

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

Charlotte Ambrozek  
UMN

Timothy K.M. Beatty  
UC Davis

Wenjia Zhan  
UC Davis

November 30, 2024

Please click [here](#) for the latest version.

## 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 streamlining benefit redemption and thereby reducing 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 among mothers more likely to be WIC-eligible following the implementation of WIC EBT. Additionally, we 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)

---

\*Charlotte Ambrozek is an assistant professor at Department of Applied Economics, University of Minnesota. Timothy K.M. Beatty is a professor and Wenjie Zhan is a Ph.D. candidate, both at Department of Agricultural and Resource Economics, University of California, Davis. We thank Marianne Bitler and Richard Sexton for their helpful comments. This work is funded by the Gifford Center for Population Studies.

# 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 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 both WIC participation and birth outcomes. 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-differences (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 vary across the country, given heterogeneous policy environments in different states, and across time.



FIGURE 1: SHARE OF BIRTHS PARTICIPATING IN WIC

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

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. Additionally, 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. 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 assis-

tance 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<sup>1</sup>. 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<sup>2</sup>.

---

<sup>1</sup>Source: USDA FNS. <https://www.fns.usda.gov/wic/factsheet> (Accessed on October 27, 2024).

<sup>2</sup>Source: USDA FNS. <https://fns-prod.azureedge.us/sites/default/files/resource-files/wisummary-10.pdf> (Accessed on October 27, 2024).

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 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. 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.

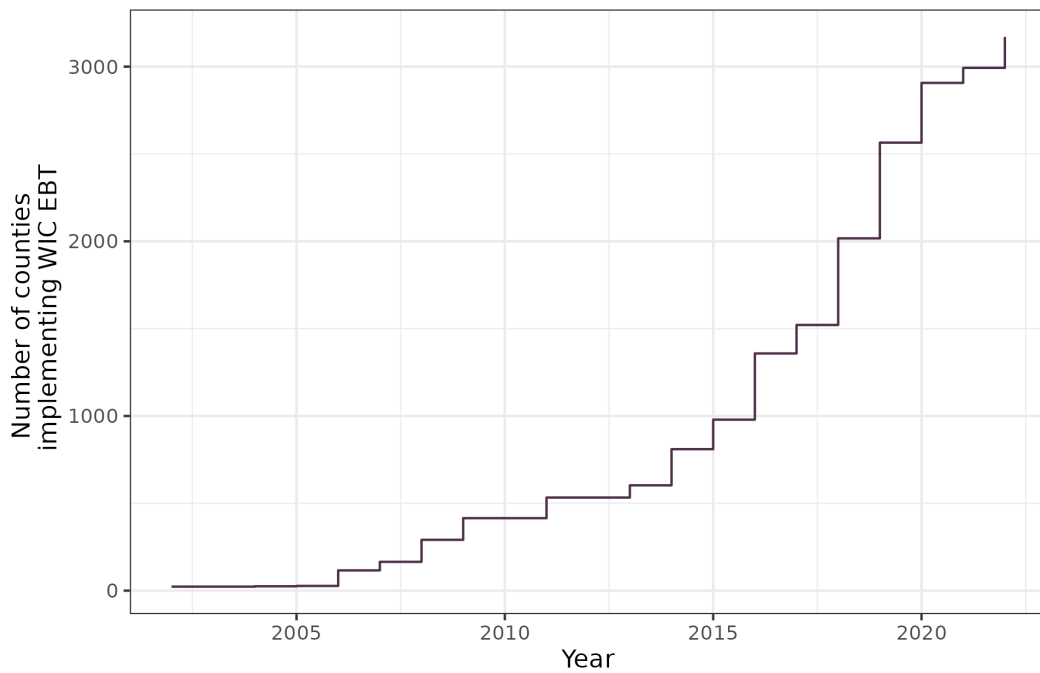
It was not until 2010 that the Healthy, Hunger-Free Kids Act (HHFKA 2010) launched a national mandate for the transition to EBT systems by October 1, 2020. This mandate provided a clear timeline for state WIC agencies nationwide<sup>3</sup>. The HHFKA 2010 directed the USDA to develop WIC EBT technical standards and operating rules for all stakeholders and to establish a national database of universal product codes for the EBT systems across all states<sup>4</sup>. 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 multiple 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 coun-

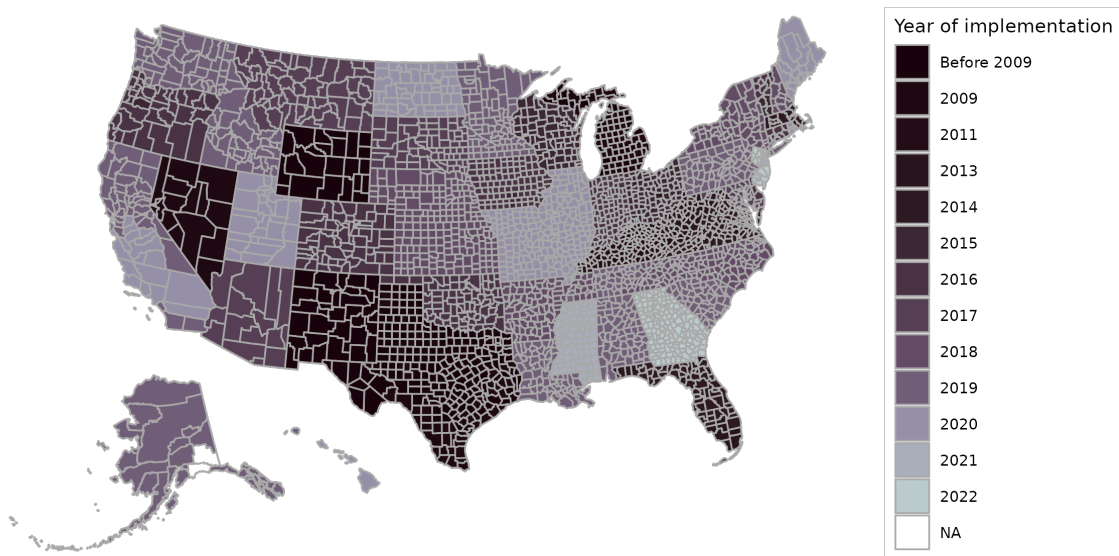
---

<sup>3</sup>Exemptions may be granted for a state facing unusual barriers to such implementation.

