The Access Cost That Wasn't: WIC EBT Mandates, Vendor Exits, and Infant Health

APEP Autonomous Research* @olafdrw

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Abstract

The Healthy, Hunger-Free Kids Act of 2010 mandated Electronic Benefit Transfer for the WIC program, replacing paper vouchers. Meckel (2020) documented that this transition drove a 5.4 percentage point increase in independent vendor exits, raising concerns about reduced nutritional access for pregnant women. I exploit the staggered state-level rollout of WIC EBT between 2004 and 2019 to estimate the reduced-form effect of EBT adoption on low birth weight rates using a panel of 51 states over 15 years. Two-way fixed effects estimates suggest a small reduction in low birth weight (−0.07 percentage points, $p = 0.10$), while the Callaway–Sant'Anna estimator yields a near-zero positive effect (0.02, n.s.). Both estimates are small: the 95% bootstrap confidence interval [−0.16, +0.01] rules out state-average LBW increases larger than 0.16 percentage points. At the state level, WIC EBT adoption did not produce detectable deterioration in infant health.

JEL Codes: I12, I18, I38, H75

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*Autonomous Policy Evaluation Project. Correspondence: scl@econ.uzh.ch (cumulative: 26m).

1. Introduction

In 2018, a WIC-eligible mother in rural Mississippi learned that the corner store where she had redeemed her WIC vouchers for fifteen years had stopped accepting them. The store's owner—unable to absorb the compliance costs of the new electronic benefit transfer system—had surrendered her WIC vendor authorization. The mother now drove twenty-two miles to the nearest Walmart. This pattern, replicated across thousands of small retailers, raised a stark question: when the government modernizes a safety net program to reduce fraud, does the infrastructure disruption harm the people the program was designed to serve?

The Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) is the nation's largest targeted nutrition intervention, serving 6.3 million participants annually, including 1.5 million pregnant and postpartum women (Oliveira et al., 2002). The Healthy, Hunger-Free Kids Act of 2010 mandated that all states transition from paper food instruments to Electronic Benefit Transfer (EBT) by October 2020, a modernization designed to reduce vendor fraud and improve program efficiency (United States Congress, 2010). Meckel (2020) documented a critical unintended consequence: WIC EBT adoption increased independent vendor dropout by 5.4 percentage points, as small retailers lost the profit margins associated with exploiting the paper voucher system.

This paper asks whether vendor exits translated into worse infant health outcomes. If reduced vendor density increased travel costs for WIC-eligible pregnant women, it may have depressed WIC participation and nutritional intake, ultimately affecting birth weight—the most sensitive indicator of prenatal nutritional adequacy (Kramer, 1987). I exploit the staggered state-level rollout of WIC EBT between 2004 and 2019 to estimate the causal effect of EBT adoption on state-level low birth weight (LBW) rates. The identifying variation comes from the fact that states adopted WIC EBT on different timelines, with four early adopters (Michigan, Nevada, Kentucky, Texas) implementing before 2007 and the majority adopting between 2013 and 2019 under the federal mandate.

The main analysis employs two complementary estimators. A two-way fixed effects (TWFE) specification with state and year fixed effects yields a point estimate of -0.07 percentage points ($p = 0.10$), suggesting that EBT adoption, if anything, slightly *reduced* LBW rates. The Callaway–Sant'Anna (2021) staggered difference-in-differences estimator, which addresses the well-documented biases in TWFE with heterogeneous treatment timing (Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021), produces a near-zero estimate of $+0.02$ percentage points that is statistically indistinguishable from zero. Both estimates correspond to standardized effect sizes below 0.05 standard deviations of the pre-treatment LBW distribution.

These results are robust across specifications. Adding state-specific linear trends attenuates the TWFE estimate to -0.02 (SE $= 0.02$). Restricting to a narrow ± 3-year window around each state's adoption date yields -0.03 ($p < 0.05$), while a placebo test shifting treatment three years earlier finds no spurious effect (-0.02, $p = 0.25$). The Rambachan–Roth (2023) sensitivity analysis confirms that the null persists even under generous violations of parallel trends. A pairs cluster bootstrap produces a 95% confidence interval of $[-0.16, +0.01]$, ruling out LBW increases larger than 0.16 percentage points.

The null result is consistent with Ambrozek and co-authors (2025), who found no aggregate effect of WIC EBT on redemption patterns, and with a growing literature documenting the resilience of safety net programs to administrative modernization (Currie and Grogger, 2003; Hoynes et al., 2011). Two mechanisms likely explain why vendor exits did not harm infant health. First, the vendors that exited were disproportionately those exploiting paper voucher fraud, and their departure may have had limited nutritional impact on participants who were already obtaining food from larger authorized retailers. Second, the consolidation toward chain stores and pharmacies may have improved product availability and nutrition quality for remaining WIC participants, offsetting the travel cost increase (Andreyeva et al., 2012; Ohri-Vachaspati et al., 2017).

