Safety Net Payment Digitization and Participant Outcomes: Evidence from the WIC EBT Transition

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Abstract

Can digital technologies improve the efficiency of social safety net programs? In this paper, we examine the impact of digitizing payments for the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) from paper vouchers to debit cards on participant outcomes. We hand-collected the rollout schedule of the payment digitization and linked it to participant outcomes from multiple datasets. We find that WIC participation increased, and birth outcomes improved, among exposed mothers. Our back-of-the-envelope calculation suggests that payment digitization is associated with \$2.15 million annual hospital cost savings in the short term and \$15.51 million additional annual adult income in the long run. We find suggestive evidence that reduced stigma at retailers is a potential mechanism for increased WIC participation, and that increased WIC participation, in turn, may contribute to improved birth outcomes through increasing the likelihood of attending prenatal care. This paper is informative for evaluating the potential of further digitization in safety net programs such as online redemption. (JEL H51, H53, I38)

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1 Introduction

The Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) is a key component of the U.S. social safety net for women and children. This program 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 (Hoynes, Page and Stevens, 2011; Chorniy, Currie and Sonchak, 2020). However, participation is declining. The share of infants enrolled in WIC has fallen from 50% in 2009 to 30% in 2021. Between 2002 and 2022, WIC payments transitioned from paper vouchers to electronic benefit transfer (EBT) cards.

WIC's EBT transition is part of a broader take-up of digital technologies in the administration of public policies. WIC's switch to EBT had three policy objectives. The first was to encourage WIC participation among eligible individuals by reducing the stigma that participants experience when redeeming WIC vouchers (Moffitt, 1983). The second was to increase redemptions. Unlike with paper vouchers, EBT allows participants to redeem WIC benefits across multiple transactions, making perishable food benefits like milk and fruits and vegetables more valuable (Hanks et al., 2019; Li et al., 2021). The final objective was to reduce fraud at stores. Evidence from Texas finds that EBT reduces fraud at the cost of lowering access to authorized stores (Meckel, 2020). Authorization of independent stores declined nationwide post-EBT (Ambrozek et al., 2024). In essence, EBT shifts administrative burden from participants to retailers. Therefore, the net effect of EBT on WIC participation – and health outcomes that follow from WIC participation – is ambiguous. Understanding the effect that this policy change, the largest change to WIC in the past few decades, had on participation and health outcomes is important.

We present the first nationwide evaluation of WIC EBT's impact on participation. The existing empirical evidence on WIC EBT's impact relies on data from individual states. Effects of the EBT transition varied across states: Ohio experienced an increase in redemptions (Hanks et al., 2019), Oklahoma showed no significant change in participation rates (Li, Saitone and Sexton, 2022), while Texas saw a decline in WIC-associated births (Meckel, 2020). Qualitative work done in several states finds that participants' subjective experience improves post-EBT (Phillips et al., 2014). Given that improving infant health is the central goal of the WIC program, we also investigate the impact of WIC EBT on infant health. To our knowledge, this is the first study to explore this relationship.

We quantify the effect of the nationwide roll-out of WIC EBT on participation and infant health using multiple datasets. Using a staggered-adoption difference-in-differences (DiD) approach (Callaway and Sant'Anna, 2021), we compare counties implementing WIC EBT to counties that have not yet implemented. As a first-stage evidence, we document an increase in Google search on WIC-related keywords following the EBT transition. In our main results, we observe a 0.52-percentage-point (p.p.) increase in WIC participation (1.26% at the sample mean) among exposed mothers, and WIC EBT implementation reduces the likelihood of low birth weight by 0.08 p.p. (1% at the sample mean) and preterm births by 0.48 p.p. (4.22% at the sample mean) among exposed mothers. 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 and Medicaid costs annually and contributing to substantial increases in affected children's future earnings. Finally, we provide suggestive evidence for two key potential mechanisms: reduced welfare stigma and increased likelihood of attending prenatal care. Our findings also suggest that information shock and improvements in food security appear less likely to be mechanisms driving our main results.

This paper contributes to three strands of literature. First, we add to the body of research on the effects of EBT implementation. Prior work has considered 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, Shiferaw (2020) shows that EBT implementation in SNAP increases average birth weight in California, while Wright et al. (2017) finds that the switch to EBT in the Temporary Assistance for Needy Families (TANF) program reduces crime rates in Missouri. This paper extends this literature by providing national-scale evidence on WIC EBT's effects on WIC participation and birth outcomes among mothers of newborns.

Second, this paper contributes to the large literature assessing the impact 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) affected 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.

More broadly, this paper contributes to the growing literature on the role of digital technologies in public administration. Digital technologies improve the efficiency of public administration by increasing welfare coverage (Gray, 2019), lowering redemption costs (Aker et al., 2013), facilitating monitoring and targeting (Aiken et al., 2021), and reducing errors (Muralidharan, Niehaus and Sukhtankar, 2014). Overall, digitization can improve participant outcomes in public programs (Shiferaw, 2020; Kuhn, 2021; Wang, 2021). This paper provides new empirical evidence for the role of digital technologies in public administration by showing that a nationwide digitization of safety net payments that reduces stigma and makes benefits easier to use increases participation and improves participants' well-being.

The rest of the paper is organized as follows: Section 2 provides the policy background; Section 3 describes the data; Section 4 outlines the research design; Section 5 presents the empirical results and provides the results of robustness checks; Section 6 discusses potential mechanisms; and Section 7 concludes.

2 Background

2.1 WIC

WIC was established to safeguard the health of low-income women, infants, and children up to the age of five who are at nutritional risk. The program provides a fixed quantity of nutrition-targeted foods to low-income women and young children[1] (USDA Food and Nutrition Service, 2022). WIC also provides nutrition education and referrals to health and other social services and supports overall health. Over time, WIC has become one of the most widely used food assistance programs; more than 30% of infants born in the US in FY 2021 received WIC benefits. In fiscal year 2023, the federal government spent $6.6 billion on WIC, making it the third-largest food assistance program by total spending (USDA Food and Nutrition Service, 2020).

WIC has been extensively 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; Rossin-Slater, 2013) and has contributed to long-term educational and health gains for those who participated during early childhood (Chorniy, Currie and Sonchak, 2020). When parents lose WIC benefits, they often compromise their own nutrition intake to preserve their children's (Bitler et al., 2023). Despite 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).

[1] WIC eligibility requires a household income below 185% of the federal poverty line or participation in SNAP, TANF, Aid to Families with Dependent Children (AFDC), or Medicaid.

2.2 EBT Transition

Prior to EBT, WIC participants received paper vouchers from WIC clinics redeemable for food benefits at authorized retailers. Most vouchers were redeemable for multiple items and each voucher was valid for one month. 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 participants chose to redeem only some of the items listed on a voucher, unredeemed items were forfeited. After EBT, WIC and non-WIC items do not have to be separated, checkout is simpler, and items only expire at the end of the month.

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 in Food Stamps/SNAP.

In 2010, the Healthy, Hunger-Free Kids Act (HHFKA 2010) imposed a national mandate to complete the transition to EBT by October 1, 2020. This deadline was eventually extended due to the COVID-19 pandemic. 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). By 2022, all 50 states, U.S. territories, and tribal organizations had made the switch to EBT.

3 Data

3.1 WIC EBT roll-out schedule

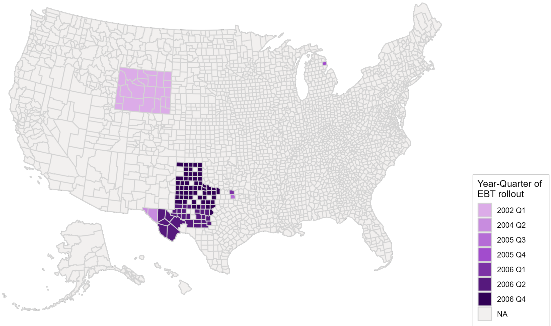
To track WIC EBT rollout timelines across U.S. counties, we collect data from multiple sources, including (archived) state websites, policy documents, and research papers. For counties reporting a range of implementation dates, we use the earliest date in the range. Figures 1a-1e show the geographic spread and temporal evolution of EBT transition. Our data captures both cross-state and within-state variation in the timing of WIC EBT implementation, with cross-state variation dominating. Most of the transition took place after 2010. We do not include Indian Tribal Organizations with separate WIC EBT implementation plans. We also exclude Nevada, which was an early adopter of WIC EBT but underwent a redesign and reimplementation of the system in 2009. 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 food, while Vermont had home delivery of food benefits. We include these states in our main analysis to estimate the average treatment effect on the treated; we show that excluding them does not change our main conclusions in Section 5.4.

Most of the implementation dates we collected reflect the planned rollout dates reported by state agencies. However, we cannot verify whether the actual implementation occurred on the exact planned date. To account for possible delays and the time required for both retailers and participants to fully transition to the EBT system, we define exposure based on the year of implementation rather than more granular timing. This approach improves the accuracy of our treatment assignment. We focus on counties that implemented the EBT transition before 2020 to avoid complications due to the COVID-19 lockdown.

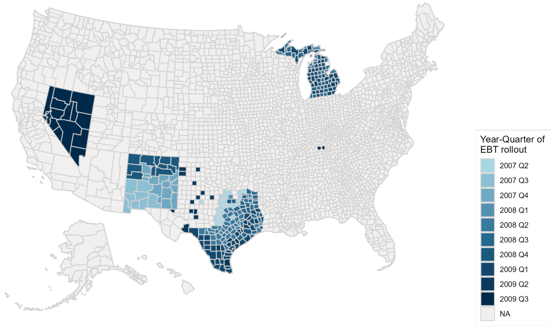
3.2 Birth certificate data

Our data on WIC participation and infant health come from the Vital Statistics Natality Data (birth certificate data), which provides detailed information on births and parental characteristics. These include the county of maternal residence, year of birth, maternal age, educational attainment, marital status, and the mother's WIC participation status, among other variables. Using natality data avoids the well-documented misreporting of WIC participation status in survey data (Meyer, Mok and Sullivan, 2015; Meyer and Mittag, 2019). The 2003 revision of the birth certificate required the inclusion of the mother's WIC status, 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 tractable. Our sample period spans 2009-2021 (National Center for Health Statistics, 2009-2021).

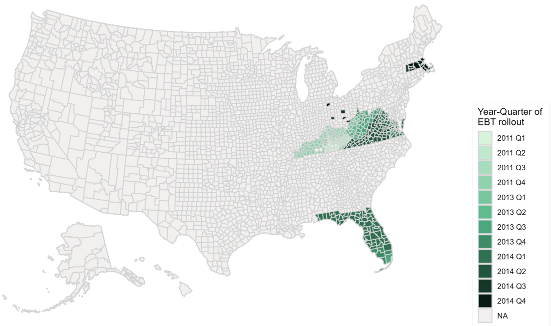
We validate the birth certificate data from Vital Statistics against birth data from the Texas Department of State Health Services (Texas DSHS) as used in Meckel (2020). Meckel (2020) uses Texas DSHS natality data covering births in counties that implemented WIC EBT before April 2009 (239 counties) from January 2005 to December 2009. Our natality data covers births



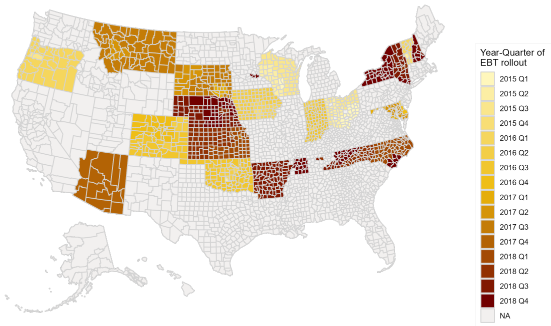
(A) WIC EBT rollout 2002-2006



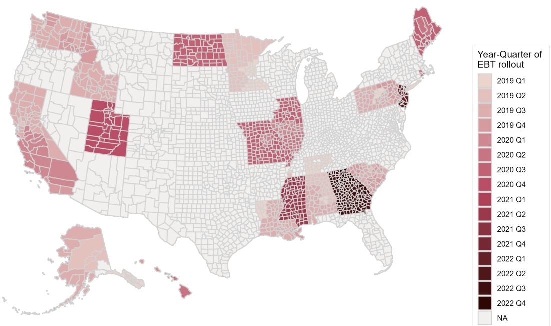
(B) WIC EBT rollout 2007-2010



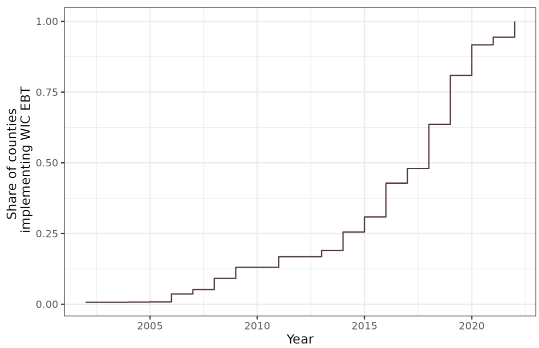
(C) WIC EBT rollout 2011-2014



(D) WIC EBT rollout 2015-2018



(E) WIC EBT rollout 2019-2022



(F) Share of counties implementing WIC EBT over time

FIGURE 1: WIC EBT ROLL-OUT SCHEDULE ACROSS U.S. COUNTIES

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 A2.

3.3 Alternative WIC participation data

One concern with WIC participation data from birth certificates is that it relies on self-reported information and only covers mothers of newborns. To address this, we replicate our research design using state agency–level data on monthly WIC participation from USDA Food and

Nutrition Service (2009-2021). The USDA's Food and Nutrition Service (FNS) publishes administrative data on monthly participation and program costs at the state level. These data include all participant categories—pregnant women, fully or partially breastfeeding women, postpartum women, fully or partially breastfed infants, and children aged 1–4—and represent the total number of active participants in a given month, not just new enrollees. While the USDA FNS website typically hosts the most recent five years of data, we use the Wayback Machine to extend our dataset back to 2009, covering the period through 2021.