<sup>4</sup>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

ties 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 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. Specifically, we consider a retailer-consumer equilibrium framework in which consumers choose to participate by optimizing 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 total value of a composite bundle of all other goods ( $G_i$ ). All 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  $\phi_i$  captures total psychological disutility associated with welfare stigma. For WIC participants, the optimal  $W_i$  and  $\theta_i$  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:

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

where  $\Pi_j$  represents the net benefit,  $\kappa_j$  denotes the share of all WIC-eligible goods sold by retailer  $j$ , and  $F_j$  is the compliance cost associated with WIC participation. 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  $\delta_i$  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  $\delta_i$  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 ( $\theta_i$ ) 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 ( $W_i^{\text{WIC}}$ ) and participation intensity ( $\theta_i^{\text{WIC}}$ ) for WIC participants. By substituting these values into the utility function  $U(\cdot)$ , we can determine the maximum utility for WIC participants,  $U_i^{\text{WIC}}$ . 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:

$$\Pr(U_i^{\text{WIC}} > U_i^{\text{non WIC}})^5.$$

---

<sup>5</sup> $W_i^{\text{WIC}}$  and  $W_i^{\text{non WIC}}$  are the optimal working time for WIC participants and non-WIC participants, respectively, and

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

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

where  $\theta_i^{\text{WIC}}$  is participation intensity for WIC participants. The optimal working time  $W_i^{\text{WIC}}$  and participation  $\theta_i^{\text{WIC}}$  for WIC participants satisfy:

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



By envelope theorem, we obtain:

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

$$\frac{\partial U^{WIC}}{\partial F_{\text{vicinity}, i}} = - \underbrace{\theta_i^{WIC}}_{>0} \cdot \underbrace{\frac{\partial \delta_i}{\partial F_{\text{vicinity}, i}}}_{>0} \cdot \underbrace{\frac{\partial V}{\partial L_i}}_{>0} < 0. \quad (2)$$

Thus, EBT affects WIC participation through two primary channels: (1) it reduces welfare stigma for consumers, lowering  $\phi_i$ , increasing  $U^{WIC}$ , and thus potentially raising  $\Pr(U_i^{WIC} > U_i^{\text{non WIC}})$ ; (2) it raises compliance costs for retailers, increasing  $F_{\text{vicinity}, i}$ , 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 suggests that consumers may spend 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  $\theta^{WIC}$ ), the marginal increase in the time cost of WIC redemption is sensitive to the closure of neighboring WIC vendors (large  $\frac{\partial \delta_i}{\partial F_{\text{vicinity}, i}}$ ), 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 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](#)).

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

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

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

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 moderate decline during the pandemic. 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%. Despite this evidence, we acknowledge that mothers in the natality data may still differ significantly from overall WIC participants. However, these mothers represent a nontrivial share of WIC participants and are therefore important to study. Natality data has also been used in other studies, such as [Rossin-Slater \(2013\)](#) and [Meckel \(2020\)](#), to examine WIC participation, which supports its use in this context.

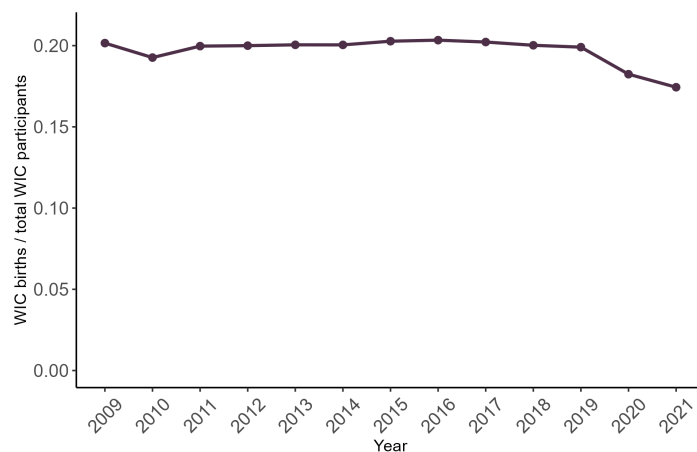


FIGURE 3: RATIO OF WIC BIRTHS TO TOTAL WIC PARTICIPANTS

Notes: Ratio of WIC Births to Total WIC Participants is calculated by dividing total number of WIC births 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.

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

TABLE 1: COMPARING NATALITY DATA WITH OTHER SURVEY DATA

	Nativity data	CPS ASEC	Mean difference (1) - (2)	SIPP	Mean difference (1) - (4)
	(1)	(2)	(3)	(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 ASEC 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.

comparison of these overlapping subsets reveals that the data are nearly identical (Figure 4).

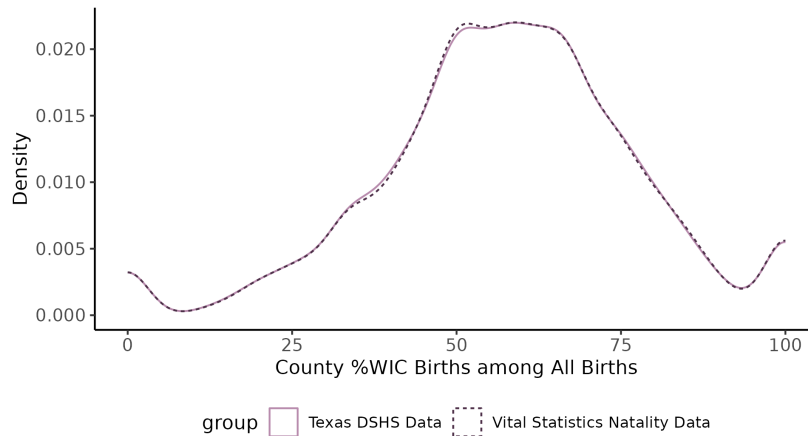


FIGURE 4: DISTRIBUTION OF COUNTY-LEVEL SHARE OF WIC BIRTH

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 compiled the WIC EBT rollout schedule across nearly all U.S. counties<sup>6</sup> using public records from state WIC agencies. For counties reporting a range of implementation dates, we 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 2,489 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

---

<sup>6</sup>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)
<i>Demographics, 2006-2008</i>					
% Black	8.84 (0.26)	12.06 (0.54)	-3.22	0.0427*** (0.0108)	-0.0014 (0.0021)
% Hispanic	5.43 (0.14)	19.46 (0.85)	-14.03	0.0480*** (0.0129)	0.0148*** (0.0031)
% Poor $\times$ under age 5	1.64 (0.02)	1.95 (0.03)	-0.31	-0.2715 (0.3121)	-0.1133*** (0.0421)
% Low birth weight	8.03 (0.05)	8.74 (0.10)	-0.71	-0.4072*** (0.0770)	-0.0157 (0.0113)
Population	96,379 (6,282)	93,937 (11,143)	2,442		
Log population				-0.0291 (0.1109)	-0.0188 (0.0161)
<i>Transfers and income, 2006-2008</i>					
Public asst. medical benefits p.p. (incl., Medicaid, \$1,000)	1.11 (0.01)	1.15 (0.02)	-0.03	0.6513*** (0.2417)	-0.0256 (0.0469)
Income maintenance benefits p.p. (incl., TANF and WIC, \$1,000)	0.18 (0.002)	0.17 (0.003)	0.01	-5.453*** (1.656)	0.4460 (0.3891)
SNAP benefits p.p. (\$1,000)	0.12 (0.002)	0.13 (0.003)	-0.01	7.038** (3.363)	1.233** (0.5087)
Income p.p.(\$1,000)	6.95 (0.03)	6.66 (0.06)	0.29	0.0166 (0.0635)	-0.0090 (0.0133)
<i>WIC vendors, 2006-2008</i>					
Number of WIC vendors (1,000)	0.04 (0.002)	0.03 (0.004)	0.004	0.4176 (0.3079)	0.1275** (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 can be considered 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:

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

where  $Y_{ct}$  is outcome variable measured for county  $c$  in year  $t$ ,  $\eta_c$  and  $\lambda_t$  are county and year fixed effects to control for national economic shocks and county time-invariant unobserved heterogeneity,  $\theta_{ct}$  is census-region-by-year fixed effect<sup>7</sup> to account for differential trends of outcomes across geographical areas,  $Z_{ct}$  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  $\varepsilon_{ct}$  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 appendix, we discuss results using other popular staggered difference-in-difference estimators as well as tradi-

---

<sup>7</sup>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 implementing WIC EBT within state.

tional 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)<sup>8</sup>. Regressions and dependent variable means are weighted using the number of births in each cell.

