{6}$$

where Y_i is the infant feeding outcome and f^+ and f^- are polynomial functions of order p defined on either side of the threshold, allowing the relationship between outcomes and income to vary flexibly for eligible and ineligible households.

Identification under this fuzzy RDD framework relies on two standard assumptions:

(i) Monotonicity: $\mathbb{P}(T_1 \geq T_0 | X = 0) = 1$ and $\mathbb{P}(T_1 > T_0 | X = 0) > 0$

(ii) Continuity: $p_z(x)$ and $g_t(x)$ are continuous at $x = 0$ for $z, t \in \{0, 1\}$

Monotonicity requires that income eligibility weakly increases participation for all households, with a strictly positive effect for some. Continuity requires that both the conditional probability of participation, $p_z(x) = \mathbb{E}[T_z \mid X = x]$, and conditional mean outcomes, $g_t(x) = \mathbb{E}[Y_t \mid X = x]$, are continuous at the threshold for $z, t \in \{0, 1\}$. Under these conditions, τ identifies the local average treatment effect (LATE) among compliers at the eligibility cutoff.

As a secondary benchmark, we implement a "doughnut" regression discontinuity design (RDD) that explicitly excludes observations with uncertain eligibility status. PRAMS reports household income in rounded brackets, making treatment assignment ambiguous for those households whose income brackets straddle the eligibility threshold. Let $\mu = \frac{1}{N}\sum_{i=1}^{N}(u_i - \ell_i)$ denote the average width of these brackets, where u_i and ℓ_i are the upper and lower bounds of household i's income bracket as a percentage of the Federal Poverty Level (FPL). In our sample, $\mu \approx 33$ percentage points.

For our doughnut approach, we exclude all observations within μ of the eligibility threshold on each side and estimate equation 6 using the subsample $|X_i - c| \in [\mu, \mu + h]$. While this approach reduces potential bias from misclassified treatment assignment, it strengthens the identifying continuity requirements. In contrast with the standard fuzzy RDD, which requires continuity of potential outcomes only at the threshold, the doughnut RDD assumes that conditional relationships estimated from observations outside the exclusion zone approximate the conditional expectation functions at the threshold. The benefit is eliminating bias from misclassified treatment assignment at the cost of relying on observations further from the discontinuity.

For both designs, we estimate equation 6 across a range of bandwidths h. For the standard RDD, we restrict the sample to observations with $|X_i - c| \leq h$, and for the doughnut RDD, we use observations $|X_i - c| \in [\mu, \mu + h]$. For each bandwidth-polynomial combination, we compute the Akaike Information Criterion (AIC) and select the specification that minimizes this criterion. While useful as benchmarks, these models rely on precise income measurement and local randomization, assumptions explicitly relaxed in our partial-identification framework.

5 Results

In this section, we present estimates of the local average treatment effect (LATE) of prenatal WIC participation on breastfeeding behaviors using our primary partial-identification framework. We begin by reporting AIC-selected bounds under empirically grounded shape restrictions, then assess their sensitivity to alternative slope constraints and validate their performance using a simulation-based approach. As benchmarks, we compare these results with conventional point estimates from standard and doughnut regression discontinuity designs.

5.1 Partial-Identification Regression Discontinuity

For the partial-identification RDD, Table 4 reports bounds on the local average treatment effect (LATE) and associated 95% confidence intervals for the initiation and duration of breastfeeding. We impose Assumptions 1, 2, and 3 to extrapolate from households with unambiguous treatment assignment and construct bounds at the eligibility threshold. Estimates are derived from AIC-selected polynomial models fit to certainly eligible and certainly ineligible subsamples. Confidence intervals correspond to the 2.5th to 97.5th percentiles of the bootstrap distribution and are reported only when the lower bound of the first-stage estimate exceeds zero in at least 50% of replications (1000 draws). Unlike conventional RDD methods, this approach does not require local continuity or randomization assumptions, thereby avoiding reliance on narrow bandwidths near the threshold—those most prone to violations of Assumption 1.

For breastfeeding initiation, AIC-selected models yield stable and informative bounds on LATE. The 95% confidence intervals from these models consistently exclude zero, with bounds ranging from –0.05 to –0.20 (Figure 10). At narrower bandwidths, estimates are more variable and more likely to violate the first-stage condition (Figure 11). This pattern persists when the allowable rate of change in mean outcomes is increased using slope multipliers of 1.5 and 2.0, though the bounds widen slightly and the set of valid specifications narrows.

For breastfeeding duration, AIC-selected models identify the sign of the treatment effect, with 95% confidence intervals typically ranging from –2 to –10 weeks (Figure 10). When the slope constraint is relaxed, the estimates remain statistically significant and negative, reinforcing our main findings. As with initiation, narrower bandwidths are more prone to instability and frequently violate the first-stage condition (Figure 11).

For both outcomes, the bounds are consistently negative and provide strong evidence that WIC participation reduces breastfeeding initiation and duration. These results are robust to faster rates of change in mean outcomes. To further assess the credibility of our estimates, we turn to a trio of sensitivity tests. These checks explicitly model income uncertainty, vary the implementation of slope constraints, and examine robustness to alternative modeling assumptions. Together, they provide complementary evidence on the credibility and stability of our partial-identification results.

5.1.1 Simulation RDD

As a first sensitivity check, we implement a simulation-based regression discontinuity design (RDD) that explicitly models income uncertainty. For each household with reported income bracket $[\ell_i, u_i]$, expressed as a percentage of the Federal Poverty Level (FPL), we draw simulated values from a uniform distribution:

$$X^{\star}_{i,s} \sim \text{Uniform}(\ell_i, u_i),$$

where $s \in \{1, ..., 1000\}$ indexes the simulation draws. This procedure yields 1,000 datasets, each with point-identified income values drawn from the reported household-specific brackets.

For each simulated dataset s, we assign treatment eligibility $Z_{i,s} = \mathbb{1}[X^{\star}_{i,s} \leq c]$ and estimate equation 6 as a traditional fuzzy RDD. This yields 1,000 treatment effect estimates, $\{\hat{\tau}_s\}_{s=1}^{1000}$. We report the mean estimate, $\bar{\tau} = \frac{1}{1000}\sum_{s=1}^{1000}\hat{\tau}_s$, as our simulation-based LATE. The 95% confidence interval is defined by the 2.5th and 97.5th percentiles of the empirical distribution.

This approach retains all observations—including those nearest the threshold, which are most informative for identification—and explicitly accounts for uncertainty in treatment assignment. By drawing from the full support of each household's income bracket, it avoids the attenuation bias introduced by midpoint proxies. The primary limitation is the uniform distributional assumption. Although the true within-bracket distribution of income is unknown, the uniform distribution offers a transparent baseline that assigns equal probability to all values within the reported range. For each simulation, we select the optimal polynomial order using the Akaike Information Criterion to ensure consistency with our other designs.

Table 4 reports simulation-based local average treatment effects (LATE), 95%

confidence intervals, and first-stage F-statistics for the initiation and duration of breastfeeding. For initiation, AIC-selected LATE estimates are uniformly negative and exhibit limited variation across bandwidths, with magnitudes ranging from -0.04 to -0.055. Despite this stability, these estimates are statistically insignificant across bandwidths (Figure 12a). Mean first-stage F-statistics exceed 10 across most specifications, indicating sufficient instrument strength (Figure 12c). Simulation-based estimates are contained within the partial-identification bounds, lending support to our main results under the assumption that income is uniformly distributed within each bracket.

For breastfeeding duration, simulation LATE estimates are again consistently negative and stable across bandwidths, with magnitudes ranging from -2.6 to -3.4 weeks. Statistically significant effects emerge only at the widest bandwidths (Figure 12b). Mean first-stage F-statistics exceed 10 across bandwidths, indicating adequate instrument strength (Figure 12d). As with initiation, simulation-based point estimates fall within the partial-identification bounds, reinforcing the credibility of our main results.

5.1.2 Sensitivity to Slope Bound Assumptions

We next examine the sensitivity of our partial-identification results to the manner in which the bounded rate of change is operationalized (Table 4). In our main specification, the slope is calculated as the average change in the estimated outcome function over a wide interval ($[-100, 0]$ for eligible and $[0, 100]$ for ineligible households). Here, we instead use the slope of the AIC-selected model evaluated at the midpoint of each reference bin, providing a localized rather than an averaged measure of variation.

Figure 13 plots LATE bounds for breastfeeding initiation and duration under this alternative slope restriction with multipliers $\in \{1.0, 1.5, 2.0\}$. The results closely match those from the baseline mean-slope specification: estimated bounds remain statistically significant and negative across the majority of bandwidths, and the 95% confidence intervals generally overlap with those from our main results. The similarity of findings under both slope implementations reinforces the stability of our results and supports the conclusion that WIC participation reduces breastfeeding.

5.1.3 Sensitivity to Slope Linearization

As an additional robustness check, we examine the sensitivity of our partial-identification results to the linearization of the estimated outcome functions (Table 4). In our main specification, the slope is constrained by a constant upper bound. Here, we instead use the full estimated polynomial function to evaluate the variation between the reference bin and the threshold, allowing for curvature in the potential outcome functions.

We re-estimate the bounds on the local average treatment effect (LATE) using this nonlinear slope procedure for the baseline slope multiplier (1.0) and under relaxed values $\{1.5, 2.0\}$. Across both breastfeeding initiation and duration, the results closely match those obtained under linearization (Figure 14). The 95% confidence intervals remain negative and statistically significant for intermediate and wide bandwidths, and the bounds continue to contain the point estimates from the simulation RDD design.

These findings indicate that our inference is not sensitive to the particular choice of slope approximation. Whether imposing a constant upper bound on slope magnitude or permitting curvature in the estimated outcome functions, the results consistently show that WIC participation reduces both breastfeeding initiation and duration.

5.2 Traditional and Doughnut Regression Discontinuity

We next present results from the traditional and doughnut regression discontinuity designs (RDDs), which serve as benchmarks for our partial-identification approach. Both designs estimate zero-, first-, and second-order polynomials across a range of bandwidths, with the optimal order selected by minimizing the Akaike Information Criterion (AIC). In the traditional RDD, the estimation sample includes all observations within the selected bandwidth on either side of the income eligibility threshold, $|X_i| \in [0, h]$. In the doughnut RDD, the sample is restricted to observations $|X_i| \in [\mu, 185 + \mu]$, where μ is the mean width of the FPL brackets. Tables 5 and 6 present detailed local average treatment effect (LATE) estimates and first-stage F-statistics across select specifications. Figures 15 and 16 summarize the key patterns: AIC-optimal estimates and corresponding first-stage F-statistics across all bandwidths. Both approaches yield predominantly negative effects of WIC participation on breastfeeding outcomes, though they differ in statistical power and consistency. The doughnut RDD produces stronger first-stage relationships and more stable estimates across bandwidths, suggesting that measurement error near the threshold substantially affects traditional RDD inference.

For both breastfeeding outcomes, the traditional RDD yields predominantly negative but often statistically insignificant estimates. For initiation, narrow bandwidths produce insignificant effects, with significance emerging only at the widest windows where estimates range from –0.05 to –0.15 (Figure 15a). Duration estimates follow a similar pattern of insignificance at narrow bandwidths, though point estimates become less negative as the bandwidth widens, ranging from -8 to -1 weeks (Figure 15b). Across both outcomes, first-stage F-statistics fall below 10 for most bandwidths narrower than 4 percentage points, indicating persistent weak-instrument concerns that undermine reliable inference (Figure 16).

The doughnut RDD produces stronger and more consistent evidence of negative WIC effects across both outcomes. For initiation, point estimates are uniformly negative, ranging from -0.01 to -0.10, with statistical significance emerging at bandwidths as narrow as 2 percentage points (Figure 15c). Duration estimates are negative, clustering around -5 weeks, and achieve significance at narrower bandwidths than the traditional approach (Figure 15d). First-stage F-statistics generally exceed 10 across bandwidths for both outcomes, alleviating the weak-instrument concerns that plague the traditional RDD (Figure 16).