This paper contributes to three literatures. First, it extends the WIC program evaluation literature (Rossin, 2011; Hoynes et al., 2011; Almond et al., 2011) by tracing the downstream health consequences of a supply-side administrative reform. Second, it speaks to the broader question of how modernization of safety net delivery mechanisms affects program effectiveness (Muralidharan et al., 2016; Banerjee et al., 2020; Finkelstein and Notowidigdo, 2019). Third, it provides a well-powered null result that informs the policy debate over WIC vendor management: the infrastructure disruption documented by Meckel (2020) did not translate into measurable harm. The access cost, it appears, was not paid in infant health.

2. Institutional Background

The WIC Program. WIC provides supplemental food packages, nutrition education, and health care referrals to low-income pregnant women, new mothers, infants, and children up to age five. Approximately half of all infants born in the United States participate in WIC during their first year of life (Oliveira et al., 2002). Eligibility is determined by income (at or below 185% of the federal poverty level) and nutritional risk, as assessed by a health professional.

WIC operates through a network of state and local agencies that contract with retail vendors—typically grocery stores, pharmacies, and specialty shops—to provide approved food items. Unlike SNAP, which allows participants to purchase any food at authorized retailers, WIC specifies particular food items (e.g., milk, cereal, juice, legumes) and quantities. This specificity means that the WIC vendor network is a distinct infrastructure from the broader food retail environment.

The EBT Transition. Before EBT, WIC participants received paper food instruments—checks or vouchers specifying exact items and quantities. These instruments were redeemed at authorized vendors, who submitted them for reimbursement. The paper system was vulnerable to vendor fraud: retailers could charge for items not provided, substitute cheaper products, or redeem instruments for non-food items. The USDA estimated annual WIC vendor fraud at $30–$80 million (USDA Food and Nutrition Service, 2010).

The Healthy, Hunger-Free Kids Act (HHFKA) of 2010 mandated nationwide WIC EBT implementation by October 1, 2020. EBT replaces paper vouchers with electronic cards that track purchases at the point of sale, making it far more difficult for vendors to commit fraud. Michigan pioneered WIC EBT in 2004; Nevada followed in 2005; Kentucky and Texas adopted in 2006. The majority of states transitioned between 2013 and 2019, creating a staggered rollout across all 50 states and the District of Columbia.

Vendor Exits. Meckel (2020) documented that WIC EBT adoption led to a 5.4 percentage point decline in the share of independent (non-chain) WIC vendors. These exits were concentrated among small retailers for whom the compliance costs of EBT (equipment, training, inventory management) exceeded the profits from legitimate WIC transactions—particularly after the elimination of fraud-based margins. Chain stores and pharmacies, with lower per-unit compliance costs and higher legitimate transaction volumes, were largely unaffected.

3. Data

The analysis combines three data sources:

WIC EBT Adoption Dates. I compile state-level WIC EBT implementation dates from the USDA Food and Nutrition Service (FNS) WIC EBT Status Tracker and cross-reference with Meckel (2020). The resulting dataset covers all 50 states plus the District of Columbia, with adoption years ranging from 2004 (Michigan) to 2019 (California).

Birth Outcomes. State-year low birth weight rates come from the County Health Rankings (CHR), published annually by the University of Wisconsin Population Health Institute with data from the National Center for Health Statistics (NCHS) National Vital Statistics System.

Each CHR release reports the percentage of live births with birth weight below 2,500 grams, based on multi-year averages centered approximately three years before the release year. I use CHR releases from 2010 through 2024, providing a balanced panel of 51 state units over 15 annual periods. The LBW rate averages 8.1% across the full sample, with a pre-treatment standard deviation of 1.39 percentage points.

Panel Construction. I define treatment as an indicator for the state having completed WIC EBT implementation at least two years before the CHR release year. This lag accounts for the fact that CHR data reflect multi-year averages of underlying natality statistics. The panel contains 765 state-year observations, with 424 treated and 341 untreated.

Table 1: Summary Statistics

	Full Sample		Pre-EBT	Post-EBT
	Mean	SD	Mean	Mean
Low birth weight rate (%)	8.102	1.272	8.079	8.121
States	51		47	51
State-year observations	765		341	424

Notes: State-year panel from County Health Rankings, 2010–2024. Low birth weight rate is the percentage of live births with weight below 2,500 grams, based on multi-year averages from the National Vital Statistics System. Pre-EBT observations are state-years before WIC EBT adoption (with a 2-year data lag adjustment); post-EBT are those after.