## 5.2 High-impact groups

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. We may lack sufficient statistical power to detect the effects of WIC EBT using the full sample, as only WIC-eligible individuals are directly impacted by this intervention. 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<sup>9</sup>. 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 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

---

<sup>8</sup>We report both standard errors whenever possible; when inconvenient to do so, we report the standard errors clustered on state.

<sup>9</sup>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.



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, ruling out the possibility that variations in WIC-eligible shares across maternal characteristic groups are 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. 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 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 WIC EBT increased 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 on file, 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 on file, 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 on file.

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 (0.0058)** (0.0156)	0.0168 (0.0051)*** (0.0097)*	0.0126 (0.0056)** (0.0120)	0.0268 (0.0081)*** (0.0120)**	0.0291 (0.0080)*** (0.0107)***	0.0156 (0.0073)** (0.0092)*	0.0275 (0.0079)*** (0.0086)***	0.0336 (0.0074)*** (0.0059)***	0.0162 (0.0065)** (0.0053)***
County fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Census region $\times$ year		✓	✓		✓	✓		✓	✓
Baseline char. $\times$ year		✓	✓		✓	✓		✓	✓
Employment rate <sub>ct</sub>			✓			✓			✓
Observations	34,566	33,873	28,023	33,964	33,329	27,485	32,496	31,890	26,227
R <sup>2</sup>	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 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 further validate this conclusion through additional tests presented later.

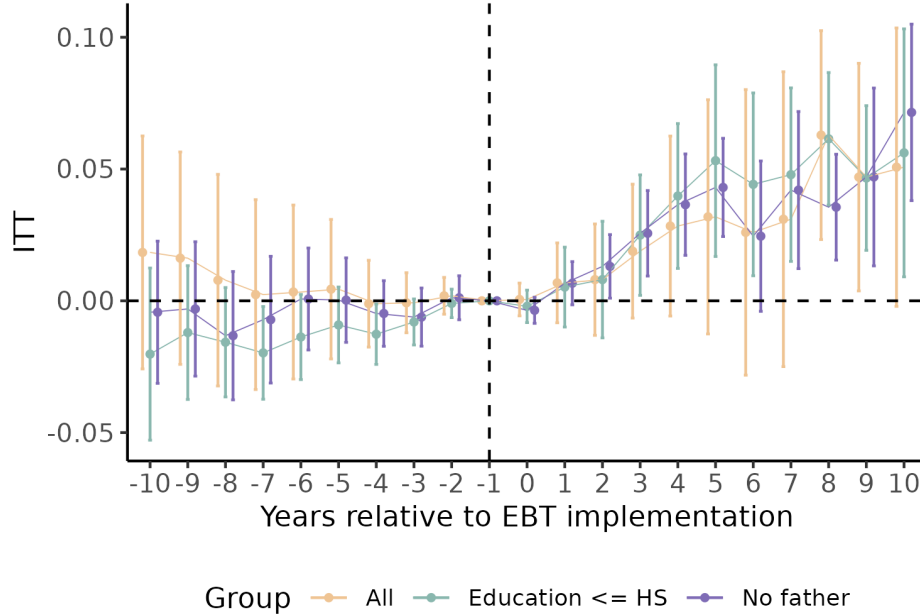


FIGURE 5: DYNAMIC EFFECTS OF WIC EBT ON WIC PARTICIPATION

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. The results are presented in Table [A2](#). Our analysis reveals that the 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 EBT effect on WIC participation is substantially higher in rural areas. The latter two findings support the intuition that EBT effects are more pronounced for mothers from disadvantaged backgrounds.

## 6.2 WIC EBT reduced adverse birth outcomes

Having established the positive effects of WIC EBT on WIC participation, we now turn to its impact on birth outcomes. Technically, WIC EBT can enhance WIC participation through both the extensive and intensive margins, potentially contributing to improvements of 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 are unlikely to be 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). Our findings indicate that EBT implementation significantly reduces adverse birth outcomes for high-impact groups. These results further reinforce the observed positive effects of WIC EBT on WIC participation.

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 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 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.

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≤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 (2.269) <4.955>	4.532 (2.812) <3.600>	4.812 (3.894) <4.441>	-0.0009 (0.0008) <0.0016>	-0.0031 (0.0012)*** <0.0010>***	-0.0040 (0.0019)** <0.0015>**	-0.0012 (0.0011) <0.0020>	-0.0035 (0.0015)** <0.0013>***	-0.0053 (0.0024)** <0.0019>***
Observations	28,021	27,482	26,224	28,021	27,482	26,224	28,023	27,485	26,227
R <sup>2</sup>	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.

Figures 6a–6c indicate that pre-implementation trends are flat for the full sample and for mothers with a high school education or less, suggesting no prior systematic changes in outcomes. The effects observed for mothers without an infant’s father on file 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.