Another limitation is that we do not observe WIC eligibility or the variables needed to estimate it on birth certificates. To address this, we turn to WIC participation data from the Current Population Survey Food Security Supplement (CPS-FSS) (Flood et al., 2024), which is collected only from likely-eligible households—those with incomes below 185% of the federal poverty line or who experienced food insecurity in the past year and have either a child under age 5 or a woman aged 15–45 in the household.

3.4 Google Trends data

We use Google Trends data to explore whether EBT implementation increases awareness of and interest in WIC. Google Trends is a publicly available database that tracks the relative popularity of search terms at the city, designated market area (DMA), state, and national levels. The data portal returns an index that normalizes the share of searches relative to the maximum search share within the chosen time frame and region. We use DMA-by-year data, which provides the normalized share of searches across 210 DMAs starting from 2004. The raw data downloaded from the Google Trends data portal represent the relative popularity of a search term within a DMA-year (Burchardi, Chaney and Hassan, 2019). One alternative would be to use city-by-year data, but Google Trends only reports search data above certain thresholds (Stephens-Davidowitz and Varian, 2014) and many search terms of interest are suppressed at the more disaggregated city-by-year level.

We collect search data on the general term "WIC" to measure overall awareness of WIC and terms such as "apply for WIC," "WIC application," "qualify for WIC," "WIC benefits," and "WIC foods" to capture intent to participate in WIC.[2] Due to Google Trends' reporting threshold, only the term "WIC" has sufficient search volume to generate a complete DMA-level panel between 2004 and 2021. To address this issue, we follow Burchardi, Chaney and Hassan (2019) and Alsan and Yang (2022) in aggregating the search data by taking a simple

[2]These five terms were selected because Google Trends only allows the comparison of five terms at a time, and they are more frequently searched than other WIC-related terms such as "WIC qualification" or "WIC clinic."

average of these terms.

3.5 County characteristics data

We collect data on county characteristics from various sources. Demographic data on race and age of county population is from the Intercensal Population Estimates (US Census Bureau, 2020). Data on poverty status and income are from the American Community Survey (ACS) Public Use Microdata Sample (Ruggles et al., 2025).[3] To measure some of the welfare programs that automatically convey WIC eligibility, we collect data on transfers from the Bureau of Economic Analysis's Regional Economic Information System (REIS), which includes these programs (Bureau of Economic Analysis, 2006-2008).[4] Finally, we collect county-level data on poverty rates and the under-five population from the Small Area Income and Poverty Estimates (SAIPE) Program (US Census Bureau, 2006-2008), the share of low birthweight births from the restricted-use Birth certificate data (National Center for Health Statistics, 2009-2021), and the net increase in WIC vendors from the WIC Integrity Profiles (TIP) (USDA Food and Nutrition Service, 2006-2008).

4 Methods

4.1 Empirical strategy

To identify the effects of WIC EBT implementation, we compare cohorts born before and after WIC EBT implementation in counties that implemented EBT with counties that have not yet implemented WIC EBT. One conventional approach is two-way-fixed-model as follows:

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

where Y_{ct} is an outcome variable measured for county c in year t, EBT_{ct} is a dummy for exposure to WIC EBT transition for county c in year t, η_c and λ_t are county and year fixed effects, 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 Callaway and Sant'Anna (2021), a standard two-way fixed effects (TWFE) OLS estimator with staggered treatment timing and

[3] 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 person weights. We use the 2000 crosswalk between counties and PUMAs provided by the Missouri Census Data Center. See https://mcdc.missouri.edu/applications/geocorr.html. Note that county-to-PUMA is a many-to-many relationship. The crosswalk includes an allocation factor to help align PUMAs with counties. Observations from PUMAs with populations under 100,000 are excluded as geographic identifiers are suppressed for these PUMAs.

[4] In the REIS data set, 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. SNAP benefits are reported separately.

heterogeneous treatment effects will implicitly make comparisons to all other units, aggregating these comparisons 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 group-time estimator proposed by Callaway and Sant'Anna (2021) in our baseline results to avoid this issue. The CS estimator is based on a group-time average treatment effect on the treated (ATT):

$$ATT(g,t) = E[Y_{g,t}(1) - Y_{g,t}(0)|G = g],$$

where $Y_{g,t}(1)$ and $Y_{g,t}(0)$ represent the potential outcomes under treatment and no treatment, respectively, and $G = g$ denotes the group that first received treatment in period g. Following Callaway and Sant'Anna (2021), we aggregate group-time ATTs to obtain overall and dynamic measures of the treatment effect. The overall effect across all treated groups and time periods is given by:

$$ATT^{overall} = \frac{1}{G}\sum_{g}\frac{1}{T_g}\sum_{t\geq g} ATT(g,t),$$

where G is the number of groups, and T_g represents the number of periods after the group g adopts treatment. Dynamic treatment effects over time—which show how the estimated impact evolves post-treatment—are calculated as:

$$ATT^{dynamic}(t) = \frac{1}{|G_t|}\sum_{g\in G_t} ATT(g,t),$$

where G_t represents the set of groups treated in periods t, allowing us to track treatment effects over time and assess potential dynamics of effects of WIC EBT transition.

For the CS estimator, the composition of treated and control groups naturally changes across cohorts by construction. As a result, restricting the sample to an event-time-balanced panel is less meaningful. However, we limit our analysis to a relatively narrow event-time window—four periods before (periods –4 to –1) and three periods after the event (periods 0 to 3)—to avoid highly imbalanced comparisons. For instance, in cohorts far from the event, the available control group may include only a small number of counties, which can reduce reliability.

To maintain consistency with the existing literature on the infant health impacts of federal food assistance programs (Almond, Hoynes and Schanzenbach, 2011; Hoynes, Page and Stevens, 2011), we define exposure based on the beginning of the third trimester. This choice is supported by medical research indicating that the third trimester is the most critical period for fetal development and infant health (Rush, Stein and Susser, 1980; Kramer, 1987*a*,*b*). Additionally, approximately 50% of pregnant WIC participants certify during the first trimester, 40% during the second, and only 10% during the third (Thorn et al., 2016). This suggests that exposure to EBT prior to the third trimester is more likely to influence behavior as well. Nonetheless, we demonstrate that our results remain robust when defining exposure based on alternative timings, including the first trimester, second trimester, and at birth (see Section 5.4).

In our baseline analysis, we use not-yet-treated areas as the control group, do not include covariates, and rely on an unbalanced panel. We test the robustness of our results to alternative specifications in Section 5.4. We also show that alternative estimation methods, including the two-stage DiD method proposed by Gardner (2022) and the imputation method proposed by Borusyak, Jaravel and Spiess (2024), yield results consistent with our main findings using the CS estimator.

4.2 Identifying assumptions

The validity of our research design relies on two key assumptions: the parallel trends assumption and the no-anticipation assumption. The parallel trends assumption requires that, in the absence of treatment, the average outcomes of the treated group would have followed a similar trajectory to those of the control group. While this assumption is not directly testable, we conduct several partial tests that provide support for its plausibility.

First, we examine whether the timing of WIC EBT rollout is correlated with baseline county characteristics. A violation of the parallel trends assumption could occur if EBT implementation is systematically related to factors that also influence WIC participation or infant health. For instance, if poorer counties experiencing economic downturns adopted EBT earlier—and those downturns independently increased WIC participation due to rising food insecurity—our estimates might simply reflect underlying economic trends rather than the effect of EBT itself. We collect baseline county characteristics from 2006–2008, three years prior to the start of our sample. These include demographic variables, economic conditions, government transfers, and the number of WIC vendors (as described in Section 3.5). We then regress the timing of WIC EBT implementation on these baseline characteristics. Table A1 shows that while some county baseline characteristics are strongly correlated with the timing of WIC EBT implementation, these characteristics as a whole explain less than 10% 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. Overall, our analysis suggests that the timing of the WIC EBT rollout seems plausibly exogenous.

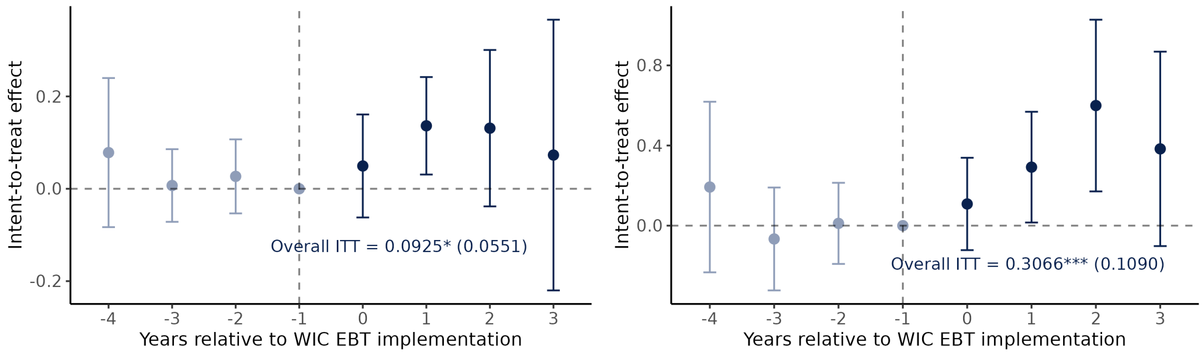
Second, we present event-study-style dynamic effects to examine the presence of any pre-treatment trends. In most cases, we do not observe evidence of such trends. To strengthen the credibility of our findings, we take two additional steps. First, we incorporate pre-treatment covariates into our estimation to relax the unconditional parallel trends assumption underlying our main results. With these covariates included, the parallel trends assumption needs only to hold conditional on them, despite being subject to additional assumptions discussed in Section 5.4. Second, we conduct the sensitivity analysis proposed by Rambachan and Roth (2023) to assess how sensitive our results are to potential violations of the parallel trends assumption.

The no-anticipation assumption requires that participants in eventually treated counties do not change their behavior in anticipation of the treatment. In our setting, anticipatory behavior is not likely, as it was technically impossible to use EBT cards before the official rollout. Consistent with this, we also do not observe any evidence for anticipatory behavior in our event-study results.

5 Results

5.1 First stage evidence from Google Search

We begin by asking whether EBT implementation increases awareness of and interest in WIC. To this end, we match the earliest EBT implementation date among all counties within a DMA to Google Trends data on the relative popularity of WIC-related search terms. Figure 2a shows that the relative popularity of searches for "WIC" increases by 0.09 standard deviations, suggesting a rise in awareness of the WIC program following EBT implementation. As in Figure 2b, we find that EBT implementation increases searches for WIC application-related keywords, including "apply for WIC", "WIC application", "qualify for WIC", "WIC benefits", and "WIC foods", by 0.31 standard deviations. This suggests that EBT implementation increases intent to participate in WIC. Both figures show that the increase in relative popularity of searches for WIC-related terms is not driven by pre-existing trends between DMAs that have implemented EBT and those that have not yet done so. The first-stage evidence reinforces the plausibility of our main results: observing an increase in intent to participate in WIC makes the rise in actual participation more convincing.



(A) Search "WIC" (B) Search WIC-application-related terms

FIGURE 2: EFFECTS OF WIC EBT ON WIC-RELATED GOOGLE SEARCHES

Notes: WIC-application-related terms include "apply for WIC", "WIC application", "qualify for WIC", "WIC benefits", and "WIC foods". We present point estimates of the dynamic effects using the group-time estimator developed by Callaway and Sant'Anna (2021), along with their 95% confidence intervals adjusted for multiple testing. The unit of observation is DMA-by-year cells. Standard errors are clustered at the DMA level. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$.

5.2 WIC participation

Figure 3 shows that the overall intent-to-treat effect (ITT) of WIC EBT transition during the third trimester on the share of mothers participating in WIC is 0.52 p.p. (1.26% at the sample mean). The ITT effect begins to increase from period 1 rather than period 0, which likely reflects the time needed for retailers and participants to transition to the EBT system or for the target population to learn the new technology. The dynamic effects grow larger over time following the EBT rollout, supporting the presence of a learning process. We do not observe any noticeable pre-treatment trends in WIC participation. Nonetheless, we conduct the sensitivity analysis proposed by Rambachan and Roth (2023) to assess how robust this estimate is to potential violations of the parallel trends assumption, even though no such violations are pbserved in the data.

We summarize the results from different specifications in Table 1. Column (1) presents our baseline estimates. Column (2) shows that, given our focus on a narrow event window, the results are very similar when using an event-time-balanced panel. Column (3) uses counties treated after 2019 as the control group. The estimated effect is larger than our baseline estimates. In Column (4), we control for pre-treatment covariates measured between 2006 and 2008 to relax the unconditional parallel trend assumption. We use a double-robust approach to incorporate pre-treatment covariates. This method requires the correct specification of either the outcome evolution for the comparison group or the propensity score model. We control for pre-treatment maternal characteristics that are likely to satisfy one of these conditions, including an urban county indicator, share of non-white mothers, share of mothers with no more

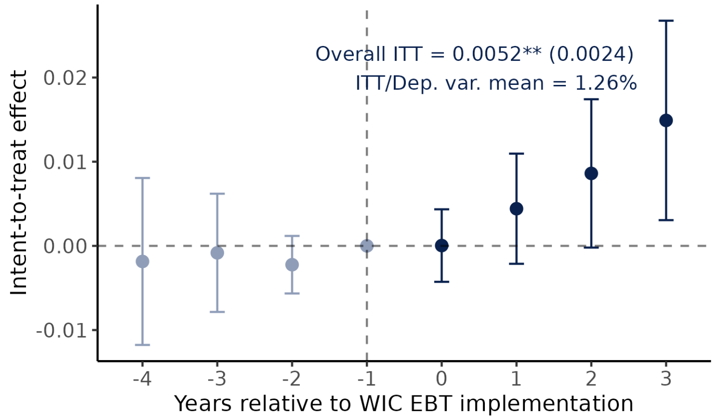


FIGURE 3: EFFECTS OF WIC EBT TRANSITION ON WIC PARTICIPATION, BIRTH CERTIFICATE DATA

Notes: We present point estimates of the dynamic effects using the group-time estimator developed by Callaway and Sant'Anna (2021), along with their 95% confidence intervals adjusted for multiple testing. Standard errors are clustered at the county level. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$. We use not-yet-treated areas as the control group and do not include covariates. The unit of observation is county-by-year cells. The regression is weighted by the number of births in each cell.

than a high school education, share of unmarried mothers, and share of firstborns. The resulting estimates are slightly less precise but remain similar to our main finding in Column (1).

