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

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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 birthweight 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 reducing transaction costs and welfare stigma. Empirical studies of the effects of WIC EBT on participation have found mixed results, with studies often limited to one or a few states. Without detailed WIC participation data on a large scale, results are not 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 by 2.39-2.50 percentage points following EBT implementation. Additionally, we find that WIC EBT reduces adverse birth outcomes among infants born to less-educated mothers and those without fathers listed on their birth records. Our findings suggest that facilitating the delivery of public benefits can improve program uptake and well-being of beneficiaries.

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1 Introduction

The Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) provides nutritious 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). Total WIC participation decreased by 3 million individuals in the past decade. 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 payment reform was expected to encourage WIC participation among eligible individuals by reducing the stigma that participants experienced when redeeming WIC benefits (Moffitt, 1983). Also, 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) On the other hand, WIC EBT was also billed as a fraud reducing policy transition. Prior work examining the change in authorized WIC retailers post EBT finds that small stores in particular are less likely to be authorized post EBT, potentially affecting participant access (Meckel, 2020) It is not, therefore, obvious in which direction participation will change after WIC EBT. It is important to understand the effect that this policy change – the largest change to WIC in the past few decades – had on participation and participant's outcomes.

Empirical evidence to date that tests the effects of WIC EBT on participation shows mixed findings. 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 an individual state and a shorter-run time period. We contribute estimates of WIC EBT on WIC participation using data from counties implementing WIC before 2022. We estimate nationwide effects of WIC EBT on WIC participation, finding that EBT increased participation and reduced adverse birth outcomes, especially among populations that are likely WIC eligible. 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. Using the natality data avoids misreporting of WIC participation status from survey data (Meyer, Mok and Sullivan, 2015; Meyer and Mittag, 2019). We estimate our models using a staggered-adoption difference-in-diffences (DiD) approach, following the procedure from

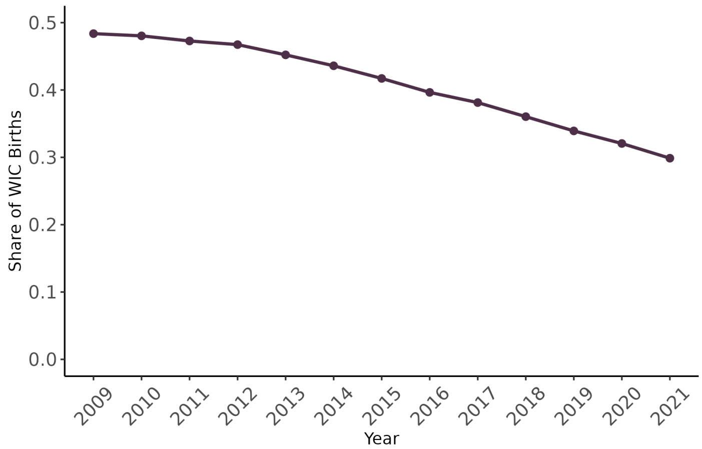


FIGURE 1: SHARE OF BIRTHS PARTICIPATING IN WIC

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

Sun and Abraham (2021). This approach allow us to disaggregate our treatment effect estimates to subgroups and time periods, to show how treatment effects vary across the country, given heterogeneous policy environments in different states, and across time, to evaluate both short-run and long-run outcomes. Overall, our results inform our understanding of take-up and benefits among participants in 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 EBT on participation rates (Meckel, 2020; Li, Saitone and Sexton, 2022), redemption patterns (Hanks et al., 2019), the retail environment (Meckel, 2020; Ambrozek et al., 2024), and crime rates (Wright et al., 2017) within the contexts of WIC, SNAP, or TANF. In her dissertation, Shiferaw (2020) found that SNAP EBT increased average birth weight in California. This paper extends this literature by providing national-scale evidence on 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, the infant health results of this paper contribute to the literature on the impacts of food assistance programs on infant health. 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 million 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). Conversely, when parents lose WIC benefits, they often compromise their own nutrition intake to preserve their

[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).

children's (Bitler et al., 2023).

Despite this 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 state agencies[3]. Although some pioneering WIC EBT projects began as early as 1995, the concept of a large-scale EBT transition plan was introduced in 2003, following the successful implementation of EBT systems in other federal food assistance programs, such as SNAP.

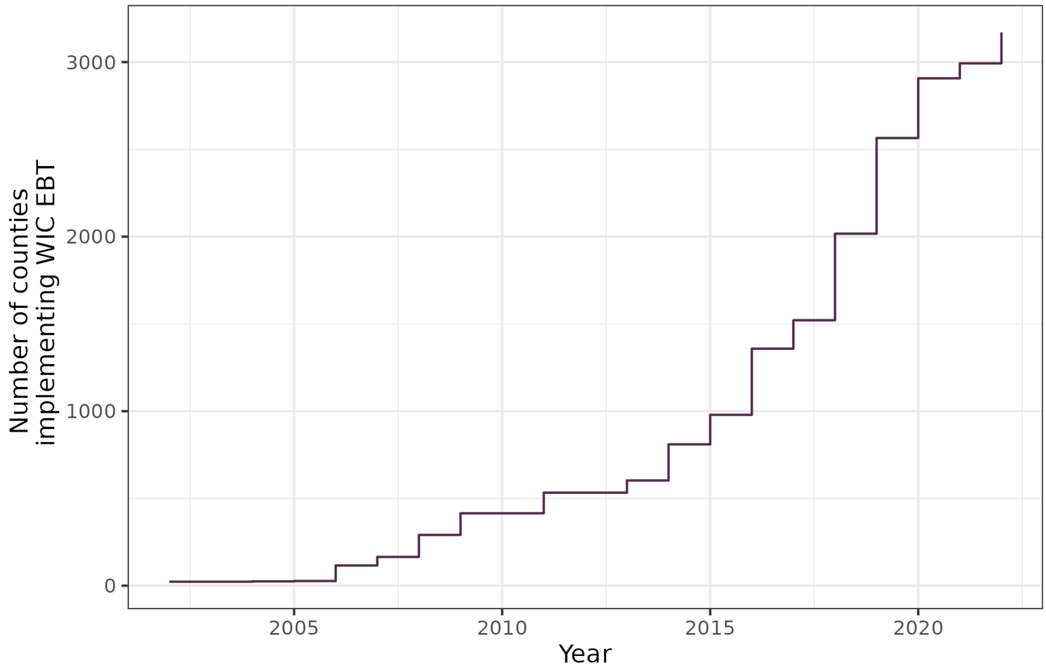
It was not until 2010, after the success of pilot programs in several states including Kentucky, Michigan, and Nevada, that the Healthy, Hunger-Free Kids Act (HHFKA 2010) launched a national mandate for the transition to EBT systems by October 1, 2020. This mandate provided a clear timeline for state WIC agencies nationwide[4]. 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[5]. 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 like 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).