These results demonstrate the fundamental issues in conventional regression discontinuity designs with a rounded running variable. Although widening the bandwidth strengthens the first stage, it undermines the local-randomization (exogeneity) assumption essential for identification. The doughnut RDD alleviates weak-instrument concerns by excluding ambiguous observations near the threshold, but relies more heavily on observations farther from the cutoff, where the fundamental assumptions of local-randomization and continuity of counterfactual outcomes are less plausible.

5.3 Discussion

Our analysis uses partial identification to address bracket reporting (or rounding) in the running variable, a common challenge in regression discontinuity designs (RDD). As formalized in Section 3, bracketed reporting creates two distinct difficulties: (1) measurement error in the running variable and (2) misclassification of treatment assignment for observations whose brackets straddle the policy threshold. In such a setting, conventional RDDs struggle to identify the local average treatment effect. Including observations near the threshold introduces misclassification bias from households with an observed running variable, X, and true running variable, $X^{\star}$, on opposite sides of the cutoff. Excluding these observations relies on increasingly tenuous identification assumptions. Our partial-identification framework sidesteps this dilemma by focusing on households with unambiguous treatment assignment and imposing theoretically grounded shape restrictions to bound effects at the threshold.

Three key advantages distinguish our approach from conventional methods. First, our framework explicitly addresses misclassification when observed treatment assignment, $\tilde{Z} = \mathbb{1}[X \leq 0]$, differs from true assignment, $Z = \mathbb{1}[X^{\star} \leq 0]$. With such misclassification, the conventional estimator $\tilde{\tau}$ captures an unidentified weighted average of eligible and ineligible households, whereas our bounds rely only on certainly eligible households (where $u < 0$) and certainly ineligible households (where $\ell > 0$), eliminating misclassification bias entirely. Second, we replace the strong continuity assumption required for point identification with weaker shape restrictions. Assumptions 2 and 3 impose only monotonicity and bounded rates of change, conditions that are both theoretically motivated and empirically supported by the smooth evolution of socioeconomic characteristics in our setting (Figure 8). Third, our bounds avoid reliance on localized comparisons and local randomization near the eligibility threshold, where measurement error and potential manipulation pose the greatest threat.

Applying this framework to WIC and infant feeding practices yields substantive findings that illustrate the method's utility. For breastfeeding initiation, our second-order polynomial bounds consistently exclude zero across most bandwidths, identifying reductions of at least 3 percentage points. These bounds remain robust when we vary the slope constraint κ or adopt alternative functional forms. For breastfeeding duration, we find even stronger evidence of negative effects, with bounds implying reductions of 2 to 10 weeks. Notably, our bounds encompass most conventional RDD point estimates, revealing the substantial uncertainty that point identification masks.

The theoretical foundation of our approach—formalized in Section 3—provides transparent guidance on the strength of identification. Tighter bounds emerge from stronger restrictions on κ and λ, the maximum rates at which outcomes and treatment can change with income. This transparency contrasts with conventional RDD approaches where researchers choose bandwidths and polynomial orders without clear guidance on how these choices affect the validity of the underlying assumptions. Our framework makes explicit that identification strength depends on the plausibility of the imposed shape and monotonicity restrictions, allowing researchers to assess the robustness of conclusions to alternative assumptions. The trade-off is that bounds may be wide under more conservative rate-of-change assumptions. However, by excluding observations near the threshold and extrapolating from manipulation free regions, our approach is more robust to sorting than conventional RDD, which relies on local randomization exactly where manipulation incentives are strongest. For the broad class of empirical settings with rounding in the running variable, our partial identification approach provides valid inference in the presence of misclassified treatment assignment.

6 Conclusion

This paper develops a partial-identification regression discontinuity design (RDD) for settings with rounding in the running variable. Our approach uses shape restrictions on conditional expectations to bound the local average treatment effect (LATE) without requiring local continuity, random assignment near the threshold, or precise measurement of the running variable. The framework's core assumptions—monotonicity and bounded rates of change—are derived from economic theory and empirical patterns, allowing flexible application to a broad class of settings with data limitations or sorting behavior.

Applying this framework to WIC participation and infant feeding practices, we find evidence that program participation reduces both the initiation and duration of breastfeeding. These findings suggest that despite WIC's substantial investments in breastfeeding promotion through counseling and peer support programs, the in-kind transfer of infant formula creates stronger countervailing incentives. While our partial-identification approach cannot pinpoint precise effect magnitudes, it provides policymakers with credible bounds on the trade-offs inherent in WIC's current benefit structure.

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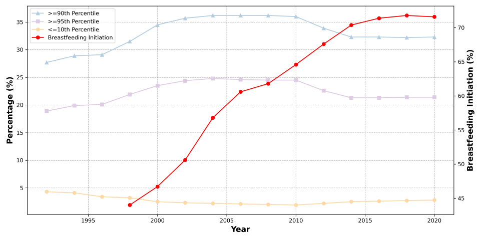
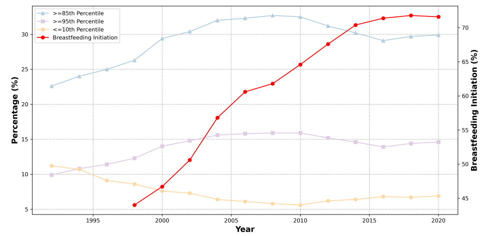
A Figures

Figure 1: Breastfeeding Initiation Rates and Participant Weight Distributions

Panel A: Children

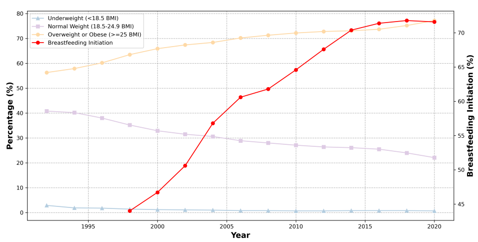


(a) 1-year-old Children

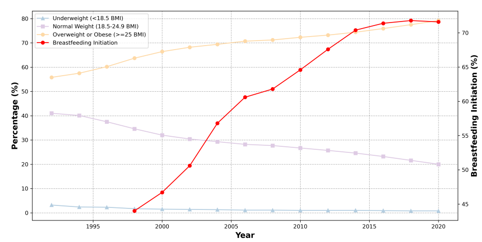


(b) 2- to 4-year-old Children

Panel B: Women



(c) Breastfeeding Women

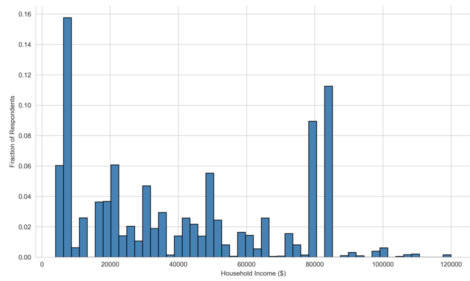


(d) Postpartum Women

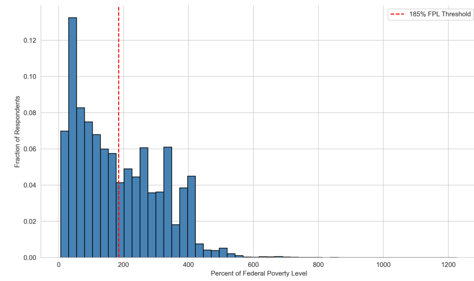
Notes: This figure presents breastfeeding initiation rates and the distribution of growth measurements for WIC participating women, infants, and children using data from the WIC Participant and Program Characteristics 2020 Appendix. Panel A shows anthropometric distributions for children: Subfigure (a) displays the distribution of 1-year-old children by standardized WHO Weight-for-Length percentiles; Subfigure (b) shows the distribution of 2- to 4-year-old children by standardized WHO BMI percentiles. Panel B presents maternal anthropometrics: Subfigure (c) displays the BMI distribution for breastfeeding women (those with fully or partially breastfeeding infants); Subfigure (d) shows BMI distribution for postpartum women without breastfeeding infants. All figures include breastfeeding initiation rates for participating 7- to 11-month-old infants. Percentiles above the 85th represent overweight and above the 95th represent obesity according to CDC standards.

Figure 2: Breastfeeding Behaviors by Household Income (%FPL)

Panel A: Distribution of Household Income

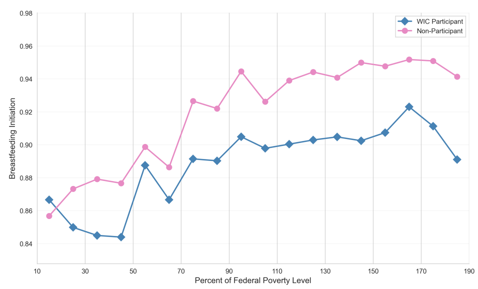


(a) Dollars ($)

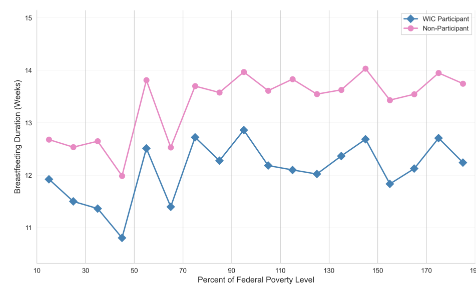


(b) Percentage of FPL (%FPL)

Panel B: Initiation and Duration of Breastfeeding



(c) Initiation

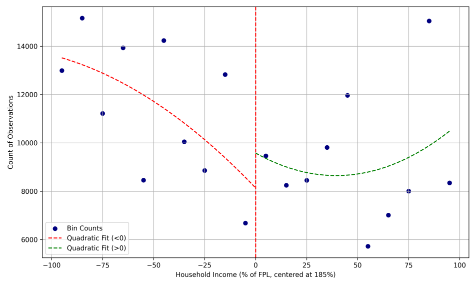


(d) Duration

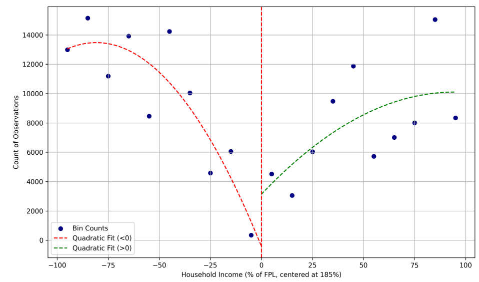
Notes: This figure presents the distribution income and infant feeding practice by income in the Pregnancy Risk Assessment Monitoring System (PRAMS). Panel A presents the distribution of household income—the midpoint of observed brackets as proxy—in dollars and as a percentage of the Federal Poverty Level (FPL). The vertical dashed line indicates the WIC eligibility threshold, set nationally at 185% FPL. Panel B displays breastfeeding initiation rates and duration by household income for WIC participants (blue diamonds) and income-eligible non-participants (pink circles). Duration is conditional on breastfeeding initiation.

