Stigma and Welfare Participation: Evidence from WIC EBT Transition*

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Abstract

Policymakers have an interest in lowering barriers to participation in the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC). WIC has been shown to increase birth weight for participating mothers and improve long-run outcomes for children who participate. 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 reducing welfare stigma. Empirical studies of the effects of WIC EBT on participation have found mixed results, with prior work limited to EBT limitation in a single state. Given a lack of national data on WIC participation, results may not be generalizable. In this paper, we evaluate the nationwide impact of WIC EBT implementation on WIC participation nationwide by linking the WIC EBT roll-out schedule to Vital Statistics Natality Data across virtually all counties in the U.S. We document a significant increase in WIC participation following the implementation of WIC EBT among mothers who are more likely to be WIC-eligible. We also find that WIC EBT reduces adverse birth outcomes for infants born to these mothers. Finally, we provide suggestive evidence that reducing welfare stigma is a likely mechanism explaining EBT's effect on WIC participation. (JEL H51, H53, I38)

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1 Introduction

The Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) provides food 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). Policymakers are interested in program changes that can stem these declines.

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

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

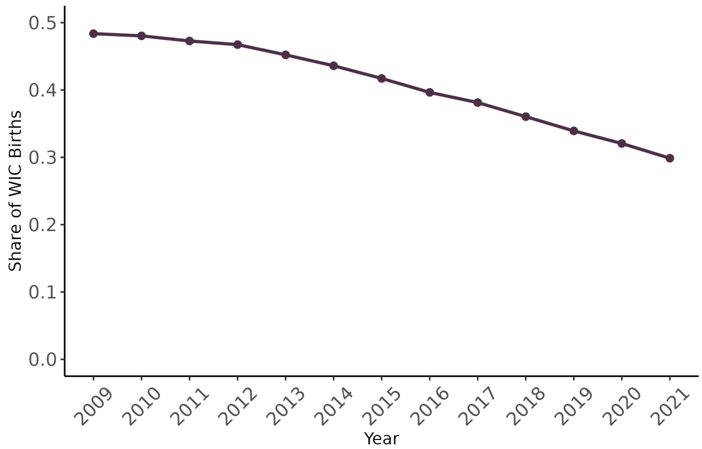


FIGURE 1: SHARE OF BIRTHS PARTICIPATING IN WIC

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

groups.

We document a somewhat noisy increase in WIC participation among all mothers of newborns, which is unsurprising, given that only WIC-eligible individuals can be impacted by the EBT transition. However, WIC eligibility is not reported in the natality data. To focus on target populations, we restrict our analysis to high-impact groups that are more likely to be WIC-eligible. We identify these groups by shares of likely WIC-eligible individuals based on overlapping maternal characteristics observed in both the natality data and the Survey of Income and Program Participation (SIPP). We find that mothers with no more than a high school education and those without an infant's father listed on the birth certificate—each making up around 40% of the full sample—are substantially more likely to be WIC-eligible.

Focusing on these two groups, we observe a 2.39-percentage-point increase in WIC participation rates among mothers with no more than a high school education and a 2.5-percentage-point increase among mothers without an infant's father listed on the birth certificate in counties that implemented WIC EBT compared to those that had not yet adopted. We also find that WIC EBT is biologically important. WIC EBT implementation reduces the likelihood of low birth weight by 0.47 percentage points and preterm births by 0.54 percentage points among infants born to mothers with no more than a high school education. Among infants without fathers listed on their birth certificates, the likelihood of low birth weight decreases by 0.62 percentage points, and preterm births by 0.82 percentage points. Based on these results, our back-of-the-envelope calculation suggests that WIC EBT lifts thousands of births out of low birth weight and preterm status, saving millions of dollars in hospital costs annually.

We provide suggestive evidence that increased participation is primarily driven by lowering stigma experienced by participants when redeeming benefits. Stigma is generally higher in rural areas, more Republican-leaning areas, and areas with potentially more customers or non-WIC customers in WIC stores. In Section 8, we find larger treatment effects in counties with these characteristics compared to counties that are urban, Democratic-leaning, or have fewer customers or non-WIC customers in WIC store. 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; Vasan et al., 2021), 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) finds that TANF EBT implementation reduces crime rates in Missouri, while Shiferaw (2020) shows that SNAP EBT increases average birth weight in California. This paper extends this literature by providing national-scale evidence on WIC EBT's effects on WIC participation among mothers of newborns and birth outcomes.

Second, 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.

Lastly, this work relates to the broader literature on the role of stigma as a determinant of food assistance participation in the U.S. We highlight that a program change that reduced the visibility, and thus stigma, of WIC participants at checkout increased participation, and that these effects are concentrated in places with higher welfare stigma. Our results echo prior qualitative work that highlights that participants had more discreet and faster checkout after WIC EBT (Chauvenet et al., 2019; Zimmer, Beaird and Steeves, 2021). In the public policy literature, a prior paper using 2015 Virginia data finds that even among EBT transactions, the more flexible the transaction was the more likely benefits were to be redeemed (Zhang et al., 2022). Negative experiences at checkout constitute "redemption costs" that vary with the third-party agent redeeming the benefits (Barnes, 2021). We contribute to understanding the WIC participation response to a program change that reduced redemptions costs, including lowering stigma costs, which informs policymakers as they consider other program changes like online WIC redemption.

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 (USDA Food and Nutrition Service, 2022). 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 time, 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 (USDA Food and Nutrition Service, 2020).

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

Despite extensive evidence on the health and social benefits of WIC, the program faces challenges such as declining participation and difficulties in reaching some of the most vulnerable groups (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

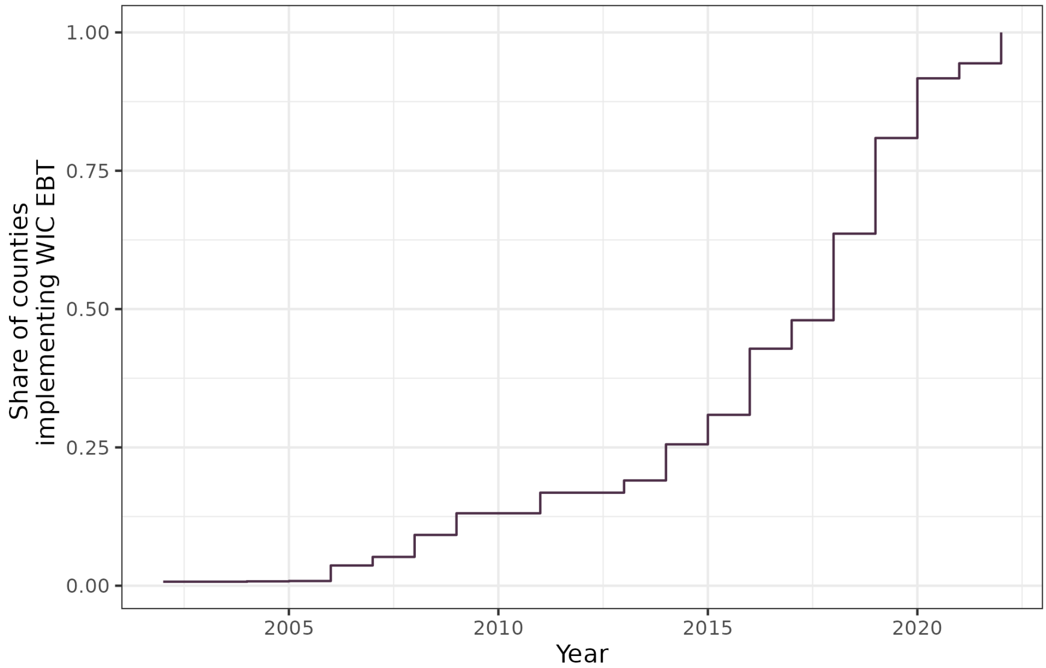
Before EBT, WIC participants received paper vouchers at WIC clinics every three months for specific foods tailored to their life stage and nutritional needs. However, these benefits could only be redeemed on a month-by-month basis. To use the vouchers, recipients had to shop at WIC-authorized stores and select only the foods listed on their vouchers.[1] At checkout, WIC items had to be separated from non-WIC items, and cashiers were responsible for ensuring that each item met the voucher's requirements, including brand, size, and quantity. If recipients mistakenly selected non-WIC-eligible items, they had to either return the items, pay for them out of pocket, or go back to the shelves to find the correct items and rejoin the checkout line. Once all items were verified, the cashier would ask the recipient to sign the voucher, collect it, and complete the transaction. If recipients chose to redeem only some of the items listed on a voucher, they forfeited the unredeemed items.

The transition to WIC EBT was a USDA Food and Nutrition Service (FNS) initiative aimed at modernizing WIC benefit delivery. Primary goals included streamlining business practices, simplifying transactions to reduce stigma, and improving program monitoring for WIC state agencies. Although some early WIC EBT projects began as early as 1995, the national WIC EBT transition plan was introduced in 2003, following the successful implementation of EBT systems in other federal food assistance programs, such as SNAP.