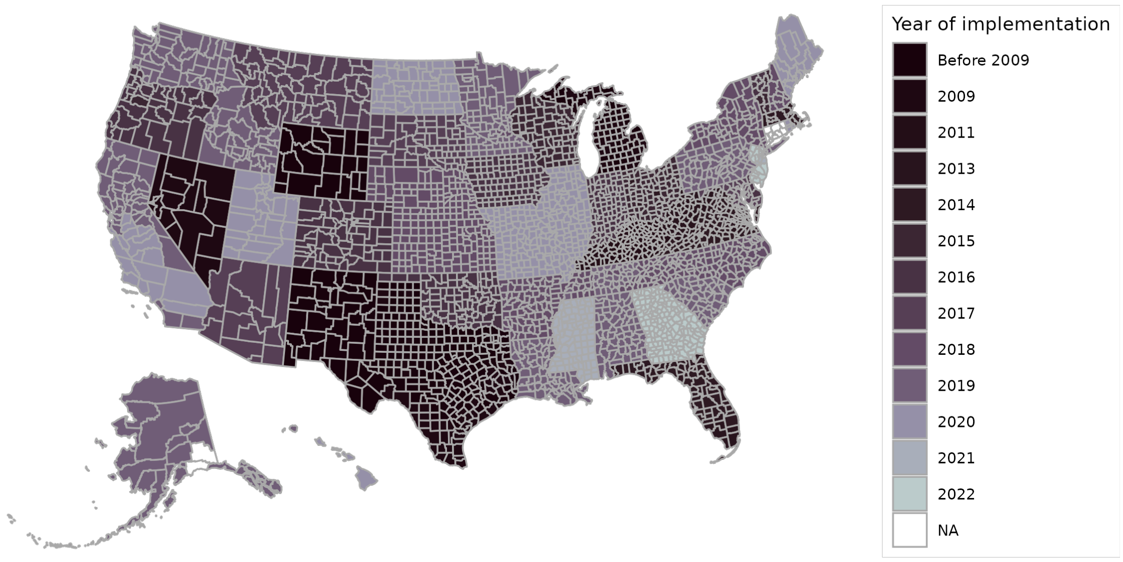
[3]Source: National WIC Association. https://media.nwica.org/2019-wic-ebt.pdf (Accessed on October 27, 2024).

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

[5]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 theoretically ambiguous. On one hand, EBT may encourage eligible individuals to participate in WIC by reducing welfare stigma and transaction costs. On the other hand, its anti-fraud features might drive stores away from the WIC program, thus discouraging participation as WIC stores become less accessible. This section outlines a simple framework to explore this dynamic.

We begin by considering a retailer j's decision to participate in WIC:

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

where κ_j represents the share of all WIC-eligible goods that retailer j sells. A retailer will join WIC if $\kappa_j \sum_i P_i Z_i > F_j$, meaning the revenue from WIC transactions must outweigh compliance costs. Thus, the probability S_j that a retailer participates in WIC can be expressed as:

$$S_j = \Pr\left(\kappa_j \sum_i P_i Z_i \geq F_j\right) = g\left(\kappa_j \sum_i P_i Z_i - F_j\right),$$

where $g(\cdot)$ is a function mapping the revenue-cost difference to store participation probability. The individual's time cost δ_i for redeeming WIC benefits depends on the availability of nearby WIC-participating retailers. Let $\bar{S}_i$ represent the average participation rate of retailers near individual i:

$$\bar{S}_i = \frac{1}{N} \sum_{j \in \text{vicinity of } i} g\left(\kappa_j \sum_i P_i Z_i - F_j\right).$$

Then, δ_i is a decreasing function of $\bar{S}_i$, as a higher probability of WIC-participating retailers nearby reduces the individual's travel or time burden. This relationship can be represented as:

$$\delta_i = \delta_0 + \eta(1 - \bar{S}_i) = \delta_i(P_i, \mathbf{F}_{\text{vicinity}}).$$

where $\mathbf{F}_{\text{vicinity}}$ represents a vector that includes all F_j values for retailers located near individual i. This vector captures the compliance costs faced by nearby stores, which can affect the local availability of WIC-participating retailers and, consequently, the individual's participation costs.

Next, we set up a utility maximization framework for a typical WIC-eligible consumer, modeling both consumer and retailer responses to EBT implementation. This setup aims to capture the dual effects of EBT: increased WIC participation through reduced stigma and transaction costs for consumers, and potentially decreased retailer participation due to compliance costs.

Following the approach in Manchester and Mumford (2010) and Li, Saitone and Sexton (2022), let U_i represent the utility for a WIC-eligible individual i, where utility depends on leisure L_i and consumption C_i. Let G_i be WIC-eligible goods, and Z_i be a composite bundle of all other goods. Then, consumption is represented as $C_i = G_i + P_i Z_i$, where $P_i \in [0, 1]$ represents WIC participation. WIC participation provides access to F at a subsidized or zero cost but may involve time and stigma costs. The individual has a time endowment, T, allocated to leisure L, work W, and time required for WIC redemption δ: $T = L_i + W_i + P_i \delta_i$. Let w denote the wage. If all income comes from work, then $C_i = w \cdot W + P_i Z_i$. The individual's utility is given by:

$$U(L_i, C_i) = U(L_i, C_i) - P_i \phi_i,$$

where ϕ captures total psychological disutility associated with welfare stigma.