Figure 3: Binned Scatter of Observation Count by FPL Bin

Panel A: 20 Bins (Width of 10 pct)

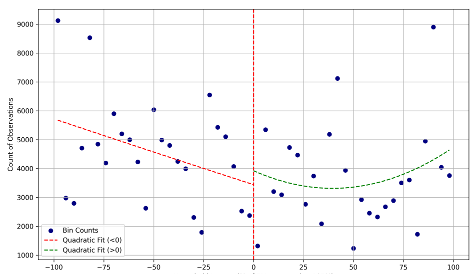


(a) Including indeterminate Assignment

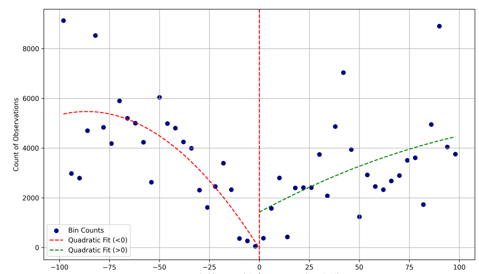


(b) Excluding indeterminate Assignment

Panel B: 50 Bins (Width of 4 pct)

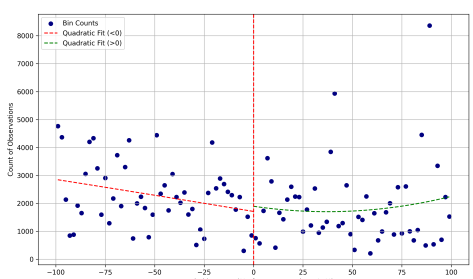


(c) Including indeterminate Assignment

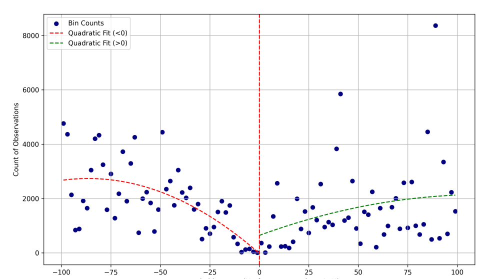


(d) Excluding indeterminate Assignment

Panel C: 100 Bins (Width of 2 pct)



(e) Including indeterminate Assignment

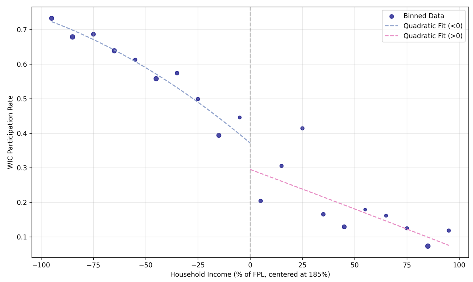


(f) Excluding indeterminate Assignment

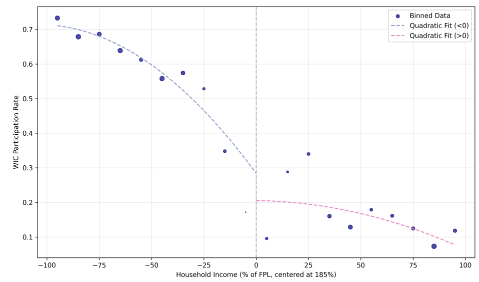
Notes: This figure displays the distribution of observations by household income, as a percentage of the Federal Poverty Level (FPL), in the Pregnancy Risk Assessment Monitoring System (PRAMS). Observations are assigned to particular FPL bins using the midpoint of their observed income bracket $(\text{FPL}_\ell + \text{FPL}_u)/2$. Household income, as a percentage of FPL, is centered at the WIC income eligibility threshold of 185%, which is marked vertically in red. Panels vary by bin width: (A) 10 percentage points, (B) 4 percentage points, and (C) 2 percentage points. Left figures include all observations; right figures exclude households with brackets straddling the threshold ($\text{FPL}_\ell \leq 185 < \text{FPL}_u$). Fitted curves show quadratic approximations.

Figure 4: Binned Scatter of WIC Participation by FPL Bin

Panel A: 20 Bins (Width of 10 pct)

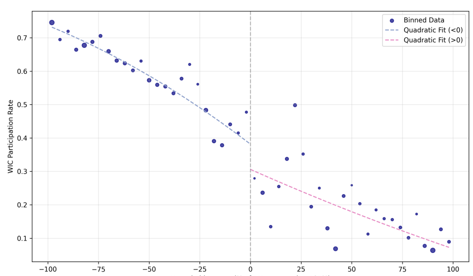


(a) Including indeterminate Assignment



(b) Excluding indeterminate Assignment

Panel B: 50 Bins (Width of 4 pct)



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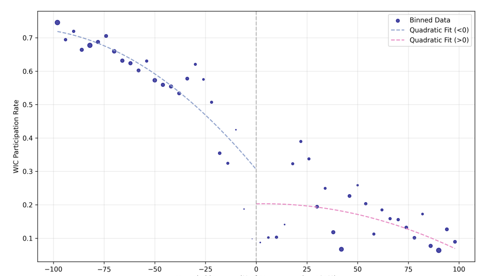
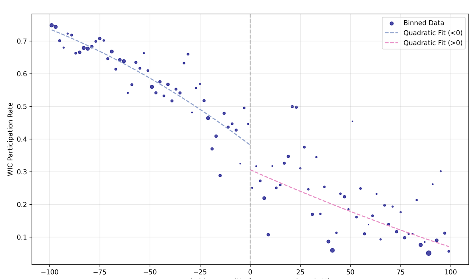


(d) Excluding indeterminate Assignment

Panel C: 100 Bins (Width of 2 pct)



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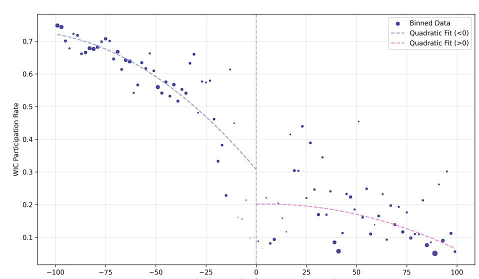
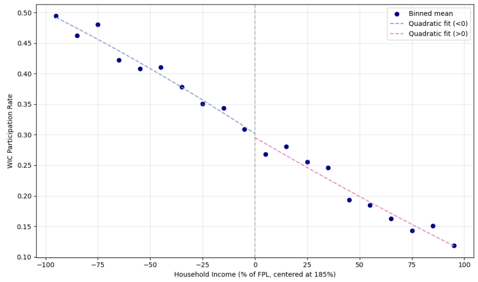


(f) Excluding indeterminate Assignment

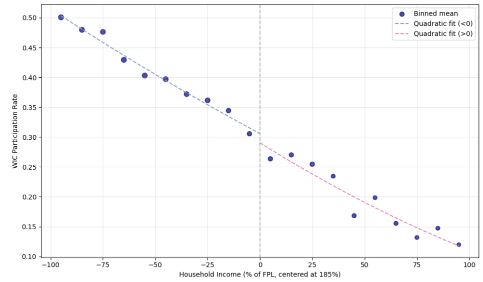
Notes: This figure displays the distribution of participation in WIC by household income, as a percentage of the Federal Poverty Level (FPL), in the Pregnancy Risk Assessment Monitoring System (PRAMS). Observations are assigned to particular FPL bins using the midpoint of their observed income bracket $(\text{FPL}_\ell + \text{FPL}_u)/2$. Household income, as a percentage of FPL, is centered at the WIC income eligibility threshold of 185%, which is marked vertically in red. Panels vary by bin width: (A) 10 percentage points, (B) 4 percentage points, and (C) 2 percentage points. Left figures include all observations; right figures exclude households with brackets straddling the threshold $(\text{FPL}_\ell \leq 185 < \text{FPL}_u)$. Fitted curves show quadratic approximations.

Figure 5: Binned Scatter of WIC Participation by FPL Bin: CPS Data

Panel A: 20 Bins (Width of 10 pct)

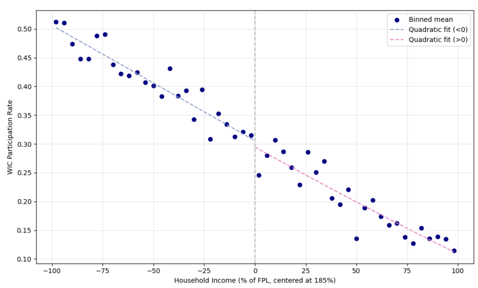


(a) Unweighted

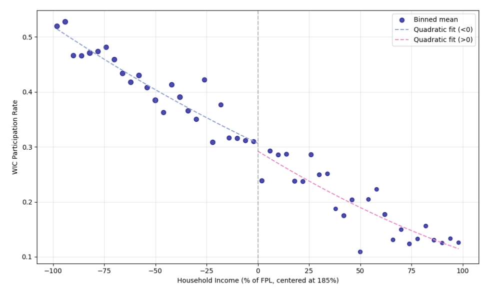


(b) Weighted

Panel B: 50 Bins (Width of 4 pct)

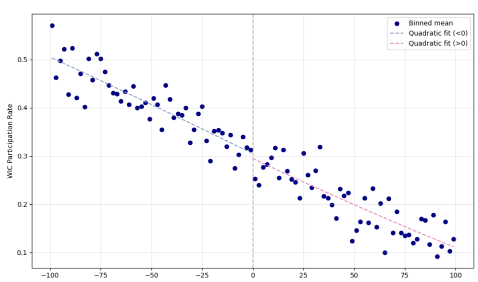


(c) Unweighted

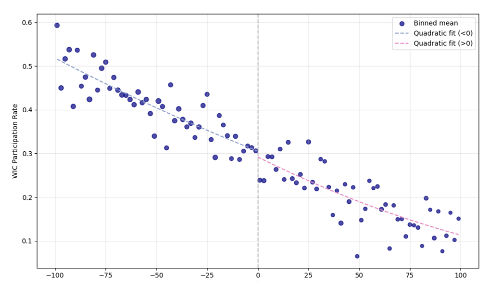


(d) Weighted

Panel C: 100 Bins (Width of 2 pct)



(e) Unweighted

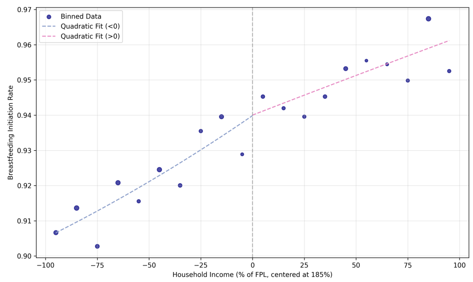


(f) Weighted

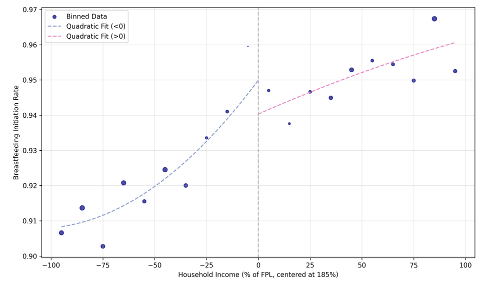
Notes: This figure displays the distribution of participation in WIC by household income, as a percentage of the Federal Poverty Level (FPL), in the Current Population Survey Annual Social and Economic Supplement (CPS-ASEC). Observations are assigned to particular FPL bins using point-identified household income. Household income, as a percentage of FPL, is centered at the WIC income eligibility threshold of 185%, which is marked vertically in red. Panels vary by bin width: (A) 10 percentage points, (B) 4 percentage points, and (C) 2 percentage points. Left figures show unweighted quadratic fits; right figures show quadratic fits weighted by bin size.