Columns (5)–(7) present results using alternative data sources on WIC participation. Because these data only contain state identifiers, we assign each state the earliest EBT implementation date among its counties. In Column (5), the dependent variable is the share of WIC participants—calculated as the number of participants divided by the total population of women ages 19–45, infants, and children under 5 (the WIC target population). The estimate is slightly smaller and less precise, likely due to the coarser variation at the state level. In Figure A5b, we disaggregate the analysis by participant type and find that the increase is primarily driven by infants and children. Column (6) uses CPS data to estimate the share of WIC households, defined as the number of households with at least one WIC participant divided by the number of likely eligible households—those with income belows185% of the poverty line or that experienced food hardship in the past year, and that include a child under 5 or a woman aged 15–45. Given the sample is restricted to the targeted population, the estimated effect is nearly five times larger than in Column (1). Column (7) uses the number of WIC participants per household as the outcome, capturing both the extensive and intensive margins of participation. This estimate is nearly twice as large as that in Column (5). The corresponding event-study results for Columns (2)–(7), shown in Figures A4, A5, and A6, exhibit patterns consistent with those in Table 1.

How do our estimates on WIC participation compare to those of other papers that estimate

TABLE 1: OVERALL EFFECTS OF WIC EBT TRANSITION ON WIC PARTICIPATION

	Share of WIC mothers				Share of WIC participants	Share of WIC households	#WIC participants per household
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
WIC EBT transition	0.0052**	0.0067**	0.0138***	0.0035*	0.0021*	0.0249*	0.0554***
	(0.0024)	(0.0030)	(0.0029)	(0.0028)	(0.0013)	(0.0124)	(0.0199)
Observations	29,370	25,228	29,370	22,903	495	256,969	256,813
Number of counties	2,724	2,531	2,724	2,089	0	0	0
Number of states	45	44	45	45	45	45	45
Dep. var. mean	0.4126	0.4127	0.4126	0.4117	0.0946	0.1566	0.2492
Est./Dep. var. mean	1.26%	1.62%	3.34%	0.85%	2.22%	15.90%	22.23%
Pre-treatment covariates	N	N	N	Y	N	N	N
Balanced in event time	N	Y	N	N	N	N	N
Comparison group	Not-yet-treated	Not-yet-treated	Never-treated	Not-yet-treated	Not-yet-treated	Not-yet-treated	Not-yet-treated
Data source	Birth cert.	Birth cert.	Birth cert.	Birth cert.	USDA admin.	CPS-FSS	CPS-FSS

Notes: We present point estimates of the dynamic effects using the group-time estimator proposed by Callaway and Sant'Anna (2021). $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$. For Columns (1)-(4), (i) the unit of observation is county-by-year cells; (ii) standard errors are clustered at the county level; and (iii) regressions and dependent variable mean are weighted by the number of births in each cell. Column (1) shows results from the baseline specification, which is based on an imbalanced panel and uses not-yet-treated counties as the comparison group. In Column (4), we control for pre-treatment covariates measured between 2006 and 2008, including an urban county indicator, share of non-white mothers, share of mothers with no more than a high school education, share of unmarried mothers, and share of firstborns. For Columns (5)-(7), (i) the unit of observation is state-by-month average for Column (5) and household for Columns (6)-(7); (ii) standard errors are clustered at the state level; and (iii) regressions and dependent variable mean are weighted by state population. Sample of Current Population Survey Food Security Supplement (CPS-FSS) includes households below 185% poverty or that ran short of money for food in the past year, that have a member under the age of 5 or a female between ages 15-45.

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 p.p. increase in WIC enrollment in 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 Figure A7 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.

5.3 Birth outcomes

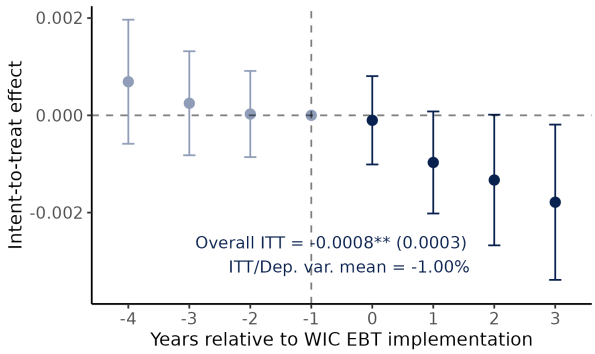
We next examine whether increased WIC participation translates into improved birth outcomes. We focus on two of the most commonly used indicators of infant health: the likelihood of low birth weight (defined as birth weight below 2,500 grams) and the likelihood of preterm birth (defined as gestation under 37 weeks). Figures 4a and 4b show that EBT implementation during the third trimester reduces the likelihood of low birth weight by 0.08 p.p. (1% at the sample mean) and preterm birth by 0.48 p.p. (4.22% at the sample mean). Similar to the WIC participation results, the dynamic treatment effects become larger over time following the EBT rollout.

For low birth weight, the effects begin to rise from period 1, consistent with the participation effects. However, for preterm birth, the effects emerge as early as period 0. One possible explanation is that EBT implementation may also improve the intensive margin of WIC participation—i.e., the extent to which enrolled participants redeem their benefits. Under the paper voucher system, participants had to redeem all benefits in a single trip, and any unredeemed items were forfeited. In contrast, the EBT system allows for multiple trips, which likely increases total benefit redemption. While we can only observe the extensive margin of WIC participation in our data (i.e., whether someone is enrolled), improvements in the intensive margin may have occurred but remain unmeasured. This could explain the earlier observed effects on preterm birth, even in the absence of a detected increase in participation in period 0. We leave the exploration of the intensive margin of WIC participation to future research.

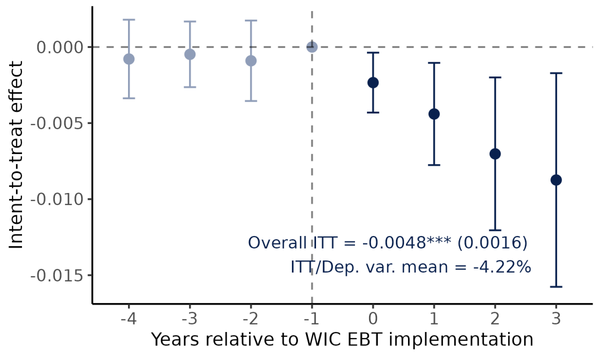
As with the participation results, we do not observe clear pre-treatment trends in either birth outcome, though there is a very subtle pre-trend in the likelihood of low birth weight. To account for this, we discuss the robustness of these estimates to potential violations of the parallel trends assumption using the sensitivity analysis proposed by Rambachan and Roth (2023) (see Section 5.4).

We summarize the birth outcome results from different specifications in Table 2. The estimates are robust to restricting the sample to an event-time-balanced panel, using counties that implemented EBT after 2019 as the control group, and including pre-treatment covariates. Additional robustness checks are discussed in Section 5.4.

We compare our estimated effects of the EBT transition on birth outcomes to those found in studies of other federal food assistance programs. Hoynes, Page and Stevens (2011) estimate almost no effect of WIC program introduction during the third trimester on the likelihood of low birth weight in the full sample, and a statistically insignificant 0.14 percentage point



(A) Low birth weight



(B) Preterm birth

FIGURE 4: EFFECTS OF WIC EBT TRANSITION ON ADVERSE BIRTH OUTCOMES

Notes: We present point estimates of the dynamic effects using the group-time estimator developed by Callaway and Sant'Anna (2021), along with their 95% confidence intervals adjusted for multiple testing. Standard errors are clustered at the county level. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$. We use an imbalanced panel, use not-yet-treated areas as the control group, and do not include covariates. The unit of observation is county-by-year cells. The regression is weighted by the number of births in each cell.

TABLE 2: OVERALL EFFECTS OF WIC EBT TRANSITION ON BIRTH OUTCOMES

	Share of low birth weight				Share of preterm births			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
WIC EBT transition	-0.0008**	-0.0012***	-0.0010**	-0.0007*	-0.0048***	-0.0050**	-0.0046***	-0.0045***
	(0.0003)	(0.0004)	(0.0004)	(0.0004)	(0.0015)	(0.0024)	(0.0016)	(0.0017)
Observations	29,370	25,228	29,370	22,903	29,370	25,228	29,370	22,903
Number of counties	2,724	2,531	2,724	2,089	2,724	2,531	2,724	2,089
Number of states	45	44	45	45	45	44	45	45
Dep. var. mean	0.0802	0.0799	0.0802	0.0800	0.1138	0.1131	0.1138	0.1132
Est./Dep. var. mean	-1.00%	-1.50%	-1.25%	-0.87%	-4.22%	-4.42%	-4.04%	-3.98%
Pre-treatment covariates	N	N	N	Y	N	N	N	Y
Balanced in event time	N	Y	N	N	N	Y	N	N
Comparison group	Not-yet-treated	Not-yet-treated	Never-treated	Not-yet-treated	Not-yet-treated	Not-yet-treated	Never-treated	Not-yet-treated
Data source	Birth cert.	Birth cert.	Birth cert.	Birth cert.	Birth cert.	Birth cert.	Birth cert.	Birth cert.

Notes: We present point estimates of the dynamic effects using the group-time estimator proposed by Callaway and Sant'Anna (2021). The unit of observation is county-by-year cells. Standard errors are clustered at the county level. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$. Column (1) shows results from the baseline specification, which is based on an imbalanced panel and uses not-yet-treated counties as the comparison group. In Columns (4) and (8), we control for pre-treatment covariates measured between 2006 and 2008, including an urban county indicator, share of non-white mothers, share of mothers with no more than a high school education, share of unmarried mothers, and share of firstborns.

(1.4% at the sample mean) decline among mothers with less than a high school education. Our estimates are slightly smaller but more precise. Almond, Hoynes and Schanzenbach (2011) find that the introduction of food stamps during the third trimester reduces the likelihood of low birth weight by 0.06 percentage points (1% at the sample mean), very close to the estimated effect of EBT on low birth weight in our study.

5.4 Other robustness checks and falsification tests

In Sections 5.2 and 5.3, we show that results based on an event-time-balanced panel, using counties treated after 2019 as the control group, or including pre-treatment covariates, are broadly consistent with our main findings. In this section, we summarize additional robustness checks and falsification tests. Full details, including tables and figures, are provided in Appendix B.

(i) Alternative estimation methods. We begin by showing that alternative estimation methods, including two-stage DiD method proposed by Gardner (2022) and the imputation method proposed by Borusyak, Jaravel and Spiess (2024), yield results consistent with our main findings using the CS estimator (see Figures B1a-B1c).

(ii) Alternative timings of exposure. Second, we find that our results are robust to alternative definitions of exposure timing, including the beginning of the first trimester, the beginning of the second trimester, and the time of birth (see Figures B2a–B2c).

(iii) Excluding Vermont and Mississippi. The WIC EBT transition coincided with changes in benefit delivery methods in Vermont and Mississippi. Figures B3a–B3c show that excluding these two states has virtually no impact on our estimates.

(iv) Longer-term dynamic effects. We extend the post-event window to include up to eight periods after treatment and find that the estimates in the later post-treatment periods remain directionally consistent with our main results (see Figures B4a–B4c).

(v) Placebo test using non-target group. We examine effects of EBT transition for a non-target group—college-educated mothers over age 25—using data from the Survey of Income and Program Participation (SIPP) to test if our estimates pick up spurious trends. This group is estimated to be 18% less likely to be WIC eligible than the full sample and thus less likely to be affected by EBT transition. As expected, Table B1 shows that the estimates are imprecise and smaller for this non-target group.

(vi) Randomization test. We randomly assign the year of WIC EBT transition 1,000 times while preserving the original distribution of rollout years to compute placebo (pseudo) treatment effects. Figures B5a–B5c show that our actual estimates consistently fall near or well into the tails of the distribution of these simulated effects, suggesting that our findings are unlikely to be driven by random noise or luck.

(vii) Sensitivity to parallel trend violation. Finally, we assess the sensitivity of our results to potential violations of the parallel trends assumption using the method proposed by Rambachan and Roth (2023). Figures B6a–B6c show that our estimates remain reasonably robust under certain hypothetical deviations from parallel trends, even though we do not observe such violations in the data.

5.5 Composition change and heterogeneity by maternal characteristics

In this section, we first examine whether the EBT transition affects the composition of maternal characteristics at the cell level, and then explore the heterogeneity of our estimates across different maternal characteristics. The characteristics of interest include maternal age, education, race and Hispanic origin, whether the mother is a first-time parent, and whether a father is listed on the birth certificate.

We might observe a spurious positive effect on WIC participation and birth outcomes if demographic change in the county coincides with the WIC EBT transition. Individuals with certain demographic characteristics are more likely to participate in WIC or have healthier infants. If these groups are growing as a share of the population at the same time that EBT is being implemented, it could confound our effects. Figure A8a shows the effects of EBT rollout on the distribution of maternal characteristics. For each group of maternal characteristics (i.e., age, education, race, Hispanic origin, first birth, and presence of a father on the birth certificate), the omitted category consists of observations with missing values for that characteristic group.

The results indicate that the EBT transition is associated with a higher share of disadvantaged mothers: more births are to mothers under age 30, with no more than a high school education, and without a father listed on the birth certificate. This pattern is consistent with the idea that, with increased WIC participation, fetuses of disadvantaged mothers are more likely to survive, leading to an increased share of such births in the population. This finding also suggests that our estimates of EBT's effects may be attenuated by compositional changes in maternal characteristics.

Next, we examine heterogeneity in the EBT effects across maternal subgroups in Figures A8b–A8d. As before, the omitted category in each case consists of observations with missing values for the relevant characteristic. We do not find a consistent pattern indicating that EBT effects are systematically larger among either disadvantaged or advantaged mothers. For example, the effect of EBT on WIC participation is larger among older mothers, as well as among those with no more than a high school education. One possible explanation is that these heterogeneous effects are themselves influenced by composition changes: if EBT has larger impacts among disadvantaged mothers, but the overall share of disadvantaged births increases at the same time, this may attenuate the observed effects, leading to the ambiguous patterns we observe.