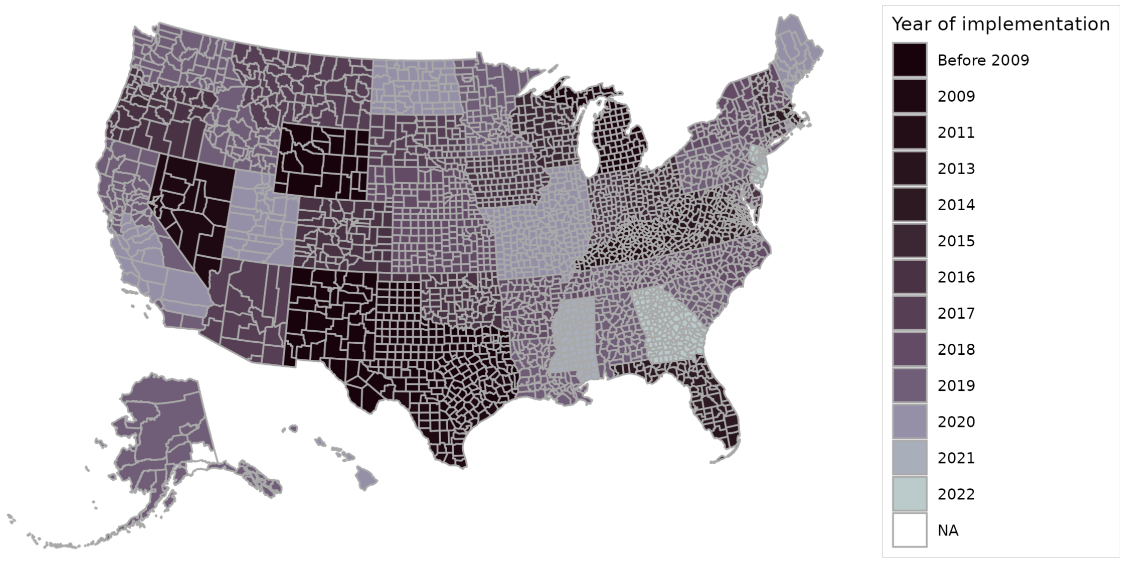
In 2010, the Healthy, Hunger-Free Kids Act (HHFKA 2010) imposed a national mandate for the transition to EBT systems by October 1, 2020. This mandate provided a clear timeline for state WIC agencies nationwide. Exemptions would be granted only to states encountering unusual barriers to implementation. 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 (S.3307 — 111th Congress, 2010). 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 collect data from mutiple sources including (archived) state websites, policy documents, and research papers. Most of the transition took place after 2010 (see Figure 2a). Figure 2b shows the geographic spread of EBT adoption, highlighting both similarities and differences in timing across counties within states. By 2022, all 50 states, U.S. territories, and tribal organizations had made the switch to

[1]Two states did not use authorized retailers to deliver WIC food benefits prior to EBT. Mississippi had participants travel to a distribution center to pick up their foods, while Vermont had home delivery of food benefits. We include these states in our estimation to obtain average treatment effects on the treated.



(A) Share 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

EBT. The pace of adoption depended on factors such as technical issues, available funding, cost-sharing plans, state agency efficiency, acceptance by local retailers, and the retail setup in each area (USDA Food and Nutrition Service, 2016).

3 Conceptual Framework

The net impact of EBT on WIC participation is, as a priori, ambiguous. EBT may encourage eligible individuals to participate in WIC by reducing welfare stigma and transaction costs. In contrast, anti-fraud features may discourage store participation in the WIC program, as they could reduce the potential for illegal profits from committing fraud, making WIC stores less accessible. For example, prior to EBT implementation, vendors had an incentive to charge WIC customers higher prices than non-WIC customers, as WIC goods are reimbursed by the government. This practice, prohibited by WIC program rules, is made more difficult by EBT, which allows the government to monitor prices directly and ensure compliance (Saitone, Sexton and Volpe III, 2015). This section outlines a simple framework to explore this dynamic. Specifically, we consider a retailer-consumer equilibrium framework in which consumers choose to participate by maximizing their utility subject to both budget 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). Specifically, we represent consumption as $C_i = G_i + \theta_i Z_i$, where $\theta_i \in [0, 1]$ captures WIC participation, allowing for partial redemption of benefits. Participation in WIC provides access to eligible goods at a subsidized or no cost but may also involve time and stigma costs. Each individual has a fixed time endowment, T, which is allocated among leisure (L_i), work (W_i), and the time required to redeem WIC benefits ($\theta_i \delta_i$), such that $T = L_i + W_i + \theta_i \delta_i$. Assuming income is entirely derived from work, consumption can be expressed as $C_i = w \cdot W_i + \theta_i Z_i$, where w represents the wage rate. The individual's utility is given by:

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

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

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

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

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

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

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

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

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

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

$$\Pr(U_i^{WIC} > U_i^{\text{non WIC}})^{2}.$$

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

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

By envelope theorem, we obtain:

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

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

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

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

4 Data

4.1 Vital Statistics Natality Data

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

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

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

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

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

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

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

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

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

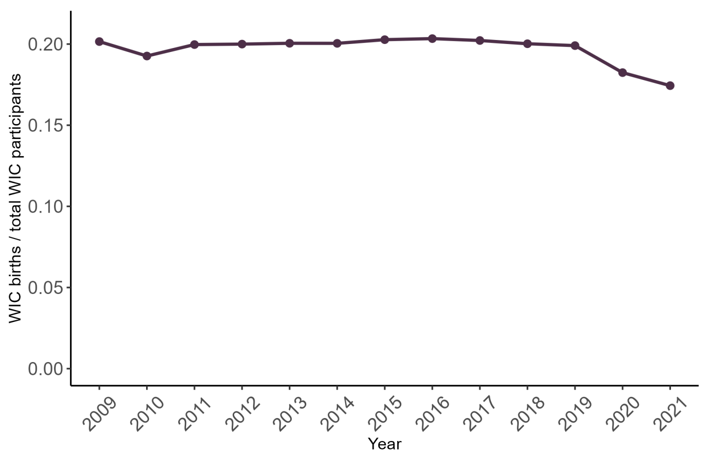


FIGURE 3: RATIO OF WIC BIRTHS TO TOTAL WIC PARTICIPANTS

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

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

TABLE 1: COMPARING NATALITY DATA WITH OTHER SURVEY DATA

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

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

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

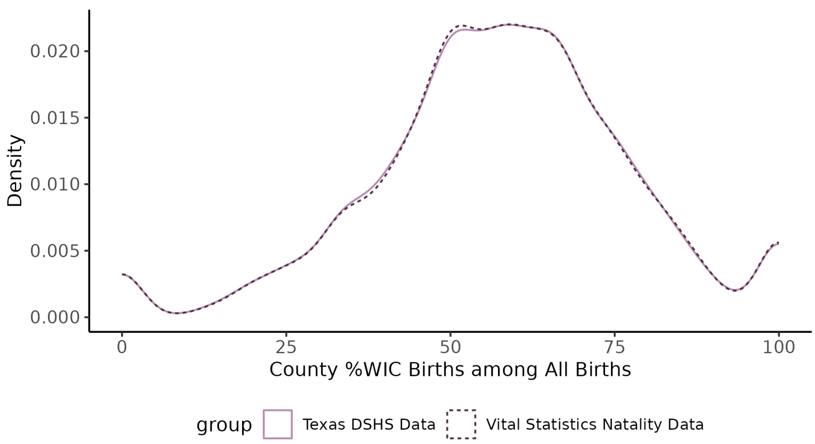


FIGURE 4: DISTRIBUTION OF COUNTY-LEVEL SHARE OF WIC BIRTH

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

4.2 WIC EBT roll-out

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

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

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

TABLE 2: TIMING OF WIC EBT IMPLEMENTATION AND COUNTY BASELINE CHARACTERISTICS

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

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

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

5 Methods

5.1 Empirical strategy

To estimate effects of WIC EBT implementation, we compare cohorts born before and after the EBT implementation in counties that implemented WIC EBT with counties that have not yet implemented WIC EBT. Our baseline regression model is:

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

where Y_{ct} is outcome variable measured for county c in year t, η_c and λ_t are county and year fixed effects to control for national economic shocks and county time-invariant unobserved heterogeneity, θ_{ct} is census-region-by-year fixed effect[3] to account for differential trends of outcomes across geograhical areas, $Z_c t$ is county baseline characteristics listed in Table 2 interacted with linear time trend to control for differential trends across regions with different baseline characteristics, X_{ct} is county-by-year employment rate to control for county-by-year-level local economic conditions, and ε_{ct} is an error term.

As documented in de Chaisemartin and D'Haultfœuille (2020) as well as Goodman-Bacon (2021), Imai and Kim (2021), and Sun and Abraham (2021), a standard two-way fixed effects (TWFE) OLS estimator with staggered treatment timing and heterogeneous treatment effects will implicitly make comparisons to all other units, aggregating these comparisons up with weights that may be negative. As a result, the TWFE estimator is not consistent for the estimand of interest - the average treatment effect on the treated (ATT). We use the interaction weighted (IW) estimator proposed by Sun and Abraham (2021) in our baseline results to avoid this issue. The IW estimator uses the last-treated counties as the control group. 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) and Lin and Zhang (2022) show that the IW estimator is consistent under assumptions of parallel trends conditional on covariates, no anticipation, and the outcomes of the comparison group (last-treated counties) in a given period are only

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

linearly correlated with the contemporary covariates. In Section 7.6, we discuss results using other popular staggered difference-in-difference estimators as well as traditional TWFE estimators. Our results are not driven by estimation method.

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

5.2 High-impact groups

To estimate an ATT, our analysis would be ideally limited to WIC-eligible mothers. However, birth certificates do not provide data on WIC eligibility or maternal income. As an alternative, we restrict our sample to subgroups more likely to be eligible for WIC, defined by specific maternal characteristics. Alternative might involve using machine learning to train a predictive model for the probability of being WIC-eligible, based on all overlapping covariates in the natality data and SIPP. This model could then be used to estimate WIC eligibility probabilities in the natality data. However, this approach is not feasible in this context due to the limited number of overlapping covariates.

We focus on the overlapping covariates in the natality data and SIPP—maternal age, education, marital status, race, and Hispanic origin—as these are the most commonly reported demographic characteristics. The SIPP provides valuable insight into the demographic characteristics of WIC-eligible individuals, as it includes information on household income and program participation[4]. 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 high-impact groups.

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

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 estimated WIC eligibility on individual maternal characteristics, controlling for state and panel fixed effects. These regression results align with the sample means reported in the other columns, suggesting that variations in WIC-eligible shares across maternal characteristic groups may not be driven by unobserved state or panel factors. When we discuss EBT's effects on WIC participation and birth outcomes, we present results for these two groups in addition to those for the full sample. 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 main results focus on the effects of WIC EBT implementation on WIC participation rates of mothers of newborns for both full sample and high-impact groups. We then explore the heterogeneity of these effects across gender, race, ethnicity (Hispanic or non-Hispanic), birth order, and income quantiles. Lastly, we examine the effects of WIC EBT on birth outcomes, as improving birth outcomes is the ultimate goal of the program. The expectation was that EBT would increase both WIC participation and redemption rates, thereby improving maternal nutrition and, consequently, birth outcomes.

6.1 Primary results: WIC EBT increases WIC participation among mothers of newborns

Table 4 shows that ITTs of EBT on WIC participation are 1.26, 1.56, and 1.62 percentage points for all mothers, mothers with no more than a high school education, and mothers without an infant's father listed on the birth certificate, 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 listed on the birth certificate, the shares of WIC-eligible individuals are 65.29% and 64.81%, respectively. Therefore, in terms 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 listed on the birth certificate.