Figure 6: Binned Scatter of Breastfeeding Initiation by FPL Bin

Panel A: 20 Bins (Width of 10 pct)

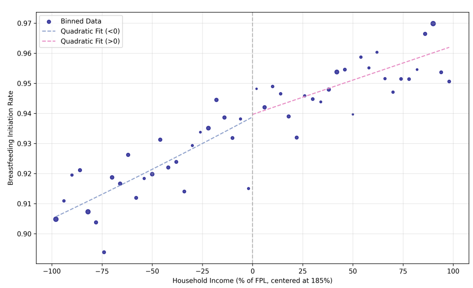


(a) Including indeterminate Assignment



(b) Excluding indeterminate Assignment

Panel B: 50 Bins (Width of 4 pct)



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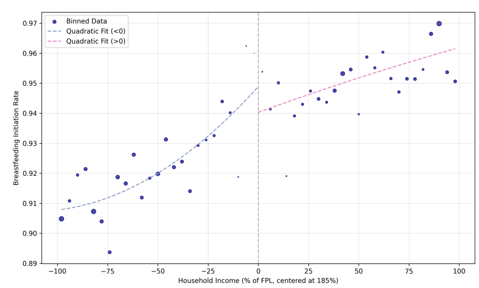
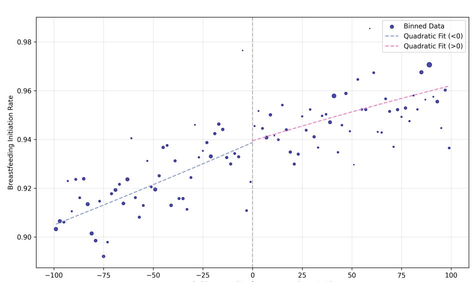
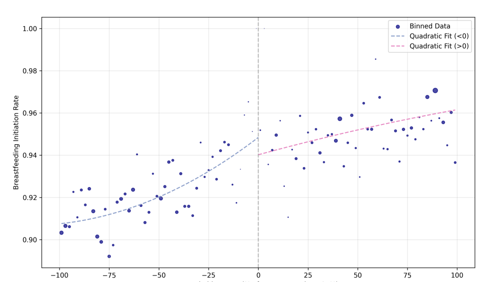


(d) Excluding indeterminate Assignment

Panel C: 100 Bins (Width of 2 pct)



(e) Including indeterminate Assignment

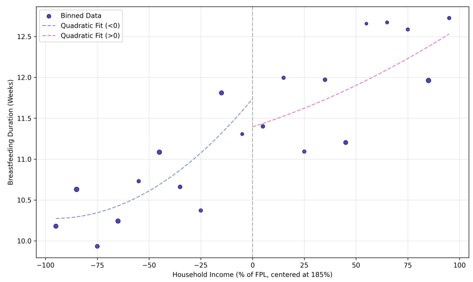


(f) Excluding indeterminate Assignment

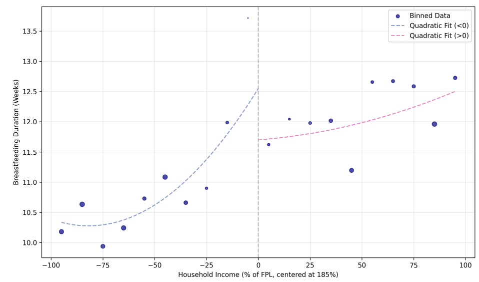
Notes: This figure displays the distribution of breastfeeding initiation by household income, as a percentage of the Federal Poverty Level (FPL), in the Pregnancy Risk Assessment Monitoring System (PRAMS). Observations are assigned to particular FPL bins using the midpoint of their observed income bracket $(\text{FPL}_\ell + \text{FPL}_u)/2$. Household income, as a percentage of FPL, is centered at the WIC income eligibility threshold of 185%, which is marked vertically in red. Panels vary by bin width: (A) 10 percentage points, (B) 4 percentage points, and (C) 2 percentage points. Left figures include all observations; right figures exclude households with brackets straddling the threshold $(\text{FPL}_\ell \leq 185 < \text{FPL}_u)$. Fitted curves show quadratic approximations.

Figure 7: Binned Scatter of Breastfeeding Duration by FPL Bin

Panel A: 20 Bins (Width of 10 pct)

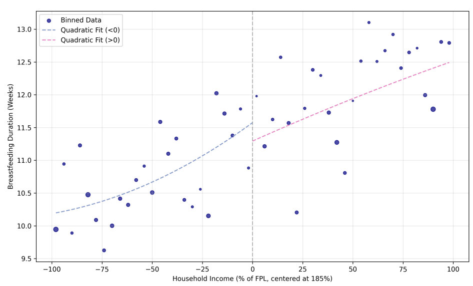


(a) Including indeterminate Assignment

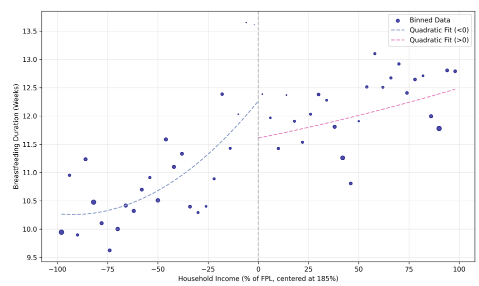


(b) Excluding indeterminate Assignment

Panel B: 50 Bins (Width of 4 pct)



(c) Including indeterminate Assignment



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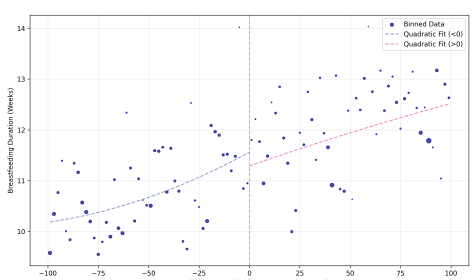
Panel C: 100 Bins (Width of 2 pct)



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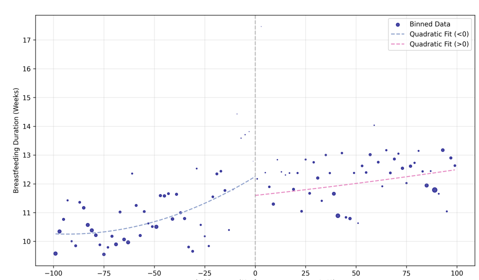
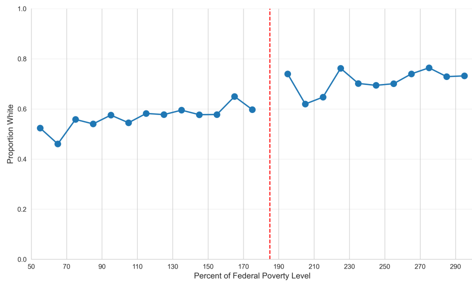


(f) Excluding indeterminate Assignment

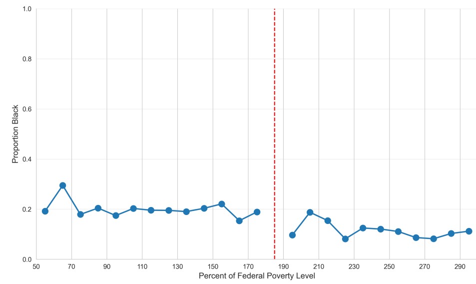
Notes: This figure displays the distribution of breastfeeding duration by household income, as a percentage of the Federal Poverty Level (FPL), in the Pregnancy Risk Assessment Monitoring System (PRAMS). Observations are assigned to particular FPL bins using the midpoint of their observed income bracket $(\text{FPL}_\ell + \text{FPL}_u)/2$. Household income, as a percentage of FPL, is centered at the WIC income eligibility threshold of 185%, which is marked vertically in red. Panels vary by bin width: (A) 10 percentage points, (B) 4 percentage points, and (C) 2 percentage points. Left figures include all observations; right figures exclude households with brackets straddling the threshold $(\text{FPL}_\ell \leq 185 < \text{FPL}_u)$. Fitted curves show quadratic approximations.

Figure 8: Maternal Characteristics by Household Income

Panel A: Race/Ethnicity

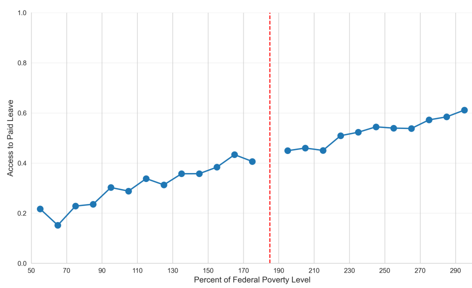


(a) White

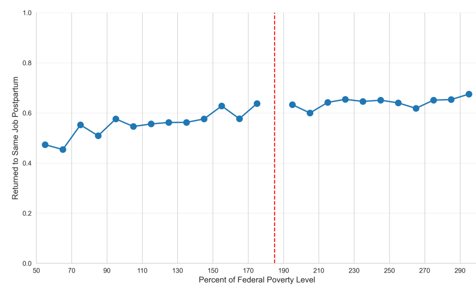


(b) Black

Panel B: Employment and Leave

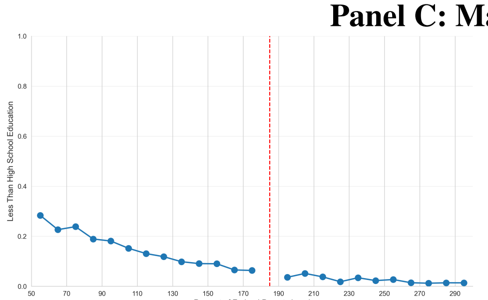


(c) Access to Paid Leave

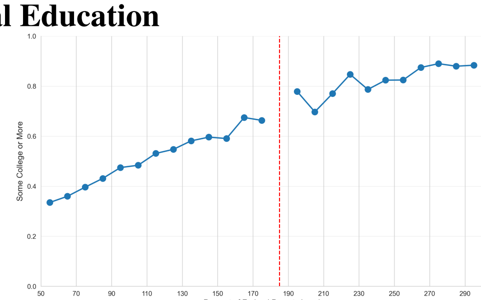


(d) Returned to Job Postpartum

Panel C: Maternal Education



(e) Less than High School

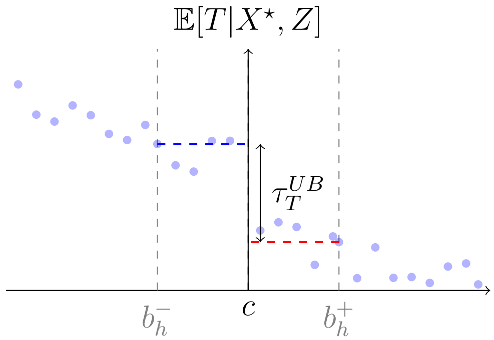


(f) Some College or More

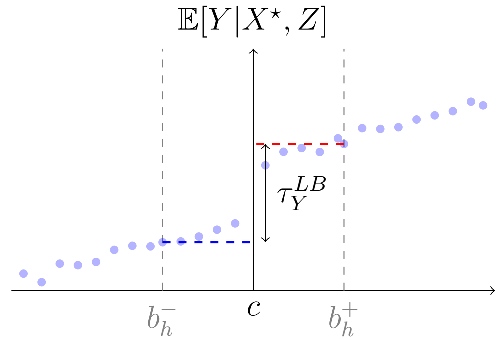
Notes: This figure shows how selected maternal characteristics evolve with household income, expressed as a percentage of the Federal Poverty Level (FPL). Each subplot presents binned proportions for a binary variable by FPL bracket using PRAMS data. The vertical dashed red line denotes the WIC income eligibility threshold at 185% FPL. The bin spanning 180–190% is excluded to avoid overlap with the threshold, and the plotted lines are segmented accordingly. All variables are defined as binary indicators and plotted as proportions within each income bin.

