The Impact of Electronic Benefit Transfer on WIC Participation: Evidence from Natality Data*

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December 19, 2024

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Abstract

Policymakers have an interest in ensuring participation in the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC). WIC has been shown to increase birth weight for participating mothers and improve long-run outcomes for children who participate in the first years of life. Between 2002 and 2022, WIC transitioned from paper vouchers to electronic benefit transfer (EBT) cards. This payment reform was expected to encourage WIC participation by streamlining benefit redemption and thereby reducing welfare stigma. Empirical studies of the effects of WIC EBT on participation have found mixed results, with prior work limited to one state. With national WIC participation data absent, the results may not be generalizable. In this paper, we evaluate the impact of WIC EBT implementation on WIC participation nationwide by linking the WIC EBT roll-out schedule to Vital Statistics Natality Data across virtually all counties in the U.S. We document a significant increase in WIC participation following the implementation of WIC EBT among mothers who are more likely to be WIC-eligible. We also find that WIC EBT reduces adverse birth outcomes for infants born to these mothers. Our findings suggest that facilitating the delivery of public benefits can improve program uptake and well-being of participants in need. (JEL H51, H53, I38)

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1 Introduction

The Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) provides foods and nutrition counseling for low-income pregnant or postpartum women, infants, and children under the age of five. WIC participation has been linked to improved birth outcomes and long-run education and health gains for individuals that participated in early childhood (Hoynes, Page and Stevens, 2011; Chorniy, Currie and Sonchak, 2020). However, the share of U.S.-born infants enrolled in WIC has declined from 50% in 2009 to 30% in 2021 (Figure 1), representing a reduction to 3 million individuals. Policymakers are interested in program changes that can stem these declines.

Between 2002 and 2022, WIC transitioned from paper vouchers to electronic benefit transfer (EBT) cards. This reform was intented to encourage WIC participation among eligible individuals by reducing the stigma that participants experienced when redeeming WIC benefits (Moffitt, 1983). Participants may perceive benefits as more valuable after WIC EBT implementation when they can redeem a food instrument across multiple transactions (Hanks et al., 2019; Li et al., 2021; Ambrozek et al., 2024). At the same time, WIC EBT was billed as a fraud reducing policy transition. Prior work finds that small stores are less likely to be authorized post EBT, potentially affecting participant access (Meckel, 2020). The net effect on WIC participation is, therefore, unclear. Understanding the effect that this policy change – the largest change to WIC in the past few decades – had on participation and participant's outcomes is important.

Empirical evidence of WIC EBT's effect on participation is mixed. For example, Hanks et al. (2019) find that WIC EBT increases WIC redemptions in Ohio. Li, Saitone and Sexton (2022) find no significant impact of WIC EBT on the share of WIC enrollment in Oklahoma. Finally, Meckel (2020) finds WIC EBT decreases the number of WIC births in Texas. A common feature of previous work is a focus on a single state and a short time period. We link the WIC EBT roll-out schedule across virtually all counties in the U.S. to Vital Statistics Natality Data, which began reporting WIC status of live births in 2009, to examine effect of WIC EBT on WIC participation. Using the natality data avoids misreporting of WIC participation status from survey data (Meyer, Mok and Sullivan, 2015; Meyer and Mittag, 2019). Given that WIC's ultimate goal is to improve infant health, we also examine the effects of WIC EBT on birth outcomes to assess whether EBT's impact on WIC participation translates into improved infant health. If WIC EBT increases WIC participation, WIC redemption, or both, improved maternal nutrition is likely to lead to better infant health on average. We estimate our models using a staggered-adoption difference-in-diffences (DiD) approach, following the procedure from Sun and Abraham (2021). This approach allow us to disaggregate our treatment effect estimates to subgroups and time periods, to show how treatment effects

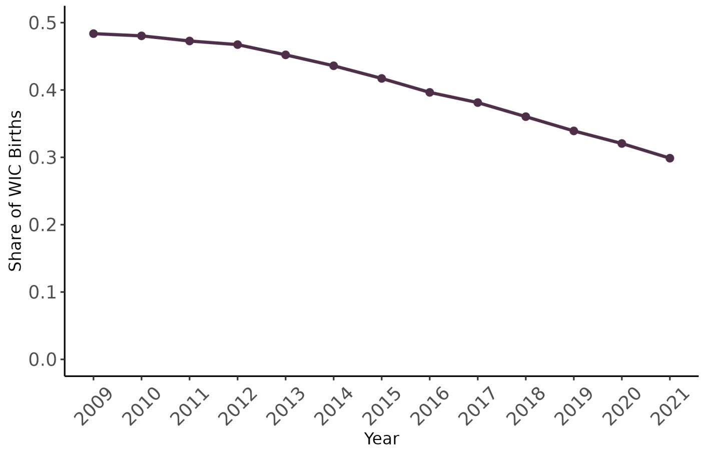


FIGURE 1: SHARE OF BIRTHS PARTICIPATING IN WIC

Notes: The share of WIC births is calculated by diving the number of WIC births by all live births from Vital Statistics Natality Data.

vary across the country, given heterogeneous policy environments in different states, and across time.

We document a somewhat noisy increase in WIC participation across all births, which is unsurprising, given that only WIC-eligible individuals are directly impacted by the EBT transition. However, WIC eligibility is not reported in the natality data. To improve statistical power, we restrict our analysis to high-impact groups that are more likely to be WIC-eligible. This method is a standard approach for studying policy impacts when data do not directly identify the policy's target population (Meckel, 2020; Alsan and Yang, 2022; East et al., 2023). We identify these groups by estimating the shares of likely WIC-eligible individuals based on overlapping maternal characteristics observed in both the natality data and the Survey of Income and Program Participation (SIPP). We find that mothers with no more than a high school education and those without an infant's father listed on the birth certificate—each comprising around 40% of the full sample—are substantially more likely to be WIC-eligible.

Focusing on these two groups, we observe a 2.39-percentage-point increase in WIC participation rates among mothers with no more than a high school education and a 2.5-percentage-point increase among mothers without an infant's father listed on the birth certificate in counties that implemented WIC EBT compared to those that had not yet adopted it. We also find that WIC EBT is biologically important. WIC EBT implementation reduces the likelihood of low birth weight by 0.47 percentage points and preterm births by 0.54 percentage points among infants born to mothers with no more than a high school education. Among infants without fathers listed on their birth certificates, the likelihood of low birth weight decreases by 0.62 percentage points, and preterm births by 0.82 percentage points. Based on these results, our back-of-the-envelope calculation suggests that WIC EBT lifts thousands of births out of low birth weight and preterm status, saving millions of dollars in hospital costs annually. We provide suggestive evidence that these improvements are primarily driven by reduced welfare stigma, which appears to outweigh any potential negative effects of declining WIC vendor access. Overall, our findings contribute to a better understanding of program take-up and the associated benefits for participants in social safety net programs.

This paper contributes to three strands of literature. First, it adds to the body of research on the effects of Electronic Benefit Transfer (EBT) implementation. Existing studies have examined the impacts of WIC EBT on WIC participation rates (Meckel, 2020; Li, Saitone and Sexton, 2022), WIC redemption patterns (Hanks et al., 2019), and the retail environment for WIC vendors (Meckel, 2020; Ambrozek et al., 2024). Beyond WIC EBT, Wright et al. (2017) found that implementing TANF EBT reduced crime rates in Missouri, while Shiferaw (2020) showed that SNAP EBT increased average birth weight in California. This paper extends this literature by providing national-scale evidence on WIC EBT's effects on WIC participation and birth outcomes.

Second, this paper relates to the broader literature on the determinants of food assistance participation in the U.S. For example, Swann (2010) finds that economic conditions, Medicaid expansion, and migration are associated with changes in WIC eligibility and participation. For WIC, factors such as the type of vendors (McLaughlin, Saitone and Sexton, 2019) and vendor accessibility (Rossin-Slater, 2013; Ambrozek, 2022) also influence participation rates. Additionally, policy design elements, such as work requirements (Gray et al., 2023) and tax exemptions (Zhao, Kaiser and Zheng, 2022), play a role in participation decisions. This study contributes to this scholarship by providing empirical evidence on the effects of payment reform, specifically EBT implementation, on program participation.

Finally, this paper contributes to the literature on the impacts of food assistance programs on birth outcomes. Previous research has explored how the introduction of SNAP (Almond, Hoynes and Schanzenbach, 2011) and WIC (Bitler and Currie, 2005; Figlio, Hamersma and Roth, 2009; Hoynes, Page and Stevens, 2011; Chorniy, Currie and Sonchak, 2020; Bitler et al., 2023) affects birth outcomes, generally finding that food assistance programs improve these outcomes. This study builds on this literature by examining the effects of WIC's transition to EBT on birth outcomes.

The rest of the paper is organized as follows: Section 2 provides the policy background; Section 3 presents the conceptual framework; Section 4 describes the data; Section 5 outlines the research design; Section 6 presents the empirical results; Section 7 provides the results of robustness checks; Section 8 discusses potential mechanisms; Section 9 discusses magnitudes of our estimates; and Section 10 addresses study limitations and concludes.

2 Background

2.1 WIC

WIC was established in 1974 as a permanent program to safeguard the health of low-income women, infants, and children up to the age of five who are at nutritional risk. The program's mission is to provide nutritious foods, nutrition education, and referrals to health and other social services to address common nutrition deficiencies and support the overall health of women and young children[1]. WIC eligibility requires a household income below 185% of the federal poverty line or participation in the Supplemental Nutrition Assistance Program (SNAP), Temporary Assistance for Needy Families (TANF), Aid to Families with Dependent Children (AFDC), or Medicaid. Over the years, WIC has become one of the most widely used food assistance programs: in fiscal year 2023, the federal government spent 6.6 billion dollars on WIC, making it the third-largest food assistance program by total spending[2].

The impacts of WIC have been widely studied. For example, WIC has been linked to lower food insecurity (Kreider, Pepper and Roy, 2016) and improved diet quality (Smith and Valizadeh, 2024) among children. Additionally, WIC participation has shown positive effects on birth outcomes (Hoynes, Page and Stevens, 2011) and has contributed to long-term educational and health gains for those who participated during early childhood (Chorniy, Currie and Sonchak, 2020). WIC also benefits parents, as it has been associated with increased breastfeeding initiation at hospital discharge (Rossin-Slater, 2013). When parents lose WIC benefits, they often compromise their own nutrition intake to preserve their children's (Bitler et al., 2023).

Despite extensive evidence on the health and social benefits of WIC, the program faces challenges such as declining participation and difficulties in reaching some of the most vulnerable populations (Neuberger, Hall and Sallack, 2024). Addressing these challenges is essential to ensure the successful delivery of WIC benefits to those most in need.

2.2 EBT Transition

The transition to WIC EBT was a USDA Food and Nutrition Service (FNS) initiative aimed at modernizing WIC benefit delivery. Its primary goals included streamlining business practices, simplifying transactions to reduce stigma, and improving program monitoring for WIC

[1]Source: USDA FNS. https://www.fns.usda.gov/wic/factsheet (Accessed on October 27, 2024).
[2]Source: USDA FNS. https://fns-prod.azureedge.us/sites/default/files/resource-files/wisummary-10.pdf (Accessed on October 27, 2024).

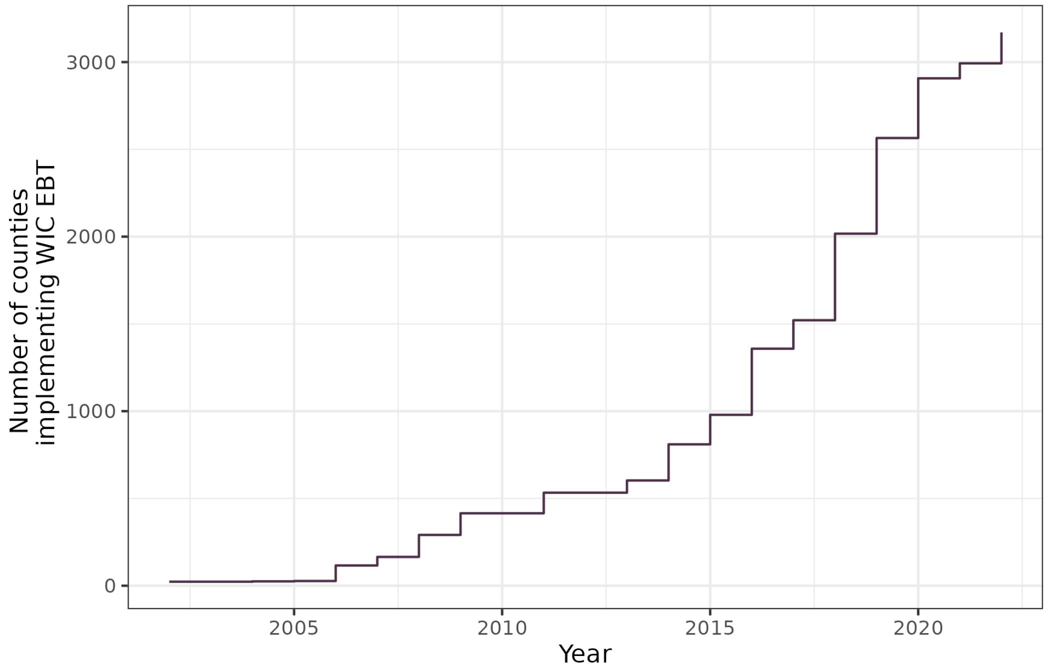
state agencies. Although some early WIC EBT projects began as early as 1995, the national WIC EBT transition plan was introduced in 2003, following the successful implementation of EBT systems in other federal food assistance programs, such as SNAP.