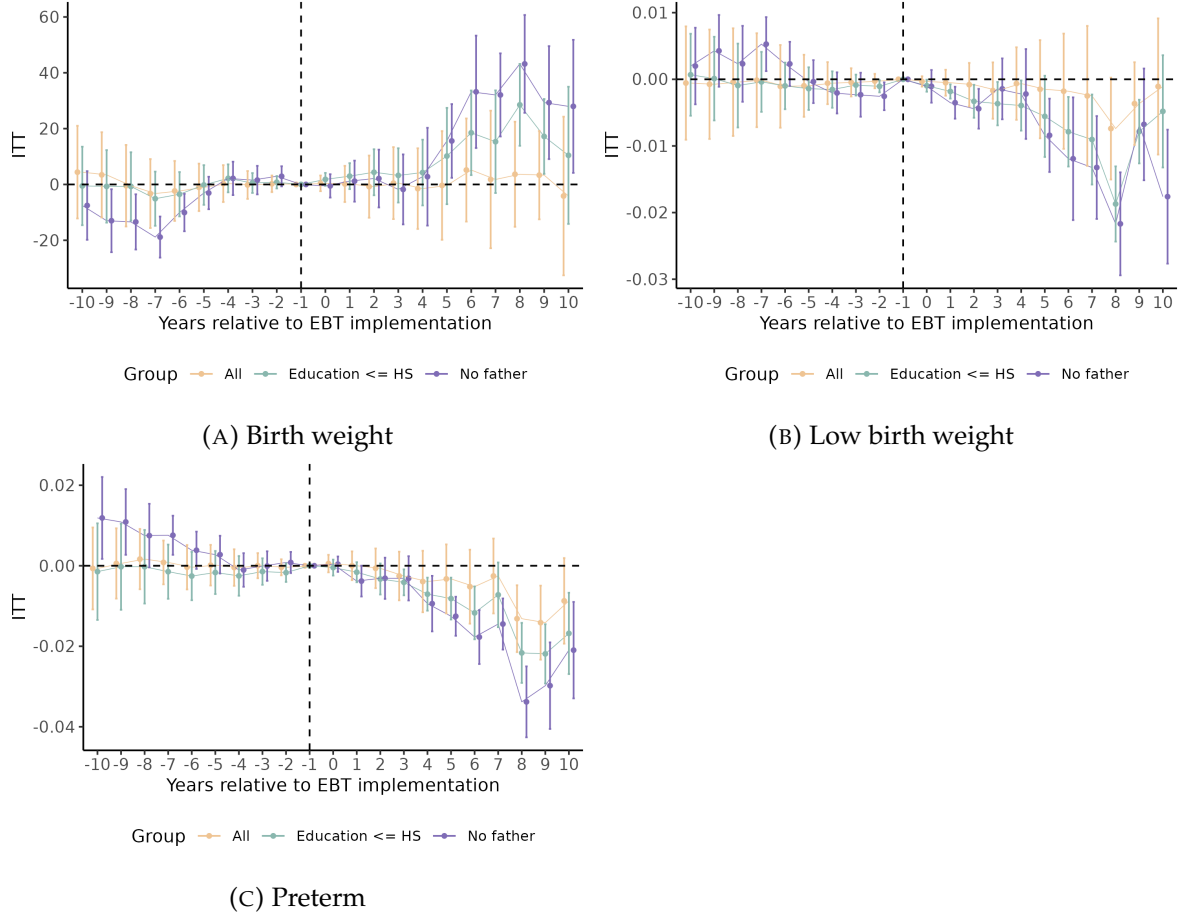


FIGURE 6: DYNAMIC EFFECTS OF WIC EBT ON BIRTH OUTCOMES

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.

## 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 clus-

tered 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 (0.0038)* (0.0114)	0.0085 (0.0032)*** (0.0060)	0.0086 (0.0035)** (0.0060)
County fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Census region $\times$ year		✓	✓
Baseline char. $\times$ year		✓	✓
Employment rate <sub>ct</sub>			✓
Observations	34,238	33,562	27,712
R <sup>2</sup>	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

The observed positive effect on WIC participation could be influenced by the demographic composition of the cell. Specifically, if over time the treated cell includes more mothers who are inclined to participate in WIC, this shift would also result in an increase in WIC participation. 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

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<sup>10</sup>. If our results do not capture any

<sup>10</sup>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 7: COMPOSITION CHANGE

	Maternal characteristics used to define subgroups			Other maternal characteristics						
	Edu ≤ HS	No father	Adv. mothers	Age ≤ 22	College gradu- ates	Unmarried	White	Black	Asian	Hispanic
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Born after EBT	-0.0003 (0.0031) (0.0037)	$8 \times 10^{-6}$ (0.0027) (0.0054)	-0.0015 (0.0032) (0.0034)	-0.0023 (0.0019) (0.0039)	0.0042 (0.0029) (0.0059)	0.0002 (0.0042) (0.0046)	-0.0156 (0.0087)* (0.0056)***	0.0143 (0.0081)* (0.0115)	0.0035 (0.0067) (0.0081)	0.0033 (0.0029) (0.0103)
Observations	28,014	28,023	28,019	28,023	28,014	28,022	28,023	28,023	28,023	28,023
R <sup>2</sup>	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<sup>11</sup>. 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.

<sup>11</sup>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 in our main estimation 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 A4 presents results for a balanced panel from period  $-4$  to period 4, which generally 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 A3, which are also consistent with our previous findings. However, this balanced panel includes only 844 counties, much 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 confirm that our findings are not driven by changes in the composition of counties over event time.

## 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.

## 8 Potential Effect Channels

In this section, we examine two potential effect channels outlined in Section 3: the welfare stigma channel and the vendor access channel. The 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.

### 8.1 Increased WIC participation after EBT is likely driven by reduced welfare stigma

As we lack direct data on consumers' welfare stigma, we instead leverage the heterogeneity of WIC participation effects across urban and rural areas to provide suggestive evidence for the welfare stigma channel. Intuitively, welfare stigma tends to be more pronounced among rural residents, who are more likely to recognize fellow shoppers.

Table 8 shows that the effects on WIC participation are substantially larger and more precise in rural areas. Additionally, the effects for high-impact groups are smaller than those for all births in rural areas. These results suggest that reduced welfare stigma may be an important channel through which WIC EBT positively impacts WIC participation.

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 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 (0.0059)* (0.0087)	0.0127 (0.0089) (0.0088)	0.0148 (0.0080)* (0.0055)**	0.0305 (0.0059)*** (0.0049)***	0.0295 (0.0071)*** (0.0053)***	0.0223 (0.0089)** (0.0063)***
Observations	8,904	8,742	8,549	19,118	18,742	17,677
R <sup>2</sup>	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 EBT reduced WIC vendor access

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 9 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. In summary, this result, combined with the findings presented in Table 8, suggests that the positive effects on WIC participation are likely driven by a reduction in welfare stigma, which outweighs the impact of reduced vendor accessibility.

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 (0.9280)* (1.029)*	-0.0130 (0.0053)** (0.0062)**	-0.5843 (0.0889)*** (0.0739)***	-0.0037 (0.0008)*** (0.0011)***
Observations	17,012	17,012	11,349	11,349
R <sup>2</sup>	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 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 the differing conclusions from [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](#)).

If [Meckel \(2020\)](#)’s estimates indeed capture negative effects of EBT on WIC participation in Texas, one possible explanation could be that access to federal support enabled by the HHFKA of 2010 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.

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 suppose 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 in unobservable factors. 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 the availability of USDA support. However, we believe our estimates remain insightful, 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 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 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 period of interest helps explain why our findings differ from those of [Meckel \(2020\)](#) but are consistent with [Li, Saitone and Sexton \(2022\)](#), among other potential explanations.

Given the observed increase in WIC participation and improvements in birth outcomes, calculating a cost-benefit ratio for WIC EBT is appealing. However, our understanding of both the benefits and costs of WIC EBT remains limited. On the cost side, we need to consider both federal investment and expenses incurred by state and local agencies. For fed-

eral investment, we can approximate using the 2016 funding allocation under the HHFKA of 2010, which provided \$220 million to support EBT transitions across remaining states by 2020<sup>12</sup>. Assuming this budget was intended to cover 2016–2020, the annual federal investment in EBT transition would be approximately \$44 million. This figure aligns closely with the \$30.5 million allocated in FY 2013 for EBT transition (USDA Food and Nutrition Service, 2017). This brings the estimated total federal investment in the EBT transition to \$440 million over a 10-year period—significantly smaller than total WIC expenditures over the same time frame, with food costs alone exceeding \$76 billion from 2011 to 2020<sup>13</sup>. Costs shared by state and local agencies are smaller and more long-term, covered by Nutrition Services and Administration (NSA) grants from USDA, even though it is unclear what portion of these grants is allocated specifically to EBT transition and maintenance. Additionally, we should consider potential savings in food costs (Li, Saitone and Sexton, 2022) and possibly in NSA expenditures as a result of the EBT transition. On the benefit side, one approach could be to translate increased WIC participation or a reduction in adverse birth outcomes into potential future earnings gains. However, increased WIC participation and improved birth outcomes may yield other benefits, such as long run health and educational gains, that are more difficult to quantify at this stage. Nevertheless, this study suggests that the EBT transition has facilitated WIC benefit delivery, with this more efficient system contributing to the improved well-being of participants in need.