For WIC participants, the optimal W_i and P_i maximize utility subject to the constraints $T = L_i + W_i + P_i \delta_i$ and $C_i = w \cdot W + P_i Z_i$. The optimal working time W_i^{WIC} and participation intensity P_i^{WIC} for WIC participants satisfy:

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

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

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

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

Thus, the probability of WIC participation for individual i is given by:

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

where

$$U^{WIC} = U(T - W_i^{WIC} - P_i^{WIC} \delta_i(P_i^{WIC}, \mathbf{F}_{\text{vicinity}}), w \cdot W_i^{WIC} + P_i^{WIC} Z_i) - P_i^{WIC} \phi_i,$$

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

Applying the envelope theorem, we obtain:

$$\frac{\partial U^{WIC}}{\partial \phi_i} = -P_i^{WIC} < 0,$$

$$\frac{\partial U^{WIC}}{\partial \mathbf{F}_{\text{vicinity}}} = -P_i^{WIC} \cdot \frac{\partial \delta_i}{\partial \mathbf{F}_{\text{vicinity}}} \cdot \frac{\partial U}{\partial L_i} < 0.$$

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

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 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. Second, we find no significant differences in observable characteristics between 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 mothers of infants (postpartum women) in the Survey of Income and Program Participation (SIPP). Table 1 shows that the differences in the proportions of Black and Hispanic mothers, educational backgrounds, and regional residence between the natality data and CPS ASEC, as well as between the natality data and SIPP, are within 5%.

We further validate our data by cross-referencing 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 before 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 (Figure 4).

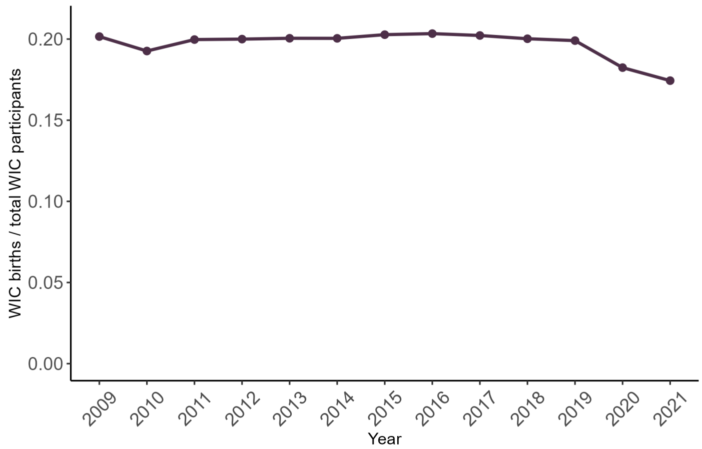


FIGURE 3: SHARE OF WIC PARTICIPANTS THAT SHOW UP IN OUR SAMPLE

Notes: Share of WIC participants that show up in our sample is calculated by dividing total number of WIC births in our sample by total WIC participants. 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 way-back machine to extract historical data.

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.

4.2 WIC EBT roll-out

We compiled the WIC EBT rollout schedule across nearly all U.S. counties[6] using public records from state WIC agencies. For counties reporting a range of implementation dates, we

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

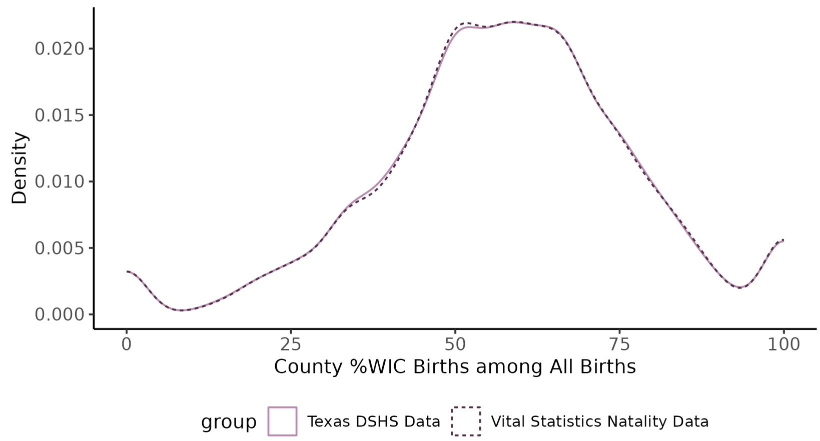


FIGURE 4: DISTRIBUTION OF COUNTY-LEVEL SHARE OF WIC BIRTH

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.

used 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 did not report WIC participation, our final sample includes 2,549 counties, covering 81.24% of the U.S. population and accounting for 79.10% of births.

We then examined the correlations between the WIC EBT rollout schedule and baseline county characteristics. We collected baseline data for the years 2006-2008 from various sources. Data on the share of Black and Hispanic populations and income per capita were sourced from the American Community Survey (ACS) Public Use Microdata Sample. We constructed 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 were excluded due to suppressed geographic identifiers. While we could not find county-level data on all welfare programs that automatically qualify participants for WIC, except for SNAP, we gathered data on transfers from the Bureau of Economic Analysis's Regional Economic Information System (REIS), which include these welfare programs. Public assistance medical benefits encompass 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. Additionally, we included 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 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 can be considered plausibly exogenous.

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.

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:

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

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[7] to account for differential trends of outcomes across geograhical areas, $Z_c t$ is county baseline characteristics 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 Goodman-Bacon (2021) as well as de Chaisemartin and D'Haultfœuille (2020), 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. Sun and Abraham (2021) show that the IW estimator is consistent under assumptions of parallel trends and no anticipation. In appendix, we discuss results using other popular staggered difference-in-difference estimators as well as traditional TWFE estimators. We find that our results are not driven by the choice of 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).[8] Regressions and dependent variable means are weighted using the number of births in each cell.

[7]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.

[8]For convenience, we report both standard errors for main results and falsification/robustness tests except for event study plots; for the other analysis, we only report standard errors clustered on county.

5.2 High-impact subgroups

Ideally, to estimate an ATT, our analysis would be 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. 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). The results we estimate directly are intent-to-treat (ITT) effects.

We focus on maternal age, education, marital status, race, and Hispanic origin, as these are the most commonly reported demographic characteristics. To validate these characteristics as proxies for WIC eligibility, we utilize data from SIPP. The SIPP provides valuable insight into the demographic characteristics of WIC-eligible individuals, as it includes information on household income and program participation[9]. 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 focus on 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 comprise approximately 40% of the full sample and are about 17% more likely to be WIC-eligible than mothers overall (Table 3). 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.

[9] 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.

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 ≤ 22	19.41%	58.11%	9.88%	0.1264***
				(0.0069)
Education $\leq$ high school	37.17%	65.29%	17.06%	0.2281***
	(0.84)			00
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. 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. 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 infant health, as improving infant health 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, infant health.