5.6 Economic significance

How much does the improvement in birth outcomes translate into economic benefits? We provide the back-of-the-envelope estimates of both short-term hospital cost savings and long-term adult income increases associated with the WIC EBT transition. Our estimates are based on low birth weight results due to the availability of estimated parameters in existing literature. First, we obtain the average number of births per year from 2009-2019, which is 3,943,146. Second, from the SIPP data, we estimate the total share of the WIC-eligible among mothers with infants to be 55.06%. Therefore, we estimate that WIC EBT lifts 5,729 births out of low birth weight each year.[5]

We use estimates from Almond, Chay and Lee (2005) to calculate hospital cost savings associated with WIC EBT. 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. We estimate $2.15 million (in 2000 dollars) of the annual hospital cost savings from the reduction in low birth weight incidents due to the WIC EBT transition. When compared to public expenditure, the hospital cost savings from reduced low birth weight alone amount to 9.52% of the USDA's annual EBT investment.[6]

Improved birth outcomes are also associated with long-run gains in labor market outcomes (Behrman and Rosenzweig, 2004). To provide a back-of-the-envelope estimate of the potential economic benefits, we draw on Johnson and Schoeni (2011), who finds that avoiding low birth weight increases annual adult earnings by $4,583 in 1997 dollars. Using this estimate and not accounting for general equilibrium effects, we estimate that the WIC EBT transition leads to an annual increase in adult earnings of approximately $15.51 million (in 2000 dollars) in total.[7] If we assumed 30 years of lifetime labor supply, the cumulative increase in adult earnings would amount to roughly $465.33 million for each birth cohort, far beyond the $30.5 million annual investment of the WIC EBT transition in 2013 (USDA Food and Nutrition Service, 2017).

[5] 5,729 = the average number of births per year (3,943,146) × ITT of EBT transition on the likelihood of low birth weight (0.0008) divided by share of the WIC-eligible (55.06%).

[6] 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 9.52% is: $\frac{2.15\times1.35}{30.5}$.

[7] $4,583 in 1997 dollars is equivalent to $4,917.09 in 2000 dollars. $4,917.09 × 0.0008 (the estimated effect of the WIC EBT on the likelihood of low birth weight) × 3,943,146 (average annual number of births in the sample) = $15,511,043.01.

6 Mechanisms

We begin by providing both qualitative and/or suggestive quantitative evidence for three mechanisms driving increased WIC participation: reduced welfare stigma and information shock. We then examine potential pathways beyond WIC participation that could explain improvements in birth outcomes, focusing on changes in food consumption and prenatal care visits. Our findings suggest that reduced welfare stigma is a plausible mechanism driving increased WIC participation, which in turn improves birth outcomes by raising the likelihood of attending prenatal care.

6.1 Welfare stigma

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 lessens the stigma participants experience from feeling like they are holding up the checkout line (Chauvenet et al., 2019; Isaacs, Shriver and Haldeman, 2020). EBT cards can also be used with self-checkout machines. Anecdotal evidence also suggests that EBT reduces stigma for WIC participants (Phillips et al., 2014).[8]

Examining the effect of EBT on welfare stigma is challenging due to the lack of systematic data on both self-reported and objective measures of stigma. Instead, we identify three county groups where welfare stigma may be particularly salient for participants: (1) rural counties, (2) counties with lower peer engagement in WIC redemption, and (3) counties with a higher share of Republican voters.

Theory from economists, sociologists, and other disciplines suggests that welfare stigma may be particularly salient in specific communities. 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 reduce willingness to participate in welfare programs. They note that rural families worry that accepting welfare could harm their

[8] 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."

family reputation, which is important for securing work opportunities in rural communities. Anecdotal evidence 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). Second, Celhay, Meyer and Mittag (2022) find that welfare stigma is most salient when fewer peers engage in the stigmatized behavior. To get at this, we construct a measure that captures whether WIC participants are likely to shop in stores with other WIC shoppers. Lastly, Republicans are more likely to view participation in welfare programs negatively (Levy, 2021; Goenka and Thomas, 2022). 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."

We use the number of non-WIC mothers per WIC vendor as a proxy for peer engagement, defining counties in the top quartile of this measure as high-stigma areas and the remainder as low-stigma areas. To capture political attitudes, we use two measures: the share of voters who supported the Republican candidate in the 2008 presidential election, based on data from Morris (2016), and the last year the Republican Party won the presidential election in each county, using data from Leip (2025). Counties in the top quartile of Republican vote share in 2008 or where the GOP has won the presidential election since 2008 are classified as high-stigma areas, with the remaining counties classified as low-stigma.

If stigma reduction is a mechanism driving increased WIC participation post-EBT, increases in WIC participation should be larger in high-stigma regions. Alsan and Yang (2022) uses a similar strategy to provide 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 5, we divide the sample by high- and low-stigma groups and present the Callaway and Sant'Anna (2021) estimators for EBT's effect on WIC participation for each group. We find that the effect of EBT implementation is generally larger in these counties, particularly in areas with a high number of non-WIC peers. We interpret these findings as suggestive evidence that reducing welfare stigma is a mechanism for increasing WIC participation.

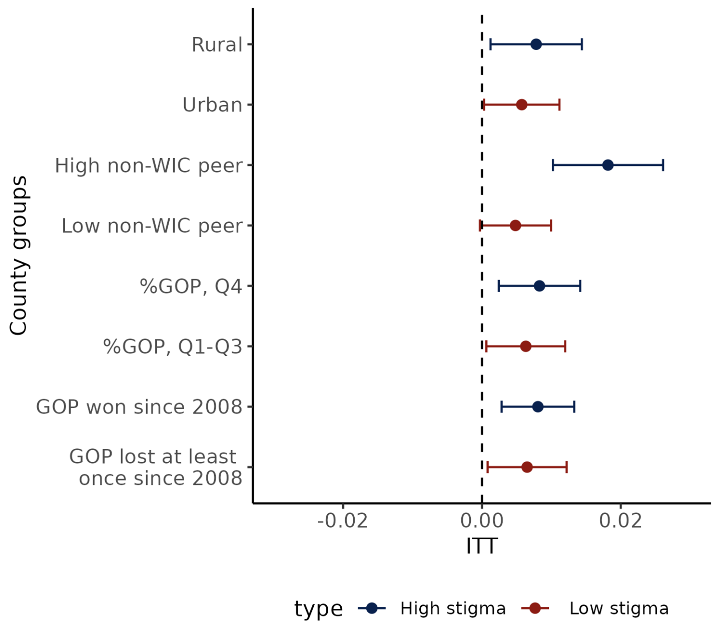


FIGURE 5: WIC EBT'S EFFECT ON WIC PARTICIPATION BY HIGH- AND LOW-STIGMA AREAS

Notes: We present point estimates of the dynamic effects and their 95% confidence intervals using the group-time estimator proposed by Callaway and Sant'Anna (2021). The unit of observation is county-by-year cells. Standard errors are clustered at the county level.

6.2 Information shock

Information shock is another potential mechanism through which the WIC EBT transition may have increased WIC participation. Following a similar approach to Alsan and Yang (2022), we partially test this channel by comparing WIC participation among likely WIC-eligible households who do and do not receive SNAP using CPS FSS data. SNAP participants are generally more familiar with the broader social safety net and may be more knowledgeable about WIC eligibility and application procedures. If information were a key mechanism, we would expect to see a larger effect of EBT among non-SNAP households. However, results in Table 6.2 suggest the opposite: the effect of EBT on WIC participation is larger and more precisely estimated among SNAP households. Section 5.5 provides another evidence for the information channel by comparing first-time and experienced mothers in birth certificate data. Experienced mothers are more likely to be aware of resources available to pregnant women and young children. If increased information were driving WIC participation, we would expect the EBT effect to be larger among first-time mothers. Yet, as shown in Figure A8b, the effect of EBT on WIC participation is again larger and more precisely estimated among experienced mothers. These findings suggest that information shocks are less likely to be the primary mechanism driving the increase in WIC participation following the EBT transition.

6.3 Food security and consumption behaviors

We next explore potential mechanisms that may explain improved birth outcomes. We first examine whether the WIC EBT transition improved birth outcomes by enhancing food security, using data from the CPS Food Security Supplement (FSS). Table A4 reports the effects of the WIC EBT transition on the food insecurity indicator and the raw food insecurity score across all infants, likely WIC-eligible infants, and infants from low-income households. Both food insecurity measures for infants are measured based on responses to specific questions designed by the USDA. We find no clear evidence that the EBT transition improved food security among infants. Neither do we find any significant effects of the transition on where people obtain food or on their use of low-cost or free food from other sources (see Table A5 and Table A6). These findings suggest that improvements in food security or changes in consumption behaviors are less likely to be the primary mechanism driving the increase in WIC participation following the EBT transition.

6.4 Medicaid/CHIP enrollment and prenatal care

Another potential mechanism behind improved birth outcomes is an increase in prenatal care visits driven by higher WIC participation. As part of WIC services, nutrition counseling may encourage pregnant women to adopt healthier practices and seek prenatal care. For instance, Rossin-Slater (2013) finds that the presence of a WIC clinic in a pregnant woman's ZIP code increases both WIC participation and the likelihood of initiating breastfeeding at hospital discharge. Some WIC clinics are located within or near large hospitals, which may further motivate participants to attend prenatal care visits more frequently. Using birth certificate data, Figure 6 shows a 0.13 percentage point increase (0.13% at the sample mean) in the likelihood of attending prenatal care visits following the WIC EBT transition.

To further explore this mechanism, we examine whether the WIC EBT transition led to increased enrollment in Medicaid or the Children's Health Insurance Program (CHIP) among infants, using data from the CPS Annual Social and Economic Supplement (ASEC). Many low-income pregnant women rely on these public insurance programs to cover the cost of prenatal care. Figure 7a shows that enrollment in Medicaid or CHIP increases by 7.4 percentage points (an 18.13% increase at the sample mean) following the WIC EBT transition. The effect is particularly pronounced for CHIP: enrollment rises by 2.94 percentage points, representing a 40.89% increase at the sample mean. The relatively large treatment effect on CHIP enrollment reflects the fact that not all states offer CHIP. In states without CHIP, enrollment is zero, which lowers the overall sample mean. In contrast, all states had Medicaid programs during the period of

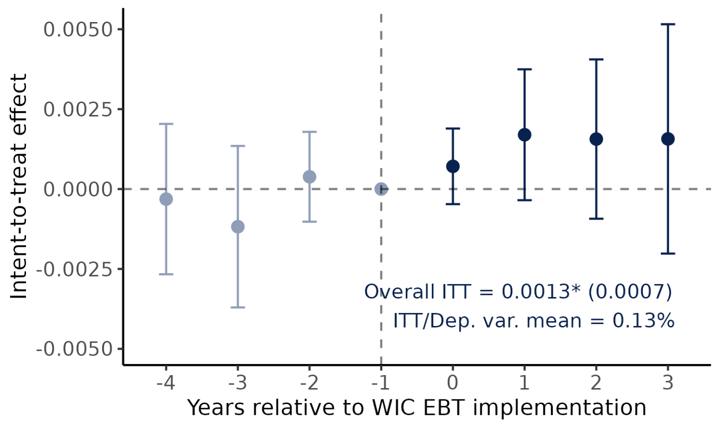
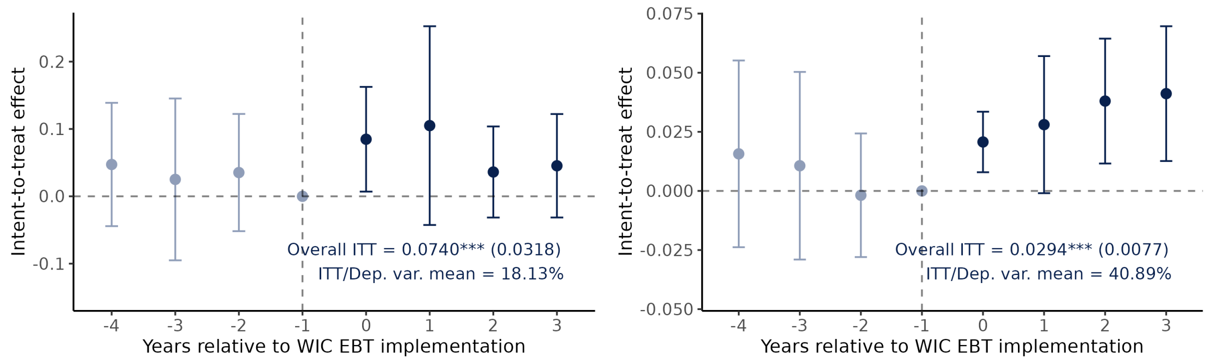


FIGURE 6: EFFECTS OF WIC EBT ON PRENATAL VISIT

Notes: We present point estimates of the dynamic effects using the group-time estimator developed by Callaway and Sant'Anna (2021), along with their 95% confidence intervals adjusted for multiple testing. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$. The unit of observation is county-by-year cells. Standard errors are clustered at the county level.

our study.

One potential concern is whether the observed increase in Medicaid/CHIP enrollment coincides with the timing of Medicaid or CHIP expansion. Table A7 compares the rollout dates of WIC EBT and Medicaid/CHIP across all states. The programs were implemented in different years for most states, with the exception of Massachusetts, Ohio, and Utah. After excluding these three states and re-estimating our models, the results remain virtually unchanged (see Figures A9a and A9b).



(A) Medicaid/CHIP enrollment

(B) State CHIP enrollment

FIGURE 7: EFFECTS OF WIC EBT ON MEDICAID/CHIP ENROLLMENT AMONG INFANTS

Notes: We present point estimates of the dynamic effects using the group-time estimator developed by Callaway and Sant'Anna (2021), along with their 95% confidence intervals adjusted for multiple testing. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$. The unit of observation is county-by-year cells. Standard errors are clustered at the county level.