Figure 9: Partial Identification Bounds

Panel A: Monotonicity Assumptions

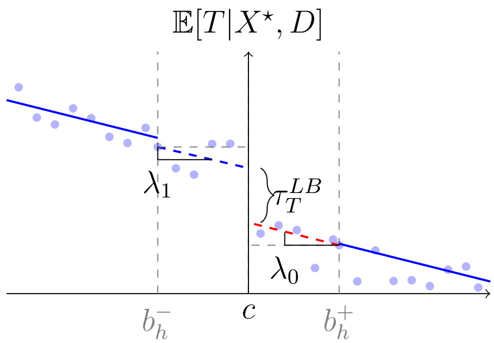


(a) Decreasing in Income

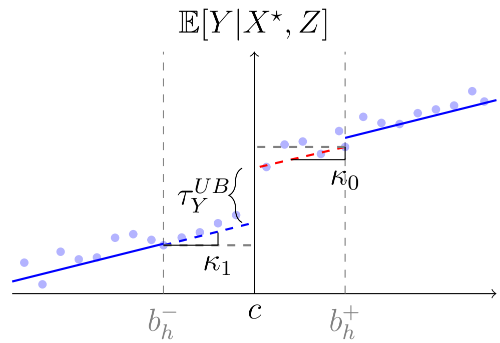


(b) Increasing in Income

Panel B: Bounded Slope Assumptions

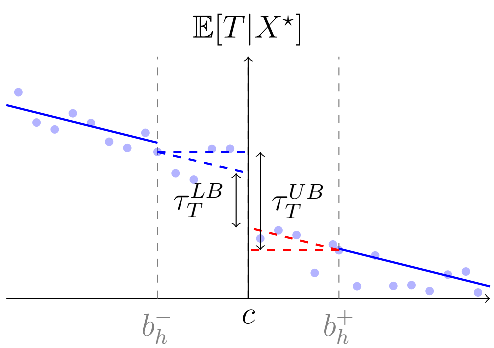


(c) Decreasing in Income

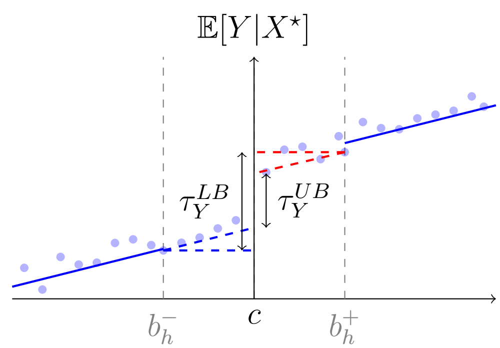


(d) Increasing in Income

Panel C: Combined



(e) Decreasing in Income

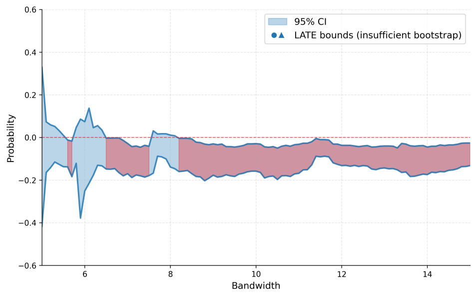


(f) Increasing in Income

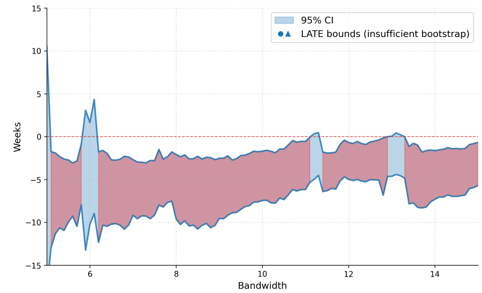
Notes: This stylized figure illustrates the bounds on discontinuities τ_T for WIC participation T and τ_Y for breastfeeding outcomes Y at the income eligibility threshold imposed by partial-identification Assumptions 2 and 3. The horizontal axis shows household income as a percentage of FPL. Panel A: Monotonicity bounds—upper bound (τ_T^{UB}) for WIC participation, lower bound (τ_Y^{LB}) for breastfeeding outcomes. Panel B: Bounded rate of change constraints—lower bound (τ_T^{LB}) for WIC participation, upper bound (τ_Y^{UB}) for breastfeeding outcomes. Panel C: Combined bounds, showing the cone of uncertainty as income approaches the threshold.

Figure 10: LATE Bounds from Partial Identification RDD: Mean Slope Projection

Panel A: Slope Multiplier - 1.0

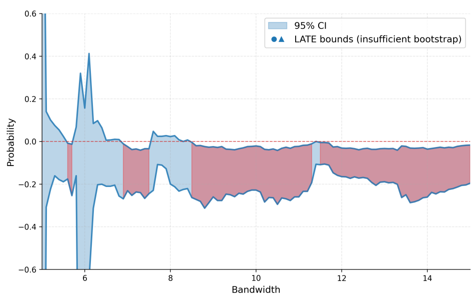


(a) Breastfeeding Initiation

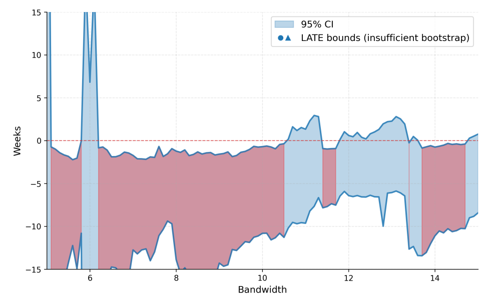


(b) Breastfeeding Duration (weeks)

Panel B: Slope Multiplier - 1.5

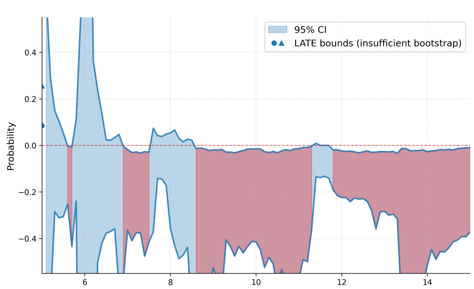


(c) Breastfeeding Initiation

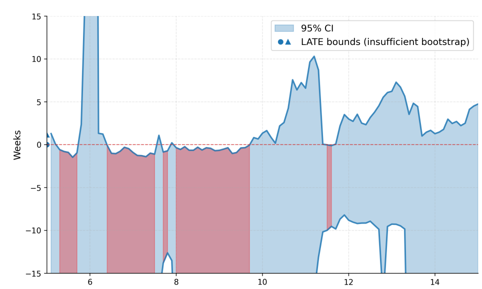


(d) Breastfeeding Duration (weeks)

Panel C: Slope Multiplier - 2.0



(e) Breastfeeding Initiation

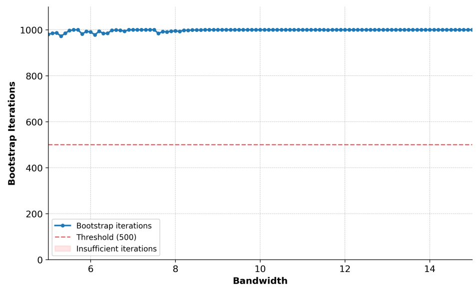


(f) Breastfeeding Duration (weeks)

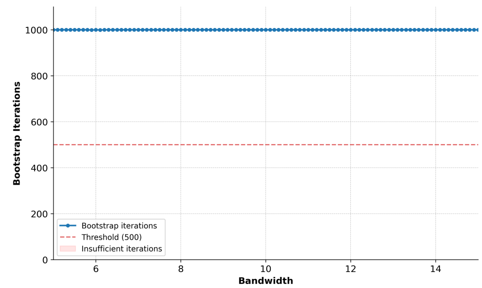
Notes: This figure reports bounds on the local average treatment effect (LATE) of WIC participation on breastfeeding outcomes. Estimates are derived using the partial-identification regression discontinuity design (RDD) and Assumptions 1–3. The bounded rate of change (Assumption 3) is computed using a linearized slope based on the average derivative of the conditional expectation function across the projection interval. Panels A–C correspond to slope multipliers $\{1.0, 1.5, 2.0\}$, with all estimates based on the polynomial model selected by the Akaike Information Criterion (AIC). For bandwidths with at least 500 successful bootstrap replications, shaded regions indicate 95% confidence intervals from the 2.5th to 97.5th percentiles of the bootstrap distribution. Red shaded areas indicate statistically significant and strictly negative bounds. At bandwidths with fewer than 500 valid replications, only the upper and lower bound estimates of LATE are displayed. See Section 4 for estimation details.

Figure 11: Successful Bootstrap Iterations by FPL Bandwidth

Panel A: Slope Multiplier - 1.0

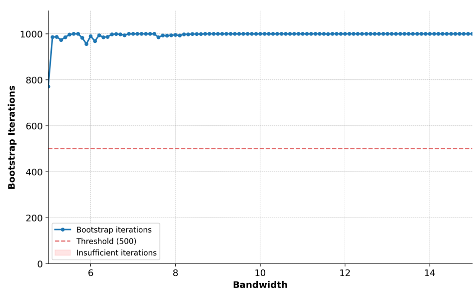


(a) Breastfeeding Initiation

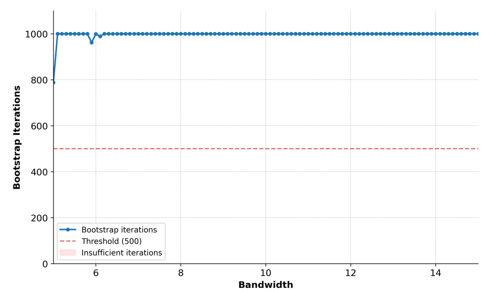


(b) Breastfeeding Duration (weeks)

Panel B: Slope Multiplier - 1.5

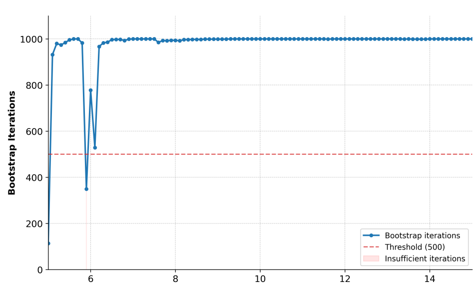


(c) Breastfeeding Initiation

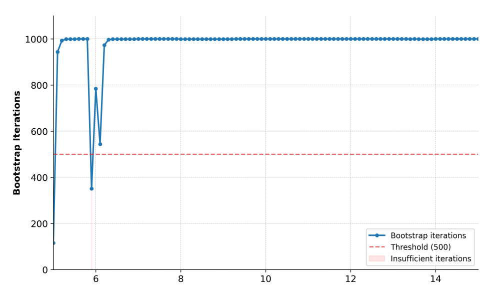


(d) Breastfeeding Duration (weeks)

Panel C: Slope Multiplier - 2.0



(e) Breastfeeding Initiation

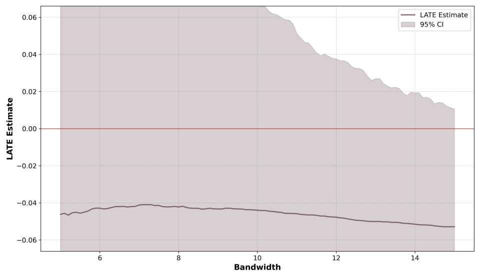


(f) Breastfeeding Duration (weeks)

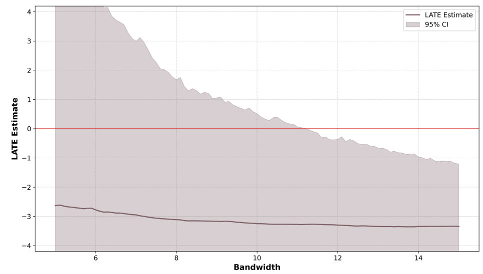
Notes: This figure reports the count of valid bounds on the local average treatment effect (LATE) from 1000 bootstrap iterations of the partial-identification regression discontinuity design (RDD). A bootstrap draw is excluded if the estimated first-stage lower bound fails to exceed zero, violating Assumption 1. Panels A–C correspond to slope multipliers $\{1.0, 1.5, 2.0\}$, with all estimates based on the polynomial model selected by the Akaike Information Criterion (AIC).