---

<sup>12</sup>Source: USDA FY 2019 Budget Summary. <https://www.usda.gov/sites/default/files/documents/usda-fy19-budget-summary.pdf> (Accessed on November 14, 2024).

<sup>13</sup>Source: USDA FNS WIC Tables. <https://www.fns.usda.gov/pd/wic-program> (Accessed on November 17, 2024). Historical WIC tables are available in the archived USDA website.



## References

- Abadie, Alberto, Susan Athey, Guido W Imbens, and Jeffrey M Wooldridge.** 2023. "When should you adjust standard errors for clustering?" *The Quarterly Journal of Economics*, 138(1): 1–35.
- Adukia, Anjali, Sam Asher, and Paul Novosad.** 2020. "Educational investment responses to economic opportunity: evidence from Indian road construction." *American Economic Journal: Applied Economics*, 12(1): 348–376.
- Almond, Douglas, Hilary W Hoynes, and Diane Whitmore Schanzenbach.** 2011. "Inside the war on poverty: The impact of food stamps on birth outcomes." *Review of Economics and Statistics*, 93(2): 387–403.
- Alsan, Marcella, and Crystal S Yang.** 2022. "Fear and the safety net: Evidence from secure communities." *Review of Economics and Statistics*, 1–45.
- Ambrozek, Charlotte.** 2022. "WIC Participant Responses to Vendor Disqualification."
- Ambrozek, Charlotte, Timothy Beatty, Marianne Bitler, Matthew Rabbitt, and Xinzhe Cheng.** 2024. "Unanticipated Effects of Electronic Benefits Transfer on WIC Stores and Redemptions: Evidence from Administrative Data on Vendors."
- Bitler, Marianne, Janet Currie, Hilary Hoynes, Krista Ruffini, Lisa Schulkind, and Barton Willage.** 2023. "Mothers as insurance: Family spillovers in WIC." *Journal of Health Economics*, 91: 102784.
- Bitler, Marianne P, and Janet Currie.** 2005. "Does WIC work? The effects of WIC on pregnancy and birth outcomes." *Journal of Policy Analysis and Management: The Journal of the Association for Public Policy Analysis and Management*, 24(1): 73–91.
- Bitler, Marianne P, Janet Currie, and John Karl Scholz.** 2003. "WIC eligibility and participation." *Journal of Human Resources*, 1139–1179.
- Borusyak, Kirill, Xavier Jaravel, and Jann Spiess.** 2024. "Revisiting event-study designs: robust and efficient estimation." *Review of Economic Studies*.
- Callaway, Brantly, and Pedro HC Sant'Anna.** 2021. "Difference-in-differences with multiple time periods." *Journal of Econometrics*, 225(2): 200–230.
- Chorniy, Anna, Janet Currie, and Lyudmyla Sonchak.** 2020. "Does prenatal WIC participation improve child outcomes?" *American Journal of Health Economics*, 6(2): 169–198.

- de Chaisemartin, Clément, and Xavier D'Haultfœuille.** 2020. "Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects." *American Economic Review*, 110(9): 2964–2996.
- East, Chloe N, Annie L Hines, Philip Luck, Hani Mansour, and Andrea Velasquez.** 2023. "The labor market effects of immigration enforcement." *Journal of Labor Economics*, 41(4): 957–996.
- Figlio, David, Sarah Hamersma, and Jeffrey Roth.** 2009. "Does prenatal WIC participation improve birth outcomes? New evidence from Florida." *Journal of Public Economics*, 93(1-2): 235–245.
- Fisher, Ronald A.** 1936. "'The coefficient of racial likeness' and the future of craniometry." *The Journal of the Royal Anthropological Institute of Great Britain and Ireland*, 66: 57–63.
- Goodman-Bacon, Andrew.** 2021. "Difference-in-differences with variation in treatment timing." *Journal of Econometrics*, 225(2): 254–277.
- Gray, Colin, Adam Leive, Elena Prager, Kelsey Pukelis, and Mary Zaki.** 2023. "Employed in a SNAP? The impact of work requirements on program participation and labor supply." *American Economic Journal: Economic Policy*, 15(1): 306–341.
- Hanks, Andrew S, Carolyn Gunther, Dean Lillard, and Robert L Scharff.** 2019. "From paper to plastic: understanding the impact of eWIC on WIC recipient behavior." *Food Policy*, 83: 83–91.
- Hoynes, Hilary, Marianne Page, and Ann Huff Stevens.** 2011. "Can targeted transfers improve birth outcomes?: Evidence from the introduction of the WIC program." *Journal of Public Economics*, 95(7-8): 813–827.
- Imai, Kosuke, and In Song Kim.** 2021. "On the Use of Two-Way Fixed Effects Regression Models for Causal Inference with Panel Data." *Political Analysis*, 29(3): 405–415.
- Kose, Esra, Siobhan O'Keefe, and Maria Rosales-Rueda.** 2024. "Does the Delivery of Primary Health Care Improve Birth Outcomes?: Evidence from the Rollout of Community Health Centers." *Journal of Human Resources*.
- Kreider, Brent, John V Pepper, and Manan Roy.** 2016. "Identifying the effects of WIC on food insecurity among infants and children." *Southern Economic Journal*, 82(4): 1106–1122.
- Li, Xuemei, Patrick W. McLaughlin, Tina L. Saitone, and Richard J. Sexton.** 2021. "The Magnitude and Determinants of Partial Redemptions of Food Benefits in the Special Supplemental Nutrition Program for Women, Infants and Children (WIC)." *American Journal of Health Promotion*, 35(6): 775–783.

- Li, Xuemei, Tina L Saitone, and Richard J Sexton.** 2022. "Impacts of Electronic Benefit Transfer on the Women, Infants and Children Program: Evidence from Oklahoma." *Journal of Agricultural and Resource Economics*, 47(2): 373–389.
- Manchester, Colleen Flaherty, and Kevin J Mumford.** 2010. "Welfare stigma due to public disapproval." *University of Minnesota and Purdue University, unpublished working paper.*
- McLaughlin, Patrick W, Tina L Saitone, and Richard J Sexton.** 2019. "The economics of food vendors specialized to serving the women, infants, and children program." *American Journal of Agricultural Economics*, 101(1): 20–38.
- Meckel, Katherine.** 2020. "Is the cure worse than the disease? unintended effects of payment reform in a quantity-based transfer program." *American Economic Review*, 110(6): 1821–1865.
- Meyer, Bruce D., and Nikolas Mittag.** 2019. "Misreporting of Government Transfers: How Important Are Survey Design and Geography?" *Southern Economic Journal*, 86(1): 230–253.
- Meyer, Bruce D., Wallace K. C. Mok, and James X. Sullivan.** 2015. "Household Surveys in Crisis." *Journal of Economic Perspectives*, 29(4): 199–226.
- Moffitt, Robert.** 1983. "An Economic Model of Welfare Stigma." *The American Economic Review*, 73(5): 1023–1035.
- National Center for Health Statistics.** 2021. "All-county natality files for 2009-2021." as compiled from data provided by the 57 vital statistics jurisdictions through the Vital Statistics Cooperative Program.
- Neuberger, Zoë, Lauren Hall, and Linnea Sallack.** 2024. "WIC's Critical Benefits Reach Only Half of Those Eligible: States Missing Out on Opportunity to Improve Pregnancy-Related, Child Health." Center on Budget and Policy Priorities.
- Nguimkeu, Pierre, Augustine Denteh, and Rusty Tchernis.** 2019. "On the estimation of treatment effects with endogenous misreporting." *Journal of Econometrics*, 208(2): 487–506.
- Rambachan, Ashesh, and Jonathan Roth.** 2023. "A more credible approach to parallel trends." *Review of Economic Studies*, 90(5): 2555–2591.
- Rossin-Slater, Maya.** 2013. "WIC in your neighborhood: New evidence on the impacts of geographic access to clinics." *Journal of Public Economics*, 102: 51–69.
- Roth, Jonathan, Pedro HC Sant'Anna, Alyssa Bilinski, and John Poe.** 2023. "What's trending in difference-in-differences? A synthesis of the recent econometrics literature." *Journal of Econometrics*, 235(2): 2218–2244.