6.5 Other potential mechanisms

There may be other mechanisms behind our findings that we either do not know about or cannot test directly or indirectly. For example, the WIC EBT transition reduces hassle costs by streamlining the checkout process, which likely increases WIC participation. However, we lack direct or indirect measures of hassle cost reductions to confirm this mechanism. Greater flexibility in benefit redemption may also play a role. Under the paper voucher system, participants had to redeem all benefits at once, often causing waste or inconvenience. In contrast, the EBT system allows partial redemption, enabling participants to shop more flexibly and make better use of their benefits. The EBT transition may also simplify the eligibility and recertification process, reducing administrative and compliance burdens for both participants and program administrators, and encouraging participation on both sides.

As for birth outcomes, some of the improvement may come from reduced financial and logistical stress during pregnancy. Easier access to food and public assistance can help ease these stressors, leading to healthier pregnancies and better outcomes for infants. Exploring these and other potential mechanisms in greater depth is an important avenue for future research.

7 Discussion and Conclusion

We provide the first national evidence on the effects of WIC payment digitization—known as the WIC EBT transition—on participant outcomes. Using hand-collected data on WIC EBT rollout at the county level, linked with birth certificate records, administrative datasets, and national survey data, we find that the transition to EBT increases the share of mothers participating in WIC by 0.52 percentage points, representing 1.26% of the sample mean. It also reduces the incidence of low birth weight by 0.08 percentage points (1% of the sample mean) and preterm birth by 0.48 percentage points (4.22% of the sample mean). Our identification strategy leverages the staggered county-level rollout over two decades, estimated using the difference-in-differences approach of Callaway and Sant'Anna (2021). Our main results are robust to a broad set of empirical choices, including balanced versus unbalanced panels, different definitions of control groups, inclusion of pre-treatment covariates, alternative estimation strategies, different definitions of EBT exposure timing, exclusion of atypical states, extension of the event window, and allowance for linear violations of the parallel trends assumption. Falsification tests using either non-target populations or pseudo-treated counties further strengthen the credibility of our findings. Based on the reduction in low birth weight, we estimate that the WIC EBT transition is associated with approximately $2.15 million in short-term annual hospital cost savings and an additional $15.51 million in long-term adult earnings, which suggests the economic benefits of the policy.

We also offer new insight into the mechanisms through which EBT improves participant outcomes. Increased interest may be due to less stigma at retailers after EBT implementation. We identify counties where stigma is more likely to be salient – rural counties, counties with lower peer engagement in WIC, and heavily Republican counties – and show that the effect of EBT on participation is largest where WIC is likely most stigmatized. We show some evidence that lower hassle costs could be a potential mechanism, but it should be interpreted more cautiously. We also find that increased prenatal care attendance likely contributes to improved birth outcomes, as indicated by higher rates of prenatal care utilization and Medicaid/CHIP enrollment following EBT adoption.

One limitation of our empirical approach 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 Callaway and Sant'Anna (2021) approach constructs a series of classical DiD estimates and aggregates, so we speculate that this attenuation effect may still hold.

Our work contributes to a broader literature on adopting digital technologies in public programs. Like prior papers in the literature, we show that program changes to incorporate digital technologies that make benefit redemption more accessible – in our setting both directly and through reduced stigma and encouraged prenatal care attendance – improve participation and participant outcomes. Declining WIC enrollment among eligible groups has been a focus for leaders of WIC state and federal agencies in the past ten years. In a policy environment where stigma from program participation is on the rise and the health of women, infants, and children is more important than ever, it is important that policymakers understand the factors that can make food assistance programs more efficient. Funding and facilitating use of technology to improve the participant experience is one such policy.

Online shopping is the next big step in the digitization of WIC. Allowing participants to select items and check out online eliminates key barriers to program participation, such as trying to purchase ineligible items and long checkout processes. Online WIC 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 were it available and the most common reason for not redeeming benefits fully was a lack of access to online shopping – (Ritchie et al., 2021). While online WIC shopping requires substantial updates to program rules and existing technology, our results suggest that this change in WIC is likely to boost WIC participation and improve birth outcomes.

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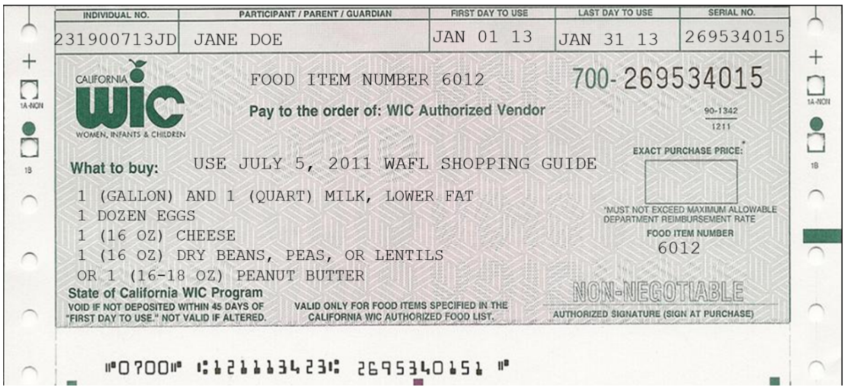
Appendix

A Figures and tables

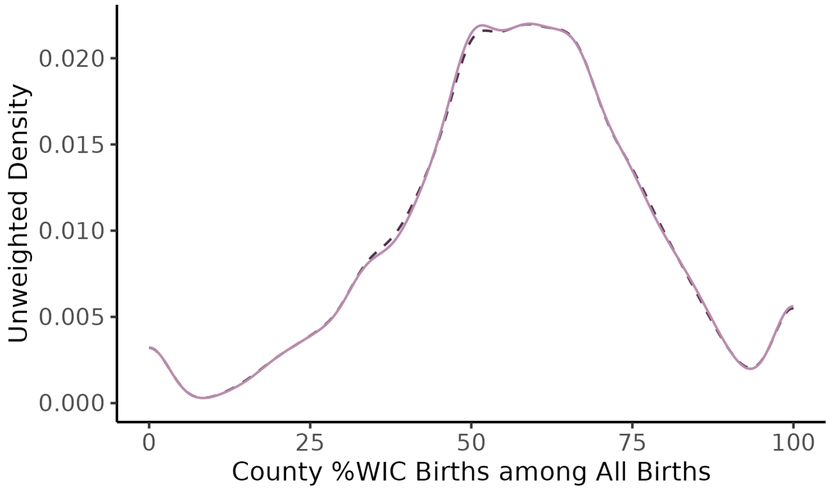
FIGURE A1: WIC PAPER VOUCHER AND EBT CARD IN CALIFORNIA



(A) WIC paper voucher

(B) WIC EBT card

FIGURE A2: VALIDATE BIRTH DATA FROM THE VITAL STATISTICS NATALITY DATA (VSND) AGAINST THE TEXAS DEPARTMENT OF STATE HEALTH SERVICES



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 dataset. The overlapped subsets cover 239 counties in Texas from January 2005 to December 2009.

FIGURE A3: DISTRIBUTION OF EVENT TIME

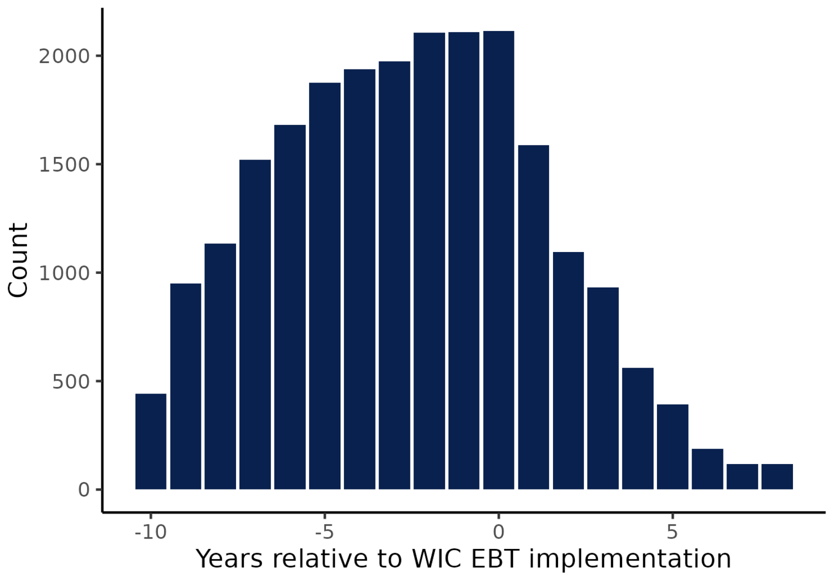
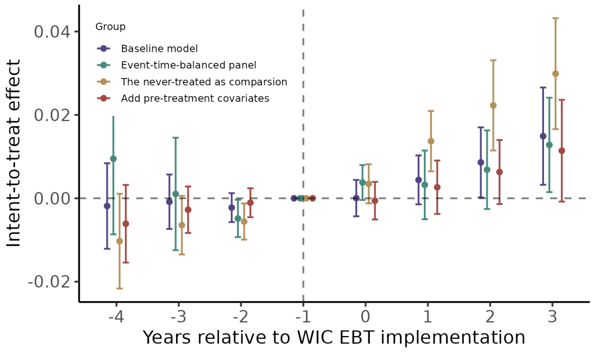


TABLE A1: CORRELATION OF THE TIMING OF WIC EBT TRANSITION AND COUNTY BASELINE CHARACTERISTICS

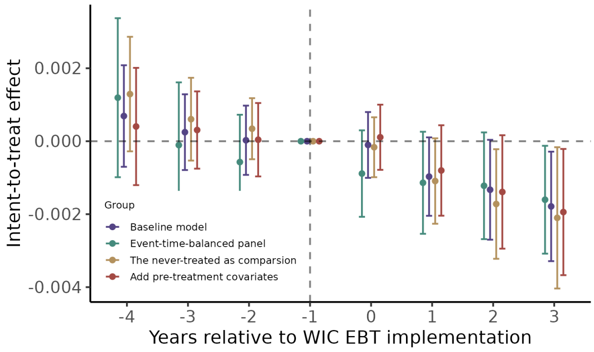
	Year of WIC EBT tranistion	
	(1)	(2)
Demographics, 2006-2008		
Share of non-white	0.0416**	-0.0029
	(0.0163)	(0.0027)
Share of Hispanics	-0.0536**	-0.0009
	(0.0218)	(0.0073)
Share of female	-0.1577	-0.0221
	(0.1374)	(0.0187)
Share of population under 5	-0.1645	0.0434
	(0.3014)	(0.0389)
Share of population over 65	-0.1788*	-0.0176
	(0.1039)	(0.0172)
Log population	0.3764	0.0723**
	(0.2878)	(0.0283)
Economic conditions, 2006-2008		
Unemployment rate	-0.0051	0.0642**
	(0.2019)	(0.0257)
Income per capita	0.1735	0.0118
	(0.2123)	(0.0158)
Government transfers per capita, 2006-2008		
Retirement and disability benefits per capita	0.9007	0.0499
	(0.7899)	(0.1172)
Medical benefits per capita	0.5978**	0.1314
	(0.3004)	(0.0896)
Income maintenance benefits per capita	-0.1646	-0.1001
	(1.789)	(0.2378)
Other benefits per capita	-0.0822	-0.3404
	(1.152)	(0.2973)
WIC vendors, 2006-2008		
Number of WIC vendors per capita	-106.2	58.73
	(617.8)	(81.58)
Observations	2,954	2,954
R^2	0.0856	0.9899
Within R^2		0.0805
Dep. var. mean	2016.52	2016.52

Notes: This table shows the means and standard errors of the group with characteristics listed in the first column. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$. Demographic data are from the Intercensal Population Estimates; unemployment rates are from the Local Area Unemployment Statistics, Bureau of Labor Statistics; income per capita is from the American Community Survey Public Use Microdata Sample; government transfer data are from the Bureau of Economic Analysis, Regional Economic Information System; and WIC vendor counts are from the WIC Integrity Profiles. Units of transfer are dollars. All variables represent three-year averages for 2006-2008. Each regression is weighted by the mean county population from 2006 to 2008. Standard errors are heteroskedasticity-robust.

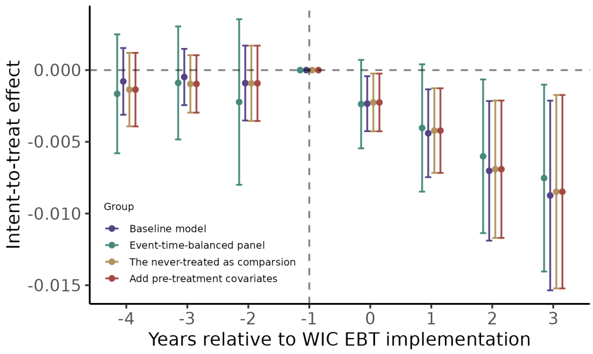
FIGURE A4: ROBUSTNESS: ALTERNATIVE SPECIFICATIONS



(A) Share of WIC mothers



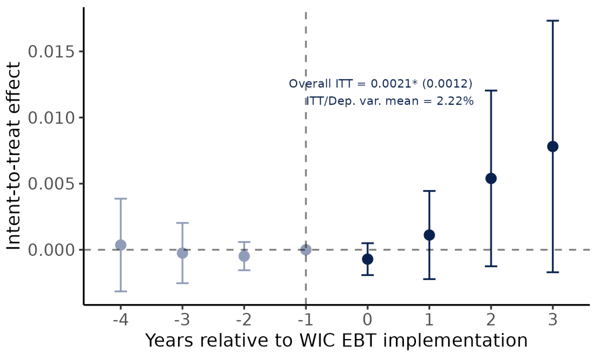
(B) Low birth weight



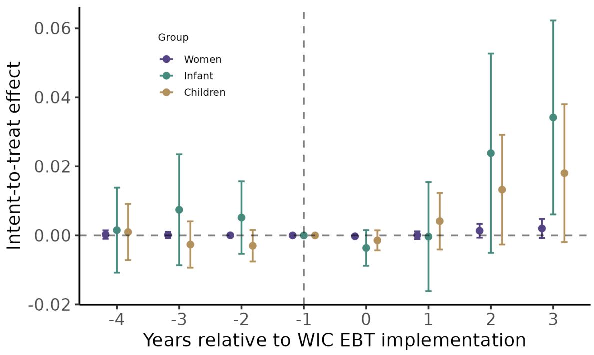
(C) Preterm births

Notes: We present point estimates of the dynamic effects using the group-time estimator developed by Callaway and Sant'Anna (2021), along with their 95% confidence intervals adjusted for multiple testing. The unit of observation is county-by-year cells. Standard errors are clustered at the county level.