TABLE 4: EFFECTS OF WIC EBT ON WIC PARTICIPATION

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

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

Figure 5 indicates that pre-EBT trends are relatively flat, suggesting minimal differential trends before EBT implementation. We further test the sensitivity to potential violations of the parallel trend assumption in Section 7.5. Although the WIC-eligible may have anticipated the EBT implementation, Figure 5 shows that any such anticipation did not affect their participation decisions, as the relative increase in WIC participation only occurs after EBT implementation. Finally, we test the assumption that WIC participation in the last-treated counties is linearly related to these covariates (Sun and Abraham, 2021; Lin and Zhang, 2022). This assumption is relatively trivial since polynomial terms of these covariates can be incorporated into the model (Lin and Zhang, 2022). To confirm this, we further control for quadratic and cubic terms of all covariates. The results are shown in Figure A2a. We do not observe any substantial changes in results.

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 unlikely to reflect regional trends. We explore this hypothesis further through additional tests presented below.

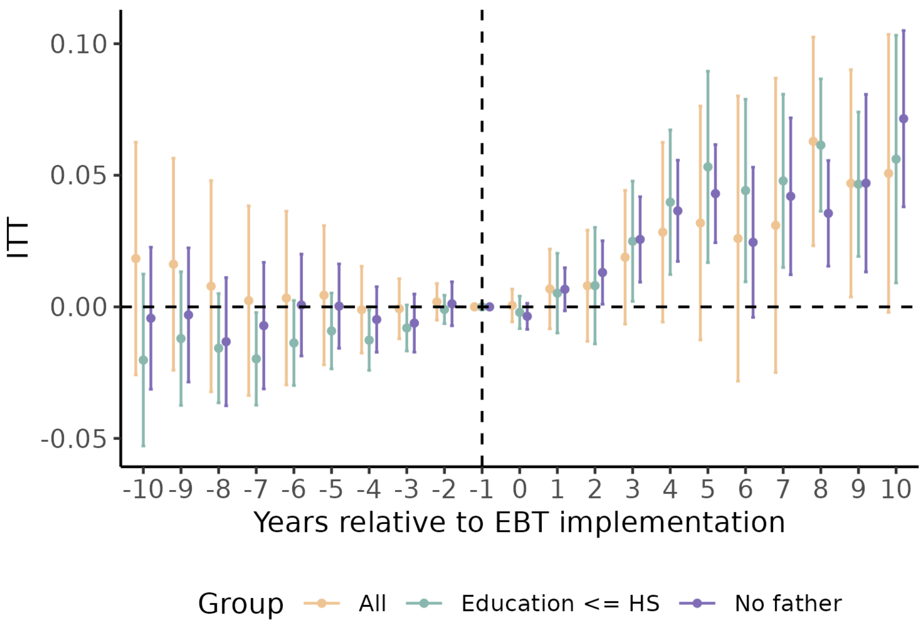


FIGURE 5: DYNAMIC EFFECTS OF WIC EBT ON WIC PARTICIPATION

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. Observations ten or more years after EBT implementation are captured by the dummy for year 10. Similarly, observations ten or more years before EBT implementation are captured by the dummy for year −10. 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 investigate the heterogeneity of EBT effects across maternal race, ethnicity, age, birth order, and income quantiles: results are presented in Table A2. We find that observed effects are primarily driven by white mothers, younger mothers under the age of 30, and mothers residing in low-income counties. The finding that white mothers benefit most from the EBT transition aligns with observations in Section 8, which show that the effect of EBT on WIC participation is substantially higher in rural areas.

6.2 Secondary results: WIC EBT reduces adverse birth outcomes

Given the positive effects of WIC EBT on WIC participation among mothers of newborns, we now turn to its impact on birth outcomes. WIC EBT can increase WIC participation among mothers of newborns through both the extensive margin (encouraging more WIC-eligible individuals to participate) and the intensive margin (existing participants redeem a greater share of their WIC benefits), potentially contributing to improved birth outcomes. However, we do not 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 provides some evidence that observed changes in birth outcomes are less likely to be attributable to an increase in the share of WIC benefits redeemed and more likely to be driven by an increase in participation. We now consider the effects of WIC EBT on three key birth outcomes: birth weight, the likelihood of low birth weight (defined as birth weight < 2500 grams), and the likelihood of preterm birth (gestation < 37 weeks). We find that EBT implementation significantly reduces adverse birth outcomes for high-impact groups.

Table 5 shows that while the effects of WIC EBT on birth outcomes are not precisely estimated for the full sample, they are statically significant for groups more likely to be WIC-eligible, mirroring earlier findings on WIC participation. Specifically, the ITT effects of EBT on the likelihood of low birth weight are -0.31 and -0.4 percentage points for mothers with no more than a high school education and mothers without an infant's father listed on the birth certificate, 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. By multiplying the average number of births per year by the TOT effect of EBT, we estimate that WIC EBT lifts 6,774 (2,456) births by mothers with no more than a high school education (mothers without an infant's father listed on the birth certificate) out of low birth weight each year, and 7,621 (3,248) births out of preterm status annually.

Figures 6a–6c indicate that pre-implementation trends are flat for the full sample and

Table 5: Effects of WIC EBT on birth outcomes

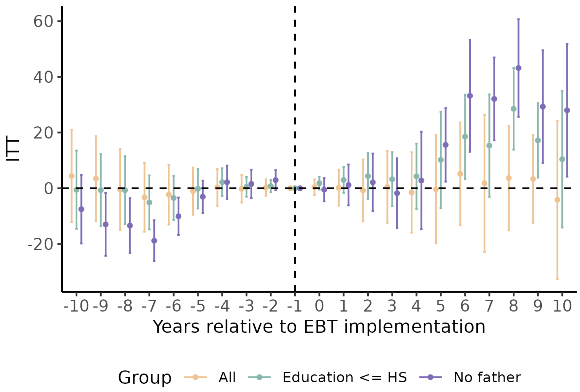
	Birth weight (grams)			Low birth weight (birth weight $<$ 2500 grams)			Preterm (gestation $<$ 37 weeks)		
	All births (1)	Edu$\leq$HS (2)	No father (3)	All births (4)	Edu$\leq$HS (5)	No father (6)	All births (7)	Edu$\leq$HS (8)	No father (9)
Born after EBT	-0.1545	4.532	4.812	-0.0009	-0.0031	-0.0040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

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

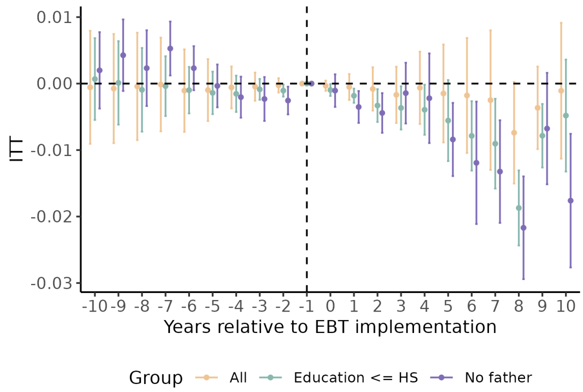
for mothers with no more than a high school education, suggesting no prior systematic changes in outcomes prior to EBT implementation. The effects observed for mothers without an infant's father listed on the birth certificate may be potentially influenced by pre-existing trends. However, these trends occur well before the EBT implementation and do not fully account for the observed impacts. We also examine the sensitivity of these outcomes to potential violations of the parallel trend assumption in Section 7.5. As shown in Figures A2b-A2d, we do not observe substantial changes in the results when adding quadratic and cubic terms of covariates.

How much does the reduction in adverse birth outcomes translate into hospital cost savings? Using estimates from Almond, Chay and Lee (2005), we provide a back-of-the-envelope estimate of hospital cost savings associated with WIC EBT, focused solely on low birth weight. Almond, Chay and Lee (2005)'s estimates account for the omitted variable bias in the cross-sectional estimates reported by most of the scientific literature. They do not provide similar estimates for preterm births. Table A3 shows that for mothers with no more than a high school education (mothers without an infant's father listed on the birth certificate), the annual hospital cost savings are estimated at \$4.92 million (\$2.8 million). When compared to public expenditure, the hospital cost savings from reduced low birth weight alone amount to 21.78% (12.39%) of the USDA's annual EBT investment[5].

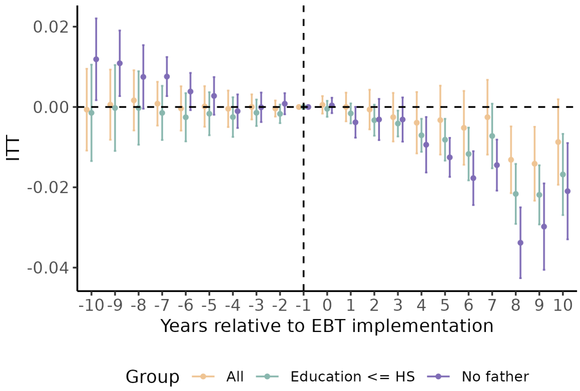
[5] The USDA's investment in the EBT transition was \$30.5 million during the 2013 fiscal year (USDA Food and Nutrition Service, 2017). We convert \$30.5 million to 2000 dollars by dividing it by 1.35. The calculation for 21.78% is: $\frac{4.92\times1.35}{30.5}$. Given that improved birth outcomes have been linked to various long-run outcomes, such as higher educational attainment (Behrman and Rosenzweig, 2004) and adult income (Bharadwaj, Lundborg and Rooth, 2018), WIC EBT is likely to generate a positive net benefit in the long run.



(A) Birth weight



(B) Low birth weight



(C) Preterm

FIGURE 6: DYNAMIC EFFECTS OF WIC EBT ON BIRTH OUTCOMES

Notes: We estimate dynamic effects using interaction weighted estimators proposed by Sun and Abraham (2021). We collapse birth data to county-of-maternal-residence-by-year-of-birth cells. Observations ten or more years after EBT implementation are captured by the dummy for year 10. Similarly, observations ten or more years before EBT implementation are captured by the dummy for year -10. 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 asking that whether advantaged mothers—defined as those with more than a high school education and a father listed on the infant's birth certificate—are less affected by WIC EBT implementation, given the variability observed in the full sample. Results in Table A4 show that, for advantaged mothers, the estimates are statistically significantly different from zero with standard errors clustered at the county level but are not statistically significant when clustered at the state level; the effect sizes for this group are also substantially smaller than those observed in high-impact groups. Advantaged mothers could be affected because maternal education and the presence of the infant's father on the birth certificate, though highly correlated with WIC eligibility, are not perfect proxies.