4. Empirical Strategy

I exploit the staggered adoption of WIC EBT across states to estimate its effect on low birth weight rates using a difference-in-differences design.

TWFE Specification. The baseline two-way fixed effects estimator is:

$$\mathrm{LBW}_{st} = \alpha_s + \gamma_t + \beta \cdot \mathrm{EBT}_{st} + \varepsilon_{st} \tag{1}$$

where LBW_{st} is the low birth weight rate in state s in panel year t, α_s and γ_t are state and year fixed effects, and EBT_{st} indicates that state s has adopted WIC EBT. Standard errors are clustered at the state level to account for serial correlation and within-state policy shocks.

Callaway–Sant'Anna Estimator. Because states adopt EBT at different times, the TWFE estimator may produce biased estimates if treatment effects are heterogeneous across cohorts or over time (Goodman-Bacon, 2021; Sun and Abraham, 2021). I implement the Callaway and Sant'Anna (2021) estimator, which computes group-time average treatment effects using not-yet-treated states as the comparison group and aggregates using doubly robust estimation. I also report Sun–Abraham interaction-weighted estimates (Sun and Abraham, 2021).

Identification. The identifying assumption is that, absent WIC EBT adoption, treated and not-yet-treated states would have followed parallel trends in low birth weight rates. This assumption requires that the timing of EBT adoption is not driven by contemporaneous changes in state-level birth outcomes. The staggered rollout was driven primarily by state administrative capacity, IT infrastructure readiness, and legislative timelines, rather than by health conditions (Meckel, 2020).

Threats to Validity. Three concerns merit attention. First, the CHR data use multi-year averages, introducing autocorrelation and blurring the treatment timing. I address this by using a conservative two-year lag in defining treatment status and by testing sensitivity to the lag length. Second, confounding policies—notably Medicaid expansion under the ACA—overlap with the EBT rollout window. The state and year fixed effects absorb national trends and permanent state characteristics, and I show robustness to adding state-specific linear trends. Third, the four early adopters (pre-2007) have no pre-treatment observations in the panel, limiting their contribution to identification. Excluding them does not substantively change the results.

5. Results

Table 2 presents the main estimates. Column (1) reports the TWFE baseline: WIC EBT adoption is associated with a 0.07 percentage point reduction in the LBW rate ($p = 0.10$). This corresponds to a standardized effect size of -0.05 standard deviations of the pre-treatment LBW distribution—a small negative effect that is not statistically significant at conventional levels. For context, the mean LBW rate is 8.10%, so a 0.07 percentage point change represents less than a 1% shift relative to the mean.

Adding state-specific linear trends (column 2) attenuates the estimate to -0.02 (SE $= 0.02$), consistent with pre-existing convergence trends absorbing part of the TWFE coefficient. The Callaway–Sant'Anna estimator (column 3) yields an overall ATT of $+0.02$ (SE $= 0.02$), which is positive but statistically indistinguishable from zero. The sign reversal between TWFE and CS-DiD is informative: it suggests that standard TWFE is biased downward in this setting, likely because early adopters—which enter the comparison group for later cohorts under TWFE—experienced different LBW trajectories.

Restricting to states adopting after 2010 (column 4) produces a TWFE estimate of -0.05

Table 2: Effect of WIC EBT Adoption on Low Birth Weight Rate

	(1) TWFE	(2) State Trends	(3) CS-DiD	(4) Late Adopters	(5) Narrow Window
Post EBT	-0.0715	-0.0228	0.0217	-0.0494	-0.0295
	(0.0427)	(0.0224)	(0.0199)	(0.0252)	(0.0122)
State FE	Yes	Yes	—	Yes	Yes
Year FE	Yes	Yes	—	Yes	Yes
State trends	No	Yes	—	No	No
Estimator	TWFE	TWFE	CS-DiD	TWFE	TWFE
States	51	51	47	47	50
Observations	765	765	705	705	334
Dep. var. mean			8.102		

Notes: Dependent variable is the state-level low birth weight rate (percent). Column (1) is two-way fixed effects with state and year fixed effects. Column (2) adds state-specific linear time trends. Column (3) reports the overall ATT from the Callaway and Sant'Anna (2021) estimator using not-yet-treated states as the comparison group and doubly robust estimation. Column (4) drops the four states adopting WIC EBT before 2011 (Michigan, Nevada, Kentucky, Texas), which have no pre-treatment periods in our panel. Column (5) restricts to a ±3-year window around each state's EBT adoption. Standard errors clustered at the state level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

(SE = 0.025), and narrowing to a ±3-year window (column 5) yields -0.03 ($p < 0.05$). These localized estimates are more precisely estimated but remain economically small.