In 2010, the Healthy, Hunger-Free Kids Act (HHFKA 2010) imposed a national mandate for the transition to EBT systems by October 1, 2020. This mandate provided a clear timeline for state WIC agencies nationwide[3]. The HHFKA 2010 directed the USDA to develop WIC EBT technical standards and operating rules for all stakeholders and to establish a national database of universal product codes for the EBT systems across all states[4]. The USDA shared the costs of EBT implementation with state agencies, with each state submitting a plan for how costs would be split. This plan allowed states to access grants for the transition, covering a range of participating stakeholders (USDA Food and Nutrition Service, 2016).

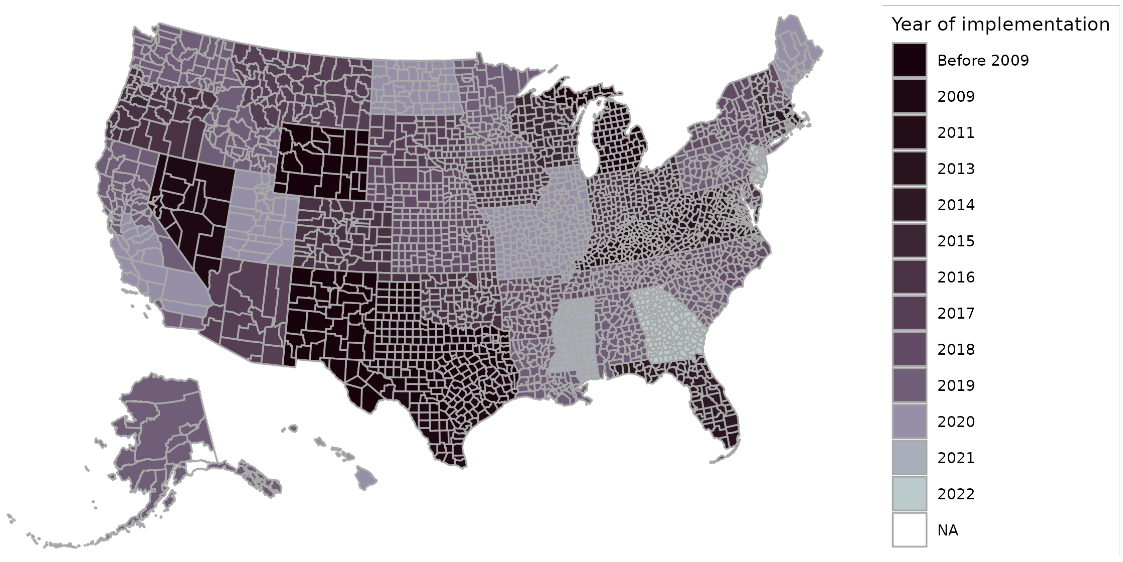
To track WIC EBT rollout timelines across U.S. counties, we collected data from mutiple sources including (archived) state websites, policy documents, and research papers. Most of the transition took place after 2010 (see Figure 2a). Figure 2b shows the geographic spread of EBT adoption, highlighting both similarities and differences in timing across counties within states. By 2022, all 50 states, U.S. territories, and tribal organizations had made the switch to EBT. The pace of adoption depended on factors such as technical issues, available funding, cost-sharing plans, state agency efficiency, acceptance by local retailers, and the retail setup in each area (USDA Food and Nutrition Service, 2016).

[3]Exemptions may be granted for a state facing unusual barriers to such implementation.

[4]Source: Congress.gov. https://www.congress.gov/bill/111th-congress/senate-bill/3307 (Accessed on October 27, 2024).



(A) Number of counties implementing WIC EBT over time



(B) Geographic variation in timing of WIC EBT implementation

FIGURE 2: WIC EBT ROLL-OUT SCHEDULE SINCE 2009

3 Conceptual Framework

The net impact of EBT on WIC participation is, as a priori, ambiguous. EBT may encourage eligible individuals to participate in WIC by reducing welfare stigma and transaction costs. In contrast, anti-fraud features may discourage store participation in the WIC program, as they could reduce the potential for illegal profits from committing fraud[5], making WIC stores less accessible. This section outlines a simple framework to explore this dynamic. Specifically, we consider a retailer-consumer equilibrium framework in which consumers choose to participate by maximizing their utility subject to both budgetary and time constraints, while retailers decide to participate in WIC if the net benefits of doing so are positive.

We start by considering a utility maximization problem for a typical WIC-eligible consumer. Following the framework outlined in Manchester and Mumford (2010), let U_i denote the utility of individual i, which depends on their leisure (L_i) and consumption (C_i). Consumption is composed of the total value of WIC-eligible goods (Z_i) and the toal value of a composite bundle of all other goods (G_i). Specifically, we represent consumption as $C_i = G_i + \theta_i Z_i$, where $\theta_i \in [0, 1]$ captures WIC participation, allowing for partial redemption of benefits. Participation in WIC provides access to eligible goods at a subsidized or no cost but may also involve time and stigma costs. Each individual has a fixed time endowment, T, which is allocated among leisure (L_i), work (W_i), and the time required to redeem WIC benefits ($\theta_i \delta_i$), such that $T = L_i + W_i + \theta_i \delta_i$. Assuming income is entirely derived from work, consumption can be expressed as $C_i = w \cdot W_i + \theta_i Z_i$, where w represents the wage rate. The individual's utility is given by:

$$U(L_i, C_i) = V(L_i, C_i) - \theta_i \phi_i,$$

where ϕ_i captures total psychological disutility associated with welfare stigma. For WIC participants, the optimal W_i and θ_i maximize utility subject to the constraints $T = L_i + W_i + \theta_i \delta_i$ and $C_i = w \cdot W_i + \theta_i Z_i$.

Next, we analyze how a retailer's decision to participate in WIC impacts consumer behavior. The net benefit for retailer j from participating in WIC is given by:

$$\Pi_j = R_j - F_j = \kappa_j \sum_i \theta_i Z_i - F_j,$$

[5]For example, prior to EBT implementation, vendors had an incentive to charge WIC customers higher prices than non-WIC customers, as WIC goods are reimbursed by the government. This practice, prohibited by WIC program rules, is curtailed by EBT, which allows the government to monitor prices directly and ensure compliance (Saitone, Sexton and Volpe III, 2015).

where Π_j represents the net benefit, κ_j denotes the share of all WIC-eligible goods sold by retailer j, and F_j is the compliance cost associated with WIC participation, including the loss of the potential benefits of committing fraud. A retailer will choose to participate in WIC if $\Pi_j > 0$, meaning the revenue from WIC transactions exceeds compliance costs. Thus, the probability S_j that a retailer participates in WIC can be expressed as:

$$S_j = \Pr\left(\kappa_j \sum_i \theta_i Z_i > F_j\right).$$

For consumers, the time cost δ_i of redeeming WIC benefits depends on the availability of nearby WIC-participating retailers. Let $\bar{S}_i$ denote the average participation rate of retailers near individual i:

$$\bar{S}_i(\theta_i, \mathbf{F}_{\text{vicinity, i}}) = \frac{1}{N_i} \sum_{j \in \text{vicinity of } i} \Pr\left(\kappa_j \sum_i \theta_i Z_i > F_j\right),$$

where N_i is the number of retailers near individual i, and $\mathbf{F}_{\text{vicinity, i}}$ is a vector of compliance costs (F_j) for retailers in the vicinity of i. This vector captures the compliance cost landscape near the consumer, influencing the likelihood of retailers participating in WIC. The consumer's time cost δ_i decreases as $\bar{S}_i$ increases, meaning that a higher probability of nearby WIC-participating retailers reduces the travel or time burden associated with redeeming WIC benefits. This relationship can be formalized as:

$$\delta_i = \delta_i[\bar{S}_i(\theta_i, \mathbf{F}_{\text{vicinity, i}})] = \delta_i(\theta_i, \mathbf{F}_{\text{vicinity, i}}),$$

with the assumptions $\frac{\partial \delta_i}{\partial \theta_i} > 0$ and $\frac{\partial \delta_i}{\partial \mathbf{F}_{\text{vicinity, i}}} > 0$. These assumptions reflect that the individual's time cost is positively related to their level of WIC participation (θ_i) and the compliance cost environment of nearby retailers ($\mathbf{F}_{\text{vicinity, i}}$).

Finally, substituting $\delta_i = \delta_i(\theta_i, \mathbf{F}_{\text{vicinity, i}})$ into the time constraint yields $T = L_i + W_i + \theta_i \delta_i(\theta_i, \mathbf{F}_{\text{vicinity, i}})$. We then solve for the optimal working time (W_i^{WIC}) and participation intensity (θ_i^{WIC}) for WIC participants. By substituting these values into the utility function $U(\cdot)$, we can determine the maximum utility for WIC participants, U_i^{WIC}. Similarly, setting $\theta_i = 0$, we calculate the utility for non-WIC participants, $U_i^{\text{non WIC}}$. The probability that individual i participates in WIC is then:

$$\Pr(U_i^{WIC} > U_i^{\text{non WIC}})^{6}.$$

[6] W_i^{WIC} and W_i^{nonWIC} are the optimal working time for WIC participants and non-WIC participants, respectively, and

$$U^{WIC} = V\left[T - W_i^{WIC} - \theta_i^{WIC}\delta_i(\theta_i^{WIC}, \mathbf{F}_{\text{vicinity, i}}), w \cdot W_i^{WIC} + \theta_i^{WIC} Z_i\right] - \theta_i^{WIC}\phi_i,$$

By envelope theorem, we obtain:

$$\frac{\partial U^{WIC}}{\partial \phi_i} = -\underbrace{\theta_i^{WIC}}_{>0} < 0, \tag{1}$$

$$\frac{\partial U^{WIC}}{\partial \mathbf{F}_{\text{vicinity, i}}} = -\underbrace{\theta_i^{WIC}}_{>0} \cdot \underbrace{\frac{\partial \delta_i}{\partial \mathbf{F}_{\text{vicinity, i}}}}_{>0} \cdot \underbrace{\frac{\partial V}{\partial L_i}}_{>0} < 0. \tag{2}$$

Thus, EBT affects WIC participation through two primary channels: (1) it reduces welfare stigma for consumers, lowering ϕ_i, increasing U^{WIC}, and thus potentially raising $\Pr(U_i^{WIC} > U_i^{\text{non WIC}})$; (2) it raises compliance costs for retailers, increasing $\mathbf{F}_{\text{vicinity, i}}$, decreasing U^{WIC}, and potentially lowering $\Pr(U_i^{WIC} > U_i^{\text{non WIC}})$.

Equation 1 indicates that the strength of the first channel depends on the intensity of optimal WIC participation: the welfare stigma channel is most effective for those with higher levels of WIC participation, reflected by a higher redemption rate. A higher redemption rate might translate into more time at the checkout counter, potentially increasing their experience of welfare stigma. Equation 2 shows that the retailer compliance cost channel is strongest when WIC benefit utilization is high (large θ^{WIC}), the marginal increase in the time cost of WIC redemption is sensitive to the closure of neighboring WIC vendors (large $\frac{\partial \delta_i}{\partial \mathbf{F}_{\text{vicinity, i}}}$), and consumers place a higher value on leisure (large $\frac{\partial V}{\partial L_i}$).

4 Data

4.1 Vital Statistics Natality Data

Natality data, coded from birth certificates, provide detailed birth and parental information, including the county of maternal residence, year of birth, maternal age, educational attainment, marital status, and WIC participation, among other variables. The 2003 revision of

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$$U^{\text{non WIC}} = V(T - W_i^{\text{non WIC}}, w \cdot W_i^{\text{non WIC}}),$$

where θ_i^{WIC} is participation intensity for WIC participants.The optimal working time W_i^{WIC} and participation θ_i^{WIC} for WIC participants satisfy:

$$\frac{\partial V}{\partial L_i}(W_i^{WIC}, \theta_i^{WIC}) = w \cdot \frac{\partial V}{\partial C_i}(W_i^{WIC}, \theta_i^{WIC}),$$

$$\left[\delta_i(\theta_i^{WIC}, \mathbf{F}_{\text{vicinity, i}}) + \theta_i^{WIC} \frac{\partial \delta_i}{\partial \theta_i}(\theta_i^{WIC}, \mathbf{F}_{\text{vicinity, i}})\right] \cdot \frac{\partial V}{\partial L_i}(W_i^{WIC}, \theta_i^{WIC}) + \phi_i = \frac{\partial V}{\partial C_i}(W_i^{WIC}, \theta_i^{WIC}) \cdot Z_i.$$

For non-WIC participants, setting $\theta_i = 0$, the optimal working time $W_i^{\text{non WIC}}$ satisfies:

$$\frac{\partial V}{\partial L_i}(W_i^{\text{non WIC}}) = w \cdot \frac{\partial V}{\partial C_i}(W_i^{\text{non WIC}}).$$

the birth certificate required the inclusion of the mother's WIC participation, though this information did not become available until 2009. We collapse the birth-level natality data to county-of-maternal-residence-by-year-of-birth cells to make the sample size more manageable. Our sample period spans 2009-2021 (National Center for Health Statistics, 2021).