7.2 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[6]. If our results do not capture any spurious trends in the treated counties, we should observe no significant effects based on these hypothetical timings. Results in Table A5 line up with this hypothesis: the pseudo-treatment effects are statistically insignificant, small in magnitude, and occasionally have the opposite sign, suggesting that our results are unlikely to be driven by spurious pre-trends.

7.3 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[7]. This randomization test is conducted for effects on WIC participation for mothers with high school education or less and for mothers without an infant's father listed on the birth certificate 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 A3).

7.4 Event-time balanced panel

Another concern with our main results is the unbalanced panel of treated counties over event time, which could mean that our results are influenced by changes in the composition of counties across event time. However, estimates from a balanced panel also have limitations. Given the widespread implementation of EBT across states and data availability starting in 2009, 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

[6]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 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.

[7]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).

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 6 presents results for a balanced panel from period -4 to period 4, which align with our main results. In this balanced panel, the effects on WIC participation are larger and more precise. The dynamic effects based on this balanced panel are shown in Figure 7, which are also consistent with our previous findings. However, this balanced panel includes only 844 counties, far fewer than the 2,489 counties used in our main specification. Although we prefer to use all available data in our main specification, the balanced panel results provide evidence that our findings are not driven by changes in the composition of counties over event time.

TABLE 6: EFFECTS OF WIC EBT ON WIC PARTICIPATION, EVENT-TIME BALANCED PANEL

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

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

7.5 Pretrend test

Some of our estimates of dynamic effects might be influenced by pre-existing differential trends, potentially compromising identification. In this section, we assess the power of our pre-trend test. Following the procedure outlined in Roth (2022), we estimate that we can detect a positive linear pre-trend in WIC participation among mothers with no more than a high school education (and mothers without an infant's father listed on the birth certificate) with a slope of 0.49 (0.48) percentage points with 80 percent power, and of 0.31 (0.31) percentage points with 50 percent power. The resulting biases for all post-periods are 0.86 (0.95) percentage points with 80 percent power and 0.55 (0.61) percentage points with 50 percent power[8]. Our overall ITT estimate is 1.56 (1.62), which is 1.81 (1.71) times as large as this potential bias with 80 percent power, and 2.84 (2.66) times as large with 50 percent power.

[8] We calculate the biases following the formula presented in Roth (2022), which takes into account the additional bias introduced by passing a pretest.

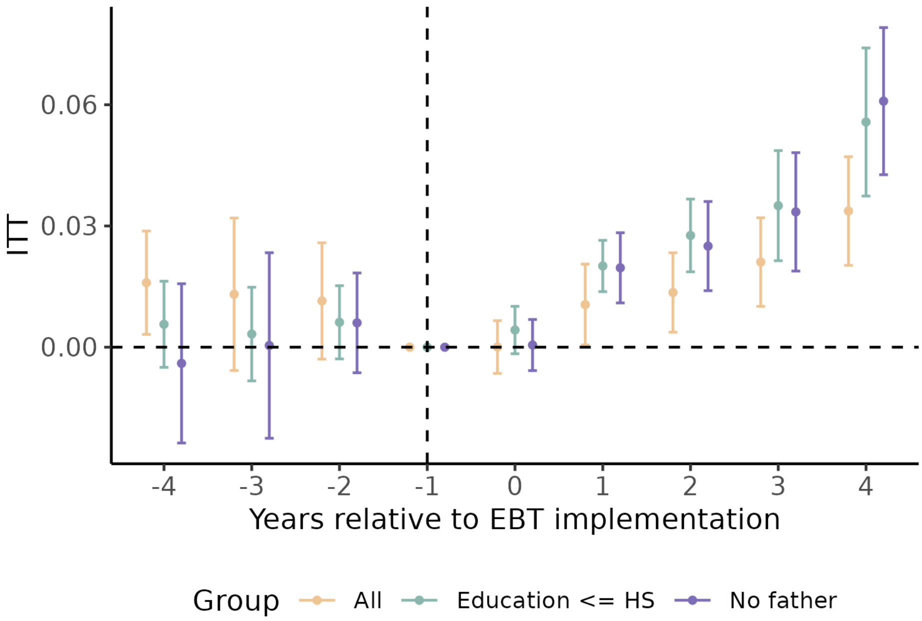


FIGURE 7: DYNAMIC EFFECTS OF WIC EBT ON WIC PARTICIPATION, EVENT-TIME BALANCED PANEL

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

In terms of birth outcomes, we estimate that we can detect a negative linear pre-trend in the likelihood of low birth weight among mothers with no more than a high school education (and mothers without an infant's father listed on the birth certificate) with a slope of -0.07 (-0.12) with 80 percent power, and of -0.04 (-0.07) with 50 percent power. The resulting biases for all post-periods are -0.12 (-0.24) with 80 percent power and -0.07 (-0.14) with 50 percent power. Our overall ITT estimate is -0.31 (-0.4), which is 2.58 (1.67) times as large as this potential bias with 80 percent power, and 4.43 (2.86) times as large with 50 percent power, both in absolute value. Similarly, we estimate that we can detect a negative linear pre-trend in the likelihood of preterm births among mothers with no more than a high school education (and mothers without an infant's father listed on the birth certificate) with a slope of -0.14 (-0.16) with 80 percent power, and of -0.09 (-0.1) with 50 percent power. The resulting biases for all post-periods are -0.25 (-0.32) with 80 percent power and -0.16 (-0.19) with 50 percent power. Our overall ITT estimate is -0.35 (-0.53), which is 1.4 (1.66) times as large as this potential bias with 80 percent power, and 2.19 (2.79) times as large with 50 percent power, both in absolute value.

Figures A4a-A6d overlay our event study estimates alongside the hypothesized differential trends and the counterfactual estimates conditional on not finding a significant pre-trend if the true pre-trend were the hypothesized trend. Our estimates are significantly larger than the counterfactual estimates in absolute value, suggesting that our results are not caused by pre-trends.

In summary, we find that the potential bias from hypothesized differential trends is substantially smaller than the treatment effects, suggesting that our findings on WIC participation, low birth weight, and preterm births for high-impact groups are not attributable to pre-trends.

7.6 Robustness to estimation methods

We also present results using alternative staggered difference-in-difference methods, including traditional two-way fixed effects estimators (Figure A7a), estimators from Callaway and Sant'Anna (2021) using never-treated or not-yet-treated groups as the control group (Figures A7b and A7c), and imputation estimators by Borusyak, Jaravel and Spiess (2024) (Figure A7d). 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.

7.7 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 A6, we present estimates defining exposure at the beginning of the first, second, or third trimester instead of at the time of birth. Estimates generally become larger and more precise, as we change the definition of exposure.

8 Potential Mechanisms

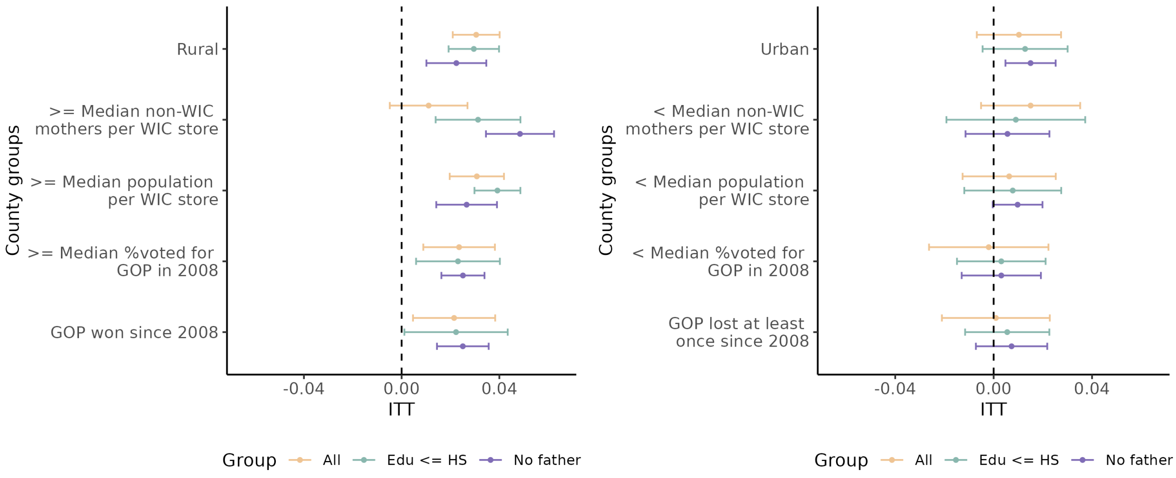
8.1 EBT's effect on WIC participation is larger in counties where participants may experience greater welfare stigma before EBT

Welfare stigma refers to the feelings of shame or degradation associated with receiving welfare benefits (Horan and Austin, 1974). Welfare stigma can deter participation in welfare programs (Moffitt, 1983). EBT can reduce welfare stigma by making WIC redemption less visible (Pukelis, Heath and Holcomb, 2024), as the EBT card closely resembles a regular credit or debit card. EBT also shortens checkout times (Hanks et al., 2019), which can minimize potential discomfort even if cashiers and other shoppers recognize a recipient's welfare status. Anecdotal evidence also suggests that EBT reduces stigma for WIC participants (Phillips et al., 2014)[9].

Examining the effect of EBT on welfare stigma is challenging due to the lack of large-scale data on both self-reported and objective measures of stigma. Instead, we identify three county groups where participants may experience greater welfare stigma: (1) rural counties; (2) counties with a potentially larger proportion of non-WIC customers or larger customer bases; and (3) counties with a higher share of Republican voters. If reducing welfare stigma is the driving mechanism, EBT would lead to a larger increase in WIC participation in regions with higher levels of existing stigma. Alsan and Yang (2022) use a similar strategy to provide suggestive evidence that fear of a family member or close contact being deported may be an explanatory mechanism for the reduced welfare program participation observed among Hispanic citizens following immigration enforcement. In Figure 8, we divide the sample by county groups and present the IW estimators for EBT's effect on WIC participation within each group. This approach ensures that identification conditions still hold, as opposed to triple-differences approach by interacting the EBT implementation dummy with county group dummies. We find that the effect of EBT implementation is generally larger in these counties, suggesting that reducing welfare stigma leads to increased WIC participation.