FIGURE A5: ROBUSTNESS: ALTERNATIVE WIC PARTICIPATION DATA FROM USDA FNS



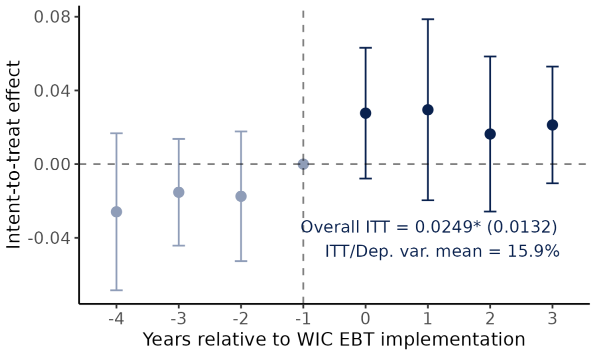
(A) Share of WIC participants



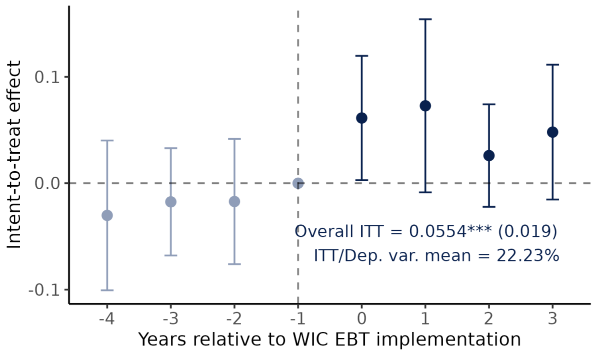
(B) Share of WIC participants by participant category

Notes: We present point estimates of the dynamic effects using the group-time estimator developed by Callaway and Sant'Anna (2021), along with their 95% confidence intervals adjusted for multiple testing. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$. The unit of observation is state-by-month average. Standard errors are clustered at the state level.

FIGURE A6: ROBUSTNESS: ALTERNATIVE WIC PARTICIPATION DATA FROM CPS-FSS



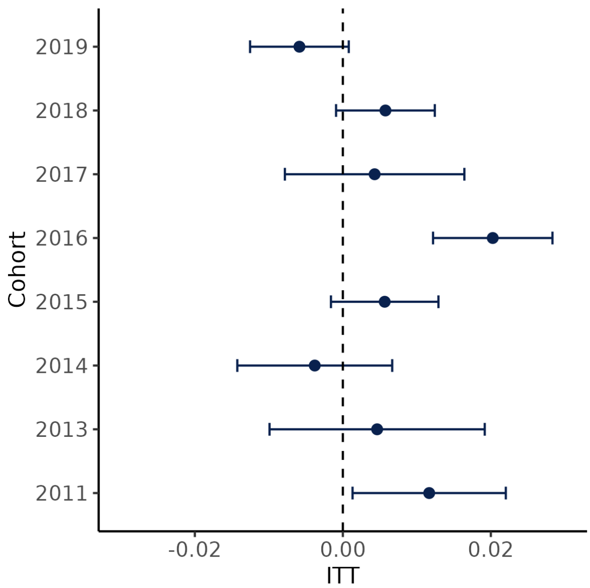
(A) Share of WIC households



(B) #WIC participants per household

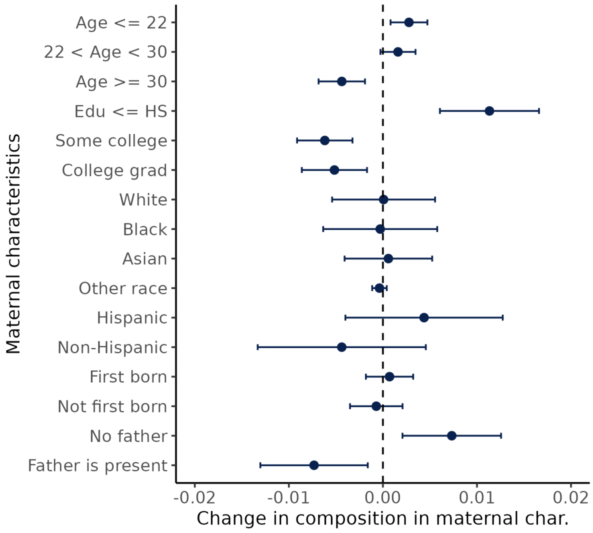
Notes: We present point estimates of the dynamic effects using the group-time estimator developed by Callaway and Sant'Anna (2021), along with their 95% confidence intervals adjusted for multiple testing. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$. The unit of observation is household. Standard errors are clustered at the state level.

FIGURE A7: COHORT-SPECIFIC EFFECTS ON WIC PARTICIPATION

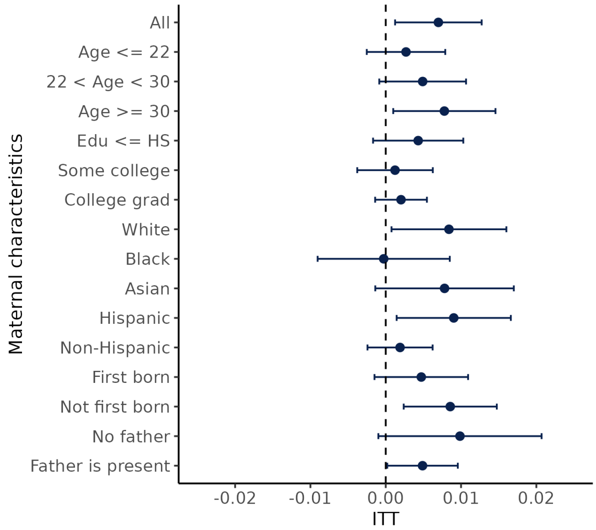


Notes: We present point estimates of the cohort-specific effects along with their 95% confidence intervals using the group-time estimator developed by Callaway and Sant'Anna (2021). Standard errors are clustered at the county level. We use not-yet-treated areas as the control group and do not include covariates. The unit of observation is county-by-year cells. The regression is weighted by the number of births in each cell.

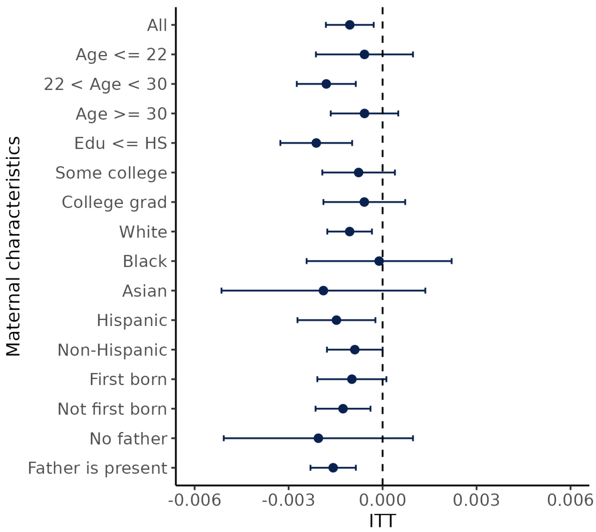
FIGURE A8: COMPOSITION CHANGE AND HETEROGENEITY BY MATERNAL CHARACTERISTICS



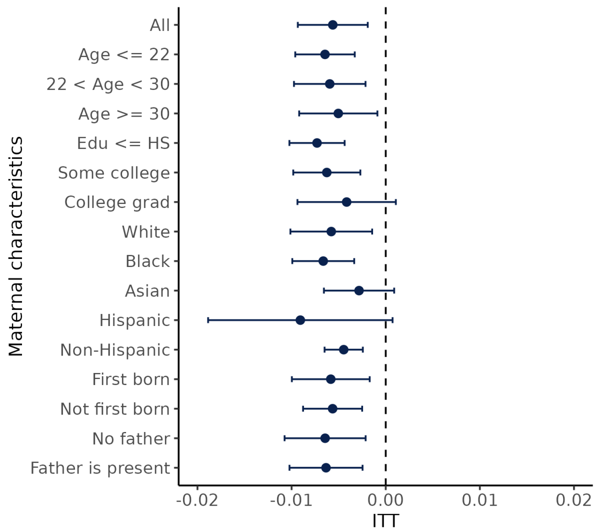
(A) Composition change in maternal char.



(B) Heterogeneous effects on Share of WIC mothers by maternal char.



(C) Heterogeneous effects on low birth weight by maternal char.



(D) Heterogeneous effects on preterm birth by maternal char.

Notes: We present point estimates of the dynamic effects and their 95% confidence intervals using the group-time estimator proposed by Callaway and Sant'Anna (2021). The unit of observation is county-by-year cells. Standard errors are clustered at the county level. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$.

TABLE A2: ESTIMATED HOSPITAL COST SAVING ASSOCIATED WITH WIC EBT FROM REDUCING LOW BIRTH WEIGHT

Birth weight segment	Excess hospital costs per mother (in 2000 dollars)	Percentage of births in each birth weight segment (%)
(1)	(2)	(3)
< 600 g	\$61,213	0.24
600-800 g	\$67,816	0.21
800-1000 g	\$36,846	0.23
1000-1500 g	\$22,309	0.75
1500-2000 g	\$6,806	1.60
2000-2500 g	\$604	5.19
Aggregated cost saved per mother	\$681.63	
Hospital cost saved per year	\$2,150,212.74	

Notes: Column (2) represents the average reduced hospital costs associated with increasing an infant's birth weight from the given birth weight category to above 2500 grams, estimated by Almond, Chay and Lee (2005). Total hospital cost saved = aggregated cost saved per mother × average number of mothers per year × reduced likelihood of low birth weight due to the new arsenic rule. Thus, total hospital cost saved per year is: aggregated cost saved per mother (\$681.63) × average births in our sample (3,943,145) × treatment effects on the incidence of low birth weight (0.0008) = \$2,150,212.74 (in 2000 dollars)

TABLE A3: THE EFFECT OF WIC EBT ON WIC PARTICIPATION FOR HOUSEHOLDS RECEIVED OR NOT RECEIVED SNAP

	Non-SNAP households		SNAP households	
	Share of WIC households	#WIC participants per household	Share of WIC households	#WIC participants per household
	(1)	(2)	(3)	(4)
WIC EBT transition	0.0033	0.0077	0.0612^{**}	0.1085^{***}
	(0.0093)	(0.0127)	(0.0299)	(0.0397)
Observations	178,071	178,020	77,850	77,763
Number of states	45	45	45	45
Dep. var. mean	0.0791	0.1241	0.2921	0.4811
Est./Dep. var. mean	4.17%	6.20%	20.95%	22.55%

Notes: We present point estimates of the dynamic effects using the group-time estimator proposed by Callaway and Sant'Anna (2021). The unit of observation is county-by-year cells. Standard errors are clustered at the county level. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$. These estimates are based on the baseline specification, which uses an imbalanced panel, excludes time-varying covariates, and uses not-yet-treated counties as the comparison group.

TABLE A4: EFFECTS OF WIC EBT ON FOOD SECURITY AMONG INFANTS

	Food insecurity			RASCH food security score		
	All	Likely eligible	Poor	All	Likely eligible	Poor
	(1)	(2)	(3)	(4)	(5)	(6)
WIC EBT transition	-0.0093	-0.0152	0.0014	-2.3467	1.0831	36.7494
	(0.0231)	(0.0446)	(0.0471)	(35.2374)	(36.9018)	(32.0584)
Observations	8,528	4,406	3,603	1,185	1,135	938
Number of counties	0	0	0	0	0	0
Number of states	45	45	45	45	45	45
Dep. var. mean	0.0691	0.1259	0.1280	542.6427	542.1878	546.8107
Est./Dep. var. mean	-13.46%	-12.07%	1.09%	-0.43%	0.20%	6.72%

Notes: We present point estimates of the dynamic effects using the group-time estimator proposed by Callaway and Sant'Anna (2021). The unit of observation is county-by-year cells. Standard errors are clustered at the county level. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$. These estimates are based on the baseline specification, which uses an imbalanced panel, excludes time-varying covariates, and uses not-yet-treated counties as the comparison group. Infants are likely eligible if their household income is below 185% of the federal poverty line or if their household ran out of money for food in the past year, and they live with a child under age 5 or a woman between the ages of 15 and 45. Poor infants are defined as those living in households with income below 185% of the federal poverty line.

TABLE A5: EFFECTS OF WIC EBT ON FOOD CONSUMPTION BY LOCATIONS

	Restaurants, cafeteria, or convenience stores	Supermarket grocery stores	Dollar stores, pharmacies, club stores, farmers markets or online	Others
	(1)	(2)	(3)	(4)
WIC EBT transition	0.0280	0.0159	-0.0249	0.0008
	(0.0533)	(0.0412)	(0.0490)	(0.0167)
Observations	4,412	5,069	5,065	4,411
Number of counties	0	0	0	0
Number of states	45	45	45	45
Dep. var. mean	0.5461	0.8983	0.3667	0.0403
Est./Dep. var. mean	5.13%	1.77%	-6.79%	1.99%

Notes: We present point estimates of the dynamic effects using the group-time estimator proposed by Callaway and Sant'Anna (2021). The unit of observation is county-by-year cells. Standard errors are clustered at the county level. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$. These estimates are based on the baseline specification, which uses an imbalanced panel, excludes time-varying covariates, and uses not-yet-treated counties as the comparison group.