Figure 12: Sensitivity Testing: Simulation RDD

Panel A: LATE Estimates

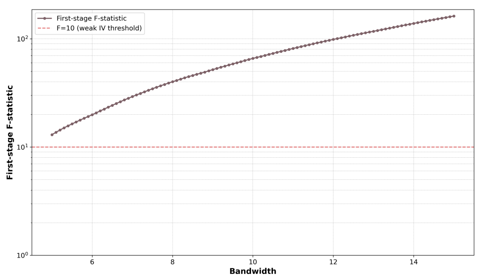


(a) Breastfeeding Initiation

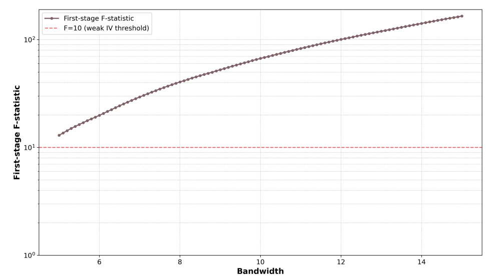


(b) Breastfeeding Duration (weeks)

Panel B: First-Stage F-Statistics



(c) Breastfeeding Initiation

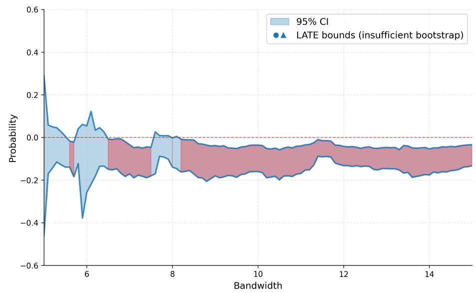


(d) Breastfeeding Duration (weeks)

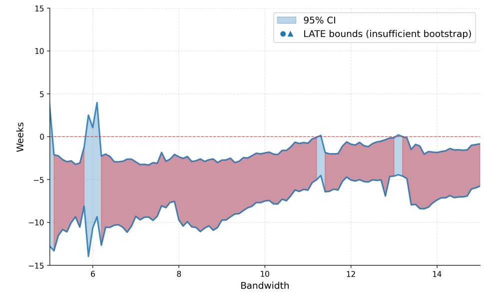
Notes: This figure presents local average treatment effect estimates (τ) from equation 6 and associated first-stage F-statistics from a simulation-based RDD. Household income is simulated using a uniform distribution: $X^{\star}_{i,s} \sim \text{Uniform}(\ell_i, u_i)$. For each bandwidth $h \in [0.5, 10.05]$, the optimal polynomial order is selected using the Akaike Information Criterion (AIC). Panel A displays displays LATE estimates, with 95% confidence intervals indicated via shaded areas. Panel B presents first-stage F-statistics, with the conventional weak instrument threshold, F=10, indicated in red on the vertical axis. See Section 4 for estimation details.

Figure 13: LATE Bounds from Partial Identification RDD: Reference Bin Slope

Panel A: Slope Multiplier - 1.0

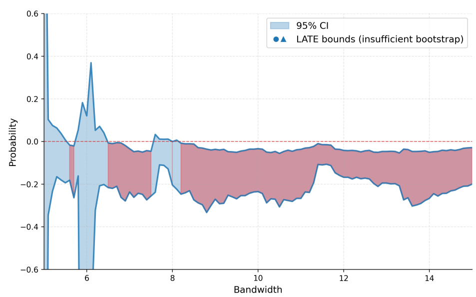


(a) Breastfeeding Initiation

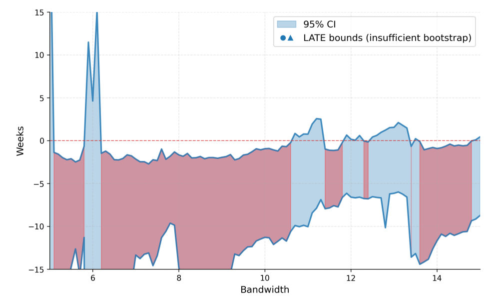


(b) Breastfeeding Duration (weeks)

Panel B: Slope Multiplier - 1.5

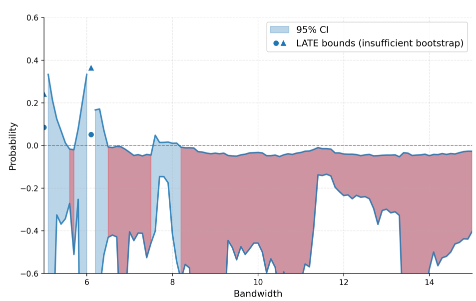


(c) Breastfeeding Initiation

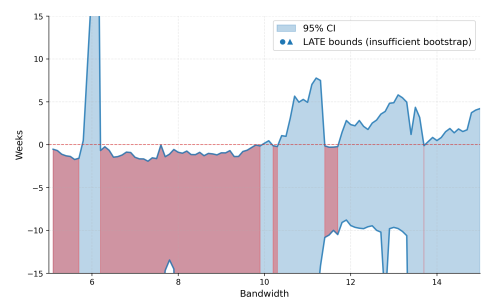


(d) Breastfeeding Duration (weeks)

Panel C: Slope Multiplier - 2.0



(e) Breastfeeding Initiation

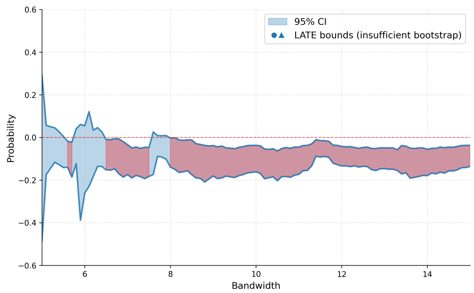


(f) Breastfeeding Duration (weeks)

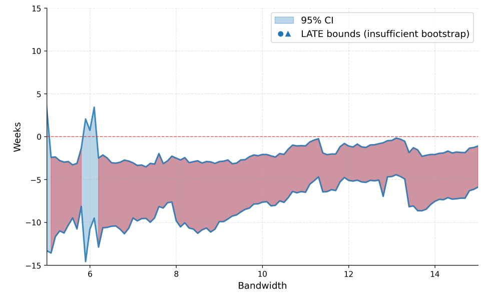
Notes: This figure reports bounds on the local average treatment effect (LATE) of WIC participation on breastfeeding outcomes. Estimates are derived using the partial-identification regression discontinuity design (RDD) and Assumptions 1–3. The bounded rate of change (Assumption 3) is estimated using a linearized slope of the conditional expectation function, computed at the midpoint of the reference bin. Panels A–C correspond to slope multipliers $\{1.0, 1.5, 2.0\}$, with all estimates based on the polynomial model selected by the Akaike Information Criterion (AIC). For bandwidths with at least 500 successful bootstrap replications, shaded regions indicate 95% confidence intervals from the 2.5th to 97.5th percentiles of the bootstrap distribution. Red shaded areas indicate statistically significant and strictly negative bounds. At bandwidths with fewer than 500 valid replications, only the upper and lower bound estimates of LATE are displayed. See Section 4 for estimation details.

Figure 14: LATE Bounds from Partial Identification RDD: Nonlinear Projection

Panel A: Slope Multiplier - 1.0

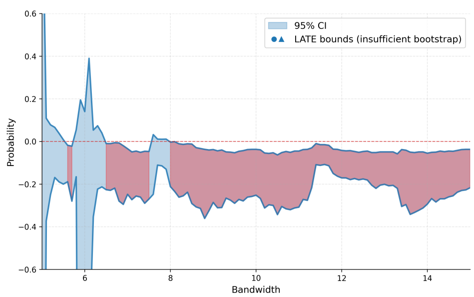


(a) Breastfeeding Initiation

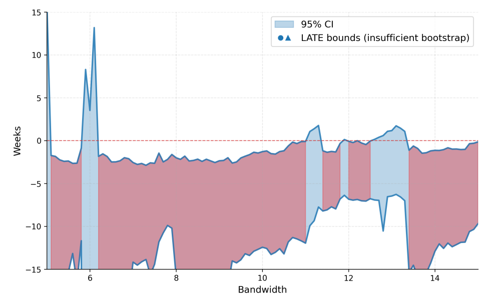


(b) Breastfeeding Duration (weeks)

Panel B: Slope Multiplier - 1.5

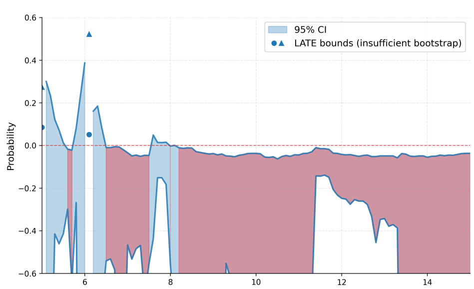


(c) Breastfeeding Initiation

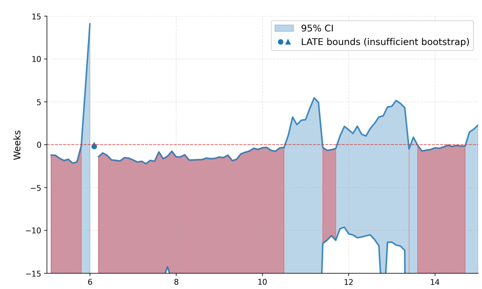


(d) Breastfeeding Duration (weeks)

Panel C: Slope Multiplier - 2.0



(e) Breastfeeding Initiation

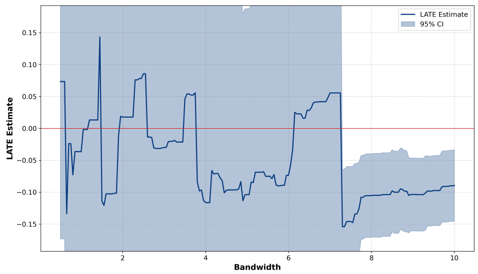


(f) Breastfeeding Duration (weeks)

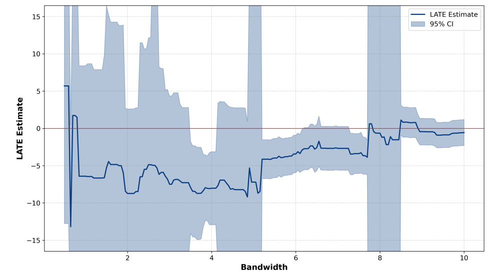
Notes: This figure reports bounds on the local average treatment effect (LATE) of WIC participation on breastfeeding outcomes. Estimates are derived using the partial-identification regression discontinuity design (RDD) and Assumptions 1–3. The bounded rate of change (Assumption 3) is estimated using a nonlinear projection of the estimated conditional expectation function. Panels A–C correspond to slope multipliers $\{1.0, 1.5, 2.0\}$, with all estimates based on the polynomial model selected by the Akaike Information Criterion (AIC). For bandwidths with at least 500 successful bootstrap replications, shaded regions indicate 95% confidence intervals from the 2.5th to 97.5th percentiles of the bootstrap distribution. Red shaded areas indicate statistically significant and strictly negative bounds. At bandwidths with fewer than 500 valid replications, only the upper and lower bound estimates of LATE are displayed. See Section 4 for estimation details.