We validate the WIC participation information from natality data by showing that it plausibly reflects changes in total WIC participation. First, as depicted in Figure 3, the ratio of WIC births to total WIC participants consistently remains at 20% throughout the study period, with the exception of a slight decline during the pandemic. Second, we find the observable characteristics are comparable across the three samples: mothers in the natality data, women aged 15-49 years in the Current Population Survey's (March) Annual Social and Economic Supplements (CPS ASEC), and postpartum women in SIPP. Table 1 shows that the differences in the proportions of Black and Hispanic mothers, educational backgrounds, and regions of residence between the natality data and CPS ASEC, as well as between the natality data and SIPP, are within 5%. Despite this evidence, we acknowledge that mothers in the natality data may still differ significantly from overall WIC participants. However, these mothers represent an important share of WIC participants. Natality data has also been used in other studies, such as Rossin-Slater (2013) and Meckel (2020), to examine WIC participation.

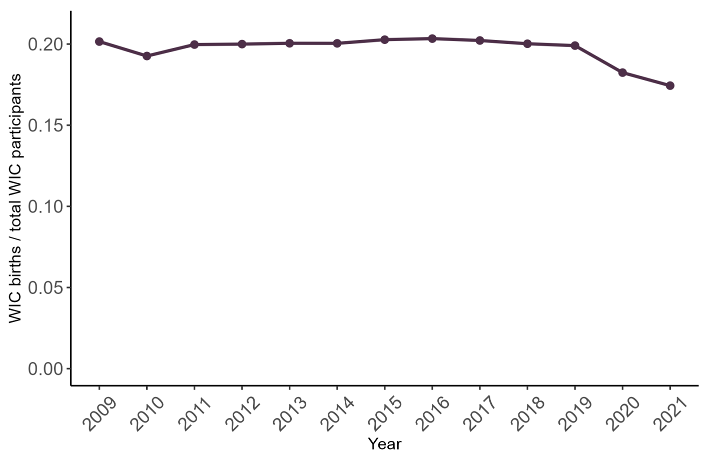


FIGURE 3: RATIO OF WIC BIRTHS TO TOTAL WIC PARTICIPANTS

Notes: Ratio of WIC Births to Total WIC Participants is calculated by dividing total number of WIC births (from natality data) by total WIC participants (from USDA FNS). Data on total WIC participants is from USDA FNS website: https://www.fns.usda.gov/pd/wic-program. The website only include most recent data. We use wayback machine to extract historical data.

We also compare the natality data from Vital Statistics with birth data from the Texas Department of State Health Services (Texas DSHS) as used in Meckel (2020). Meckel (2020) uses Texas DSHS natality data covering births in counties that implemented WIC EBT be-

Table 1: Comparing natality data with other survey data

	Natality data (1)	CPS ASEC (2)	Mean difference (1) - (2) (3)	SIPP (4)	Mean difference (1) - (4) (5)
Share of black	16.07%	15.85%	0.22%	15.37%	0.70%
Share of Hispanics	24.18%	21.54%	2.64%	20.04%	4.14%
Education $\leq$ high school	40.42%	42.91%	-2.49%	37.17%	3.25%
Education $\geq$ college	31.06%	27.79%	3.27%	32.94%	-1.88%
Northeast	14.77%	17.02%	-2.25%	17.47%	-2.70%
Midwest	21.65%	20.60%	1.05%	20.82%	0.83%
West	24.81%	24.07%	0.74%	23.08%	1.73%
Share WIC participants	40.46%	6.41%		5.65%	
Full sample size	45,910,299	432,575		80,535	

Notes: Numbers in this table, unless otherwise noted, are shares of group with characteristics listed in first column. All three data sets span 2009-2021. Observations with null value are dropped. Means from natality data are unweighted since it covers population of live births; means from CPS AESC are weighted average characteristics of women at 15-49 years old; means from SIPP are the average of weighted average characteristics of mothers of infants across SIPP panels. For SIPP means, we first take weighted average of SIPP panel and then average across panels because personal weights are not comparable across panels.

fore April 2009 (239 counties) from January 2005 to December 2009. Our natality data covers births in all Texas counties (254 counties) but only extends back to January 2009. The overlapping subset of these two datasets includes births from January to December 2009 in counties that implemented WIC EBT before April 2009. A comparison of these overlapping subsets reveals that the data are nearly identical, as in Figure 4.

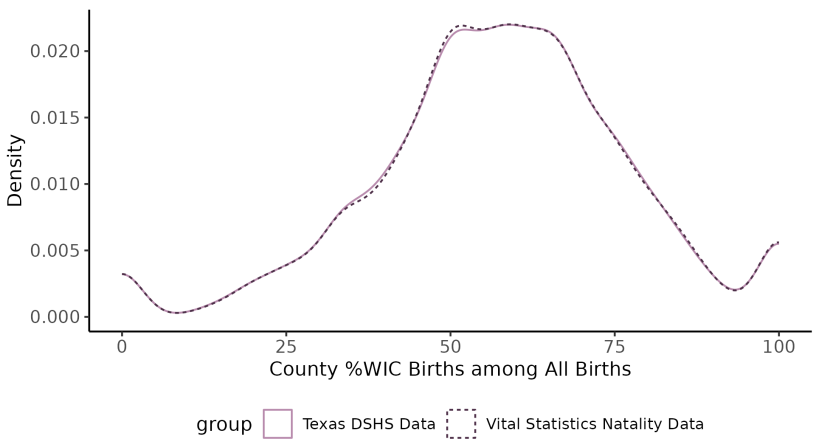


Figure 4: Distribution of county-level share of WIC birth

Notes: The dashed line represents the distribution of county shares of WIC births from the overlapped subset of Meckel (2020)'s data set. The solid line represents the distribution of county share of WIC births from the overlapped subset of our data set. The overlapped subsets cover 239 counties in Texas from January 2005 to December 2009.

4.2 WIC EBT roll-out

We compile the WIC EBT rollout schedule across nearly all U.S. counties[7] using public records from state WIC agencies. For counties reporting a range of implementation dates, we use the earliest date in the range. Our data capture both cross-state and within-state variation in the timing of WIC EBT implementation, with cross-state variation being more pronounced. After excluding counties that do not report WIC participation, our final sample includes 2,489 counties, covering 81.24% of the U.S. population and accounting for 79.10% of births.

We then examine the correlations between the WIC EBT rollout schedule and baseline county characteristics. We collect baseline data for the years 2006-2008 from various sources. Data on the share of Black and Hispanic populations and income per capita are from the American Community Survey (ACS) Public Use Microdata Sample. We construct county-level ACS data by matching individual records with Public Use Microdata Areas (PUMA) identifiers, aggregated to the county level and weighted by ACS personal weights. Observations from PUMAs with populations under 100,000 are excluded due to suppressed geographic identifiers. While we cannot find county-level data on all welfare programs that automatically qualify participants for WIC, we collect data on transfers from the Bureau of Economic Analysis's Regional Economic Information System (REIS), which include these welfare programs. Public assistance medical benefits include Medicaid and other medical vendor payments, while income maintenance benefits include TANF, WIC expenditures, and other general assistance such as tax credits, refugee assistance, foster care, adoption assistance, and energy aid. Finally, we include county-level data on poverty rates and the under-five population from the Small Area Income and Poverty Estimates (SAIPE) Program, the share of low birthweight from restricted-use Vital Statistics Natality Data, and the net increase in WIC vendors from the WIC Integrity Profiles (TIP). All variables represent three-year averages for 2006-2008, except for the net increase in WIC vendors, which is a three-year total.

Columns 1-3 of Table 2 present the baseline characteristics of our sample counties compared to those excluded. In general, included counties are not significantly better off than excluded ones. Although included counties have a smaller share of disadvantaged populations, a lower share of infants with low birthweight, and receive more income maintenance benefits per capita, they receive less SNAP benefits and have lower income per capita. We found no significant differences between included and excluded counties in terms of population size, per capita public assistance medical benefits, or net increase in WIC vendors. Columns 4 and 5 of Table 2 show that while some county baseline characteristics are strongly

[7] Indian Tribal Organizations with separate WIC EBT implementation plans are excluded.

Table 2: Timing of WIC EBT implementation and county baseline characteristics

	Included counties	Excluded counties	Mean difference (1) - (2)	Regressions of year of WIC EBT implementation on county baseline characteristics	
	(1)	(2)	(3)	(4)	(5)
Demographics, 2006-2008					
% Black	8.84	12.06	-3.22	0.0427***	-0.0014
	⟨0.26⟩	⟨0.54⟩		(0.0108)	(0.0021)
% Hispanic	5.43	19.46	-14.03	0.0480***	0.0148***
	⟨0.14⟩	⟨0.85⟩		(0.0129)	(0.0031)
% Poor × under age 5	1.64	1.95	-0.31	-0.2715	-0.1133***
	⟨0.02⟩	⟨0.03⟩		(0.3121)	(0.0421)
% Low birth weight	8.03	8.74	-0.71	-0.4072***	-0.0157
	⟨0.05⟩	⟨0.10⟩		(0.0770)	(0.0113)
Population	96,379	93,937	2,442		
	⟨6,282⟩	⟨11,143⟩			
Log population				-0.0291	-0.0188
				(0.1109)	(0.0161)
Transfers and income, 2006-2008					
Public asst. medical benefits p.p.	1.11	1.15	-0.03	0.6513***	-0.0256
(incl., Medicaid, $1,000)	⟨0.01⟩	⟨0.02⟩		(0.2417)	(0.0469)
Income maintenance benefits p.p.	0.18	0.17	0.01	-5.453***	0.4460
(incl., TANF and WIC, $1,000)	⟨0.002⟩	⟨0.003⟩		(1.656)	(0.3891)
SNAP benefits p.p. ($1,000)	0.12	0.13	-0.01	7.038**	1.233**
	⟨0.002⟩	⟨0.003⟩		(3.363)	(0.5087)
Income p.p.($1,000)	6.95	6.66	0.29	0.0166	-0.0090
	⟨0.03⟩	⟨0.06⟩		(0.0635)	(0.0133)
WIC vendors, 2006-2008					
Number of WIC vendors	0.04	0.03	0.004	0.4176	0.1275**
(1,000)	⟨0.002⟩	⟨0.004⟩		(0.3079)	(0.0537)
Fraction of population	81.27	18.73			
Fraction of births	79.10	20.08			
State fixed effects					✓
Observations				2,489	2,489
R-squared				0.1569	0.9892

Notes: This table shows cases means and, in angle brackets, standard errors, of the group with characteristics listed in first column. Data on share of black, share of Hispanic, and income per person is from American Community Survey (ACS) Public Use Microdata Sample; data on transfers is from Bureau of Economic Analysis, Regional Economic Information System (REIS); data on share of poor and under age 5 is from the Small Area Income and Poverty Estimates (SAIPE) Program; data on share of low birth weight is from restricted-use Vital Statistics Natality Data; data on the number of WIC vendors is from the WIC Integrity Profiles (TIP). In the third column are differences in means of included and excluded counties. ***, **, and * indicate that mean difference are significant at the 1%, 5%, and 10% levels with Student's T-test. Units of transfer are dollars unless otherwise specified. Fractions of the population and births do not sum up to 1 because we take into account observations without geographical identifiers. Low birth weight is when birth weight is no more than 2,500 grams. In Columns 4 and 5 are results from regressions of year of WIC EBT implementation on county baseline characteristics. Each regression is weighted by the mean population during 2006-2008. Standard errors in Columns 4 and 5 are heteroscedasticity-robust.

correlated with the timing of WIC EBT implementation, these characteristics as a whole explain only a small portion of the variation in implementation timing. Most of the variation in WIC EBT rollout timing is explained by state-level unobservables, as the R^2 value approaches 1 when state fixed effects are added. Thus, after controlling for county baseline characteristics, the timing of the WIC EBT rollout seems plausibly exogenous.

5 Methods

5.1 Empirical strategy

To estimate effects of WIC EBT implementation, we compare counties that implemented WIC EBT with counties that have not yet implemented WIC EBT. Our baseline regression model is:

$$Y_{ct} = \alpha + \mu EBT_{ct} + \eta_c + \lambda_t + \theta_{ct} + Z_c t + X_{ct} + \varepsilon_{ct},$$

where Y_{ct} is outcome variable measured for county c in year t, η_c and λ_t are county and year fixed effects to control for national economic shocks and county time-invariant unobserved heterogeneity, θ_{ct} is census-region-by-year fixed effect[8] to account for differential trends of outcomes across geograhical areas, $Z_c t$ is county baseline characteristics listed in Table 2 interacted with linear time trend to control for differential trends across regions with different baseline characteristics, X_{ct} is county-by-year employment rate to control for county-by-year-level local economic conditions, and ε_{ct} is an error term.

As documented in de Chaisemartin and D'Haultfœuille (2020) as well as Goodman-Bacon (2021), Imai and Kim (2021), and Sun and Abraham (2021), a standard two-way fixed effects (TWFE) OLS estimator with staggered treatment timing and heterogeneous treatment effects will implicitly make comparisons to all other units, aggregating these comparisons up with weights that may be negative. As a result, the TWFE estimator is not consistent for the estimand of interest - the average treatment effect on the treated (ATT). We use the interaction weighted (IW) estimator proposed by Sun and Abraham (2021) in our baseline results to avoid this issue. We first estimate the cohort-specific ITT effects in each event time (excluding period -1) using a saturated regression model that interacts event time dummies with cohort dummies, including all fixed effects and control variables. We then aggregate the coefficients on the interaction terms of event time and cohort dummies by sample shares to construct the IW estimators. Sun and Abraham (2021) show that the IW estimator is consistent under assumptions of parallel trends and no anticipation. In Section 7.7, we discuss results using other popular staggered difference-in-difference estimators as

[8] We control for census-region-by-year instead of state-by-year fixed effects to avoid singular matrix in estimation as there is nontrivial synergy of implemeting WIC EBT within state.

well as traditional TWFE estimators. We find that our results are not driven by estimation method.