TABLE A6: EFFECTS OF WIC EBT ON LOWER-COST/FREE FOODS

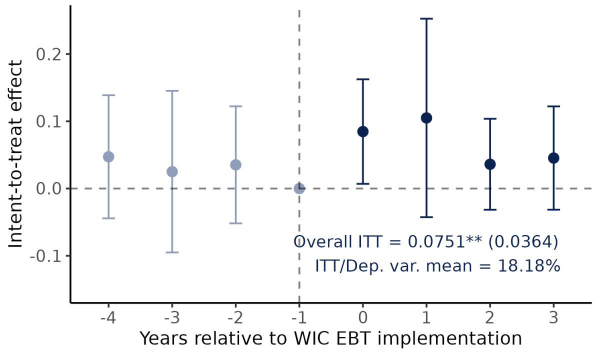
	SNAP	School lunches	Foods from community programs/centers	Foods from churches/pantries/food banks/soup kitchen
	(1)	(2)	(3)	(4)
WIC EBT transition	-0.0030	-0.0402	-0.0650	-0.0470
	(0.0479)	(0.1010)	(0.0680)	(0.0426)
Observations	4,410	2,450	303	4,358
Number of states	45	45	44	45
Dep. var. mean	0.4377	0.5899	0.0332	0.1387
Est./Dep. var. mean	-0.69%	-6.81%	-195.78%	-33.89%

Notes: We present point estimates of the dynamic effects using the group-time estimator proposed by Callaway and Sant'Anna (2021). The unit of observation is county-by-year cells. Standard errors are clustered at the county level. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$. These estimates are based on the baseline specification, which uses an imbalanced panel, excludes time-varying covariates, and uses not-yet-treated counties as the comparison group.

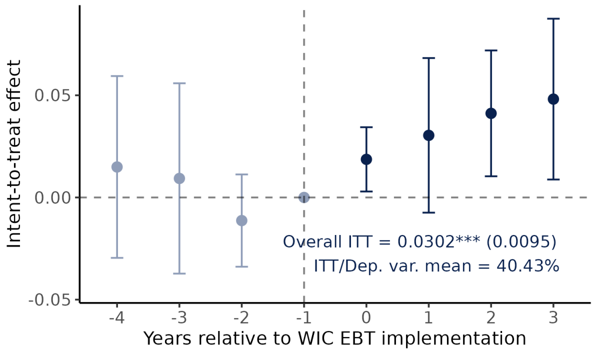
TABLE A7: ROLLOUT DATES OF WIC EBT AND MEDICAID/CHIP EXPANSION

States	WIC EBT	Medicaid/CHIP expanision
AL	3/14/2019-8/5/2019	Not adopted
AK	2/19/2019-8/19/2019	9/1/2015
AZ	10/17/2017-11/28/2017	1/1/2014
AR	4/12/2018-7/16/2018	1/1/2014
CA	6/3/2019-3/30/2020	1/1/2014
CO	4/1/2016-11/1/2016	1/1/2014
CT	2/22/2016-6/13/2016	1/1/2014
DC	6/1/2021	1/1/2014
DE	6/13/2016-10/24/2016	1/1/2014
FL	12/1/2013-3/1/2014	Not adopted
GA	6/1/2022-10/17/2022	Not adopted
HI	10/29/2019-5/8/2020	1/1/2014
ID	6/1/2019-10/1/2019	1/1/2020
IL	3/16/2020-8/31/2020	1/1/2014
IN	2/16/2016-9/5/2016	2/1/2015
IA	10/27/2015-5/23/2016	1/1/2014
KS	9/14/2017-8/30/2018	Not adopted
KY	8/11/2009-10/2/2011	1/1/2014
LA	1/14/2019-10/4/2019	7/1/2016
ME	6/22/2020-8/31/2020	1/10/2019 with coverage retroactive to 7/2/2018
MD	1/1/2017-7/1/2017	1/1/2014
MA	10/1/2014-10/29/2014	1/1/2014[a]
MI	10/1/2005-3/1/2009	4/1/2014
MN	10/17/2018-5/20/2019	1/1/2014
MS	4/12/2021-6/11/2021	Not adopted
MO	3/2/2020-8/31/2020	10/1/2021 with coverage retroactive to 7/1/2021
MT	6/8/2017-9/14/2017	1/1/2016
NE	6/4/2018-8/3/2018	10/1/2020
NV	8/1/2009	1/1/2014
NH	7/1/2018-11/1/2018	8/15/2014
NJ	10/21/2021-4/20/2022	1/1/2014
NM	7/1/2007-5/31/2008	1/1/2014
NY	4/30/2018-4/15/2019	1/1/2014
NC	3/5/2018-10/16/2018	12/1/2023
ND	5/20/2020-8/25/2020	1/1/2014
OH	7/14/2014-7/1/2015	1/1/2014[a]
OK	5/1/2015-9/8/2016	7/1/2021
OR	9/14/2015-3/7/2016	1/1/2014
PA	2/19/2019-10/28/2019	1/1/2015
RI	5/26/2020	1/1/2014
SC	5/6/2019-11/21/2019	Not adopted
SD	3/6/2017-9/5/2017	7/1/2023
TN	5/1/2018-4/1/2019	Not adopted
TX	6/1/2004-4/1/2009	1/1/2014
UT	9/1/2020-11/2/2020	1/1/2020[a]
VT	6/1/2015-3/21/2016	1/1/2014
VA	11/1/2013-5/5/2014	1/1/2019
WA	3/4/2019-11/4/2019	1/1/2014
WV	3/29/2013-10/28/2013	1/1/2014
WI	2/25/2015-9/16/2015	Not adopted
WY	1/31/2002	Not adopted

Notes: More details about Medicaid expansion by state, please refer to https://www.kff.org/status-of-state-medicaid-expansion-decisions/ (accessed on 6/30/2025). [a]: WIC EBT transition and Medicaid expansion occurred in the same year.



(A) Medicaid enrollment



(B) State CHIP enrollment

FIGURE A9: EFFECTS OF WIC EBT ON MEDICAID/CHIP ENROLLMENT AMONG INFANTS, EXCLUDING STATES WHERE MEDICIAD/CHIP EXPANSION AND WIC EBT TRANSITION HAPPENED IN THE SAME YEAR (MA, OH, AND UT)

Notes: We present point estimates of the dynamic effects using the group-time estimator developed by Callaway and Sant'Anna (2021), along with their 95% confidence intervals adjusted for multiple testing. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$. The unit of observation is county-by-year cells. Standard errors are clustered at the county level.

Online Appendix

B Other robustness checks and falsification tests

This section describes in more detail, and provides results of, the robustness and sensitivity checks that we summarized in 5.4.

B1 Alternative estimation methods

We begin by comparing estimates from alternative estimation methods, including the two-stage DiD approach proposed by Gardner (2022) and the imputation method proposed by Borusyak, Jaravel and Spiess (2024), with our main findings using the CS estimator. The two-stage DiD method proposed by Gardner (2022) compares outcomes between treated and untreated groups after removing unit and time fixed effects, which are estimated in a first-stage regression using only untreated observations. The imputation approach by Borusyak, Jaravel and Spiess differs from the Callaway and Sant'Anna (2021) estimator in that it constructs counterfactual outcomes using untreated and not-yet-treated observations, rather than comparing group-time average treatment effects to specific control cohorts. As shown in Figures B1a-B1c, while the estimators are not directly comparable due to differences in methodology, we find that both alternative approaches produce results that align closely with our baseline estimates using Callaway and Sant'Anna (2021) approach.

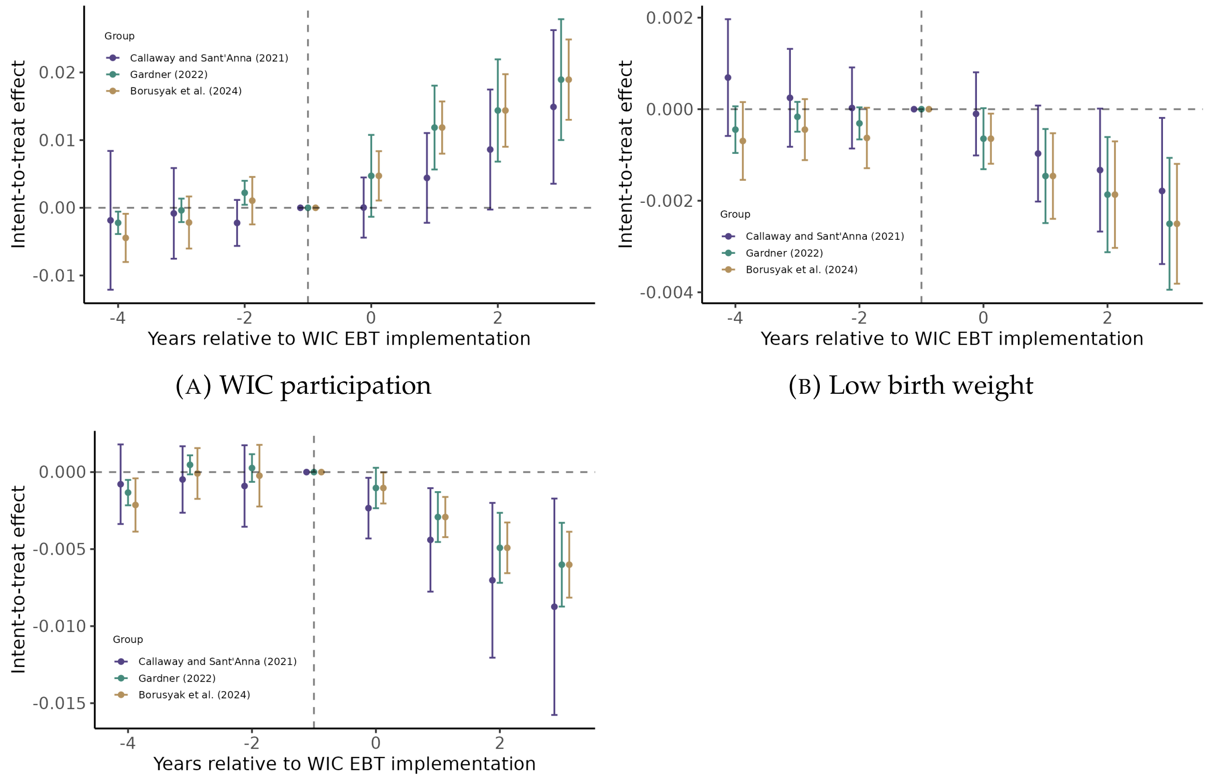
B2 Alternative timings of exposure

Second, we examine how our results vary under different definitions of exposure timing: the beginning of the first trimester, beginning of the second trimester, beginning of the third trimester (our baseline specification), and the time of birth. We define the three trimesters based on gestational length: the first three months as the first trimester, the following three months as the second trimester, and the remaining months as the third trimester. Figures B2a–B2c show that our findings are not particularly sensitive to these alternative definitions. This is reasonable, as the effect in period 0 is either very small or close to zero.

B3 Excluding Vermont and Mississippi

Before transitioning to the EBT system partnered with private retailers, Vermont used home delivery for WIC benefits, and Mississippi relied on state-owned warehouses to distribute benefits under the paper voucher system. As a result, the EBT transition in these states is confounded by simultaneous changes in delivery methods. To address this, we examine how our results change when excluding Vermont and Mississippi from the sample. Figures B3a–B3c

Figure B1: Robustness: alternative estimation methods



(A) WIC participation

(B) Low birth weight

(C) Preterm births

Notes: We present point estimates of the dynamic effects and their 95% confidence intervals using estimators proposed by Callaway and Sant'Anna (2021), Gardner (2022), and Borusyak, Jaravel and Spiess (2024). The unit of observation is county-by-year cells. Standard errors are clustered at the county level. For Borusyak, Jaravel and Spiess (2024) estimators, we use not-yet-treated and never-treated observations from periods -7 to -2 to construct the imputed counterfactual.

show that removing these states has virtually no effect on our estimates.

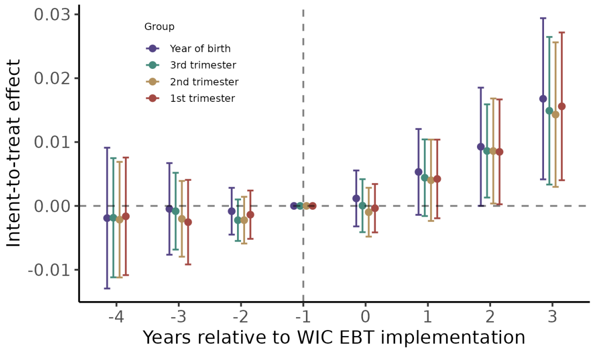
B4 Longer-term dynamic effects

Third, we extend the post-event window to examine the longer-term dynamic effects. Figures B4a–B4c present estimates that include additional post-treatment periods, up to a maximum of eight periods after treatment. The estimates in the later post-treatment periods remain directionally consistent with our main results. However, the standard errors tend to be larger, likely reflecting the more imbalanced comparisons that our baseline specification was designed to avoid.