6.1 Primary results on 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 high school education or less, and infants without documented fathers, respectively. The corresponding shares of WIC-eligible individuals are 48.23%, 65.29%, and 64.81%, yielding TOT estimates of 2.6, 2.39, and 2.5 percentage points.

TABLE 4: EFFECTS OF WIC EBT ON WIC PARTICIPATION

	All births			Education ≤ 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. In Table A1, we aggregate estimates by cohort and observe that earlier-adopting states experienced more negative changes in WIC participation post-implementation. We discuss possible reasons at the end of the article.

We explore the heterogeneity of EBT effects by maternal race, ethnicity, age, birth order, and income quantiles, with the results presented in Table A2. Our findings indicate that the observed effects are primarily driven by white mothers, younger mothers who are less than 30 years old, and mothers living in high-income counties.

6.2 Infant health

Having established the positive effects of WIC EBT on WIC participation, we now turn to its impact on infant health. 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). Our findings indicate that EBT implementation significantly reduces adverse birth outcomes for high-impact subgroups.

Table 5 shows that while the effects of WIC EBT on birth outcomes are noisy for the full sample, they are statically significant for subgroups more likely to be WIC-eligible. Specifically, the ITT effects of EBT on the likelihood of low birth weight are -0.31 (TOT = -0.47) and

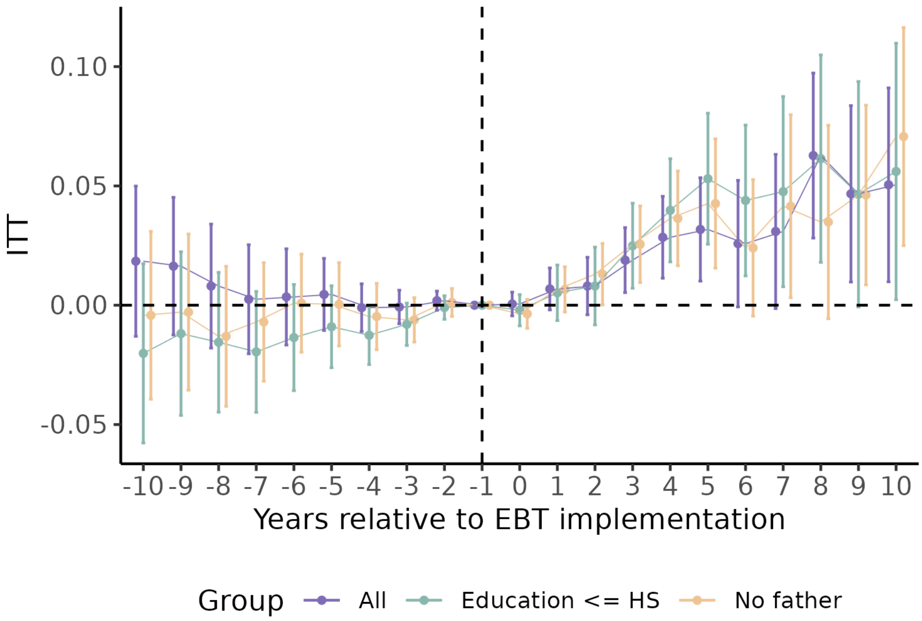


FIGURE 5: DYNAMIC EFFECTS OF WIC EBT ON WIC PARTICIPATION

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 county level.

-0.4 (TOT = -0.62) percentage points for mothers with a high school education or less and mothers without documented fathers, respectively. Similarly, the ITT effects on the likelihood of preterm births are -0.35 (TOT = -0.54) and -0.53 (TOT = -0.82) percentage points for the same groups.

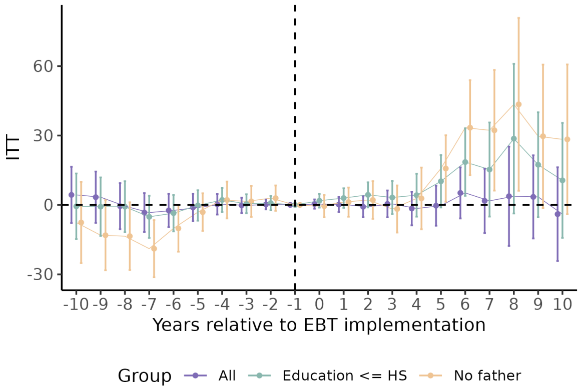
Figures 6a–6c suggest that pre-implementation trends are flat for the full sample and for mothers with a high school education or less, while the effects observed for mothers without documented fathers may be influenced by pre-existing trends. We also assess the sensitivity of these results to violations of the parallel trends assumption in Section 7.

Technically, WIC EBT can improve WIC participation through both the extensive and intensive margins. 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 alter zip-code-level WIC redemptions, suggesting that changes in the intensive margin are unlikely to explain the improvement in infant health.

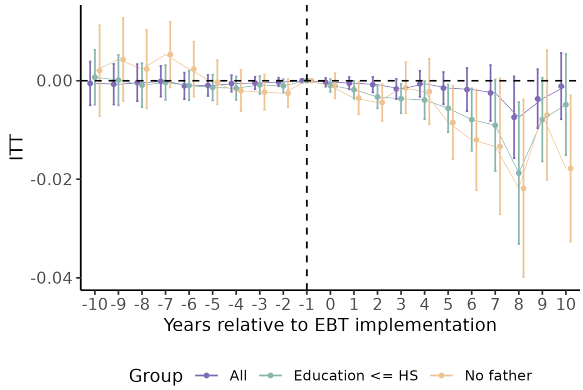
TABLE 5: EFFECTS OF WIC EBT ON INFANT HEALTH

	Birth weight (grams)			Low birth weight (birth weight < 2500 grams)			Preterm (gestation < 37 weeks)		
	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	-0.1545	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

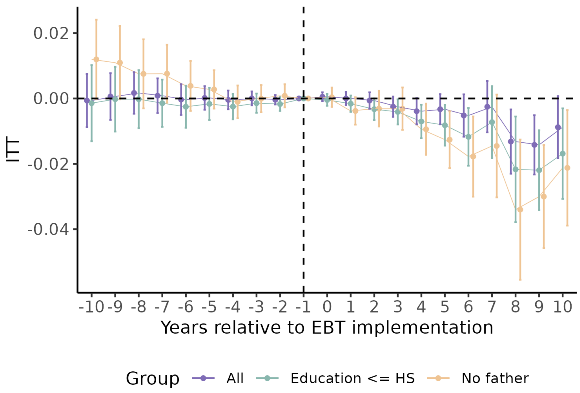
Notes: We report interaction weighted estimators proposed by Sun and Abraham (2021). We collapse birth data to county-of-maternal-residence-by-year-of-birth cells. We control for county and year fixed effects, census-region-specific linear time trend, county baseline characteristics from 2006-2009 interacted 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.