In our baseline results, we report standard errors clustered at both the county and state levels, recognizing that the unit of treatment assignment could be the county or a group of counties, while also accounting for potential correlation of errors among counties within the same state (Abadie et al., 2023)[9]. Regressions and dependent variable means are weighted using the number of births in each cell. We present results for all births, as well as for high-impact groups defined as in Section 5.2. The raw estimates from our regressions represent the ITT effects of EBT. To obtain treatment effects on the treated (TOT), we divide the ITT by the share of WIC-eligible individuals in each group, as determined from SIPP.

5.2 High-impact groups

To estimate an ATT, our analysis would be ideally limited to WIC-eligible mothers. However, birth certificates do not provide data on WIC eligibility or maternal income. As an alternative, we restrict our sample to subgroups more likely to be eligible for WIC, defined by specific maternal characteristics[10].

We focus on the overlapping covariates in the natality data and SIPP—maternal age, education, marital status, race, and Hispanic origin—as these are the most commonly reported demographic characteristics. The SIPP provides valuable insight into the demographic characteristics of WIC-eligible individuals, as it includes information on household income and program participation[11]. We identify WIC-eligible mothers based on household income below 185% of the federal poverty line or participation in SNAP, TANF/AFDC, or Medicaid. From 2009 to 2021, the average proportion of WIC-eligible mothers of infants was 48.23%, slightly lower than the 54.10% estimated for WIC-eligible pregnant and postpartum women in 1998 by Bitler, Currie and Scholz (2003). Given that we do not observe pregnant women directly, we focus on mothers of infants (children aged 0). We then use the correlation between WIC eligibility and maternal characteristics to guide the selection of subgroups.

We identify mothers with a high school education or less and mothers who are unmarried householders as subpopulations more likely to be WIC-eligible as both of them

[9] We report both standard errors whenever possible; when inconvenient to do so, we report the standard errors clustered on state.

[10] Another approach could involve using machine learning to train a predictive model for the probability of being WIC-eligible, based on all overlapping covariates in the natality data and SIPP. This model could then be used to estimate WIC eligibility probabilities in the natality data. However, this approach is not feasible in this context due to the limited number of overlapping covariates.

[11] Bitler, Currie and Scholz (2003) highlights a significant undercount of WIC participants in SIPP data, though this undercount appears to be random with respect to observable characteristics.

comprise approximately 40% of the full sample and are about 17% more likely to be WIC-eligible than mothers overall (Table 3). Column 4 of Table 3 presents the results of regressing estimated WIC eligibility on individual maternal characteristics, controlling for state and panel fixed effects. These regression results align with the sample means reported in the other columns, suggesting that variations in WIC-eligible shares across maternal characteristic groups may not be driven by unobserved state or panel factors. When we discuss effects on WIC participation and birth outcomes, in addition to the full sample, we present results for these two groups. Since natality data does not indicate whether a mother is a householder, we report results for births where the father is not listed, as a proxy for unmarried householder mothers.

TABLE 3: REGRESSIONS OF WIC ELIGIBILITY ON MATERNAL CHARACTERISTICS, SIPP

Maternal characteristics	Share of individuals with characteristic k (1)	Share of WIC-eligible individuals (S_k) (2)	$S_k - S_{all}$ (3)	Individual regressions: coefficients (std.err) (4)
Age $\leq$ 22	19.41%	58.11%	9.88%	0.1264*** (0.0069)
Education $\leq$ high school	37.17%	65.29%	17.06%	0.2281*** (0.0084)
Unmarried	56.00%	56.41%	8.18%	0.1558*** (0.0088)
Unmarried female householder	40.71%	64.81%	16.58%	0.1742*** (0.0103)
Black	15.37%	64.00%	15.77%	0.1809*** (0.0196)
Hispanic	20.04%	62.35%	14.12%	0.2220*** (0.0127)

Notes: Data is Survey of Income and Program Participation (SIPP) panels 2008, 2014, and 2018-2021. These panels cover households interviewed from 2008-2021 (those interviewed in 2008 are excluded). Dependent variables of Columns (4) are a dummy for WIC eligibility estimated with income and program participation and the estimates are from regressions of WIC eligibility on single maternal characteristics. We control for state and panel fixed effects. Standard errors are clustered at state level. ***, **, and * indicate that t-test are significant at the 1%, 5%, and 10% levels. All regressions controls for state and panel fixed effects. S_{all} denotes overall share of WIC-eligible mothers. $S_{all} = 48.23\%$.

6 Results

Our primary findings focus on the effects of WIC EBT implementation on WIC participation rates for both full sample and high-impact groups. We then explore the heterogeneity of these effects across gender, race, ethnicity (Hispanic or non-Hispanic), birth order, and income quantiles. Lastly, we examine the effects of WIC EBT on birth outcomes, as improving birth outcomes is the ultimate goal of the policy. The expectation was that EBT would increase both WIC participation and redemption rates, thereby improving maternal nutrition and, consequently, birth outcomes.

6.1 Primary results: WIC EBT increases WIC participation

Table 4 shows that ITTs of EBT on WIC participation are 1.26, 1.56, and 1.62 percentage points for all births, mothers with no more than a high school education, and mothers without an infant's father documented, respectively. These estimates are statistically significant for the high-impact groups when using standard errors clustered at the county or state level. Among mothers with no more than a high school education and those without an infant's father documented, the shares of WIC-eligible individuals are 65.29% and 64.81%, respectively. Therefore, in term of TOT, the introduction of WIC EBT increased WIC participation by 2.39 percentage points among mothers with no more than a high school education and by 2.5 percentage points among mothers without an infant's father documented.

TABLE 4: EFFECTS OF WIC EBT ON WIC PARTICIPATION

	All births			Education $\leq$ high school			No father		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Born after EBT	0.0149	0.0168	0.0126	0.0268	0.0291	0.0156	0.0275	0.0336	0.0162
	(0.0058)**	(0.0051)***	(0.0056)**	(0.0081)***	(0.0080)***	(0.0073)**	(0.0079)***	(0.0074)***	(0.0065)**
	⟨0.0156⟩	⟨0.0097⟩*	⟨0.0120⟩	⟨0.0120⟩**	⟨0.0107⟩***	⟨0.0092⟩*	⟨0.0086⟩***	⟨0.0059⟩***	⟨0.0053⟩***
County fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Census region×year		✓	✓		✓	✓		✓	✓
Baseline char.×year		✓	✓		✓	✓		✓	✓
Employment rate$_{ct}$			✓			✓			✓
Observations	34,566	33,873	28,023	33,964	33,329	27,485	32,496	31,890	26,227
R^2	0.9578	0.9635	0.9637	0.9193	0.9237	0.9290	0.8463	0.8520	0.8520
Dep. var. mean	0.3972	0.3987	0.4118	0.6395	0.6412	0.6514	0.6627	0.6641	0.6747

Notes: We report interaction weighted estimators proposed by Sun and Abraham (2021). We collapse birth data to county-of-maternal-residence-by-year-of-birth cells. We control for county and year fixed effects, census-region-specific linear time trend, county baseline characteristics from 2006-2009 interacted with linear time trend, and county-by-year employment rate. Regressions and dependent variable mean are weighted by the number of births in each cell. We report standard errors clustered on county in parentheses and standard errors clustered on state in angle brackets. ***, **, and * indicate that t-test are significant at the 1%, 5%, and 10% levels.

Figure 5 indicates that pre-EBT trends are relatively flat, suggesting minimal differential trends before EBT implementation. We further test the sensitivity to potential violations of the parallel trend assumption in Section 7.6. In Table A1, we aggregate estimates by cohort and find that the positive effects are primarily driven by counties that adopted EBT in 2013, 2016, and 2017. These cohorts include counties from states such as Arizona, Colorado, Connecticut, Delaware, Florida, Indiana, Iowa, Kansas, Maryland, Oklahoma, Oregon, South Dakota, Virginia, and West Virginia. The geographic diversity of these states suggests that the estimates are less likely to reflect regional trends. We consider this hypothesis through additional tests presented later.

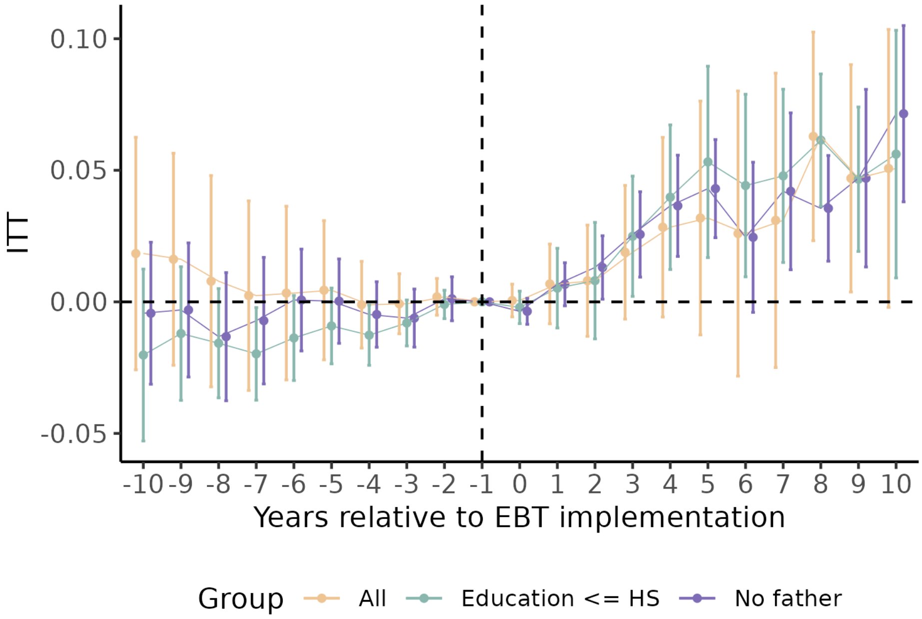


FIGURE 5: DYNAMIC EFFECTS OF WIC EBT ON WIC PARTICIPATION

Notes: We estimate dynamic effects using interaction weighted estimators proposed by Sun and Abraham (2021). We collapse birth data to county-of-maternal-residence-by-year-of-birth cells. We control for county and year fixed effects, census-region-specific linear time trend, county baseline characteristics from 2006-2009 interacted with linear time trend, and county-by-year employment rate. Regressions and dependent variable mean are weighted by the number of births in each cell. Standard errors are clustered at state level.

We further investigate the heterogeneity of EBT effects across maternal race, ethnicity, age, birth order, and income quantiles: results are presented in Table A2. We find that observed effects are primarily driven by white mothers, younger mothers under the age of 30, and mothers residing in low-income counties. The finding that white mothers benefit most from the EBT transition aligns with observations in Section 8, which show that the effect of EBT on WIC participation is substantially higher in rural areas.

6.2 Secondary results: WIC EBT reduces adverse birth outcomes

Given the positive effects of WIC EBT on WIC participation, we now turn to its impact on birth outcomes. Technically, WIC EBT can increase WIC participation through both the extensive margin (encouraging more WIC-eligible individuals to participate) and the intensive margin (enabling existing participants to redeem more WIC benefits), potentially contributing to improved birth outcomes. However, we cannot observe the intensive margin of WIC participation in the Vital Statistics Natality Data. Ambrozek et al. (2024) find that the rollout of WIC EBT does not significantly affect zip-code-level WIC redemptions. This suggests that any observed changes in birth outcomes may not be driven driven by increases in the intensive margin of WIC participation. In this section, we examine the effects of WIC EBT on three key birth outcomes: birth weight, the likelihood of low birth weight (defined as birth weight < 2500 grams), and the likelihood of preterm birth (gestation < 37 weeks). We find that EBT implementation significantly reduces adverse birth outcomes for high-impact groups.

Table 5 shows that while the effects of WIC EBT on birth outcomes are noisy for the full sample, they are statically significant for groups more likely to be WIC-eligible, mirroring the findings on WIC participation. Specifically, the ITT effects of EBT on the likelihood of low birth weight are -0.31 and -0.4 percentage points for mothers with no more than a high school education and mothers without an infant's father documented, respectively. Similarly, the ITT effects on the likelihood of preterm births are -0.35 and -0.53 percentage points for the same groups. In terms of TOT, the introduction of WIC EBT reduces the likelihood of low birth weight by 0.47 percentage points and preterm births by 0.54 percentage points among mothers with no more than a high school education. For mothers without an infant's father listed on the birth certificate, the likelihood of low birth weight decreases by 0.62 percentage points, and preterm births decline by 0.82 percentage points. Translating these results into the number of births, WIC EBT lifts 6,774[12] (2,456) births by mothers with no more than a high school education (mothers without an infant's father documented) out of low birth weight each year, and 7,621 (3,248) births out of preterm status annually.