- Shiferaw, Leah.** 2020. "Understanding the Effects of Access to the U.S. Social Safety Net." PhD diss. University of California, Berkeley.
- Smith, Travis A, and Pourya Valizadeh.** 2024. "Aging out of WIC and child nutrition: evidence from a regression discontinuity design." *American Journal of Agricultural Economics*, 106(2): 904–924.
- Sun, Liyang, and Sarah Abraham.** 2021. "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects." *Journal of Econometrics*, 225(2): 175–199.
- Swann, Christopher A.** 2010. "WIC eligibility and participation: The roles of changing policies, economic conditions, and demographics." *The BE Journal of Economic Analysis & Policy*, 10(1).
- Thorn, Betsy, Chrystine Tadler, Nicole Huret, E Ayo, and C Trippe.** 2016. "WIC participant and program characteristics final report. 2015."
- USDA Food and Nutrition Service.** 2016. "Special Supplemental Nutrition Program for Women, Infants and Children (WIC): Implementation of Electronic Benefit Transfer-Related Provisions as required by the Healthy, Hunger-Free Kids Act of 2010. Final rule." *Federal Register*, 81(40): 10433–10451.
- USDA Food and Nutrition Service.** 2017. "WIC NSA Cost Study: Final Report." by Stacy Gleason, Linnea Sallack, Loren Bell, Leslie Erickson, Benjamin Yarnoff, and Celia Eicheldinger. Project Officer: Chanchalat Chanhataasilpa. Alexandria, VA.
- Wright, Richard, Erdal Tekin, Volkan Topalli, Chandler McClellan, Timothy Dickinson, and Richard Rosenfeld.** 2017. "Less cash, less crime: Evidence from the electronic benefit transfer program." *The Journal of Law and Economics*, 60(2): 361–383.
- Zhao, Jianqiang J, Harry M Kaiser, and Yuqing Zheng.** 2022. "Do grocery food taxes incentivize participation in SNAP?" *Regional Science and Urban Economics*, 95: 103736.

# Appendix

## A Figures and tables

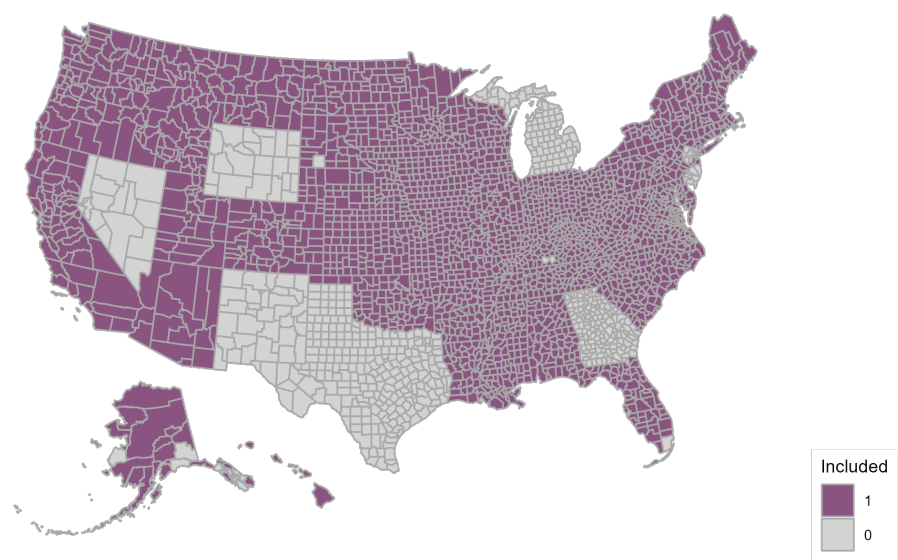


FIGURE A1: COUNTIES IN OUR SAMPLE

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 (0.0093)	0.0094 (0.0062)	0.0103 (0.0066)
Cohort = 2013	0.1730*** (0.0165)	0.1470*** (0.0071)	0.0969*** (0.0111)
Cohort = 2014	-0.0008 (0.0211)	0.0077 (0.0120)	-0.0005 (0.0096)
Cohort = 2015	-0.0027 (0.0247)	0.0198 (0.0151)	-0.0026 (0.0170)
Cohort = 2016	0.0273** (0.0126)	0.0404*** (0.0123)	0.0430*** (0.0125)
Cohort = 2017	0.0189** (0.0092)	0.0204* (0.0109)	0.0266** (0.0111)
Cohort = 2018	0.0087 (0.0073)	0.0085 (0.0097)	0.0218** (0.0101)
Cohort = 2019	-0.0045 (0.0085)	-0.0131 (0.0126)	-0.0067 (0.0068)
Cohort = 2020	0.0138 (0.0100)	0.0086 (0.0119)	0.0149 (0.0120)
Cohort = 2021	-0.0141 (0.0151)	-0.0099 (0.0238)	-0.0278 (0.0167)
Observations	28,023	27,485	26,227
R <sup>2</sup>	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 ≤ 22	22 < Age < 30	Age ≥ 30
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Born after EBT	0.0102 (0.0031)*** (0.0030)***	-0.0009 (0.0043) (0.0041)	-0.0013 (0.0047) (0.0044)	0.0106 (0.0114) (0.0125)	0.0114 (0.0047)** (0.0078)	0.0185 (0.0065)*** (0.0076)**	0.0168 (0.0057)*** (0.0079)**	0.0095 (0.0046)** (0.0076)
Observations	23,758	17,880	16,205	24,378	27,994	27,097	27,592	27,507
R <sup>2</sup>	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 (0.0052)** (0.0101)	0.0119 (0.0056)** (0.0100)	0.0283 (0.0056)*** (0.0033)***	0.0032 (0.0070) (0.0103)
Observations	27,502	27,828	18,195	9,827
R <sup>2</sup>	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 (1)	Edu≤HS (2)	No father (3)
Born after EBT	-0.0020 (0.0050) (0.0079)	0.0050 (0.0055) (0.0067)	0.0007 (0.0054) (0.0056)
Observations	28,020	27,482	26,224
R <sup>2</sup>	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.