(A) Birth weight



(B) Low birth weight



(C) Preterm

FIGURE 6: DYNAMIC EFFECTS OF WIC EBT ON INFANT HEALTH

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 county level.

7 Robustness

7.1 Results on advantaged mothers

We start by showing that advantaged mothers—defined as those with more than a high school education and a father listed on the infant's birth certificate—are expected to be less affected by WIC EBT implementation, given the variability observed in the full sample. Table 6 confirms this: the effects for advantaged mothers are statistically significantly different from zero with standard errors clustered at the county level but lose significance when clustered at the state level; the effect sizes for this group are also substantially smaller than those observed in high-impact groups.

TABLE 6: EFFECTS OF WIC EBT ON ADVANTAGED MOTHERS

	(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

Table 7 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

We conduct another placebo test by estimating results based on hypothetical treatment timings. Specifically, we re-estimate the effects assuming the treatment occurred four years

TABLE 7: 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.

earlier than it actually did. The pseudo-treatment effects are statistically insignificant, small in magnitude, and occasionally have the opposite sign (Table A3).

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. 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).

7.5 Event-time balanced panel

Due to the widespread distribution of EBT implementation across states, constructing a balanced panel would result in a significant reduction in sample size. Table A4 presents results for an event-time balanced panel spanning from period −4 to period 4. These findings generally align with our main results. The effects on WIC participation are larger and more precise in this balanced panel. The dynamic effects are shown in Figure A3.

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 A4 presents the maximum deviation from the parallel trends 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.

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 A5, we present estimates defining exposure at the beginning of the first, second, or third trimester instead of at the time of birth. As expected, our estimates generally become larger and more precise.

7.9 Controlling for variables associated with WIC Eligibility

Building on the full model, we further control for county-by-year per capita income and transfers from Medicaid, SNAP, and TANF, as these variables reflect changes in average WIC eligibility. The results remain consistent with our baseline findings (Table A6). Controlling for variables related to WIC eligibility modestly attenuates the effects of EBT on WIC participation.

8 Potential Mechanisms

8.1 Welfare stigma

Table 8 indicates that the effects on WIC participation are primarily driven by rural areas, aligning with the notion that welfare stigma is more pronounced among rural residents, where individuals are more likely to recognize fellow shoppers. The larger effects observed in rural areas might be attributed to a stronger response from poorer individuals; however, this does not explain why the effects are smaller among two particularly disadvantaged groups compared to the full sample. Another potential explanation for the larger effects in rural areas could be differences in the retail environment between urban and rural areas. However, if this were the only factor, we would expect smaller effects in rural areas, given that WIC vendors there are predominantly small, independent stores. According to Meckel (2020), these stores have a higher tendency to drop out of WIC after EBT implementation, which could hinder WIC participation or redemption.

TABLE 8: EFFECTS OF WIC EBT ON WIC PARTIPATION IN URBAN AND RURAL AREAS

	Urban areas			Rural areas		
	All births	Edu≤HS	No father	All births	Edu≤HS	No father
	(1)	(2)	(3)	(4)	(5)	(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.

8.2 WIC vendor access

The positive effects on WIC participation may also be influenced by changes in the retail environment. Specifically, if the implementation of WIC EBT expanded access to WIC vendors, it could have led to increased participation. To explore this possibility, we linked WIC EBT rollout data to WIC Integrity Profiles from 2009-2016 to assess the impact of WIC EBT on the number of net authorized WIC vendors each year. Table 9 indicates that WIC EBT actually decreases the number of WIC vendors, which aligns with the results reported by Ambrozek et al. (2024) and Meckel (2020). Even in rural areas, where the increase in WIC participation is more pronounced after the implementation of EBT, we still observe a decline in net WIC vendor authorizations. Thus, it is unlikely that the observed effects on WIC participation are driven by increased access to WIC vendors.

TABLE 9: EFFECTS OF WIC EBT ON WIC VENDORS

	All 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	-1.769	-0.0130	-0.5843	-0.0037
	(0.9280)*	(0.0053)**	(0.0889)***	(0.0008)***
	⟨1.029⟩*	⟨0.0062⟩**	⟨0.0739⟩***	⟨0.0011⟩***
Observations	17,012	17,012	11,349	11,349
R^2	0.9936	0.5984	0.9741	0.9380
Dep. var. mean	65.4593	0.0766	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 Texas birth certificate data. However, as shown in Figures B1a-B1d, when we replicate the event study estimates from (Meckel, 2020) using our natality data subset to Texas and a longer event window, we find no significant effects of EBT on participation. The dynamic effects also suggest a decreasing trend in birthweights in treated counties versus control counties in the pre-period, so we test sensitivity of the results to violations of parallel trends. 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 B2a-B2b). 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. Together, we find results that 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.

Another possible explanation for the differing conclusion from Meckel (2020) is that the HHFKA of 2010 significantly shifted the landscape: access to federal support has made retailers more inclined to continue participating in WIC. As discussed in Section 3, the negative effects of WIC EBT on vendor accessibility are likely mitigated by the technical and financial support from USDA following the HHFKA of 2010, allowing the positive welfare effects via welfare stigma reduction to dominate. As a result, we observe a 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 have learned from states that adopted the program earlier.

10 Discussion and Conclusion

10.1 Limitations

Our approach has some key 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.

Another 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 ATT 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 suppose that this attenuation effect holds.

A final 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 not represent the full population in unobservable factors.

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 substantially advances our understanding of the effects of a major policy change in WIC on WIC participants. Where prior work using data for one state and a shorter time span find negative effects of EBT on participation, we find increases in WIC participation and decline in adverse birth outcomes on average after EBT implementation.

As noted above, our data and approach allow us to capture the effects of WIC EBT on participation across the whole country and for a longer period of time. We consider our average treatment effect estimates to be 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 likely eligible individuals.