Figure 15: LATE Point-Estimates by FPL Bandwidth

Panel A: Traditional Regression Discontinuity

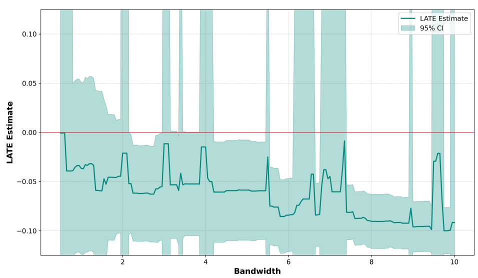


(a) Breastfeeding Initiation

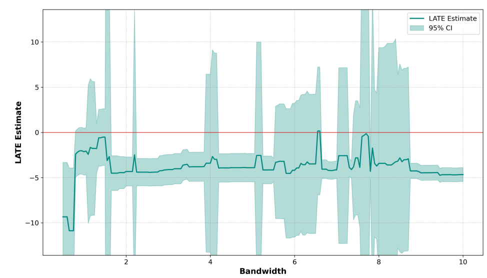


(b) Breastfeeding Duration (weeks)

Panel B: Doughnut Regression Discontinuity



(c) Breastfeeding Initiation

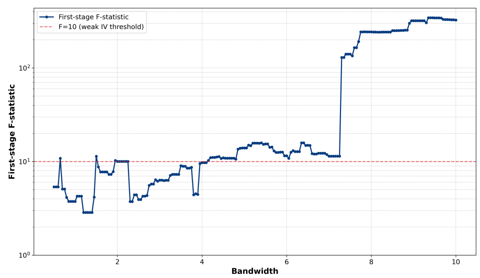


(d) Breastfeeding Duration (weeks)

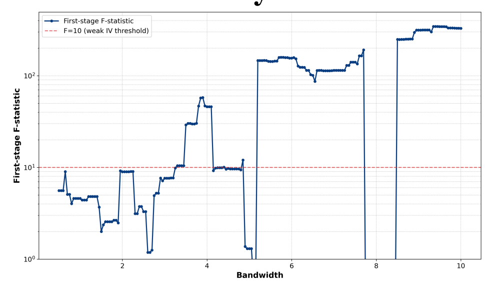
Notes: This figure presents local average treatment effect estimates (τ) from equation 6 using data from the Pregnancy Risk Assessment Monitoring System (PRAMS). For each bandwidth $h \in [0.5, 10.05]$, the optimal polynomial order is selected using the Akaike Information Criterion (AIC). Panel A displays results from a traditional fuzzy regression discontinuity design (RDD); Panel B displays results from a doughnut RDD, excluding observations within $\mu = \frac{1}{N}\sum_{i=1}^{N}(u_i - \ell_i)$ of the income eligibility threshold. Left figures show effects on breastfeeding initiation (a coefficient estimate of -0.1 represents a 10 percentage point decline in probability); right figures show effects on duration (weeks). Shaded areas indicate 95% confidence intervals. See Section 4 for estimation details.

Figure 16: First Stage F-Stat by FPL Bandwidth

Panel A: Traditional Regression Discontinuity

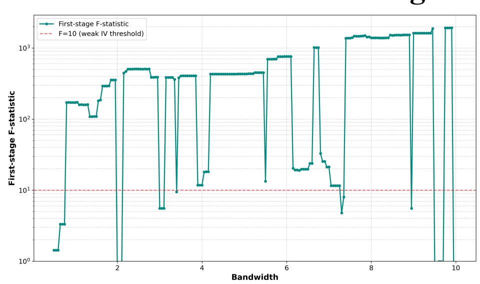


(a) Breastfeeding Initiation

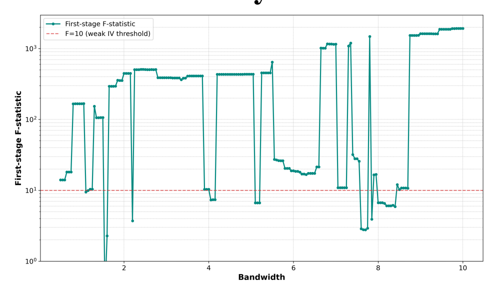


(b) Breastfeeding Duration (weeks)

Panel B: Doughnut Regression Discontinuity



(c) Breastfeeding Initiation



(d) Breastfeeding Duration (weeks)

Notes: This figure presents first-stage F-statistics for the instrumental variables regression of WIC participation on income eligibility using data from the Pregnancy Risk Assessment Monitoring System (PRAMS). For each bandwidth $h \in [0.5, 10.05]$, the optimal polynomial order is selected using the Akaike Information Criterion (AIC). Panel A displays results from a traditional fuzzy regression discontinuity design (RDD); Panel B displays results from a doughnut RDD, excluding observations within $\mu = \frac{1}{N}\sum_{i=1}^{N}(u_i - \ell_i)$ of the income eligibility threshold. The conventional weak instrument threshold, F=10, is indicated in red on the vertical axis. See Section 4 for estimation details.

B Tables

Table 1: Maximum Monthly Allowances of Supplemental Foods for Infants, Children, and Women

Panel A: Infants						
Foods	Fully Formula Fed Food Packages I and III A: 0–3 mo. B: 4–5 mo.	Fully Formula Fed Food Packages II and III 6–11 mo.	Partially Breastfed Food Packages I and III A: 0–3 mo. B: 4–5 mo.	Partially Breastfed Food Packages II and III 6–11 mo.	Fully Breastfed Food Package I 0–5 mo.	Fully Breastfed Food Package II 6–11 mo.
WIC Formula	A: Up to 806 fl. oz. B: Up to 884 fl. oz.	Up to 624 fl. oz.	A: Up to 364 fl. oz. B: Up to 442 fl. oz.	Up to 312 fl. oz.	N/A	N/A
Infant cereal	N/A	8 oz.	N/A	8 oz.	N/A	16 oz.
Baby food fruits and vegetables	N/A	128 oz.	N/A	128 oz.	N/A	128 oz.
Baby food meat	N/A	N/A	N/A	N/A	N/A	40 oz.
Panel B: Children and Women						
Foods	Children Food Package IV A: 12–23 mo.	Children Food Package IV B: 2–4 yrs	Women Food Package V Pregnant / Mostly BF	Women Food Package VI Postpartum	Women Food Package VII Fully Breastfeeding	
Juice, single strength	64 fl. oz.	64 fl. oz.	64 fl. oz.	64 fl. oz.	64 fl. oz.	64 fl. oz.
Milk	A: 12 qt. B: 14 qt.	14 qt.	16 qt.	16 qt.	16 qt.	16 qt.
Breakfast cereal	36 oz.	36 oz.	36 oz.	36 oz.	36 oz.	36 oz.
Eggs	1 dozen	1 dozen	1 dozen	1 dozen	2 dozen	2 dozen
Fruit and vegetable CVB	$26.00	A: $47.00 B: $52.00	$47.00	$47.00	$52.00	$52.00
Whole wheat bread	24 oz.	48 oz.	48 oz.	48 oz.	48 oz.	48 oz.
Fish (canned)	6 oz.	A: 10 oz. B: 15 oz.	10 oz.	10 oz.	20 oz.	20 oz.
Legumes and/or Peanut Butter	1 lb. dry / 64 oz. canned or 18 oz. PB	1 lb. / 64 oz. canned and 18 oz. PB	1 lb. / 64 oz. canned or 18 oz. PB	1 lb. / 64 oz. canned or 18 oz. PB	1 lb. / 64 oz. canned and 18 oz. PB	1 lb. / 64 oz. canned and 18 oz. PB

Notes: This table summarizes the maximum monthly quantities of WIC-provided supplemental foods by age and participant category. Panel A presents allowances for infants based on age and breastfeeding status across Food Packages I, II, and III. Panel B presents allowances for children and women across Food Packages IV–VII. All values reflect monthly values under current federal guidelines.

Table 2: Summary Statistics by WIC Participation

Variable	WIC Mean	WIC Obs	WIC SD	Non-WIC Mean	Non-WIC Obs	Non-WIC SD	Diff (P-value)
Infant Feeding Practices							
Breastfeeding Initiation	0.88	163,328	0.33	0.92	68,753	0.27	-0.05 (0.00)
Breastfeeding Duration - Any	11.84	124,245	7.15	13.38	57,495	6.80	-1.54 (0.00)
Breastfeeding Duration - Exclusive	11.52	53,987	6.71	12.68	22,315	6.37	-1.17 (0.00)
Formula Duration - Any	44.95	88,110	7.64	43.35	32,709	7.91	1.60 (0.00)
Formula Duration - Exclusive	43.20	167,006	8.03	40.83	68,900	7.96	2.36 (0.00)
Infant and Maternal Health							
Maternal BMI	28.14	143,351	7.48	27.29	61,881	6.99	0.86 (0.00)
Maternal Diabetes	0.07	173,453	0.25	0.06	71,794	0.24	0.00 (0.01)
Preterm Labor	0.29	63,868	0.45	0.25	20,721	0.43	0.04 (0.00)
Infant Birth Weight - Grams	3015	173,804	767	3041	71,900	792	-26 (0.00)
Small for Gestational Age	0.17	167,740	0.38	0.15	69,662	0.36	0.02 (0.00)
Maternal Education and Employment							
Employment Before Pregnancy	0.61	38,123	0.49	0.62	22,667	0.49	-0.00 (0.26)
Employment During Pregnancy	0.56	47,025	0.50	0.61	18,649	0.49	-0.05 (0.00)
Returned to Job After Birth	0.49	21,440	0.50	0.55	9,420	0.50	-0.06 (0.00)
Less than High School	0.05	172,168	0.22	0.03	71,207	0.17	0.02 (0.00)
Some High School	0.19	172,168	0.39	0.10	71,207	0.31	0.08 (0.00)
High School Graduate	0.39	172,168	0.49	0.31	71,207	0.46	0.08 (0.00)
Some College	0.31	172,168	0.46	0.36	71,207	0.48	-0.06 (0.00)
College Graduate or Greater	0.07	172,168	0.26	0.19	71,207	0.40	-0.12 (0.00)
Household Characteristics							
Maternal Marital Status	0.37	173,696	0.48	0.58	71,890	0.49	-0.21 (0.00)
Household Size	3.42	174,079	1.74	3.79	72,054	1.73	-0.37 (0.00)
Household Income - Midpoint	16671	174,079	11404	24748	72,054	14631	-8076 (0.00)
Maternal Race - White	0.49	168,825	0.50	0.60	70,026	0.49	-0.11 (0.00)
Maternal Race - Black	0.26	168,825	0.44	0.17	70,026	0.38	0.09 (0.00)
Maternal Ethnicity - Hispanic	0.25	168,763	0.43	0.16	69,940	0.37	0.09 (0.00)

Notes: This table reports summary statistics for infant–mother pairs in PRAMS (2009–2022), restricted to income-eligible households ($\leq$ 185% FPL). Columns list variable-specific means, sample sizes (Obs), and standard deviations (SD). Differences in means are defined as WIC $-$ Non-WIC; t-test p-values are shown in parentheses. Breastfeeding duration is measured in weeks; exclusive duration is the number of weeks until any non-breastmilk liquid or solids are introduced. These are descriptive associations and should not be interpreted causally.

Table 3: Summary Statistics by FPL Interval

Variable	Bounds Overlap 185%			Bounds Do Not Overlap			
	Mean	Obs	SD	Mean	Obs	SD	Diff (P-value)
Infant Feeding Practices							
Breastfeeding Initiation	0.93	16,773	0.25	0.89	216,308	0.32	0.05 (0.000)
Breastfeeding Duration - Any	12.97	14,348	6.74	12.27	168,178	7.11	0.70 (0.000)
Breastfeeding Duration - Exclusive	12.36	5,987	6.47	11.81	70,515	6.65	0.55 (0.000)
Formula Duration - Any	43.78	8,985	7.69	44.57	112,145	7.74	-0.80 (0.000)
Formula Duration - Exclusive	41.00	16,899	7.68	42.62	220,047	8.10	-1.62 (0.000)
Infant and Maternal Health							
Maternal BMI	27.61	14,402	7.05	27.91	191,553	7.37	-0.30 (0.000)
Maternal Diabetes	0.06	17,376	0.24	0.07	228,983	0.25	-0.00 (0.319)
Preterm Labor	0.23	6,421	0.42	0.28	78,417	0.45	-0.06 (0.000)
Infant Birth Weight - Grams	3084	17,400	777.50	3018	229,425	774.49	66 (0.000)
Small for Gestational Age	0.15	16,761	0.35	0.17	221,718	0.37	-0.02 (0.000)
Maternal Education and Employment							
Employment Before Pregnancy	0.64	5,321	0.48	0.61	55,888	0.49	0.03 (0.000)
Employment During Pregnancy	0.74	4,398	0.44	0.56	61,506	0.50	0.17 (0.000)
Returned to Job After Birth	0.62	2,640	0.49	0.50	28,334	0.50	0.12 (0.000)
Less than High School	0.01	17,268	0.11	0.05	227,117	0.21	-0.03 (0.000)
Some High School	0.05	17,268	0.22	0.17	227,117	0.38	-0.12 (0.000)
High School Graduate	0.27	17,268	0.44	0.37	227,117	0.48	-0.10 (0.000)
Some College	0.42	17,268	0.49	0.31	227,117	0.46	0.10 (0.000)
College Graduate or Greater	0.25	17,268	0.43	0.10	227,117	0.30	0.15 (0.000)
Household Characteristics							
Maternal Marital Status	0.62	17,405	0.49	0.41	229,293	0.49	0.20 (0.000)
Household Size	3.41	17,435	1.45	3.54	229,822	1.77	-0.13 (0.000)
Household Income - Midpoint	37455	17,435	10750	17636	229,822	12017	19819 (0.000)
Maternal Race - White	0.62	16,781	0.49	0.51	223,113	0.50	0.10 (0.000)
Maternal Race - Black	0.17	16,781	0.38	0.24	223,113	0.43	-0.07 (0.000)
Maternal Ethnicity - Hispanic	0.15	16,734	0.36	0.23	223,028	0.42	-0.08 (0.000)

Notes: This table compares income-eligible PRAMS respondents (2009–2022) whose reported FPL interval overlaps the 185% FPL eligibility threshold—$[\ell_i, u_i]$ such that $\ell_i \leq 185\% \leq u_i$—to those entirely below or above 185% FPL. Columns list variable-specific means, sample sizes (Obs), and standard deviations (SD). Differences are defined as Overlap $-$ No overlap; t-test p-values are shown in parentheses. Breastfeeding duration is measured in weeks; exclusive duration is the number of weeks until any non-breastmilk liquid or solids are introduced. These are descriptive associations and should not be interpreted causally.