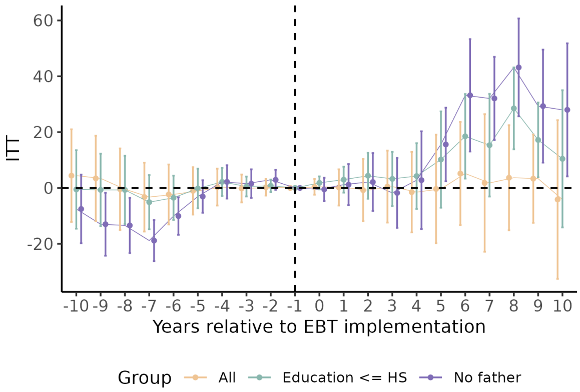
TABLE 5: EFFECTS OF WIC EBT ON BIRTH OUTCOMES

	Birth weight (grams)			Low birth weight (birth weight < 2500 grams)			Preterm (gestation < 37 weeks)		
	All births (1)	Edu$\leq$HS (2)	No father (3)	All births (4)	Edu$\leq$HS (5)	No father (6)	All births (7)	Edu$\leq$HS (8)	No father (9)
Born after EBT	-0.1545	4.532	4.812	-0.0009	-0.0031	-0.0040	-0.0012	-0.0035	-0.0053
	(2.269)	(2.812)	(3.894)	(0.0008)	(0.0012)***	(0.0019)**	(0.0011)	(0.0015)**	(0.0024)**
	⟨4.955⟩	⟨3.600⟩	⟨4.441⟩	⟨0.0016⟩	⟨0.0010⟩***	⟨0.0015⟩**	⟨0.0020⟩	⟨0.0013⟩***	⟨0.0019⟩***
Observations	28,021	27,482	26,224	28,021	27,482	26,224	28,023	27,485	26,227
R^2	0.8865	0.8324	0.6471	0.7092	0.6458	0.4203	0.6996	0.6335	0.4292
Dep. var. mean	3,269	3,217	3,121	0.0808	0.0913	0.1224	0.1153	0.1308	0.1629

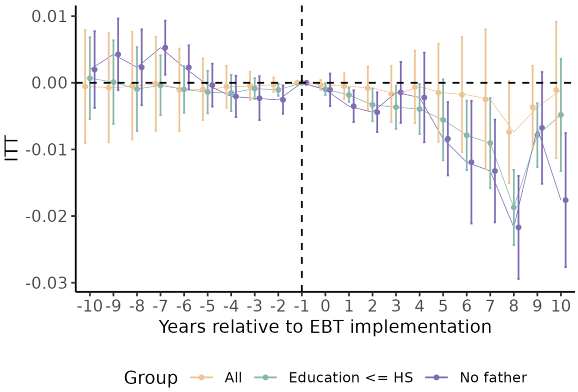
Notes: We report interaction weighted estimators proposed by Sun and Abraham (2021). We collapse birth data to county-of-maternal-residence-by-year-of-birth cells. We control for county and year fixed effects, census-region-specific linear time trend, county baseline characteristics from 2006-2009 interacted with linear time trend, and county-by-year employment rate. Regressions and dependent variable mean are weighted by the number of births in each cell. We report standard errors clustered on county in parentheses and standard errors clustered on state in angle brackets. ***, **, and * indicate that t-test are significant at the 1%, 5%, and 10% levels.

[12] This figure is calculated by multiplying the average number of births per year by the TOT effect of EBT.

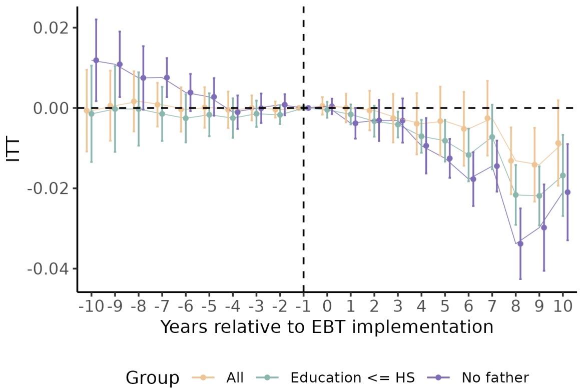
Figures 6a–6c indicate that pre-implementation trends are flat for the full sample and for mothers with no more than a high school education, suggesting no prior systematic changes in outcomes prior to EBT implementation. The effects observed for mothers without an infant's father documented may be potentially influenced by pre-existing trends. However, these trends occur well before the EBT implementation and do not fully account for the observed impacts. We also examine the sensitivity of these outcomes to potential violations of the parallel trend assumption in Section 7.6.



(A) Birth weight



(B) Low birth weight



(C) Preterm

FIGURE 6: DYNAMIC EFFECTS OF WIC EBT ON BIRTH OUTCOMES

Notes: We estimate dynamic effects using interaction weighted estimators proposed by Sun and Abraham (2021). We collapse birth data to county-of-maternal-residence-by-year-of-birth cells. We control for county and year fixed effects, census-region-specific linear time trend, county baseline characteristics from 2006-2009 interacted with linear time trend, and county-by-year employment rate. Regressions and dependent variable mean are weighted by the number of births in each cell. Standard errors are clustered at state level.

How much do reduced adverse birth outcomes translate into hospital cost savings? Using estimates from Almond, Chay and Lee (2005)[13], we provide a back-of-the-envelope

[13] Almond, Chay and Lee (2005)'s estimates account for the omitted variable bias in the cross-sectional estimates reported by most of the scientific literature. However, they do not provide similar estimates for preterm

estimation of hospital cost savings associated with WIC EBT focusing solely on low birth weight. Table 6 shows that for mothers with no more than a high school education (mothers without an infant's father documented), the annual hospital cost savings are estimated at $4.92 million ($2.8 million). Given the USDA's $30.5 million investment in the EBT transition during the 2013 fiscal year (USDA Food and Nutrition Service, 2017), these savings from reduced low birth weight alone represent 21.78%[14] (12.39%) of the USDA's annual EBT investment. Given that improved birth outcomes have been linked to various long-run outcomes, such as higher educational attainment (Behrman and Rosenzweig, 2004) and adult income (Bharadwaj, Lundborg and Rooth, 2018), WIC EBT is likely to generate a positive net benefit in the long run.

TABLE 6: HOSPITAL COST SAVING OF WIC EBT ASSOCIATED WITH LOW BIRTH WEIGHT

Birth weight segment	Excess hospital costs per mother (in 2000 dollars)	Percentage of births in each birth weight segment (%)	
		Edu≤HS	No father
(1)	(2)	(3)	(4)
< 600 g	$61,213	0.26	0.46
600-800 g	$67,816	0.23	0.35
800-1000 g	$36,846	0.25	0.36
1000-1500 g	$22,309	0.81	1.14
1500-2000 g	$6,806	1.75	2.39
2000-2500 g	$604	5.84	7.55
Aggregated cost saved per mother		$742	$1,114
Hospital cost saved per year		$4.92 million	$2.8 million

Notes: Total hospital cost saved = aggregated cost saved per mother × average number of mothers per year × reduced likelihood of low birth weight due to WIC EBT (TOT). Thus, total hospital cost saved per year for mothers with no more than a high school education is: $742 × 1,411,305 × 0.0047 = $4,921,785; the number for mothers without an infant's father documented is: $1,142 × 396,125 × 0.0062 = $2,804,723.

7 Robustness

7.1 Results on advantaged mothers

We start by asking that whether advantaged mothers—defined as those with more than a high school education and a father listed on the infant's birth certificate—are less affected by WIC EBT implementation, given the variability observed in the full sample. Results in Table 7 show that, for advantaged mothers, the estimates are statistically significantly different from zero with standard errors clustered at the county level but are not statistically signif-

births.

[14] We convert $30.5 million to 2000 dollars by dividing it by 1.35. The calculation for 21.78% is given by: $\frac{4.92\times1.35}{30.5}$.

icant when clustered at the state level; the effect sizes for this group are also substantially smaller than those observed in high-impact groups.

TABLE 7: EFFECTS OF WIC EBT ON ADVANTAGED MMOTHERS

	(1)	(2)	(3)
Born after EBT	0.0074	0.0085	0.0086
	(0.0038)*	(0.0032)***	(0.0035)**
	⟨0.0114⟩	⟨0.0060⟩	⟨0.0060⟩
County fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Census region×year		✓	✓
Baseline char.×year		✓	✓
Employment rate$_{ct}$			✓
Observations	34,238	33,562	27,712
R^2	0.9402	0.9482	0.9474
Dep. var. mean	0.2181	0.2193	0.2279

Notes: Advantaged mothers have more than high school education and father of infant on birth certificate. We report interaction weighted estimators proposed by Sun and Abraham (2021). We collapse birth data to county-of-maternal-residence-by-year-of-birth cells. We control for county and year fixed effects, census-region-specific linear time trend, county baseline characteristics from 2006-2009 interacted with linear time trend, and county-by-year employment rate. Regressions and dependent variable mean are weighted by the number of births in each cell. We report standard errors clustered on county in parentheses and standard errors clustered on state in angle brackets. ***, **, and * indicate that t-test are significant at the 1%, 5%, and 10% levels.

7.2 Composition change

Change in the demographic composition of the cell might explain observed positive effect on WIC participation. Specifically, if over time the treated cell includes more mothers who are inclined to participate in WIC, this shift would lead to an increase in WIC participation. Table 8 shows that EBT implementation does not significantly alter the composition of the subpopulations of interest, with the exception of a slight decrease in white infants following EBT. This suggests that we are comparing mothers with similar characteristics across periods, allowing us to interpret our estimates as reflecting changes in outcomes among existing WIC-eligible mothers.

7.3 Placebo treatment timing

To ensure that the observed effect on WIC participation is not due to unrelated trends in the treated counties, we conduct a placebo test by estimating results based on hypothetical treatment timings rather than actual ones. Specifically, we re-estimate the effects as if the treatment had occurred five years earlier than it did[15]. If our results do not capture any

[15] There is no strict rule for determining how many years before the actual treatment year should be used as a placebo treatment year. Economists sometimes randomly select a year that is sufficiently distant from the actual treatment year, while other times they choose the middle year of the pre-treatment period. Here, we follow the latter approach. An example of this test can be found in Kose, O'Keefe and Rosales-Rueda (2024). Note that if

TABLE 8: COMPOSITION CHANGE

	Maternal characteristics used to define subgroups			Other maternal characteristics						
	Edu $\leq$ HS	No father	Adv. mothers	Age $\leq$ 22	College graduates	Unmarried	White	Black	Asian	Hispanic
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Born after EBT	-0.0003	8×10^{-6}	-0.0015	-0.0023	0.0042	0.0002	-0.0156	0.0143	0.0035	0.0033
	(0.0031)	(0.0027)	(0.0032)	(0.0019)	(0.0029)	(0.0042)	(0.0087)*	(0.0081)*	(0.0067)	(0.0029)
	⟨0.0037⟩	⟨0.0054⟩	⟨0.0034⟩	⟨0.0039⟩	⟨0.0059⟩	⟨0.0046⟩	⟨0.0056⟩***	⟨0.0115⟩	⟨0.0081⟩	⟨0.0103⟩
Observations	28,014	28,023	28,019	28,023	28,014	28,022	28,023	28,023	28,023	28,023
R^2	0.9632	0.8992	0.9642	0.9590	0.9798	0.9245	0.9767	0.9217	0.8846	0.9926
Dep. var. mean	0.4028	0.1114	0.5611	0.1812	0.3119	0.4027	0.6482	0.1366	0.0613	0.2086

Notes: Advantaged mothers (adv. mothers) have more than high school education and father of infant on birth certificate. We collapse birth data to county-of-maternal-residence-by-year-of-birth cells. We control for county and year fixed effects, census-region-specific linear time trend, county baseline characteristics from 2006-2009 interacted with linear time trend, and county-by-year employment rate. Regressions and dependent variable mean are weighted by the number of births in each cell. We report standard errors clustered on county in parentheses and standard errors clustered on state in angle brackets. ***, **, and * indicate that t-test are significant at the 1%, 5%, and 10% levels.

spurious trends in the treated counties, we should observe no significant effects based on these hypothetical timings. Table A3 confirms this expectation: the pseudo-treatment effects are statistically insignificant, small in magnitude, and occasionally have the opposite sign, ruling out the possibility that our results are driven by spurious pre-trends.

7.4 Randomization test

To assess the robustness of our results against random noise, we compute Intent-to-Treat (ITT) effects using randomized pseudo-treatment timings. We randomly assign the year of WIC EBT implementation 1,000 times while maintaining the original distribution of rollout years[16]. This randomization test is conducted for effects on WIC participation for mothers with high school education or less and for mothers without documented fathers of infants. The estimated effects in our main analysis consistently fall well into the tails of the distribution of the simulated effects, suggesting that our findings are not likely the results of random noise (Figure A2).

the placebo test passes (i.e., no effect is found), it adds to the confidence in the validity of the original findings; if the placebo test fails (i.e., an effect is found), it raises concerns about the reliability of the original results. We do not claim that a passing placebo test directly validates the original findings.

[16]The randomization test, which traces its origins to Fisher (1936), is widely used as a placebo test in applied research such as Adukia, Asher and Novosad (2020) and Kose, O'Keefe and Rosales-Rueda (2024).

7.5 Event-time balanced panel

Another concern with our main results is the unbalanced panel of treated counties over event time, which could mean that our results are influenced by changes in the composition of counties across event time. However, estimates from a balanced panel also have limitations. Due to the broad distribution of EBT implementation across states, constructing a balanced panel requires choosing between the number of pre- and post-periods and the number of counties included in the estimation. Maximizing the former would significantly reduce the sample size, while maximizing the latter would limit our ability to observe extended pre-trends and longer-term dynamic effects. Despite these trade-offs, Table 9 presents results for a balanced panel from period -4 to period 4, which align with our main results. In this balanced panel, the effects on WIC participation are larger and more precise. The dynamic effects based on this balanced panel are shown in Figure 7, which are also consistent with our previous findings. However, this balanced panel includes only 844 counties, far fewer than the 2,489 counties used in our main specification. Although we prefer to use all available data in our main specification, the balanced panel results provide evidence that our findings are not driven by changes in the composition of counties over event time.