The results are characterized by heterogeneity across space and time of WIC EBT implementation. We find that earlier-adopting states had more negative participation changes after EBT. These results are consistent with a mental model in which states learn from other states about how to implement EBT in ways that make the transition smoother for participants. It is also possible that early adopting states have other unobserved factors that mediated the effect of EBT on participation. For instance, states that were eager to implement fraud reduction technology may impose other administrative burdens to WIC participation. However, on average, WIC EBT was effective at increasing WIC participation among eligible individuals. This indicates that the net effect of reducing stigma and enabling partial redemptions outweighed any deterrence effect from fraud reduction.

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Appendix

A Figures and tables

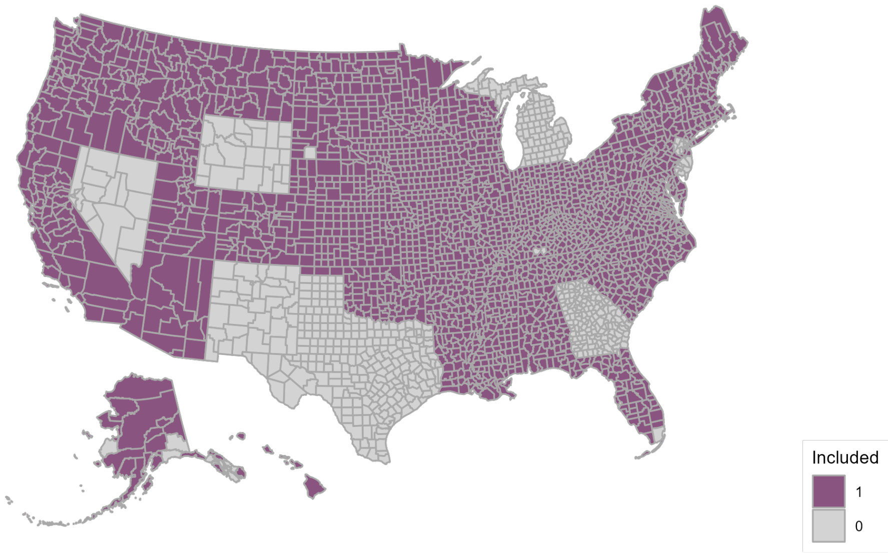
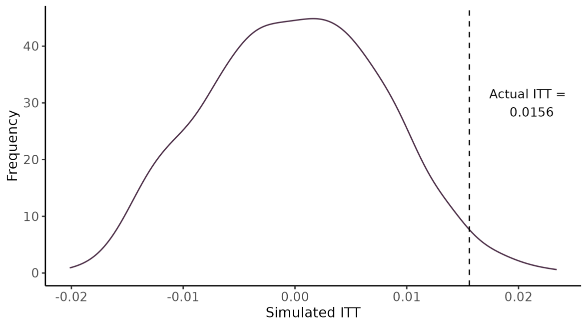


FIGURE A1: COUNTIES IN OUR SAMPLE

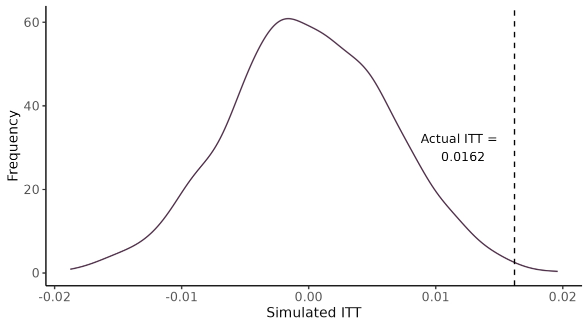
TABLE A1: COHORT-SPECIFIC EFFECTS OF EBT ON WIC PARTICIPATION

	WIC participation		
	All births (1)	Edu≤HS (2)	No father (3)
Cohort = 2011	0.0131	0.0093	0.0097
	(0.0123)	(0.0163)	(0.0132)
Cohort = 2013	0.1731***	0.1471***	0.0968*
	(0.0335)	(0.0383)	(0.0522)
Cohort = 2014	-0.0008	0.0073	-0.0007
	(0.0136)	(0.0167)	(0.0163)
Cohort = 2015	-0.0029	0.0195*	-0.0030
	(0.0110)	(0.0117)	(0.0126)
Cohort = 2016	0.0272***	0.0404***	0.0428***
	(0.0079)	(0.0105)	(0.0164)
Cohort = 2017	0.0193**	0.0207*	0.0271***
	(0.0076)	(0.0110)	(0.0101)
Cohort = 2018	0.0087	0.0085	0.0218***
	(0.0059)	(0.0092)	(0.0079)
Cohort = 2019	-0.0045	-0.0131	-0.0067
	(0.0085)	(0.0120)	(0.0089)
Cohort = 2020	0.0138**	0.0086	0.0149
	(0.0069)	(0.0088)	(0.0104)
Cohort = 2021	-0.0140	-0.0099	-0.0277*
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. 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.

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	High-income counties	Low-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	-0.0065	0.0040	-0.0016
	(0.0052)	(0.0060)	(0.0054)
	⟨0.0111⟩	⟨0.0088⟩	⟨0.0068⟩
Observations	28,021	27,483	26,225
R^2	0.9638	0.9284	0.8510
Dep. var. mean	0.4118	0.6515	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.

Table A4: Event-time Balanced Panel

	WIC participation		
	All births (1)	Edu≤HS (2)	No father (3)
Born after EBT	0.0157	0.0284	0.0279
	(0.0057)***	(0.0091)***	(0.0096)***
	⟨0.0048⟩***	⟨0.0049⟩***	⟨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.