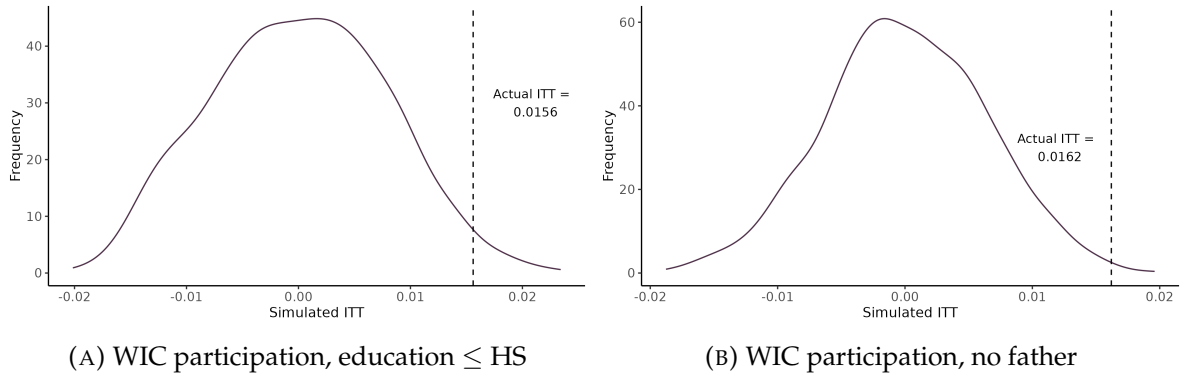


FIGURE A2: RANDOMIZATION TEST

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 A4: EVENT-TIME BALANCED PANEL

	WIC participation		
	All births (1)	Edu $\leq$ 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 <sup>2</sup>	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.

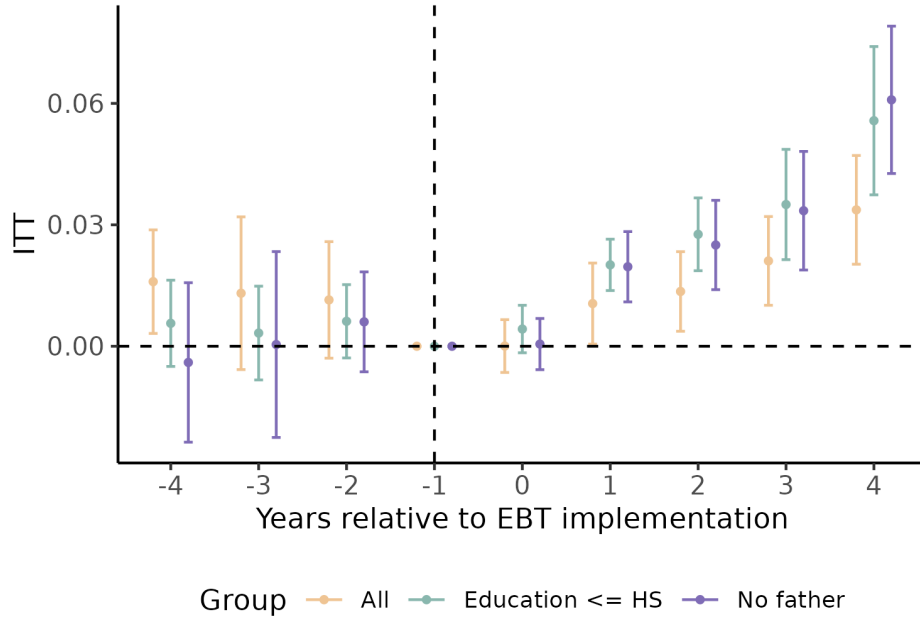


FIGURE A3: DYNAMIC EFFECTS OF WIC EBT ON WIC PARTICIPATION, EVENT-TIME BALANCED PANEL

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 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.

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 (0.0047)*** (0.0089)*	0.0213 (0.0067)*** (0.0097)**	0.0241 (0.0059)*** (0.0067)***	0.0143 (0.0049)*** (0.0089)	0.0189 (0.0070)*** (0.0095)*	0.0212 (0.0060)*** (0.0058)***	0.0135 (0.0051)*** (0.0091)	0.0186 (0.0073)** (0.0092)**	0.0211 (0.0063)*** (0.0055)***
Observations	28,340	27,904	26,713	28,320	27,905	26,789	28,291	27,892	26,738
R <sup>2</sup>	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.