TABLE 9: EFFECTS OF WIC EBT ON WIC PARTICIPATION, EVENT-TIME BALANCED PANEL

	WIC participation		
	All births (1)	Edu≤HS (2)	No father (3)
Born after EBT	0.0157 (0.0057)*** ⟨0.0048⟩***	0.0284 (0.0091)*** ⟨0.0049⟩***	0.0279 (0.0096)*** ⟨0.0053⟩***
Observations	7,103	6,905	6,603
R^2	0.95951	0.91695	0.84913
Dep. var. mean	0.37964	0.60518	0.65629

Notes: We report interaction weighted estimators proposed by Sun and Abraham (2021). We collapse birth data to county-of-maternal-residence-by-year-of-birth cells. We control for county and year fixed effects, census-region-specific linear time trend, county baseline characteristics from 2006-2009 interacted with linear time trend, and county-by-year employment rate. Regressions and dependent variable mean are weighted by the number of births in each cell. We report standard errors clustered on county in parentheses and standard errors clustered on state in angle brackets. ***, **, and * indicate that t-test are significant at the 1%, 5%, and 10% levels.

7.6 Sensitivity to parallel trend violation

Some of our estimates of dynamic effects might be influenced by pre-existing differential trends, potentially compromising identification. We assess the sensitivity of our results to violations of the parallel trends assumption using the procedure proposed by Rambachan and Roth (2023). Our focus is on the dynamic effects on outcomes for two groups more likely to be WIC-eligible. Figure A3 presents the maximum deviation from the parallel trends

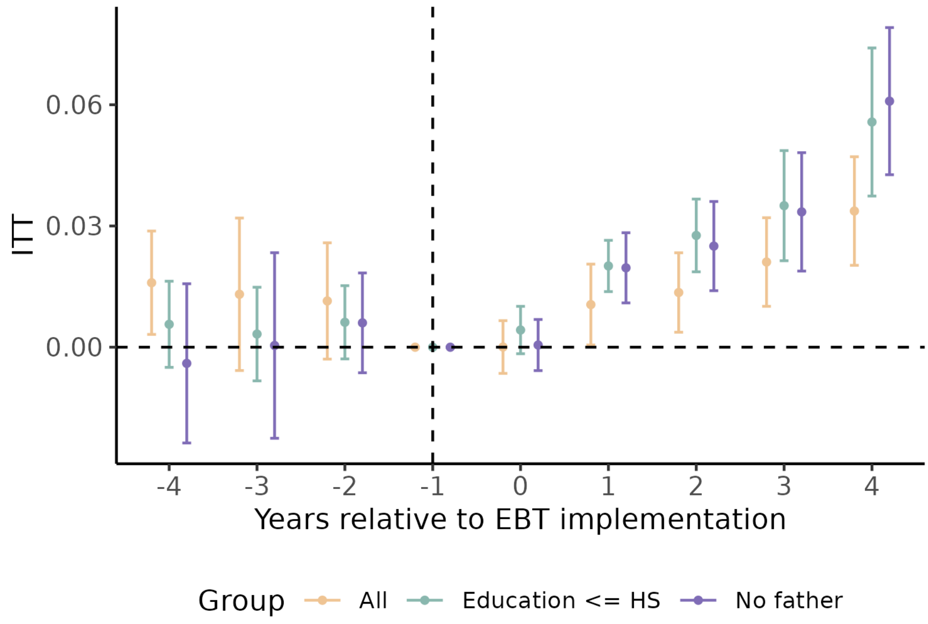


FIGURE 7: DYNAMIC EFFECTS OF WIC EBT ON WIC PARTICIPATION, EVENT-TIME BALANCED PANEL

Notes: This event study plots report results using estimators by Sun and Abraham (2021). We collapse birth data to county-of-maternal-residence-by-year-of-birth cells. Regressions and dependent variable mean are weighted by the number of births in each cell. Standard errors are clustered at state level. Since this estimator use all the whole pre-treatment period as comparison, we use shorter pre-treatment period (6 years before the treatment) to ensure the relevance.

assumption that we can tolerate while still claiming significant effects at a 10% significance level. For mothers with a high school education or less (and mothers without documented fathers of infants), the breakdown values are 0.14 (0.12) for WIC participation. This implies that our results remain robust at the 10% significance level unless we allow for the linear extrapolation across consecutive periods to deviate by more than these breakdown values.

In terms of birth outcomes, Figure A4 show that, for mothers with a high school education or less (and mothers without documented fathers of infants), the breakdown values are greater than 0.2 (0.1) for the probability of having low-birth-weight infants and 0.16 (0.12) for the probability of preterm births. The sensitivity for birth outcomes is comparable to that observed for WIC participation.

7.7 Robustness to estimation methods

We also present results using alternative staggered difference-in-difference methods, including traditional two-way fixed effects estimators (Figure A5a), estimators from Callaway and Sant'Anna (2021) using never-treated or not-yet-treated groups as the control group (Figures

A5b and A5c), and imputation estimators by Borusyak, Jaravel and Spiess (2024) (Figure A5d). While these estimators are not directly comparable due to differences in comparison groups, periods, and methods of accounting for covariates (Roth et al., 2023), we find that these alternative estimators are broadly consistent with our baseline results using the Sun and Abraham (2021) approach.

7.8 Robustness to timing of exposure

Finally, we examine the robustness of our results to the timing of exposure. In our baseline results, infants are considered treated if they are born after EBT implementation. However, this may attenuate our estimates since mothers of infants born shortly after EBT implementation might not have had enough time to obtain WIC authorization if they did not anticipate its arrival. This concern is valid, as 50% of pregnant participants certify in the first trimester, 40% in the second, and only 10% in the third (Thorn et al., 2016). In Table A4, we present estimates defining exposure at the beginning of the first, second, or third trimester instead of at the time of birth. Estimates generally become larger and more precise, as we change the definition of exposure.

8 Potential Mechanisms

In this section, we examine two potential effect mechanisms outlined in Section 3: the welfare stigma and the vendor access. Results suggest that both channels are at play, and that the observed positive effects on WIC participation are likely driven by a reduction in welfare stigma, which outweighs the effect of reduced accessibility to WIC vendors.

Examining the effect of EBT on welfare stigma is challenging, as it ideally requires direct data on welfare stigma. However, such data is not available on a large scale. Instead, we leverage the heterogeneity in WIC participation effects across urban and rural areas to provide suggestive evidence that reduced welfare stigma may be a contributing mechanism. Sociological studies suggest that welfare stigma tends to be more pronounced in rural communities (Findeis et al., 2001; Meij, Haartsen and Meijering, 2020),. For example, Findeis et al. (2001) find that smaller, more integrated networks can amplify the stigma attached to needing help, which may diminish families' willingness to participate in welfare programs[17].

Table 10 shows that the effects on WIC participation are substantially larger and more precise in rural areas, consistent with hypothesis that welfare stigma is larger in these regions. However, it is challenging to determine how much of the net positive effects of EBT on WIC participation can be attributed to reduced welfare stigma versus other mechanisms

[17] Findeis et al. (2001) note that rural families worry that accepting welfare could harm their family reputation, which is important for securing work opportunities in rural communities.

not captured in the simplified conceptual framework in Section 3, particularly given the lack of large-scale data on welfare stigma. One concern is that the larger effects observed in rural areas could be driven by a stronger response from poorer individuals. However, disentangling the effects of poverty from those of welfare stigma remains difficult.

TABLE 10: EFFECTS OF WIC EBT ON WIC PARTICIPATION IN URBAN AND RURAL AREAS

	Urban areas			Rural areas		
	All births (1)	Edu≤HS (2)	No father (3)	All births (4)	Edu≤HS (5)	No father (6)
Born after EBT	0.0103	0.0127	0.0148	0.0305	0.0295	0.0223
	(0.0059)*	(0.0089)	(0.0080)*	(0.0059)***	(0.0071)***	(0.0089)**
	⟨0.0087⟩	⟨0.0088⟩	⟨0.0055⟩**	⟨0.0049⟩***	⟨0.0053⟩***	⟨0.0063⟩***
Observations	8,904	8,742	8,549	19,118	18,742	17,677
R^2	0.9729	0.9472	0.9044	0.9316	0.8877	0.6649
Dep. var. mean	0.3976	0.6515	0.6600	0.4768	0.6512	0.7287

Notes: Urban and rural areas are defined by NCHS 2006 Urban-Rural Classification Scheme for Counties. We report interaction weighted estimators proposed by Sun and Abraham (2021). We collapse birth data to county-of-maternal-residence-by-year-of-birth cells. We control for county and year fixed effects, census-region-specific linear time trend, county baseline characteristics from 2006-2009 interacted with linear time trend, and county-by-year employment rate. Regressions and dependent variable mean are weighted by the number of births in each cell. We report standard errors clustered on county in parentheses and standard errors clustered on state in angle brackets. ***, **, and * indicate that t-test are significant at the 1%, 5%, and 10% levels.

To provide evidence of reduced WIC vendor access, we linked WIC EBT rollout data to WIC Integrity Profiles from 2009–2016 to assess the impact of WIC EBT on the net number of authorized WIC vendors each year. The WIC Integrity Profiles, a restricted-use administrative dataset provided by USDA FNS, contains the name and address of all authorized vendors by fiscal year. We convert fiscal years to calendar years to align with our previous analyses and then aggregate the vendor-level data by county and year. All regressions and mean calculations for the dependent variable are weighted by county population.

Table 11 shows that WIC EBT reduces both the total and per capita number of WIC vendors in urban and rural areas, consistent with findings reported by Ambrozek et al. (2024) and Meckel (2020). The decline in the number of WIC vendors is smaller in rural areas, which could be due to the fact that rural areas already have fewer total WIC vendors or fewer vendors per capita to begin with. Reduced WIC vendor access suggests that the observed positive effects of EBT on WIC participation are driven by other mechanisms, potentially including reduced welfare stigma discussed in Section 3.

Table 11: Effects of WIC EBT on WIC vendors

	Urban areas		Rural areas	
	Number of WIC vendors	Number of WIC vendors per person	Number of WIC vendors	Number of WIC vendors per person
	(1)	(2)	(3)	(4)
WIC EBT implementation	-3.527	-0.0247	-0.4929	-0.0027
	(2.142)*	(0.0077)***	(0.0970)***	(0.0008)***
	⟨2.914⟩	⟨0.0105⟩**	⟨0.0897⟩***	⟨0.0010⟩***
Observations	5,662	5,662	11,349	11,349
R^2	0.9933	0.5729	0.9753	0.9431
Dep. var. mean	126.0577	0.1167	5.3509	0.0368

Notes: We report interaction weighted estimators proposed by Sun and Abraham (2021). We control for county and year fixed effects, census-region-specific linear time trend, county baseline characteristics from 2006-2009 interacted with linear time trend, and county-by-year employment rate. Regressions and dependent variable mean are weighted by county-by-year population. We report standard errors clustered on county in parentheses and standard errors clustered on state in angle brackets. ***, **, and * indicate that t-test are significant at the 1%, 5%, and 10% levels.

9 Magnitudes

We compare our estimates on WIC participation with those of other papers that estimate the effect of WIC EBT on participation in individual states. Meckel (2020) finds a decline in the average number of mothers participating in WIC after the introduction of EBT, based on the birth certificate data from Texas, where EBT transition occurred between June 2005 and March 2009. In contrast, our nationwide estimates are slightly smaller than those reported by Li, Saitone and Sexton (2022), who find an 8.54-percentage-point increase in WIC participation based on WIC enrollment data from Oklahoma, where EBT transition occurred between February and August 2016. Our results are bounded between existing estimates of the effect of WIC EBT on WIC participation from individual states, which is reasonable given that we estimate an average nationwide effect rather than state-specific effects. The cohort-specific estimates in Table A1 also support the existence of heterogeneity in the effects of EBT across states that adopted the program at different times. However, no cohort exhibits a comparable negative decline in WIC participation to that observed in Texas.

We find that negative effects of EBT on WIC births reported by Meckel (2020) are likely to be driven by pre-existing trends, as shown in Figures A6a-A6d. When we replicate the event study estimates from (Meckel, 2020), the dynamic effects with a larger event window (36 months before and after EBT implementation) suggest a decreasing trend in the number of WIC births in treated counties versus control counties in the pre-period. We test sensitivity of the results to violations of parallel trends and find that, with a difference in the trend of 0.08 percentage points between groups (or more), the magnitude of the effect found could be attributable to differences in trends rather than the effect of EBT (see Figures A7a-A7b).

An alternative explanation for Meckel (2020)'s results is access to federal support under the HHFKA of 2010, which has made retailers more inclined to continue participating in WIC. As discussed in Section 3, negative effects of WIC EBT on vendor accessibility are likely mitigated by the technical and financial support from USDA following the HHFKA of 2010 (USDA Food and Nutrition Service, 2016). As a result, we observe an overall positive impact of WIC EBT on WIC participation. Learning could also contribute to the positive effects we observed, even though we have limited knowledge about whether state agencies and WIC vendors learned from early adopters.

10 Discussion and Conclusion

10.1 Limitations

Our approach has some important limitations. The first is that the natality data will not measure WIC participation among those who enter WIC after the birth certificate is filed. This may include some older siblings who enroll at the time that the pregnant individual or newly born infant joins the program. Accordingly, we are more accurately capturing changes in participation for pregnant and postpartum individuals and newly born infants, rather than children who were on the program when WIC EBT was implemented. Rates of participation have been falling fast for children and children are the largest total participating group in WIC at any time, so that understanding children's participation is still important. On the other hand, our results are directly comparable to previous work that has used natality data to measure WIC participation. Also, nutritionists and public health experts often attempt to target pregnant people and infants given the importance of nutrition in the "first 1000 days" for later life outcomes. Ensuring participation among eligible pregnant individuals and infants covers a substantial portion of the first thousand days window.