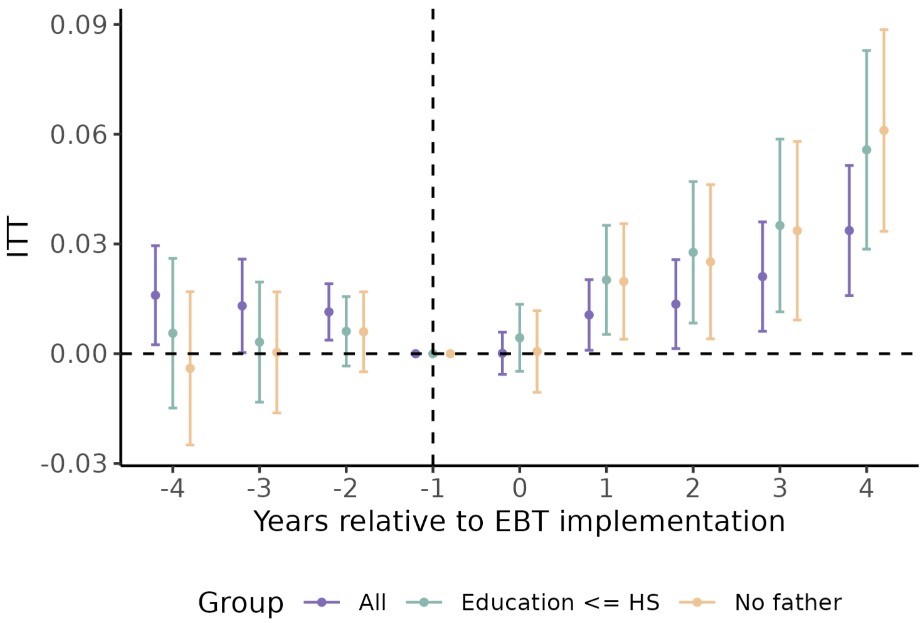
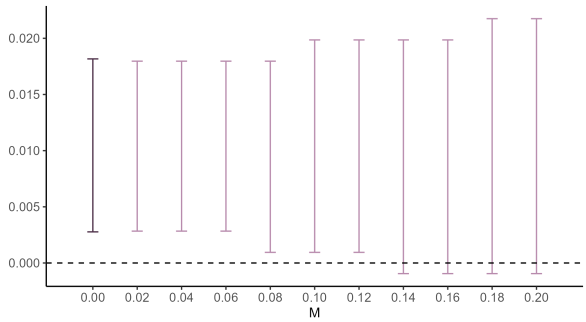
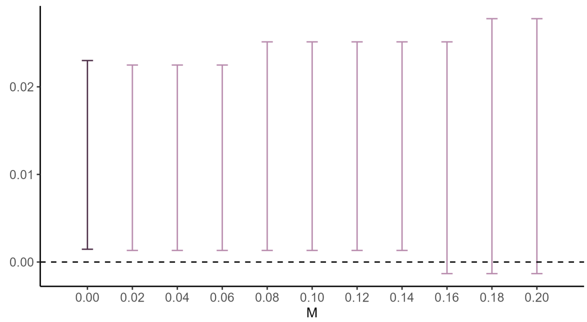


Figure A3: Dynamic Effects of WIC EBT on WIC Participation, Event-time Balanced Panel

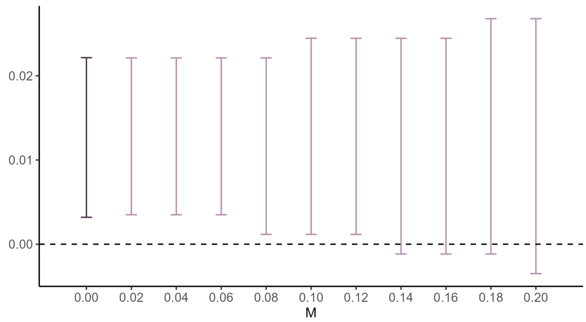
Notes: This event study plots report results using estimators by Sun and Abraham (2021). We collapse birth data to county-of-maternal-residence-by-year-of-birth cells. Regressions and dependent variable mean are weighted by the number of births in each cell. Standard errors are clustered at county level. 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.



(A) All births



(B) Edu $\leq$ HS



(C) No father

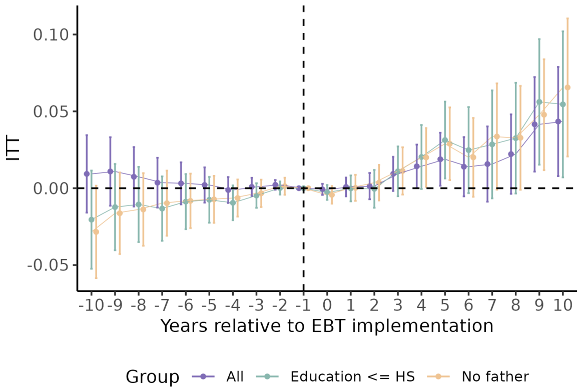
FIGURE A4: TESTING SENSITIVITY TO PARALLEL TREND VIOLATION

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

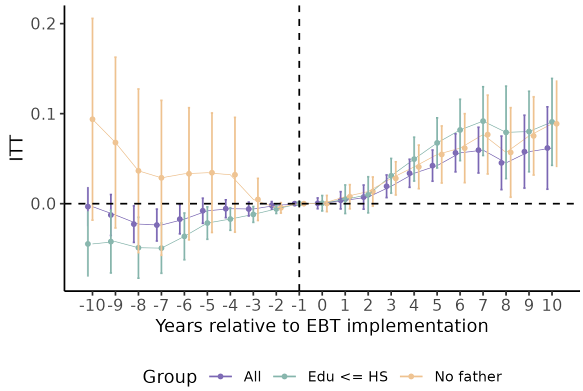
TABLE A5: ROBUSTNESS TO TIMING OF EXPOSURE

	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

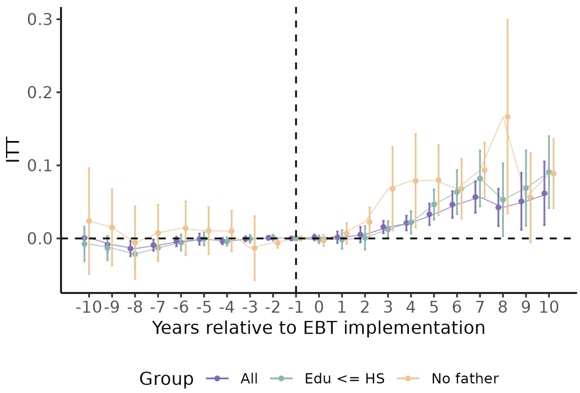
Notes: We report interaction weighted estimators proposed by Sun and Abraham (2021). The dependent variable is WIC participation rate for all regressions. We collapse birth data to county-of-maternal-residence-by-year-of-birth cells. We control for county and year fixed effects, census-region-specific 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.