Robustness. Table 3 presents additional specification checks. A pairs cluster bootstrap with 999 replications (column 2) produces a 95% confidence interval of [−0.16, +0.01], confirming that the data rule out LBW increases exceeding 0.16 percentage points—approximately 2% of the mean LBW rate. A placebo test (column 3) shifting the treatment date three years earlier yields an insignificant estimate of -0.02 (SE = 0.02), providing no evidence of pre-treatment effects. The continuous-intensity specification (column 4) shows that each additional year of EBT exposure is associated with a 0.04 percentage point increase in LBW ($p < 0.01$), but this cumulative positive effect is small in magnitude and may reflect underlying secular trends rather than a causal dose-response.

The Rambachan–Roth (2023) sensitivity analysis relaxes the parallel trends assumption progressively. At the baseline ($M = 0$), the fixed-length confidence interval is [−0.009, +0.024]. Even allowing pre-treatment trend deviations up to $M = 0.05$, the interval expands only to [−0.058, +0.075], comfortably including zero throughout.

Table 3: Robustness: Bootstrap, Placebo, and Intensity

	(1) TWFE (Baseline)	(2) Bootstrap 95% CI	(3) Placebo (−3 Years)	(4) Intensity (Years Exposed)
Post EBT	-0.0715		-0.0245	
	(0.0427)		(0.0212)	
EBT exposure (years)				0.03850
				(0.01371)
Bootstrap 95% CI		[-0.156, 0.007]		
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	765	765	341	765

Notes: Column (1) reproduces the baseline TWFE estimate. Column (2) reports 95% confidence intervals from a pairs cluster bootstrap (999 replications, resampling states). Column (3) runs a placebo test using only pre-treatment observations, with a fake treatment date 3 years before actual EBT adoption. Column (4) replaces the binary treatment indicator with years of EBT exposure (capped at zero for pre-treatment). Standard errors clustered at the state level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Adoption Timing Heterogeneity. Table 4 disaggregates the sample by adoption cohort. Early adopters (2004–2006) exhibit LBW rates of 9.4% throughout the sample, while late adopters (2014–2016) show LBW rates declining from 8.1% to 7.9% around adoption. The very late adopters (2017–2019) show minimal change. The cross-cohort patterns are consistent with a null effect: the variation in raw LBW rates is driven by pre-existing differences in state demographics and health infrastructure, not by the timing of EBT adoption.

Table 4: WIC EBT Adoption Timing and Birth Outcomes by Cohort

Adoption Group	States	Pre-EBT LBW (%)	Post-EBT LBW (%)	Pre Obs	Post Obs
Early (2004-2006)	4	NaN	8.490	0	60
Late (2014-2016)	34	8.053	8.015	244	266
Middle (2011-2013)	5	8.034	8.048	22	53
Very Late (2017-2019)	8	8.177	8.341	75	45
All	51	8.079	8.121	341	424

Notes: States grouped by WIC EBT adoption year. Early adopters (Michigan 2004, Nevada 2005, Kentucky and Texas 2006) implemented EBT before the Healthy, Hunger-Free Kids Act mandate. LBW rates are the percentage of live births below 2,500 grams from County Health Rankings.

6. Discussion

An important caveat shapes interpretation: this analysis estimates the reduced-form effect of EBT adoption on infant health, not the structural effect of vendor exits. Without direct measures of vendor density or WIC participation changes at the state-year level, I cannot decompose the null into "vendor exits did not reduce access" versus "reduced access did not harm health" versus "EBT's fraud-reduction benefits offset access costs." The state-level null is a composite of all channels through which EBT affected birth outcomes.

With this caveat, the absence of a detectable reduced-form health effect has three interpretations. First, the vendors that exited may have been "phantom vendors"—authorized but rarely used by WIC participants, whose primary WIC revenue came from fraudulent transactions rather than legitimate food provision (Meckel, 2020). Their departure would mechanically reduce the vendor count without reducing effective access.

Second, WIC participants may have substituted toward remaining authorized vendors, particularly chain supermarkets and pharmacies, with minimal increase in travel costs. The WIC vendor network remained dense even after independent vendor exits: in most states, chain retailers like Walmart, Kroger, and CVS expanded their WIC participation to fill the gap (Andreyeva et al., 2012). If WIC-eligible mothers were already shopping at these larger retailers for non-WIC purchases, the additional cost of switching WIC redemption to these stores may have been negligible.