First, sociologists have found that welfare stigma tends to be larger in rural communities (Findeis et al., 2001; Meij, Haartsen and Meijering, 2020). For example, Findeis et al. (2001) find that smaller, more integrated networks can amplify the stigma attached to needing help, which may diminish families' willingness to participate in welfare programs. They also note that rural families worry that accepting welfare could harm their family reputation, which is important for securing work opportunities in rural communities. Anecdotal evidence also documents that, in rural areas, WIC participants reported being identified as "one of them" by other shoppers or being publicly criticized by store clerks for "wasting the government's money" (Isaacs, Shriver and Haldeman, 2020). The first set of estimates from top to bottom in Figures 8a and 8b indicate that EBT's effect on WIC participation is both larger and more precise in rural counties compared to urban counties. EBT increases WIC

[9] Phillips et al. (2014) documents that, for example, a Michigan WIC participant shared: "Even now [with self-checkout]. . . you can check out on your own [with] no hassle, so you don't have to worry about people or the cashier having a fit about [your WIC].", and a Nevada WIC participant said: "[When] the cashiers see you coming with WIC, they're not like, 'Oh no.' Before, when they had to do everything ... it was kind of complicated for them, but now ... it's a lot easier for them to check us out [and] a lot faster too."



(A) Groups that may face greater welfare stigma (B) Groups that may face smaller welfare stigma

FIGURE 8: EFFECTS OF WIC EBT ON WIC PARTICIPATION BY COUNTY CHARACTERISTICS RELATED TO WELFARE STIGMA

Notes: Urban and rural areas are defined by the NCHS 2006 Urban-Rural Classification Scheme for Counties. Population data is collected from the Intercensal Population Estimates. Data on non-WIC mothers is from the Vital Statistics Natality Data. Data on WIC vendors is from the WIC Integrity Profiles for 2009–2016. Population and non-WIC mothers per vendor are calculated as the county-level average from 2009 to 2016. The share of voters who supported the Republican candidate in the 2008 presidential election is collected by Morris (2016). Data on the last time the Republican Party won in the presidential elections is collected by Leip (2025). Medians are weighted by population. We divide the sample by county groups and present the interaction weighted estimators proposed by Sun and Abraham (2021) for EBT's effect on WIC participation within each group. 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 state.

participation among all mothers by 3.05 percentage points (ITT) in rural counties, which is three times higher than the imprecise 1.03 percentage point (ITT) increase observed in urban counties. For high-impact groups, EBT's effect on WIC participation (ITT) is 2.95 percentage points for mothers with no more than a high school education and 2.24 percentage points for mothers without an infant's father listed on the birth certificate in rural counties, compared to 1.28 and 1.5 percentage points, respectively, in urban counties.

Second, Celhay, Meyer and Mittag (2022) find that welfare stigma is typically greater when fewer peers engage in the stigmatized behavior. To capture this dynamic, we calculate the number of non-WIC mothers per WIC vendor as a proxy for peer engagement in WIC redemption. A higher number of non-WIC mothers per WIC vendor indicates a greater likelihood of shopping in an environment where fewer peers are redeeming WIC benefits. We also calculate the population per WIC vendor to measure store crowdedness. Participants may feel greater stigma in a more crowded store, as more people are likely to observe their welfare status. Participants who require longer checkout times may feel greater pressure in a crowded store with longer lines behind them. The second and third sets of estimates from top to bottom in Figures 8a and 8b show that EBT's effect on WIC participation is both larger and more precise in counties with at least the median number of non-WIC mothers per WIC store and at least the median population per WIC store compared to other counties. EBT's effects on WIC participation among all mothers are both imprecise and of similar magnitudes in counties with high and low numbers of non-WIC mothers per WIC vendor. However, among high-impact groups in counties with a high number of non-WIC mothers per WIC vendor, as well as across all samples in counties with a high population per WIC vendor, EBT's effects are 2.5 to 10 times larger than those observed in the other halves of the counties. Specifically, EBT's effect on WIC participation (ITT) is 3.13 percentage points for mothers with no more than a high school education and 4.85 percentage points for mothers without an infant's father listed on the birth certificate in counties with at least the median number of non-WIC mothers per WIC store, compared to 0.9 and 0.56 percentage points, respectively, in counties with fewer than the median number of non-WIC mothers per WIC store. Similarly, EBT's effect on WIC participation (ITT) is 3.08 percentage points for all mothers, 3.92 percentage points for mothers with no more than a high school education, 2.66 percentage points for mothers without an infant's father listed on the birth certificate in counties with at least the median population per WIC store, compared to 0.63, 0.78, and 0.97 percentage points, respectively, in counties with fewer than the median population per WIC store.

Our final piece of suggestive evidence leverages the observation that Republicans are more likely to view participation in welfare programs negatively (Levy, 2021; Goenka and Thomas, 2022). This suggests that individuals may experience greater welfare stigma in areas with a higher concentration of Republican voters. A Pew Research Center report by Doherty, Kiley and Asheer (2019) finds that Republicans and Republican-leaning individuals are less likely to support expanding government assistance for people in need and are more inclined to believe statements such as "poor people have it easy because they can get government benefits without doing anything in return" and "most people can get ahead if they are willing to work hard." To capture the possible higher welfare stigma caused by the negative attitudes of Republicans towards welfare, we calculate the share of voters who supported the Republican candidate in the 2008 presidential election using data collected by Morris (2016) and collect data on the last time the Republican Party won in the presidential elections from Leip (2025). The last two sets of estimates from top to bottom in Figures 8a and 8b suggest that EBT's effect on WIC participation is both larger and more precise in counties with at least the median share of Republican voters in the 2008 presidential election, and in counties where the Republican Party has consistently won presidential elections since 2008, compared to other counties. EBT increases WIC participation among all mothers by

2.35 percentage points (ITT) in counties with at least the median share of Republican voters in the 2008 presidential election, compared to an imprecise and slight decline in participation in counties with a lower-than-median share of Republican voters. Similarly, EBT increases WIC participation among all mothers by 2.15 percentage points (ITT) in counties where the Republican Party has consistently won presidential elections since 2008, compared to an imprecise 0.09 percentage-point increase in counties where the Republican Party has lost at least one presidential election since 2008. For high-impact groups, EBT's effect on WIC participation (ITT) is 2.92 percentage points for mothers with no more than a high school education and 2.66 percentage points for mothers without an infant's father listed on the birth certificate in counties with at least the median share of Republican voters in the 2008 presidential election, compared to 0.78 and 0.97 percentage points, respectively, in counties with fewer than the median share of Republican voters in the 2008 presidential election. Similarly, EBT's effect on WIC participation (ITT) is 2.31 percentage points for mothers with no more than a high school education and 2.51 percentage points for mothers without an infant's father listed on the birth certificate in counties where the Republican Party has consistently won presidential elections since 2008, compared to 0.31 and 0.31 percentage points, respectively, in counties where the Republican Party has lost at least one presidential election since 2008.

To sum up, we find that EBT's impact on WIC participation is greater in rural counties, counties with a higher proportion of non-WIC customers or larger customer bases, and counties with a higher share of Republican voters, where welfare participants may experience larger welfare stigma. These findings suggest that reducing welfare stigma may be an important driver of EBT's positive effect on WIC participation.

8.2 WIC EBT reduces WIC vendor access

Our model in Section 3 predicts that EBT implementation would lead to a reduction in WIC vendor access. To test this, we linked WIC EBT rollout data to WIC Integrity Profiles 2009–2016 to assess the impact of WIC EBT on the number of 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 the timing of EBT implementation to fiscal years to match the WIC vendor data and then aggregate the vendor-level data by county and fiscal year. All regressions and mean calculations for the dependent variable are weighted by county population.

Table 7 shows that WIC EBT reduces both the total and per capita number of WIC vendors in urban and rural areas, which is consistent with the findings of Meckel (2020) and Ambrozek et al. (2024). 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

TABLE 7: EFFECTS OF WIC EBT ON WIC VENDORS

	Rural counties		Urban counties	
	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	-0.4929	-0.0027	-3.527	-0.0247
	(0.0970)***	(0.0008)***	(2.142)*	(0.0077)***
	⟨0.0897⟩***	⟨0.0010⟩***	⟨2.914⟩	⟨0.0105⟩**
Observations	11,349	11,349	5,662	5,662
R^2	0.9753	0.9431	0.9933	0.5729
Dep. var. mean	5.3509	0.0368	126.0577	0.1167

Notes: We report interaction weighted estimators proposed by Sun and Abraham (2021). We control for county and fiscal 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.

vendors per capita, to begin with. The overall positive effect of EBT implementation on WIC participation suggests that, while reduced WIC vendor access could potentially discourage participation, this negative effect is outweighed by the positive impact driven by reducing welfare stigma.

8.3 Mortality selection is unlikely to drive our results

Demographic composition change in the cell due to mortality selection might explain observed positive effect on WIC participation. Specifically, if over time the treated cell includes more mothers who are inclined to participate in WIC, this shift would lead to an increase in WIC participation. Table 8 shows that EBT implementation does not significantly alter the composition of maternal characteristics in the cell, except for a slight decrease in white mothers 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.

Table 8: Effects of WIC EBT on maternal characteristics

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

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

9 Magnitudes

How do our estimates on WIC participation compare to those of other papers that estimate the effect of WIC EBT on participation? Meckel (2020) finds a decline in the average number of mothers participating in WIC after the introduction of EBT in 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 the 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 suggest heterogeneity in the effects of EBT across states that adopted the program at different times. However, unlike Texas, we do not observe a significant decline in WIC participation in any other state following the implementation of EBT.