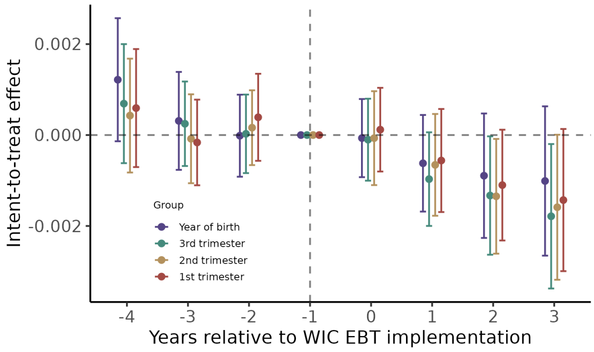
B5 Placebo test using non-target group

The WIC EBT transition should not affect individuals who are not eligible for WIC. To test whether our results are driven by spurious trends in underlying WIC participation that happen to coincide with the timing of WIC EBT implementation, we estimate the effect of WIC EBT

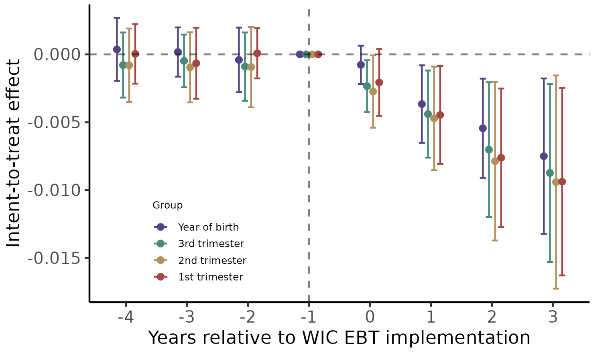
Figure B2: Robustness: alternative timings of exposure



(A) WIC participation



(B) Low birth weight



(C) Preterm births

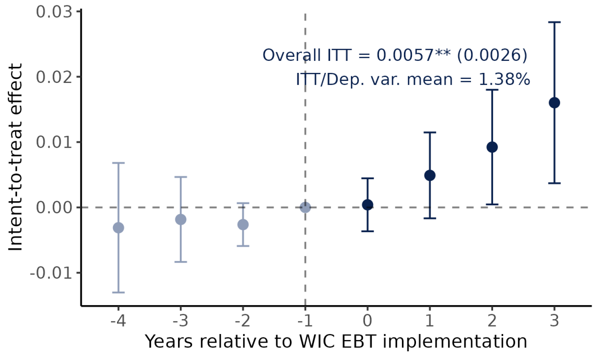
Notes: We present point estimates of the dynamic effects and their 95% confidence intervals using the group-time estimator proposed by Callaway and Sant'Anna (2021). The unit of observation is county-by-year cells. Standard errors are clustered at the county level. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$.

on WIC participation among a group that is less likely to be eligible: mothers over age 25 with a college degree. WIC eligibility can be inferred in the Survey of Income and Program Participation (SIPP), which includes information on household income, demographics, and program participation.[9] We define WIC-eligible mothers as mothers of infants (children under age 1) with household income below 185% of the federal poverty line or with participation in SNAP, TANF/AFDC, or Medicaid. Between 2009 and 2021, the average proportion of WIC-eligible mothers of infants was 48.23%, slightly lower than the 54.10% estimate for WIC-eligible pregnant and postpartum women in 1998 reported by Bitler, Currie and Scholz (2003).

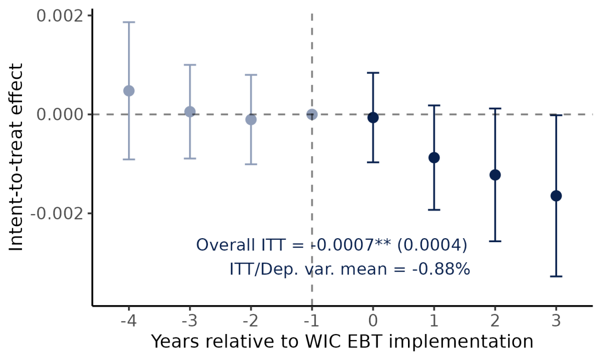
We use maternal education and age to define the non-target group because these characteristics are available in both the SIPP and birth certificate data. In the SIPP, this non-target group—college-educated mothers over age 25—makes up 30% of the full sample and is 18% less likely to be WIC eligible than the full sample. Some WIC-eligible individuals may still be included in this group, so we expect any estimated effects to be smaller and less precise, rather

[9] Bitler, Currie and Scholz (2003) documents a significant undercount of WIC participants in the SIPP, although the undercount appears to be random with respect to observable characteristics.

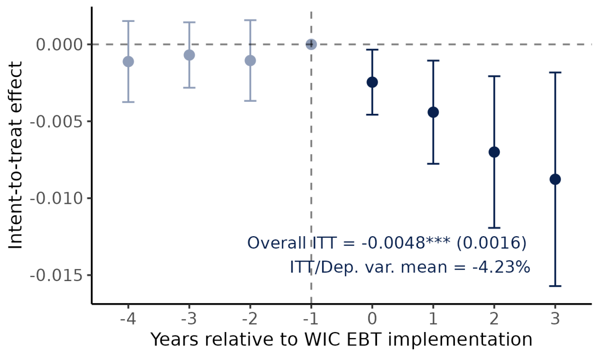
FIGURE B3: ROBUSTNESS: EXCLUDING VERMONT AND MISSISSIPPI



(A) WIC participation



(B) Low birth weight



(C) Preterm births

Notes: We present point estimates of the dynamic effects and their 95% confidence intervals using the group-time estimator proposed by Callaway and Sant'Anna (2021). The unit of observation is county-by-year cells. Standard errors are clustered at the county level. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$.

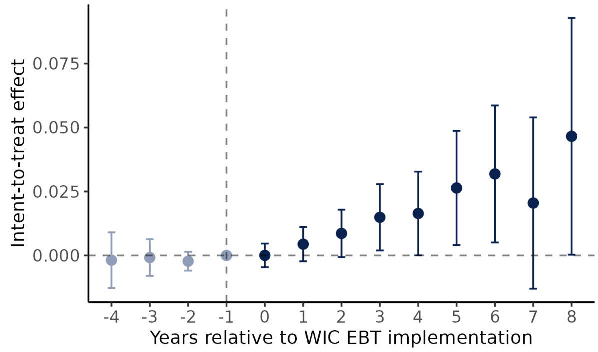
than entirely null. Table B1 reports the results for the non-target group. We find very noisy effects, consistent with the expectation that these mothers are less affected by the WIC EBT rollout.

TABLE B1: PLACEBO TEST

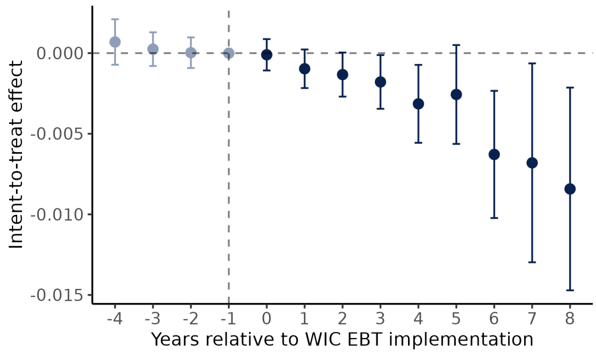
	WIC participation (1)	Low birth weight (2)	Preterm (3)
WIC EBT implementation	0.0011	-0.0006	-0.0034
	(0.0014)	(0.0006)	(0.0022)
Observations	28,729	28,729	28,729
Number of counties	2,724	2,724	2,724
Number of states	45	45	45
Dep. var. mean	0.0840	0.0665	0.0936
Est./Dep. var. mean	1.31%	-0.90%	-3.63%

Notes: We present point estimates of the dynamic effects using the group-time estimator proposed by Callaway and Sant'Anna (2021). The unit of observation is county-by-year cells. Standard errors are clustered at the county level. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$. These estimates are based on the baseline specification, which uses an imbalanced panel, excludes time-varying covariates, and uses not-yet-treated counties as the comparison group.

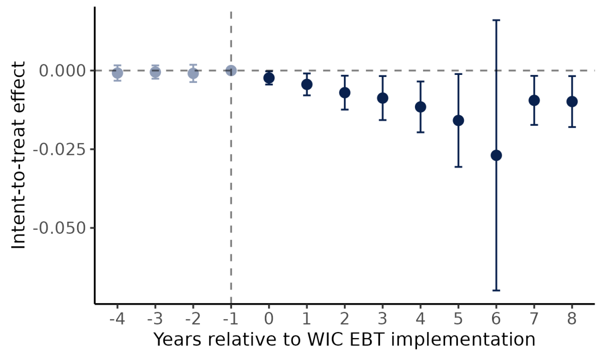
Figure B4: Robustness: longer-term dynamic effects



(A) WIC participation



(B) Low birth weight



(C) Preterm births

Notes: We present point estimates of the dynamic effects and their 95% confidence intervals using the group-time estimator proposed by Callaway and Sant'Anna (2021). The unit of observation is county-by-year cells. Standard errors are clustered at the county level. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$.

B6 Randomization test

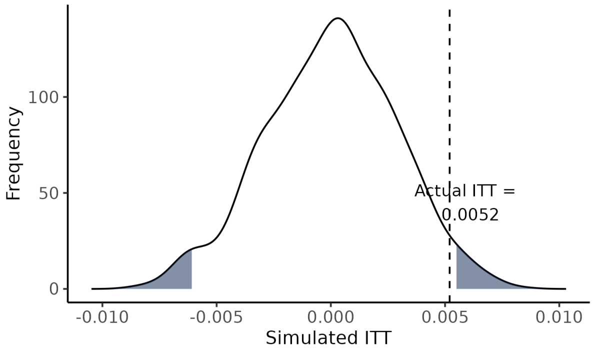
To assess the robustness of our results against random noise, we compute Intent-to-Treat (ITT) effects using randomized pseudo-treatment timings. We randomly assign the year of WIC EBT implementation 1,000 times while maintaining the original distribution of rollout years.[10] Figures B5a–B5c show that the estimated effects from our main analysis consistently lie near or well into the tails of the distribution of simulated placebo effects, suggesting that our findings are unlikely to be driven by chance.

B7 Sensitivity to potential parallel trend violation

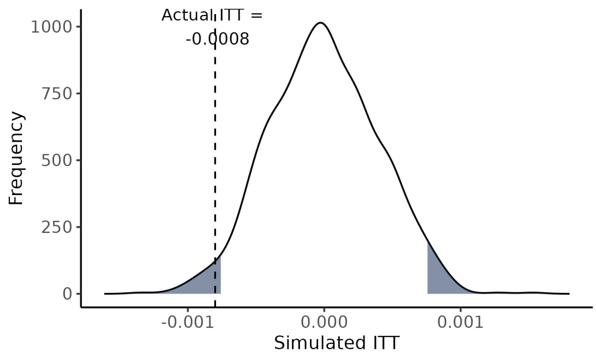
In this section, we assess the sensitivity of our results to potential violations of the parallel trends assumption using the approach proposed by Rambachan and Roth (2023). For the third year after EBT implementation, we estimate breakdown values—the magnitude of deviation in pre-trends that would render our estimates statistically insignificant at the 90% confidence

[10]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).

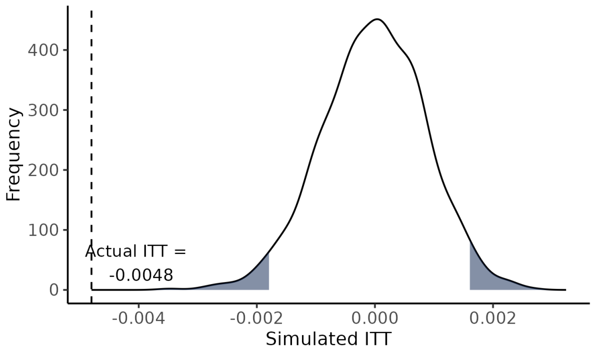
FIGURE B5: RANDOMIZATION TEST



(A) WIC participation



(B) Low birth weight

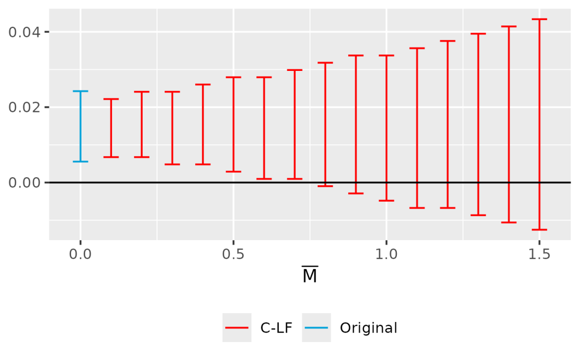


(C) Preterm birth

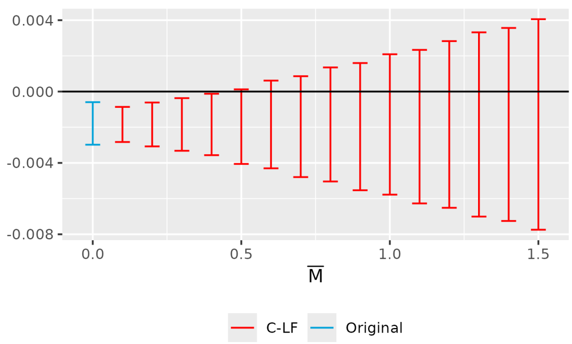
Notes: We present point estimates of the dynamic effects and their 95% confidence intervals using the group-time estimator proposed by Callaway and Sant'Anna (2021). The unit of observation is county-by-year cells. We randomize year of EBT implementation 1,000 times while keep the distribution. Regressions and dependent variable mean are weighted by the number of births in each cell. The shaded areas represent $\leq$ 2.5th and $\geq$ 97.5th percentiles of our simulated null distribution.

level—of 0.8 for WIC participation, 0.5 for the likelihood of low birth weight, and 1.3 for the likelihood of preterm birth (see Figures B6a-B6c). While we cannot entirely rule out such deviations, they appear unlikely given the observed pre-treatment dynamics, particularly for WIC participation and preterm birth. These results suggest that our estimates are reasonably robust to potential violations of parallel trends between treated and not-yet-treated counties.

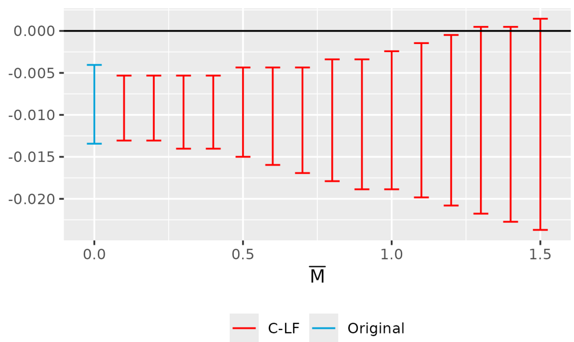
FIGURE B6: SENSITIVITY TO HYPOTHESIZED VIOLATION OF PARALLEL TREND ASSUMPTION



(A) WIC participation



(B) Low birth weight



(C) Preterm births

Notes: We present point estimates of the dynamic effects and their 90% confidence intervals using the group-time estimator proposed by Callaway and Sant'Anna (2021). The unit of observation is county-by-year cells. Standard errors are clustered at the county level. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$.