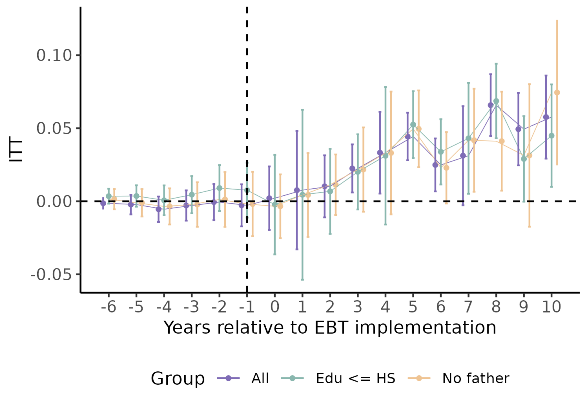
(A) Traditional TWFE estimators



(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 county 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.

TABLE A6: CONTROLLING FOR VARIABLES ASSOCIATED WITH WIC ELIGIBILITY

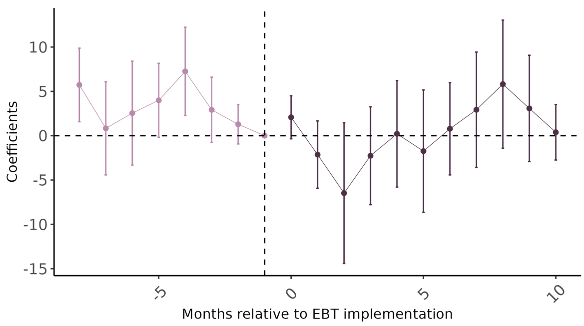
	WIC participation		
	All births	Edu≤HS	No father
	(1)	(2)	(3)
Born after EBT	0.0104	0.0133	0.0142
	(0.0052)**	(0.0073)*	(0.0064)**
	⟨0.0090⟩	⟨0.0088⟩	⟨0.0050⟩***
Observations	28,009	27,471	26,213
R^2	0.9646	0.9297	0.8530
Dep. var. mean	0.41209	0.65162	0.67495

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.

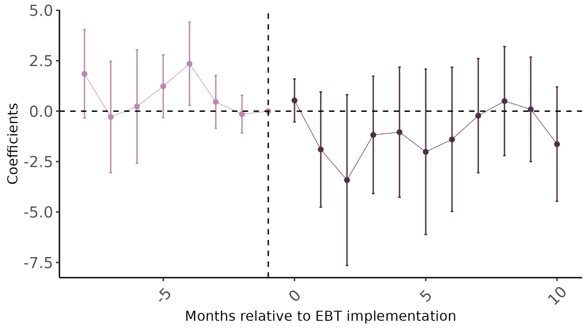
B Comparison to Meckel (2020)

In this appendix we discuss how our findings compare to those in Meckel (2020). We are able to extend both the time and geographic scope of estimates of the effect of WIC EBT on birthweight. In this section, we will make apples-to-apples comparisons across the Texas natality data we have with the results of Meckel (2020) with the same time period and with a longer time period, to understand the importance of the time dimension for results.

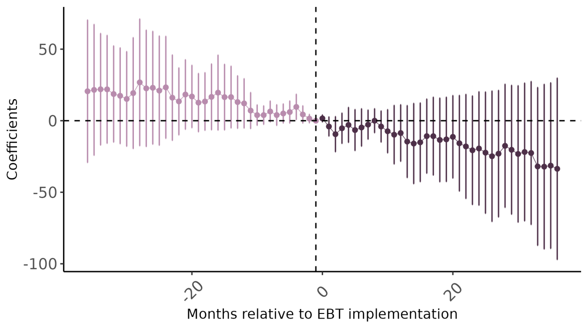
We note that with a longer time series over which to estimate treatment effects, we can capture additional trends in the data. The short run pre-trends – within 6 months prior to WIC EBT implementation – appear relatively stable around zero. However, longer run pre-trends show a path that indicates WIC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



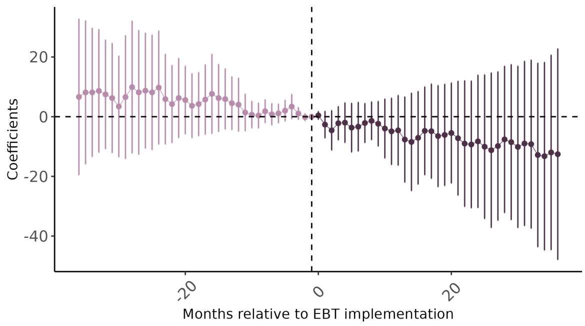
(A) EBT and WIC births per county (Figure 8 in Meckel (2020))



(B) EBT and high poverty WIC births per county (Figure 8 in Meckel (2020))

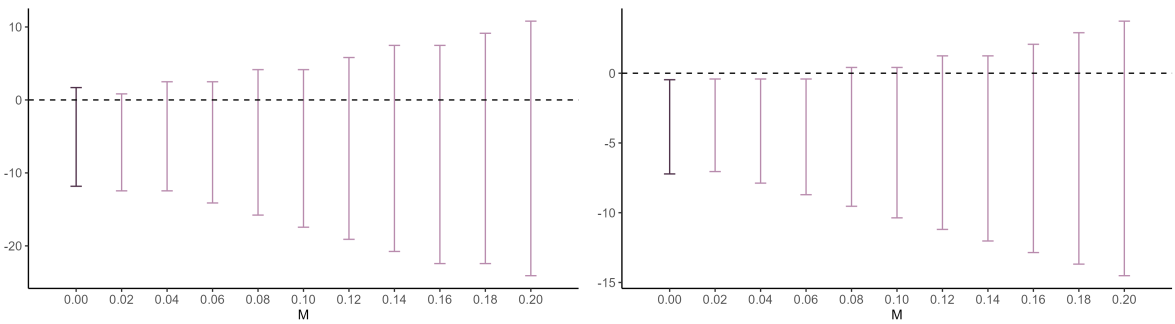


(C) EBT and WIC births per county, with larger window



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

FIGURE B1: EXTENDING EVENT STUDY PLOTS IN MECKEL (2020) TO LARGER WINDOW



(A) WIC Births

(B) High Poverty Share of WIC Births

FIGURE B2: TESTING SENSITIVITY TO PARALLEL TREND VIOLATION FOR MECKEL (2020)'S ESTIMATORS