We find that negative effects of EBT on WIC births reported by Meckel (2020) are likely to be driven by pre-existing trends, as shown in Figures A8a-A8d. We observe a decreasing trend in the number of WIC births in treated counties versus control counties in the pre-period when we replicate the event study estimates from (Meckel, 2020) while allowing a larger event window (36 months before and after EBT implementation). We perform a similar power calculation for the pre-trends in Meckel (2020)'s estimates on WIC mothers. Our analysis shows that we can detect a negative linear pre-trend in the number of WIC mothers with a slope of -0.65 with 80 percent power and -0.4 with 50 percent power. The resulting biases for all post-periods are -5.18 with 80 percent power and -3.16 with 50 percent power. Her overall ITT estimate is -3.86, which is smaller than this potential bias with 80 percent power in absolute value, and almost as large as that with 50 percent power. This suggests the magnitude of the effect she found could be attributable to differences in trends rather than the effect of EBT. Figures A9a and A9b show that Meckel (2020)'s event study estimates are very similar in magnitude to the counterfactual estimates conditional on not finding a significant pre-trend if the true pre-trend were the hypothesized trend, further supporting this possibility.

One explanation for Meckel (2020)'s results is the lack of federal support under the HHFKA of 2010, which increases the likelihood of retailers continuing to participate in WIC. As discussed in Section 3, negative effects of WIC EBT on vendor accessibility are potentially mitigated by the technical and financial support from USDA following the HHFKA of 2010 (USDA Food and Nutrition Service, 2016). As a result, we observe an overall positive impact of WIC EBT on WIC participation. Learning could also contribute to the positive effects (Ambrozek et al., 2024), though we have limited knowledge about the extent to which state agencies and WIC vendors learned from early adopters.

10 Discussion and Conclusion

10.1 Summary

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

10.2 Limitations

Our approach has some important limitations. The first is that the natality data will not measure WIC participation among those who enter WIC after the birth certificate is filed. This may include older siblings who enroll at the time that the pregnant individual or newly born infant joins the program. This implies we capture 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 understanding children's participation remains important and understudied. That said, 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 much of these important windows.

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

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

10.3 Conclusion

Our data and approach capture the effects of WIC EBT on participation for most of the United States and for a longer period of time. As a result, our average treatment effect on the treated estimates are more representative of the net effect of EBT than prior work using only one or a few states and shorter panels. 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 of EBT on WIC participation are driven by reducing welfare stigma, which outweighs the effect of reduced WIC vendor authorization.

Amid declining WIC enrollment among eligible populations, policymakers are interested in programmatic changes that can boost WIC participation. While the EBT transition is complete, our work indicates that policies that bring the WIC shopping experience closer to a "normal" food shopping experience and that can reduce stigma during WIC shopping increase WIC participation. Our results suggest the online shopping for WIC food benefits could increase WIC participation. WIC online shopping is currently being piloted at select retailers in Iowa, Massachusetts, Minnesota, Nebraska, South Dakota (Rosebud Sioux), and Washington (Center for Nutrition & Health Impact, 2024). 62% of WIC participants indicate that they would use online WIC shopping if it were available, and 53% cited lack of access to online shopping as a reason they did not redeem all of their benefits – the most common reason for not redeeming benefits fully (Ritchie et al., 2021). While online WIC shopping requires substantial updates to program rules and existing technology, our results on stigma and participation suggest that this next technological change in WIC will further boost WIC participation.

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Appendix

A Figures and tables

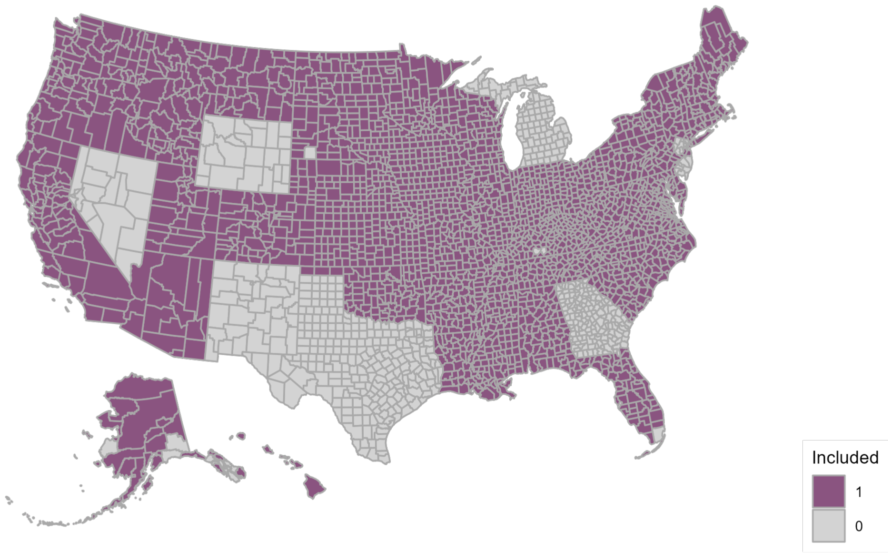


FIGURE A1: COUNTIES IN OUR SAMPLE

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

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

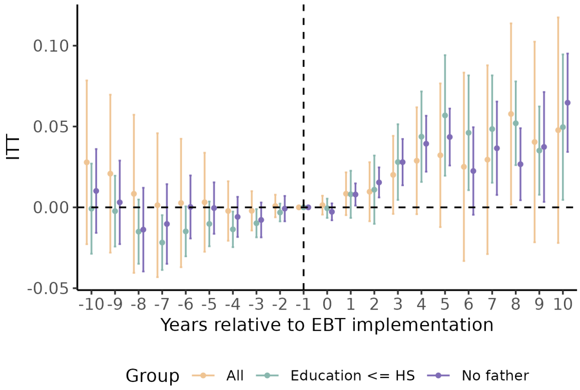
Notes: We report interaction weighted estimators proposed by Sun and Abraham (2021). We collapse birth data to county-of-maternal-residence-by-year-of-birth cells. We control for county and year fixed effects, census-region-specific linear time trend, county baseline characteristics from 2006-2009 interacted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

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

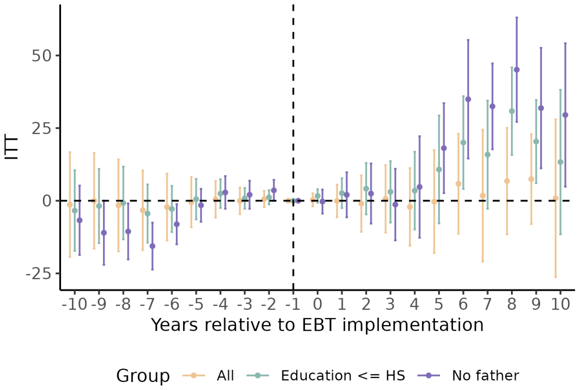
	White	Black	Asian	Hispanic	Non-Hispanic	Age $\leq$ 22	22 < Age < 30	Age $\geq$ 30
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Born after EBT	0.0102	-0.0009	-0.0013	0.0106	0.0114	0.0185	0.0168	0.0095
	(0.0031)***	(0.0043)	(0.0047)	(0.0114)	(0.0047)**	(0.0065)***	(0.0057)***	(0.0046)**
	⟨0.0030⟩***	⟨0.0041⟩	⟨0.0044⟩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

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

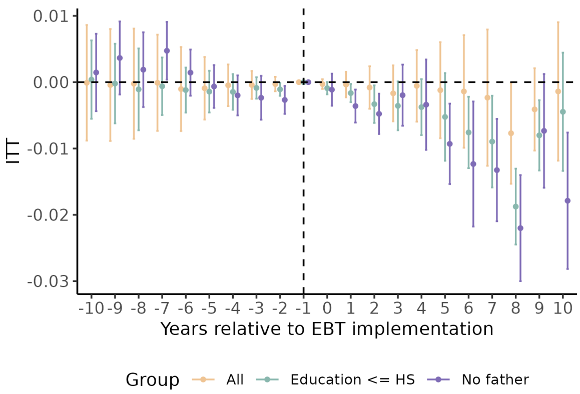
Notes: The high-income counties includes the ones where the average income between 2006 and 2008 falls within the top income quantile (1,945 counties). All other counties are categorized as the low-income counties (1,133 counties). We report interaction weighted estimators proposed by Sun and Abraham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



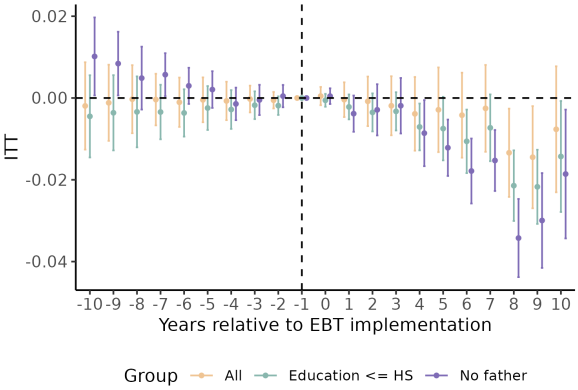
(A) WIC participation]



(B) Birth weight



(C) Low birth weight



(D) Preterm

FIGURE A2: DYNAMIC EFFECTS OF WIC EBT, ADDING QUADRATIC AND CUBIC TERMS OF COVARIATES

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. Observations ten or more years after EBT implementation are captured by the dummy for year 10. Similarly, observations ten or more years before EBT implementation are captured by the dummy for year -10. We control for county and year fixed effects, and all covariates and their quadratic and cubic terms. Regressions and dependent variable mean are weighted by the number of births in each cell. Standard errors are clustered at state level.