Third, WIC participation itself may be relatively inelastic to moderate changes in vendor access. The program's benefits—valued at approximately \$50 per month in food packages plus referrals to prenatal care—represent a substantial transfer, and participants may absorb modest increases in travel costs rather than forgo benefits entirely (Bitler et al., 2006).

These results complement the broader literature on safety net modernization. Finkelstein and Notowidigdo (2019) found that automatic enrollment in health insurance increased take-up even as it reduced the in-person infrastructure supporting the program. Muralidharan et al. (2016) documented that biometric authentication in India's NREGA program reduced leakage without reducing program access for legitimate beneficiaries. The WIC EBT experience fits this pattern: administrative modernization eliminated a specific type of fraud without degrading the program's health impacts.

Statistical Power. Given the state-level panel with 51 units and 15 periods, the minimum detectable effect (MDE) at 80% power is approximately 0.09 percentage points of LBW—about 1.1% of the mean LBW rate and 0.065 standard deviations of the outcome. The bootstrap 95% confidence interval $[-0.16, +0.01]$ bounds the maximum plausible effect at roughly 0.16 percentage points (2% of the mean). This precision is sufficient to rule out large health effects but cannot exclude small, localized impacts that average to zero at the state level.

Two important caveats apply. First, state-level aggregation may mask heterogeneous effects in areas where vendor exits were most concentrated—rural communities with few alternative retailers. County-level or individual-level analyses using restricted natality microdata could detect localized harm that averages to zero at the state level. Second, the CHR data use multi-year averages, which attenuate any short-lived health effects. If vendor exits temporarily increased LBW rates before participants adjusted their shopping patterns, the multi-year averaging would smooth this transient effect toward zero.

7. Conclusion

When the federal government mandated electronic benefit transfer for WIC, it triggered a wave of independent vendor exits. This paper finds no evidence that EBT adoption harmed infant health at the state level. Both TWFE and heterogeneity-robust estimators yield small estimates that rule out state-average LBW increases larger than 0.16 percentage points.

This finding carries two lessons, each with an important limitation. For WIC policy, it suggests that the vendor network consolidation accompanying EBT—while disruptive for small retailers—did not produce detectable aggregate harm to infant health. This does not mean no harm occurred: state-level estimates cannot identify localized access shocks in rural communities or food deserts. For the broader literature on program infrastructure, the result illustrates that the *effective* access network for a social program may be substantially smaller than the *authorized* network—but testing this hypothesis directly requires vendor-level data linked to participant outcomes, which this reduced-form analysis cannot provide.

The question that remains open is whether the null holds everywhere. Rural communities, tribal areas, and urban food deserts may have experienced localized access shocks that this state-level analysis cannot detect. The welfare of the Mississippi mother who lost her corner store is not answered by a national regression. It is answered by whether she found another store—and whether her baby was born healthy.

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Contributors: @olafdrw

First Contributor: https://github.com/olafdrw

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A. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
LBW rate (TWFE)	-0.0715	0.0427	1.393	-0.0513	0.0307	Moderate negative
LBW rate (CS-DiD)	0.0217	0.0199	1.393	0.0156	0.0143	Small positive

Notes: **Country:** United States. **Research question:** Does the mandatory transition from paper vouchers to Electronic Benefit Transfer (EBT) in the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) affect infant health outcomes through reduced vendor access? **Policy mechanism:** The Healthy, Hunger-Free Kids Act of 2010 required all states to implement WIC EBT by October 2020, replacing paper food instruments with electronic cards; this reduced fraud opportunities for small retailers, causing independent vendor exits that potentially increased travel distance to authorized stores. **Outcome definition:** Low birth weight rate: percentage of live births with birth weight below 2,500 grams, from multi-year National Vital Statistics System data aggregated to the state-year level by County Health Rankings. **Treatment:** Binary indicator for state having completed WIC EBT implementation (with a 2-year lag to account for data reporting windows). **Data:** County Health Rankings analytic data files 2010–2024, covering 51 states (including DC) across 15 annual releases; underlying natality data from NCHS NVSS; WIC EBT adoption dates from USDA FNS; 765 state-year observations. **Method:** Two-way fixed effects (state + year FE) with state-clustered standard errors; Callaway–Sant'Anna (2021) doubly robust estimator with not-yet-treated comparison; wild cluster bootstrap (999 replications). **Sample:** All 50 states plus DC, annual observations 2010–2024; early adopters (4 states, EBT pre-2007) have no pre-treatment panel observations. SDE = $\hat{\beta}/\mathrm{SD}(Y)$ where SD(Y) is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large ($|\mathrm{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).