Table 4: Partial Identification Regression Discontinuity LATE Estimates

Bandwidth:	5.0	7.0	9.0	11.0	13.0	15.0
Ref. bins [L/R]	[-30,-25]/[10,15]	[-28,-21]/[28,35]	[-36,-27]/[27,36]	[-33,-22]/[33,44]	[-39,-26]/[26,39]	[-45,-30]/[30,45]
Panel A: Breastfeeding Initiation						
Specification:						
Mean Slope	0.085, 0.135	-0.081, -0.049	-0.126, -0.066	-0.054, -0.033	-0.138, -0.071	-0.093, -0.045
	(-0.418, 0.331)	(-0.170, -0.028)	(-0.177, -0.030)	(-0.168, -0.032)	(-0.143, -0.040)	(-0.132, -0.026)
Bin Slope	0.085, 0.128	-0.083, -0.056	-0.128, -0.079	-0.055, -0.040	-0.141, -0.081	-0.094, -0.057
	(-0.462, 0.291)	(-0.171, -0.034)	(-0.180, -0.038)	(-0.169, -0.040)	(-0.146, -0.047)	(-0.133, -0.034)
Nonlinear Projection	0.085, 0.130	-0.084, -0.056	-0.131, -0.079	-0.055, -0.040	-0.145, -0.081	-0.097, -0.057
	(-0.488, 0.295)	(-0.174, -0.034)	(-0.181, -0.038)	(-0.173, -0.045)	(-0.146, -0.049)	(-0.136, -0.037)
Simulation	-0.046	-0.041	-0.043	-0.046	-0.050	-0.053
	(-0.376, 0.284)	(-0.235, 0.152)	(-0.184, 0.097)	(-0.156, 0.065)	(-0.141, 0.041)	(-0.130, 0.025)
	[13.0]	[29.2]	[51.9]	[81.4]	[117.3]	[162.2]
Panel B: Breastfeeding Duration (Weeks)						
Specification:						
Mean Slope	0.00, 0.32	-1.69, -0.61	-4.87, -2.52	-4.08, -2.54	-4.06, -1.83	-2.50, -0.98
	(-17.86, 10.61)	(-9.18, -2.69)	(-9.53, -2.52)	(-6.17, -0.56)	(-4.63, 0.05)	(-5.70, -0.67)
Bin Slope	·	-1.73, -1.17	-4.97, -3.05	-4.15, -2.90	-4.16, -2.40	-2.53, -1.40
	(·)	(-9.31, -2.94)	(-9.75, -2.74)	(-6.25, -0.76)	(-4.60, -0.13)	(-5.77, -0.83)
Nonlinear Projection	·	-1.74, -1.17	-5.08, -3.05	-4.19, -3.04	-4.26, -2.40	-2.60, -1.53
	(·)	(-9.49, -3.05)	(-9.91, -2.91)	(-6.49, -1.08)	(-4.66, -0.44)	(-5.89, -1.09)
Simulation	-2.64	-2.95	-3.17	-3.28	-3.35	-3.35
	(-13.04, 7.77)	(-9.01, 3.12)	(-7.56, 1.23)	(-6.73, 0.18)	(-6.21, -0.49)	(-5.77, -0.92)
	[13.0]	[29.4]	[52.6]	[82.9]	[119.6]	[165.5]

Notes: This table reports bounds on the local average treatment effect (LATE) of WIC participation on breastfeeding outcomes using the partial identification regression discontinuity design (Assumptions 1–3). Bootstrap 95% confidence intervals (1,000 replications) appear in parentheses. Confidence intervals are reported only when the first-stage lower bound excludes zero in at least 500 replications. Panel A presents breastfeeding initiation; Panel B presents breastfeeding duration (weeks). Column headers indicate bandwidth h with corresponding reference bin ranges $[b_h^-]/[b_h^+]$ from which Assumption 2 is imposed. The bounded rate of change (Assumption 3) is estimated via three approaches using the slope of the conditional expectation function, $\partial \hat{h}_z(x)/\partial x$: (i) *Mean Slope* uses derivatives averaged over $[-100, 0]$ for $z = 1$ and $[0, 100]$ for $z = 0$; (ii) *Bin Slope* uses derivatives evaluated at midpoints of $[b_h^-]$ and $[b_h^+]$; (iii) *Nonlinear Projection* directly projects the estimated conditional expectation function. All implementations use a slope multiplier of one. The *Simulation* row presents simulation-based RDD estimates with 95% confidence intervals in parentheses and mean first-stage F-statistics in brackets. See Section 4 for details.

Table 5: Traditional Regression Discontinuity Estimates

Bandwidth:	0.50	1.00	2.00	4.00	6.00	8.00	10.00
Panel A: Breastfeeding Initiation							
Polynomial Order:							
Zero	-0.19	0.17	-0.48	-0.25	-0.14	-0.11	-0.09
	(0.23)	(0.24)	(0.49)	(0.08)	(0.04)	(0.03)	(0.03)
	[7.3]	[5.1]	[1.9]	[47.2]	[156.2]	[242.8]	[325.5]
One	0.07	-1.40	0.02	0.18	-2.07	-0.43	-0.42
	(0.13)	(2.14)	(0.17)	(0.25)	(3.43)	(0.18)	(0.18)
	[5.4]	[0.5]	[10.0]	[5.1]	[0.4]	[12.1]	[12.3]
Two	-1.38	-0.04	·	-0.12	-0.07	1.07	·
	(3.47)	(0.13)	(·)	(0.18)	(0.16)	(2.26)	(·)
	[0.2]	[3.8]	[·]	[9.8]	[11.5]	[0.3]	[·]
Optimal Order	1	2	1	2	2	0	0
Observations	597	949	1194	3188	5055	9275	11517
Panel B: Breastfeeding Duration (Weeks)							
Polynomial Order:							
Zero	-14.43	-6.41	-12.69	-8.02	-3.43	1.06	-0.55
	(8.91)	(7.55)	(13.67)	(2.50)	(1.28)	(1.04)	(0.89)
	[5.7]	[4.6]	[1.9]	[46.0]	[155.8]	[241.5]	[328.0]
One	5.70	-76.45	-8.73	-8.47	-71.25	-21.01	-11.59
	(9.42)	(200.74)	(5.78)	(8.15)	(124.05)	(6.92)	(5.28)
	[5.6]	[0.1]	[9.0]	[4.5]	[0.3]	[12.8]	[12.5]
Two	-72.82	3.29	48.56	-8.94	-13.31	-0.64	·
	(239.48)	(11.92)	(106.30)	(6.00)	(6.19)	(29.98)	(·)
	[0.1]	[3.7]	[0.2]	[8.7]	[10.4]	[0.3]	[·]
Optimal Order	1	0	1	0	0	2	0
Observations	592	952	1198	3159	5054	9387	11634

Notes: This table presents estimates of the treatment effect (τ) from WIC participation on the initiation and duration of breastfeeding using the traditional regression discontinuity design. Panel A shows results for breastfeeding initiation, and Panel B shows results for breastfeeding duration in weeks. Each column represents a different bandwidth (h), and each row represents a different polynomial order for the regression function. Standard errors are shown in parentheses. Observation counts reflect the number of observations i such that $X_i^\star \in [c-h, c+h]$ for bandwidth h. First-stage F-test values are shown in square brackets. The optimal polynomial order according to Akaike's Information Criterion is indicated for each bandwidth. Additional estimation details can be found in Section 4.

Table 6: Doughnut Regression Discontinuity LATE Estimates

Bandwidth:	0.50	1.00	2.00	4.00	6.00	8.00	10.00
Panel A: Breastfeeding Initiation							
Polynomial Order:							
Zero	-0.22	-0.04	-0.05	-0.06	-0.08	-0.09	-0.10
	(0.07)	(0.04)	(0.03)	(0.03)	(0.02)	(0.01)	(0.01)
	[14.0]	[171.6]	[445.7]	[402.0]	[760.7]	[1394.0]	[1928.1]
One	-0.00	0.01	-0.14	-0.01	-0.26	-0.41	-0.32
	(0.08)	(0.08)	(0.15)	(0.15)	(0.11)	(0.25)	(0.13)
	[1.4]	[19.6]	[14.9]	[11.8]	[28.6]	[6.6]	[22.1]
Two	·	·	-0.02	-0.09	-0.09	-0.12	-0.09
	(·)	(·)	(0.60)	(0.10)	(0.11)	(0.16)	(0.74)
	[·]	[·]	[0.6]	[21.3]	[21.2]	[9.5]	[0.5]
Optimal Order	1	0	2	1	0	0	2
Observations	843	1679	5310	7224	8924	12412	14328
Panel B: Breastfeeding Duration (Weeks)							
Polynomial Order:							
Zero	-9.33	-2.13	-4.32	-3.90	-4.66	-4.22	-4.67
	(3.07)	(1.33)	(0.82)	(0.83)	(0.62)	(0.45)	(0.39)
	[14.0]	[166.3]	[446.6]	[405.6]	[759.9]	[1389.0]	[1918.8]
One	10.76	-4.27	-13.46	-3.40	-11.04	-3.42	-9.30
	(15.57)	(2.97)	(5.31)	(5.02)	(3.69)	(6.52)	(3.92)
	[1.4]	[19.1]	[16.0]	[10.4]	[27.9]	[6.7]	[22.0]
Two	·	·	-5.08	-9.79	-4.19	-9.90	10.84
	(·)	(·)	(22.36)	(3.95)	(3.78)	(5.74)	(29.20)
	[·]	[·]	[0.5]	[20.6]	[18.9]	[10.0]	[0.6]
Optimal Order	0	0	0	1	2	1	0
Observations	827	1655	5373	7306	9003	12461	14371

Notes: This table presents estimates of the treatment effect (τ) from WIC participation on the initiation and duration of breastfeeding using the doughnut regression discontinuity design. Panel A shows results for breastfeeding initiation, and Panel B shows results for breastfeeding duration in weeks. Each column represents a different bandwidth (h), and each row represents a different polynomial order for the regression function. Standard errors are shown in parentheses. Observation counts reflect the number of observations i such that $X_i^\star \in [c-\mu-h, c-\mu] \cup [c+\mu, c+\mu+h]$ for bandwidth h. First-stage F-test values are shown in square brackets. The optimal polynomial order according to Akaike's Information Criterion is indicated for each bandwidth. Additional estimation details can be found in Section 4.