Table A3: Hospital cost saving of WIC EBT associated with low birth weight

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

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

Table A4: Effects of WIC EBT on advantaged mothers

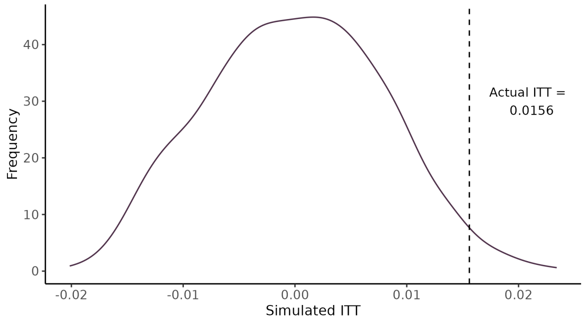
	(1)	(2)	(3)
Born after EBT	0.0074	0.0085	0.0086
	(0.0038)*	(0.0032)***	(0.0035)**
	⟨0.0114⟩	⟨0.0060⟩	⟨0.0060⟩
County fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Census region×year		✓	✓
Baseline char.×year		✓	✓
Employment rate$_{ct}$			✓
Observations	34,238	33,562	27,712
R^2	0.9402	0.9482	0.9474
Dep.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

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

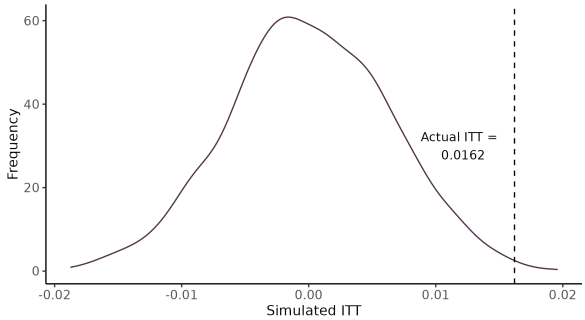
TABLE A5: PLACEBO TREATMENT TIMING

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

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



(A) WIC participation, education ≤ HS



(B) WIC participation, no father

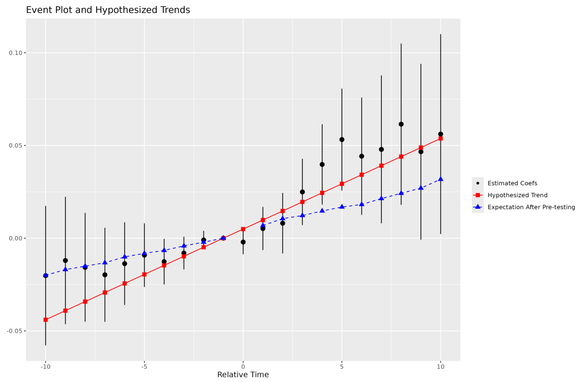
FIGURE A3: RANDOMIZATION TEST

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.

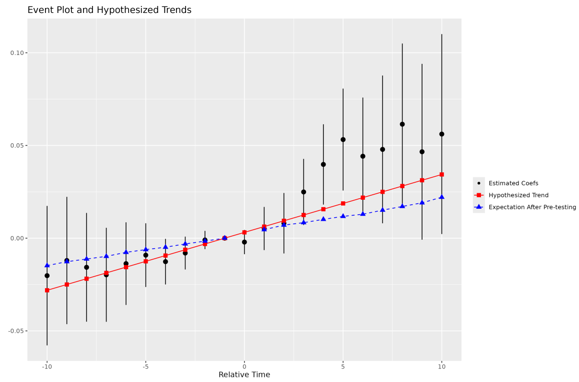
TABLE A6: ROBUSTNESS TO TIMING OF EXPOSURE

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

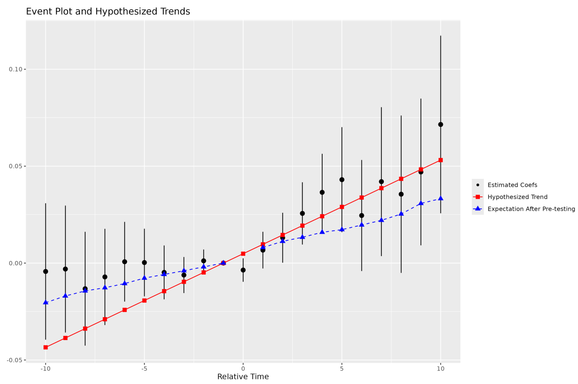
Notes: We report interaction weighted estimators proposed by Sun and Abraham (2021). The dependent variable is WIC participation rate for all regressions. We collapse birth data to county-of-maternal-residence-by-year-of-birth cells. We control for county and year fixed effects, census-region-specific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



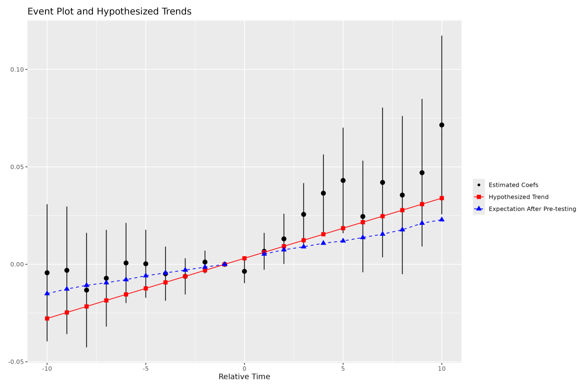
(A) Edu $\leq$ HS, with 80 percent power



(B) Edu $\leq$ HS, with 50 percent power



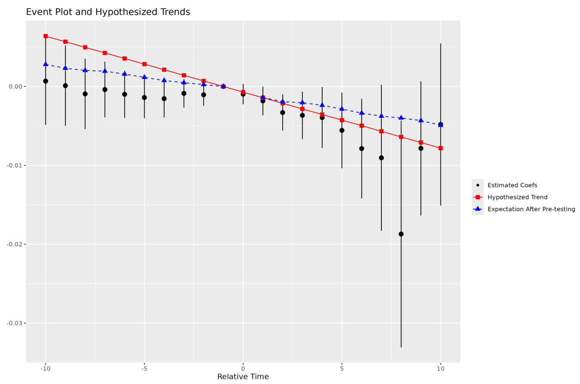
(C) No father, with 80 percent power



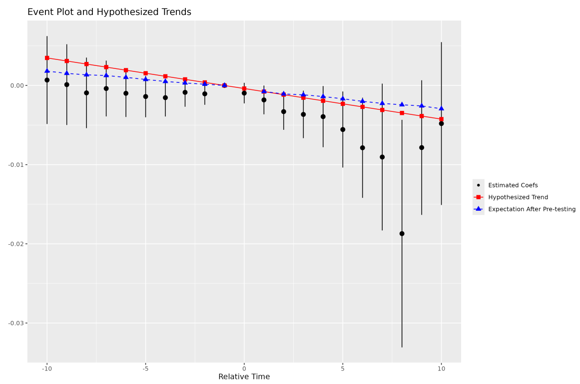
(D) No father, with 50 percent power

FIGURE A4: EXTRAPOLATING LINEAR DIFFERENTIAL PRETRENDS BY ROTH (2022), WIC PARTICIPATION

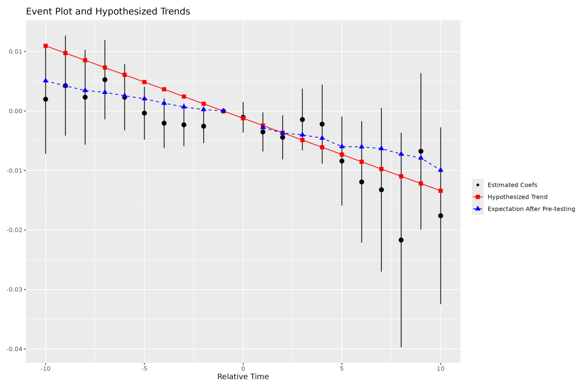
Notes: The red lines represent the hypothesized differential pre-trends. The blue lines represent what the coefficients would look like conditional on not finding a significant pre-trend if the true pre-trend were the hypothesized trend. The actual estimates are significantly larger than the blue lines, suggesting that our results are not caused by linear differential trends.



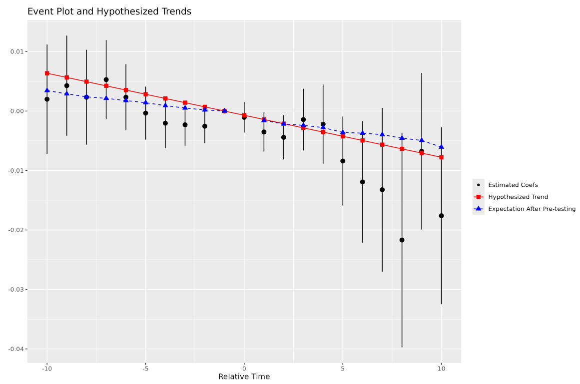
(A) Edu $\leq$ HS, with 80 percent power



(B) Edu $\leq$ HS, with 50 percent power



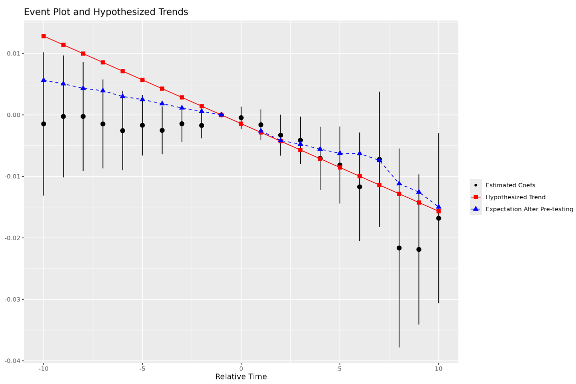
(C) No father, with 80 percent power



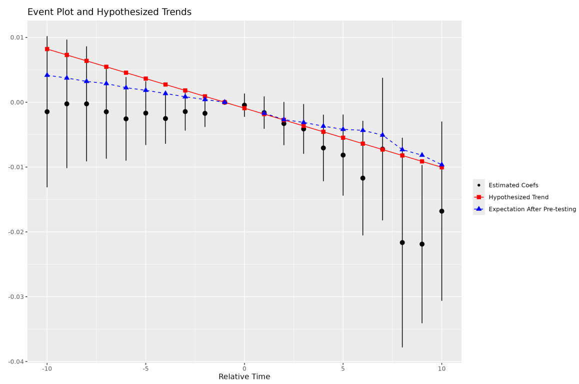
(D) No father, with 50 percent power

FIGURE A5: EXTRAPOLATING LINEAR DIFFERENTIAL PRETRENDS BY ROTH (2022), THE LIKELIHOOD OF LOW BIRTH WEIGHT

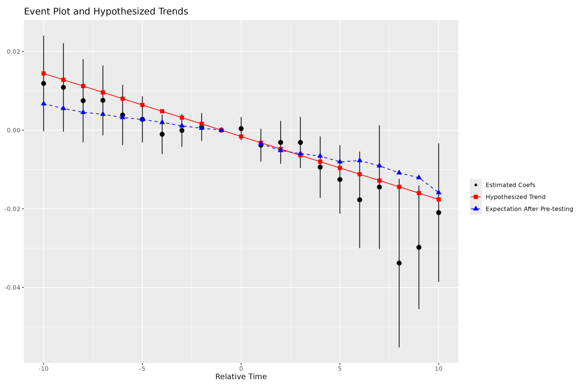
Notes: The red lines represent the hypothesized differential pre-trends. The blue lines represent what the coefficients would look like conditional on not finding a significant pre-trend if the true pre-trend were the hypothesized trend. The actual estimates are significantly larger than the blue lines, suggesting that our results are not caused by linear differential trends.