The second limitation is that we measure EBT timing at the year level with a binary treatment variable indicating whether or not the county had any EBT implementation during the year. This binary measure aggregated up over time induces some non-classical measurement error into our treatment variable, which may bias our results. We note that in our case we have only false positives – indicating that a county has EBT when EBT has not occurred yet – so that our TOT estimates in a classical DiD set up will be attenuated (Nguimkeu, Denteh and Tchernis, 2019). The Sun and Abraham (2021) approach constructs a series of classical DiD estimates and aggregates, so we speculate that this attenuation effect holds.

Another limitation of the data is that not all counties report natality data. As mentioned in Section 4, the observable characteristics of our sample of births in the natality data are close in magnitude to a comparison population in the CPS ASEC and SIPP. However, our sample may still not represent the full population. Additionally, WIC status information is only available for some states in the natality data starting from 2009, with other states beginning to report mothers' WIC status a few years later. This limits our study period to after 2008, which coincides with the passage of the HHKAT in 2010. Therefore, our results should be interpreted as estimates of the effects of WIC EBT in the context of available USDA support. However, we believe our estimates remain important, as most counties implemented WIC EBT after 2008.

10.2 Summary

In this paper, we combine Vital Statistics Natality Data from 2009-2021 with county-level data on the rollout of WIC EBT across all states. We construct the first national estimates of the effect of WIC EBT on WIC participation. This advances our understanding of the effects of a major policy change in WIC on WIC participants. We find increased WIC participation and a decline in adverse birth outcomes, on average, among groups more likely to be WIC-eligible following EBT implementation.

As noted above, our data and approach allow us to capture the effects of WIC EBT on participation across nearly the whole country and for a much longer period of time. We consider our average treatment effect on the treated estimates to be more representative of the net effect of EBT. We are also able to measure WIC participation accurately with natality data (relative to survey data). Across our main results and the sensitivity and robustness checks we find significant and positive effects of WIC EBT on WIC participation and birth outcomes among the more likely WIC-eligible individuals. Finally, we provide suggestive evidence that the observed positive effects on WIC participation are likely driven by reduced welfare stigma, which outweighs the effect of reduced accessibility to WIC vendors. Our results capture the impacts of WIC EBT following the HHKFA of 2010, when the USDA was required to provide both technical and financial support to state agencies for the EBT transition. We believe that this helps explain why our findings differ from those of Meckel (2020) but are consistent with Li, Saitone and Sexton (2022), among other potential explanations.

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Appendix

A Figures and tables

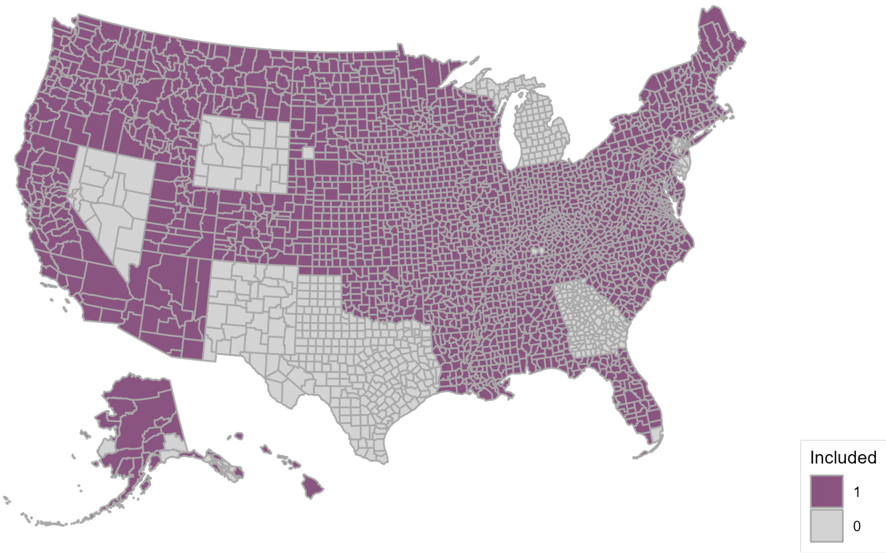


FIGURE A1: COUNTIES IN OUR SAMPLE

TABLE A1: COHORT-SPECIFIC EFFECTS OF EBT ON WIC PARTICIPATION

	WIC participation		
	All births (1)	Edu≤HS (2)	No father (3)
Cohort = 2011	0.0134	0.0094	0.0103
	(0.0093)	(0.0062)	(0.0066)
Cohort = 2013	0.1730***	0.1470***	0.0969***
	(0.0165)	(0.0071)	(0.0111)
Cohort = 2014	-0.0008	0.0077	-0.0005
	(0.0211)	(0.0120)	(0.0096)
Cohort = 2015	-0.0027	0.0198	-0.0026
	(0.0247)	(0.0151)	(0.0170)
Cohort = 2016	0.0273**	0.0404***	0.0430***
	(0.0126)	(0.0123)	(0.0125)
Cohort = 2017	0.0189**	0.0204*	0.0266**
	(0.0092)	(0.0109)	(0.0111)
Cohort = 2018	0.0087	0.0085	0.0218**
	(0.0073)	(0.0097)	(0.0101)
Cohort = 2019	-0.0045	-0.0131	-0.0067
	(0.0085)	(0.0126)	(0.0068)
Cohort = 2020	0.0138	0.0086	0.0149
	(0.0100)	(0.0119)	(0.0120)
Cohort = 2021	-0.0141	-0.0099	-0.0278
	(0.0151)	(0.0238)	(0.0167)
Observations	28,023	27,485	26,227
R^2	0.9637	0.9290	0.8520
Dep. var. mean	0.4118	0.6514	0.6747

Notes: We report interaction weighted estimators proposed by Sun and Abraham (2021). We collapse birth data to county-of-maternal-residence-by-year-of-birth cells. We control for county and year fixed effects, census-region-specific linear time trend, county baseline characteristics from 2006-2009 interacted with linear time trend, and county-by-year employment rate. Regressions and dependent variable mean are weighted by the number of births in each cell. ***, **, and * indicate that t-test are significant at the 1%, 5%, and 10% levels. Standard errors are clustered at state level.

TABLE A2: HETEROGENEITY BY MATERNAL RACE, ETHNICITY, AGE, BIRTH ORDER, AND INCOME QUANTILES

	White	Black	Asian	Hispanic	Non-Hispanic	Age $\leq$ 22	22 < Age < 30	Age $\geq$ 30
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Born after EBT	0.0102	-0.0009	-0.0013	0.0106	0.0114	0.0185	0.0168	0.0095
	(0.0031)***	(0.0043)	(0.0047)	(0.0114)	(0.0047)**	(0.0065)***	(0.0057)***	(0.0046)**
	⟨0.0030⟩***	⟨0.0041⟩	⟨0.0044⟩	⟨0.0125⟩	⟨0.0078⟩	⟨0.0076⟩**	⟨0.0079⟩**	⟨0.0076⟩
Observations	23,758	17,880	16,205	24,378	27,994	27,097	27,592	27,507
R^2	0.9706	0.9157	0.9212	0.9261	0.9646	0.8814	0.9411	0.9515
Dep. var. mean	0.3936	0.6358	0.2992	0.6388	0.3508	0.7091	0.4374	0.2659

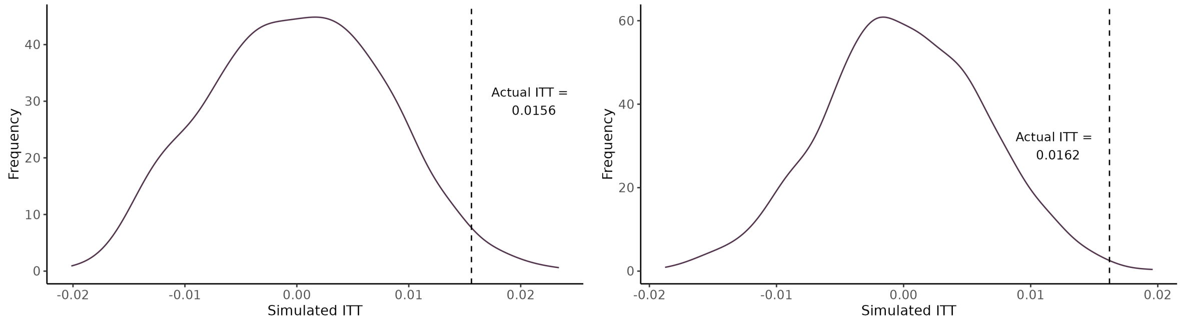
	First birth	Not first birth	Low-income counties	High-income counties
	(9)	(10)	(11)	(12)
Born after EBT	0.0119	0.0119	0.0283	0.0032
	(0.0052)**	(0.0056)**	(0.0056)***	(0.0070)
	⟨0.0101⟩	⟨0.0100⟩	⟨0.0033⟩***	⟨0.0103⟩
Observations	27,502	27,828	18,195	9,827
R^2	0.9513	0.9581	0.9317	0.9699
Dep. var. mean	0.4016	0.4160	0.5106	0.3780

Notes: The high-income counties includes the ones where the average income between 2006 and 2008 falls within the top income quantile (1,945 counties). All other counties are categorized as the low-income counties (1,133 counties). We report interaction weighted estimators proposed by Sun and Abraham (2021). We collapse birth data to county-of-maternal-residence-by-year-of-birth cells. We control for county and year fixed effects, census-region-specific linear time trend, county baseline characteristics from 2006-2009 interacted with linear time trend, and county-by-year employment rate. Regressions and dependent variable mean are weighted by the number of births in each cell. We report standard errors clustered on county in parentheses and standard errors clustered on state in angle brackets. ***, **, and * indicate that t-test are significant at the 1%, 5%, and 10% levels.

TABLE A3: PLACEBO TREATMENT TIMING

	WIC participation		
	All births	Edu≤HS	No father
	(1)	(2)	(3)
Born after EBT	-0.0020	0.0050	0.0007
	(0.0050)	(0.0055)	(0.0054)
	⟨0.0079⟩	⟨0.0067⟩	⟨0.0056⟩
Observations	28,020	27,482	26,224
R^2	0.9636	0.9282	0.8506
Dep. var. mean	0.4118	0.6514	0.6747

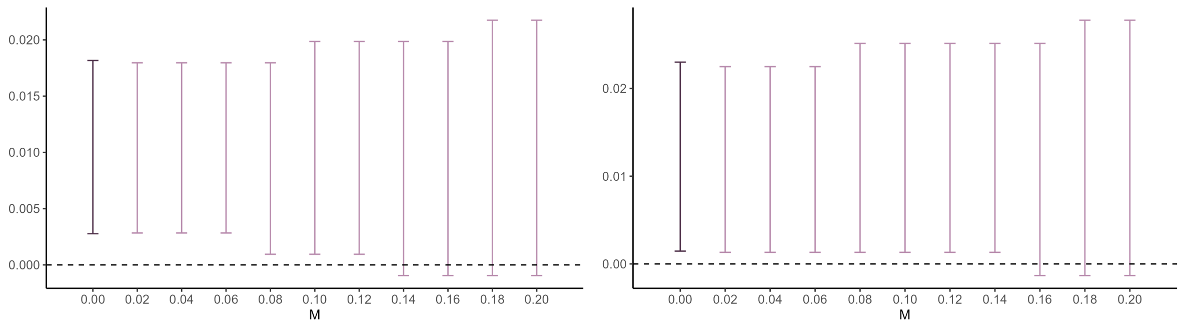
Notes: We report interaction weighted estimators proposed by Sun and Abraham (2021). We collapse birth data to county-of-maternal-residence-by-year-of-birth cells. We control for county and year fixed effects, census-region-specific linear time trend, county baseline characteristics from 2006-2009 interacted with linear time trend, and county-by-year employment rate. Regressions and dependent variable mean are weighted by the number of births in each cell. We report standard errors clustered on county in parentheses and standard errors clustered on state in angle brackets. ***, **, and * indicate that t-test are significant at the 1%, 5%, and 10% levels.



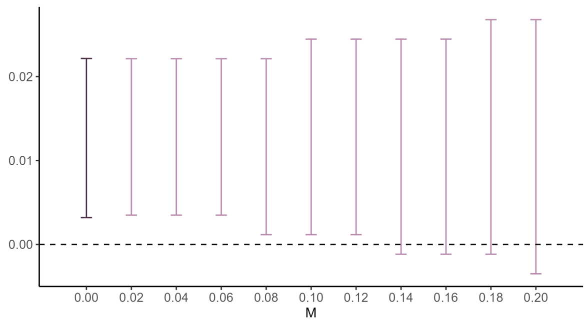
(A) WIC participation, education ≤ HS (B) WIC participation, no father

FIGURE A2: RANDOMIZATION TEST

Notes: These event study plots report results using estimators by Sun and Abraham (2021). We randomize year of EBT implementation 1,000 times while keep the distribution. We collapse birth data to county-of-maternal-residence-by-year-of-birth cells. Regressions and dependent variable mean are weighted by the number of births in each cell. Standard errors are clustered at county level. We enforce balanced panel. We do not allow covariates because we do not know the set of covariates that can correctly specify either the outcome evolution for the comparison group or the propensity score model.



(A) All births (B) Edu ≤ HS



(C) No father

FIGURE A3: TESTING SENSITIVITY TO PARALLEL TREND VIOLATION

Notes: These figures presents the results of sensitivity of parallel trend assumption proposed by Rambachan and Roth (2023).