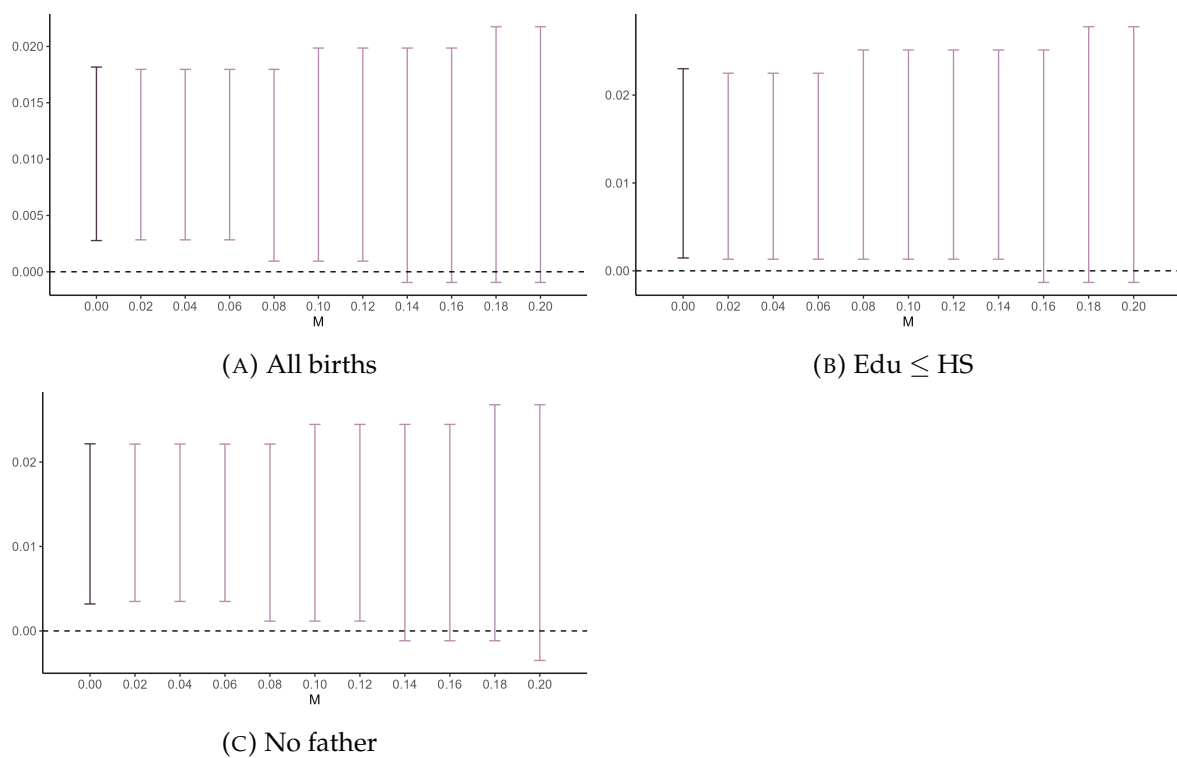


FIGURE A4: TESTING SENSITIVITY TO PARALLEL TREND VIOLATION

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

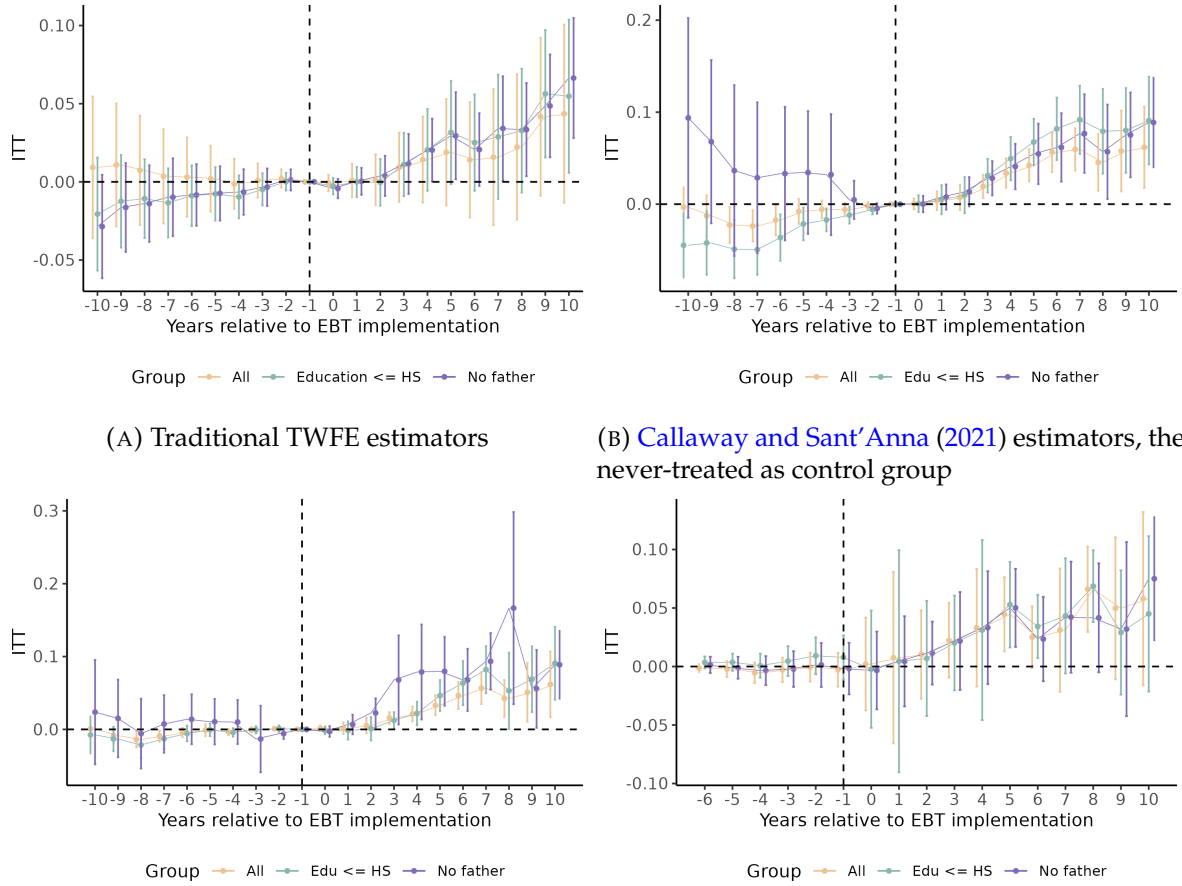
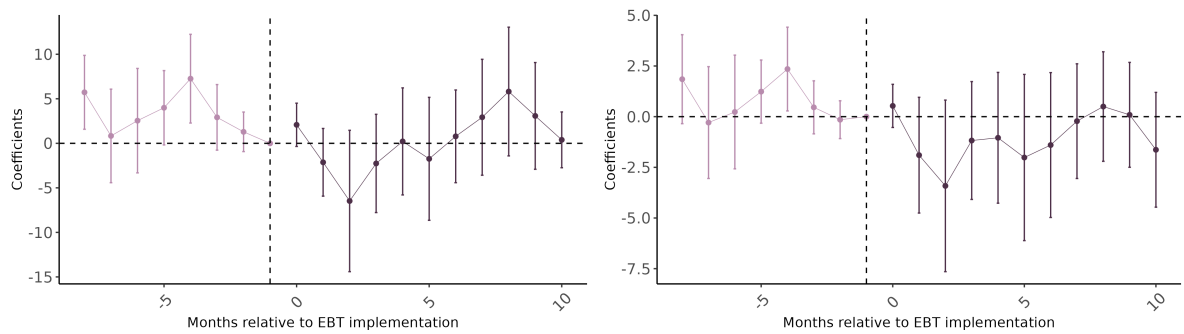


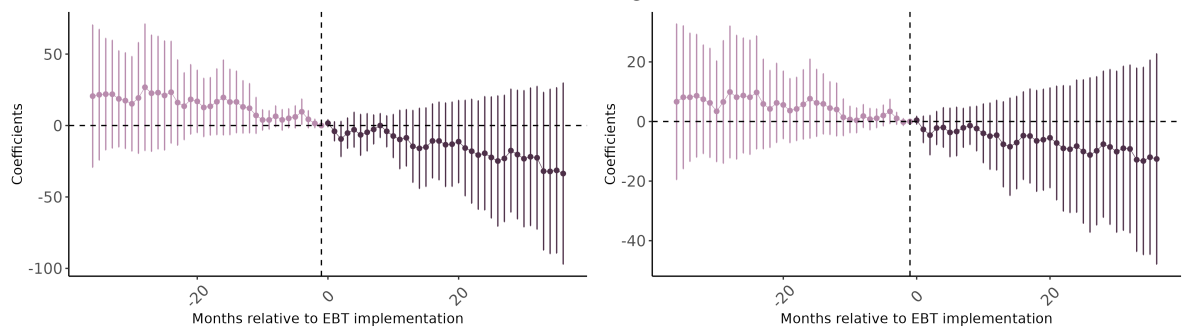
FIGURE A5: DYNAMIC EFFECTS OF WIC EBT BY ESTIMATION METHODS

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))

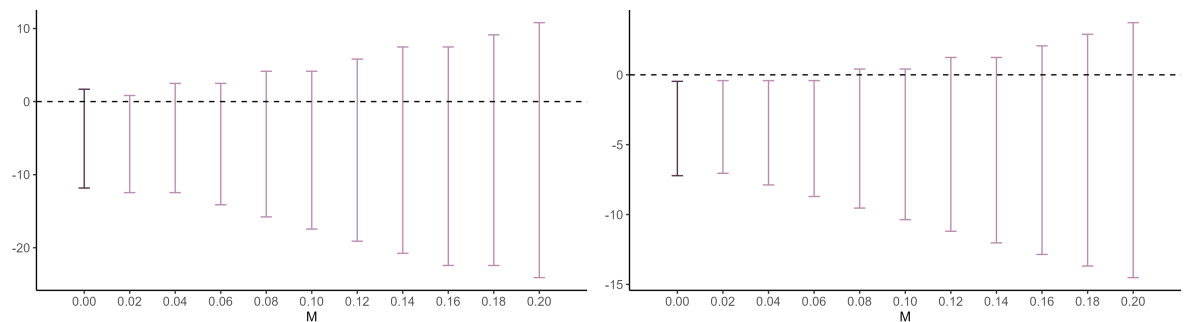


(C) EBT and WIC births per county, with a larger event window

(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