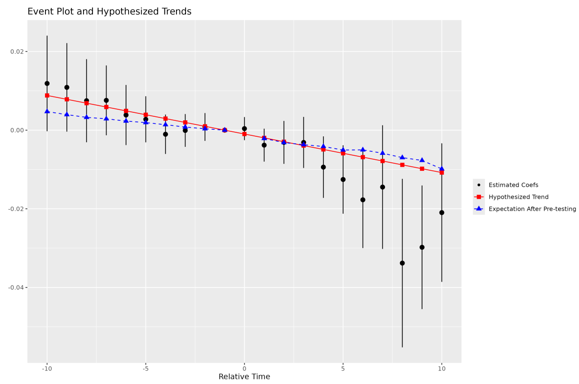
(A) Edu $\leq$ HS, with 80 percent power



(B) Edu $\leq$ HS, with 50 percent power



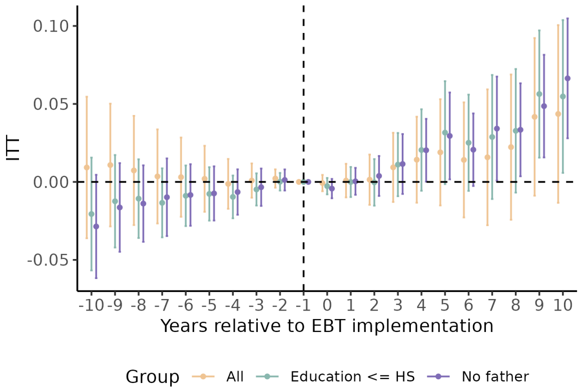
(C) No father, with 80 percent power



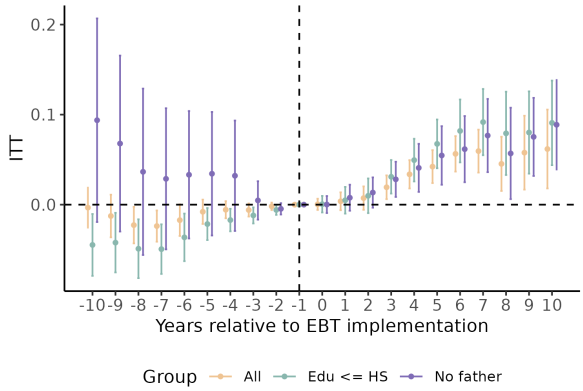
(D) No father, with 50 percent power

FIGURE A6: EXTRAPOLATING LINEAR DIFFERENTIAL PRETRENDS BY ROTH (2022), THE LIKELIHOOD OF PRETERM BIRTHS

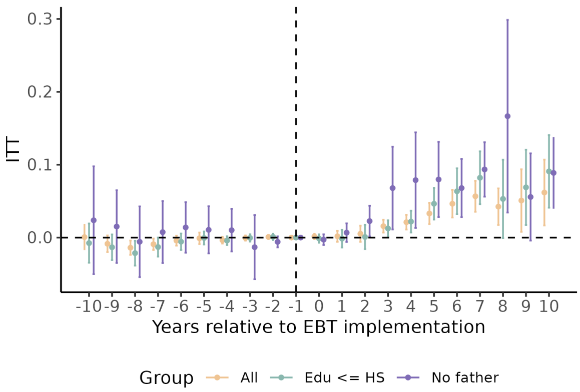
Notes: The red lines represent the hypothesized differential pre-trends. The blue lines represent what the coefficients would look like conditional on not finding a significant pre-trend if the true pre-trend were the hypothesized trend. The actual estimates are significantly larger than the blue lines, suggesting that our results are not caused by linear differential trends.



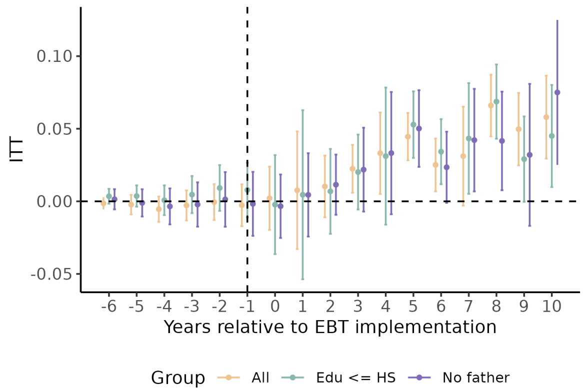
(A) Traditional TWFE estimators



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



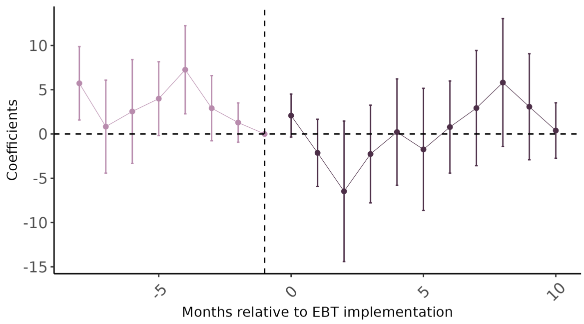
(C) Callaway and Sant'Anna (2021) estimators, the not-yet-treated as control group



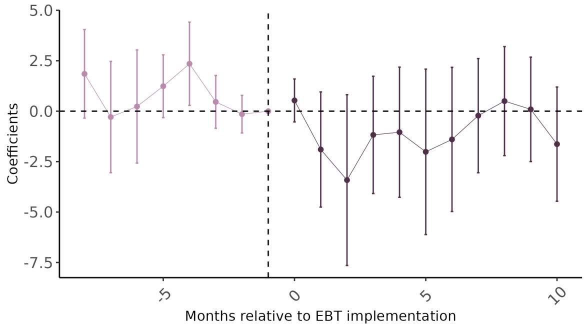
(D) Borusyak, Jaravel and Spiess (2024) estimators

FIGURE A7: DYNAMIC EFFECTS OF WIC EBT BY ESTIMATION METHODS

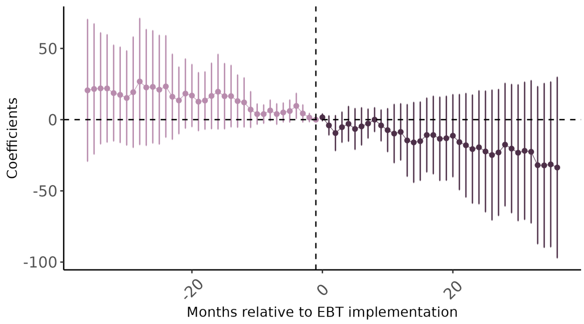
Notes: For all regressions, we collapse birth data to county-of-maternal-residence-by-year-of-birth cells. Regressions and dependent variable mean are weighted by the number of births in each cell. Standard errors are clustered at state level. Observations ten or more years after EBT implementation are captured by the dummy for year 10. Similarly, observations ten or more years before EBT implementation are captured by the dummy for year -10. 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 a 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 a shorter pre-treatment period (6 years before the treatment) to ensure relevance since this estimator use all the whole pre-treatment period as a comparison.



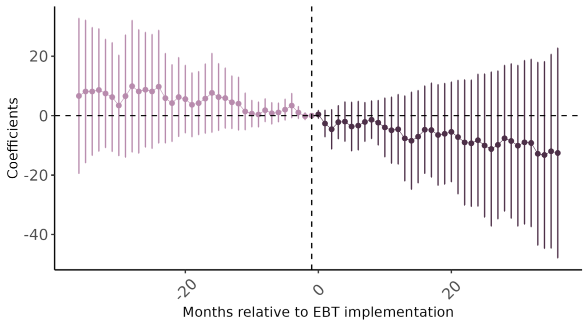
(A) EBT and WIC births per county (Figure 8 in Meckel (2020))



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



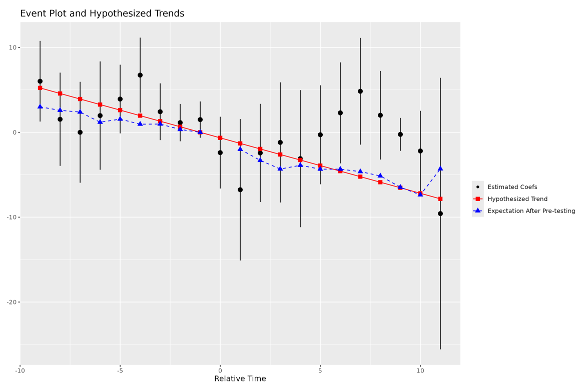
(C) EBT and WIC births per county, with a larger event window



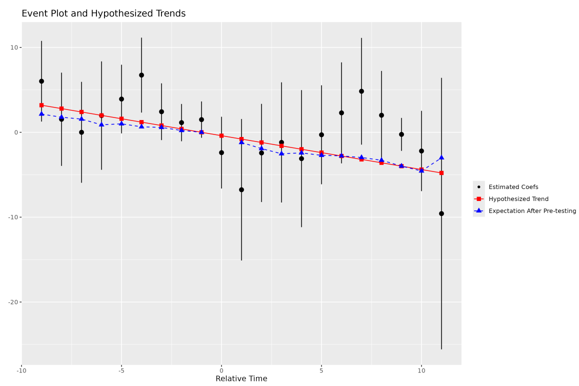
(D) EBT and high poverty WIC births per county, with a larger event window

FIGURE A8: 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) Edu $\leq$ HS, with 80 percent power



(B) Edu $\leq$ HS, with 50 percent power

FIGURE A9: EXTRAPOLATING LINEAR DIFFERENTIAL PRETRENDS BY ROTH (2022), NUMBER OF WIC MOTHERS IN TEXAS

Notes: The red lines represent the hypothesized differential pre-trends. The blue lines represent what the coefficients would look like conditional on not finding a significant pre-trend if the true pre-trend were the hypothesized trend. The actual estimates are significantly larger than the blue lines, suggesting that our results are not caused by linear differential trends.