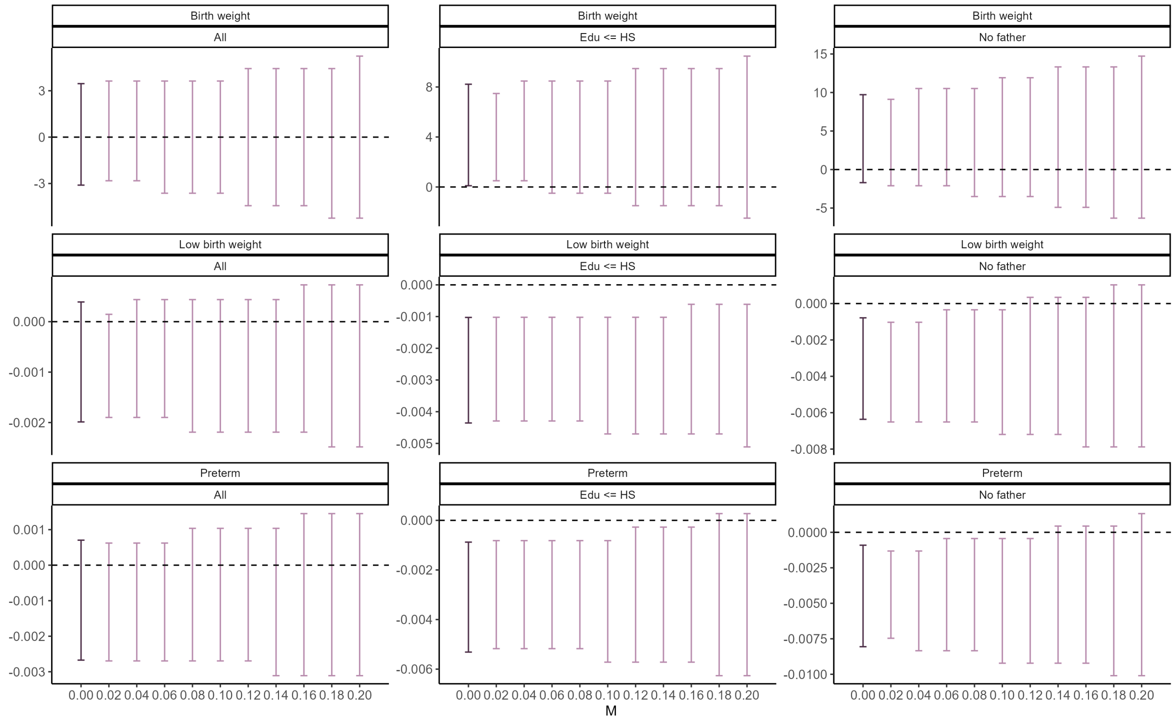


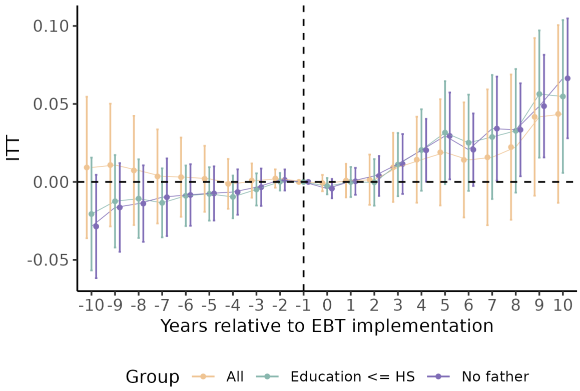
FIGURE A4: TESTING SENSITIVITY TO PARALLEL TREND VIOLATION, BIRTH OUTCOMES

Notes: These figures presents the results of sensitivity of parallel trend assumption proposed by Rambachan and Roth (2023).

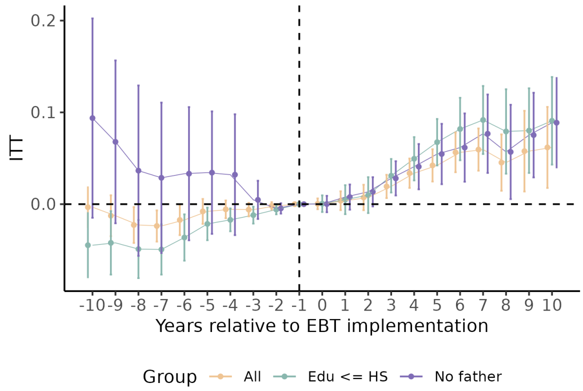
TABLE A4: ROBUSTNESS TO TIMING OF EXPOSURE

	First trimester			Second trimester			Third trimester		
	All births (1)	Edu≤HS (2)	No father (3)	All births (4)	Edu≤HS (5)	No father (6)	All births (7)	Edu≤HS (8)	No father (9)
Born after EBT	0.0161	0.0213	0.0241	0.0143	0.0189	0.0212	0.0135	0.0186	0.0211
	(0.0047)***	(0.0067)***	(0.0059)***	(0.0049)***	(0.0070)***	(0.0060)***	(0.0051)***	(0.0073)**	(0.0063)***
	⟨0.0089⟩*	⟨0.0097⟩**	⟨0.0067⟩***	⟨0.0089⟩	⟨0.0095⟩*	⟨0.0058⟩***	⟨0.0091⟩	⟨0.0092⟩**	⟨0.0055⟩***
Observations	28,340	27,904	26,713	28,320	27,905	26,789	28,291	27,892	26,738
R^2	0.9660	0.9305	0.8507	0.9655	0.9294	0.8505	0.9651	0.9290	0.8501
Dep. var. mean	0.4089	0.6488	0.6731	0.4100	0.6499	0.6740	0.4109	0.6508	0.6747

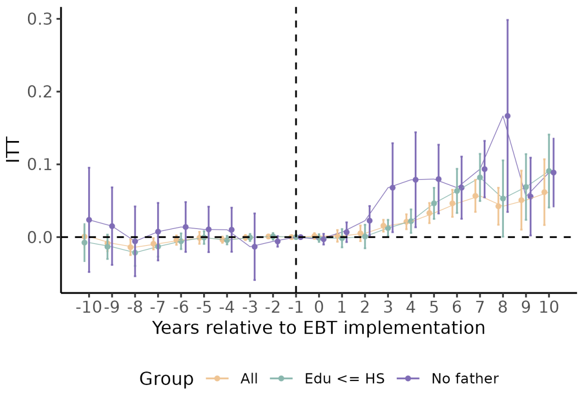
Notes: We report interaction weighted estimators proposed by Sun and Abraham (2021). The dependent variable is WIC participation rate for all regressions. We collapse birth data to county-of-maternal-residence-by-year-of-birth cells. We control for county and year fixed effects, census-region-specific linear time trend, county baseline characteristics from 2006-2009 interacted with linear time trend, and county-by-year employment rate. Regressions and dependent variable mean are weighted by the number of births in each cell. We report standard errors clustered on county in parentheses and standard errors clustered on state in angle brackets. ***, **, and * indicate that t-test are significant at the 1%, 5%, and 10% levels.



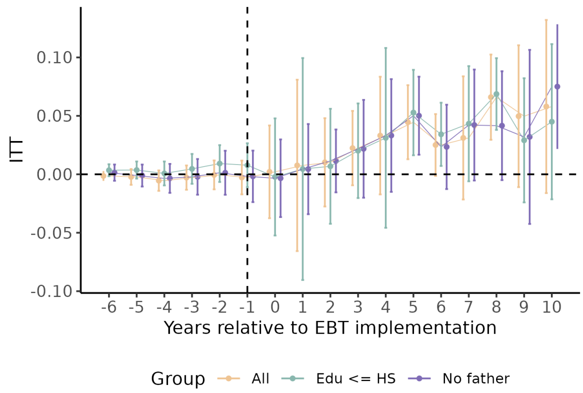
(A) Traditional TWFE estimators



(B) Callaway and Sant'Anna (2021) estimators, the never-treated as control group



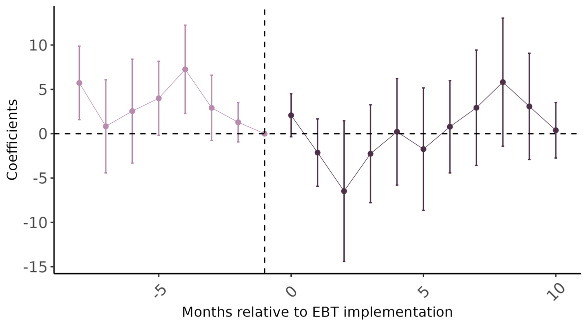
(C) Callaway and Sant'Anna (2021) estimators, the not-yet-treated as control group

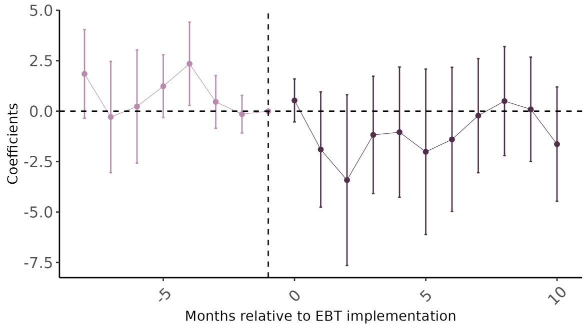


(D) Borusyak, Jaravel and Spiess (2024) estimators

FIGURE A5: DYNAMIC EFFECTS OF WIC EBT BY ESTIMATION METHODS

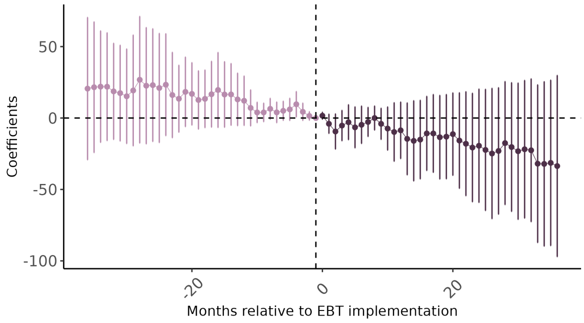
Notes: For all regressions, we collapse birth data to county-of-maternal-residence-by-year-of-birth cells; regressions and dependent variable mean are weighted by the number of births in each cell; and standard errors are clustered at state level. For traditional TWFE estimators, we control for county and year fixed effects, census-region-specific linear time trend, county baseline characteristics from 2006-2009 interacted with linear time trend, and county-by-year employment rate. For Callaway and Sant'Anna (2021) estimators, We enforce balanced panel. We do not allow covariates because we do not know the set of covariates that can correctly specify either the outcome evolution for the comparison group or the propensity score model. For Borusyak, Jaravel and Spiess (2024) estimators, we use shorter pre-treatment period (6 years before the treatment) to ensure the relevance since this estimator use all the whole pre-treatment period as comparison.

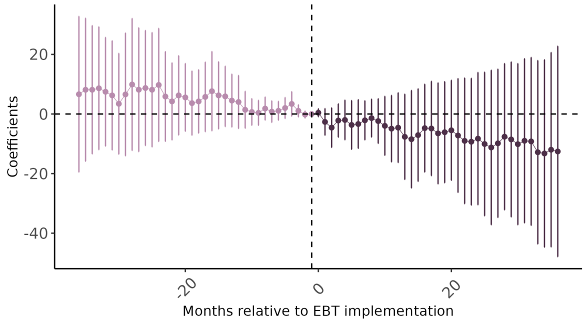




(A) EBT and WIC births per county (Figure 8 in Meckel (2020))

(B) EBT and high poverty WIC births per county (Figure 9 in Meckel (2020))



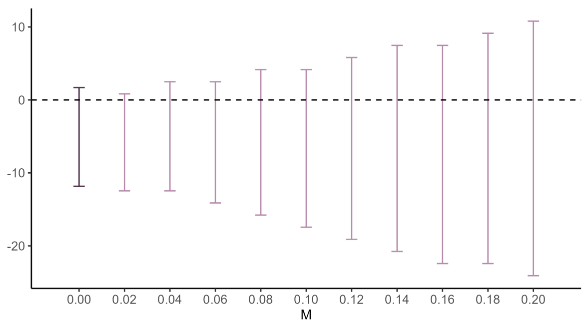


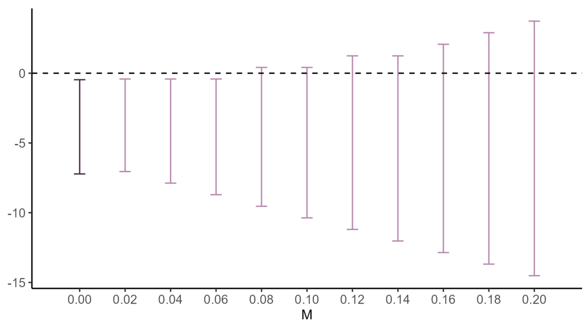
(C) EBT and WIC births per county, with a larger event window

(D) EBT and high poverty WIC births per county, with a larger event window

FIGURE A6: EXTENDING EVENT STUDY PLOTS IN MECKEL (2020) TO LARGER WINDOW

Notes: With a longer time series over which to estimate treatment effects, we can capture additional trends in the data. The short run pre-trends – within 6 months prior to WIC EBT implementation – appear relatively stable around zero. However, longer run pre-trends show a path that indicates WIC EBT timing may coincide with declining birth rates, picking up a spurious relationship.





(A) WIC Births

(B) High Poverty Share of WIC Births

FIGURE A7: TESTING SENSITIVITY TO PARALLEL TREND VIOLATION FOR MECKEL (2020)'S ESTIMATORS