Bottle Redemption, Wealth Transfers, and Informal Wages

Maya A. Norman*

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Abstract

This paper shows that waste policy can improve birth outcomes in marginalized populations to a similar extent as EITC, a widely studied welfare program. Between 1973 and 1990, ten states introduced deposit refund programs for beverage containers. Policy introductions are associated with a .6-3.7% reduction in the incidence of low birth weight among mothers with less than a high school education. A simple labor supply model implies that deposit refund programs create opportunities for informal labor among the working poor. This paper's results indicate that informal work in recycling alleviated gaps in welfare policy during the study period.

Key words: Deposit Refund, Incidence of Low Birth Weight
JEL Codes: Q52, Q53, I38, I12

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*Sustainable Development, Columbia University, email: man2185@columbia.edu

1 Introduction

> *"Now when you reap the harvest of your land, you shall not reap the very corners of your field, nor shall you gather the gleanings of your harvest. Nor shall you glean your vineyard, nor shall you gather the fallen fruit of your vineyard; you shall leave them for the needy and the stranger."*
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> —Leviticus 19:9-10

This paper indicates that waste collection and processing can create important and overlooked wealth transfers. One need not look further than the Old Testament, for a historical reference to the waste stream's perceived importance in the social safety net. Today, in the global south, roughly 1% of people find employment in informal recycling.[1] In the global north, scavenging through waste is predominantly illegal.

Deposit refund programs for beverage containers, known as "bottle bills" (BBs), have the potential to cultivate informal markets around recycling in places where waste foraging has been suppressed. Bottle bills provide a stable price for recyclables and require redemption opportunities be easily accessible. For example, in New York City, one redemption center processed 5 million containers weekly in 2021, amounting to $250,000 in refunds. This redemption center is one of 40 operating in the area. Redemption centers purchase empties from building employees and container collectors on New York City's streets. An estimated 4,000 individuals work full time as container collectors in NYC making roughly $60,000 per person annually. Many other laborers collect cans throughout the city, earning far less each year.[2]

BBs are increasingly popular. In 2005, BBs covered 250 million people globally. In 2025, they will cover 500 million in 59 different jurisdictions.[3] The motivation for BBs is waste management oriented; the policy improves recycling rates and yields higher value recycled material. Moreover, economic theory argues that deposit refund programs are the most efficient way for the market to incorporate waste disposal's social costs. The policy taxes waste production and incentivizes proper disposal. Other policies, such as Pigouvian taxes, create incentives to illegally dump waste (Fullerton and Wolverton 2000).

BBs require consumers to pay deposits on beverages at purchase and return deposits to people who properly return empties. Importantly, the person who pays the tax does not need to be the same person to collect the return. If everyone does not collect their refunds, BBs increase the value of the waste stream, a common pool resource, exploitable through recycling. Consider the following simple model for empty collection. Let the recycling wage be the value of all discarded empties divided by the number of collectors on a given day. An earner who's market wage falls below the recycling wage will choose to recycle (Ashenmiller 2011). An earner who's market wage exceeds the transaction costs of redemption will discard their personal empties without collecting refunds. In short, relatively higher wage earners create the empties reservoir, and relatively lower wage earners recycle empties from the reservoir for refunds. Theoretically, recycling markets transfer wealth from high to low wage earners and BBs increase recycling markets' value, enhancing the markets' total wealth transfer.

In reality, people who's market wage exceeds the recycling wage also recycle for cash. These people may be constrained by the amount they can formally work or have access to large amounts of empties through their job. Or, these people may value their time collecting empties differently than their time

[1] World Bank. 2008. "The Informal Recycling Sector in Developing Countries." https://documents1.worldbank.org/curated/en/227581468156575228/pdf/472210BRI0Box31ing1sectors01PUBLIC1.pdf.

[2] These statistics are from an informational interview with a redemption center owner and industry expert.

[3] Reloop. 2020. "Global Deposit Book 2020." https://www.reloopplatform.org/wp-content/uploads/2020/12/2020-Global-Deposit-Book-WEB-version-1DEC2020.pdf.

formally working. If a person receives positive utility from recycling, then they may recycle even if the recycling wage falls below their market wage (Ashenmiller 2009). Alternatively, if a person receives negative utility from recycling, then they may choose not to recycle even if the recycling wage exceeds their market wage. Survey evidence from redemption centers in California suggests that while a diverse group of people recycle for cash, redeemers' income distribution is significantly skewed left relative to the local population (Ashenmiller 2009, Ashenmiller 2011).

In this paper, I present suggestive evidence that informal recycling markets transfer wealth to low income communities. Today, we know little empirically about informal employment in recycling. Informal labor markets are difficult to study because there is often no data documenting the employment. Moreover, data documenting waste is also poor. In the case of BBs, there is little data documenting when and where deposits are paid and refunds are received. I use two strategies to extract information from this data sparse environment. First, I use BB implementations to proxy for shocks in the recycling wage (Ashenmiller 2010). Second, I employ the incidence of low birth weight (ILBW) to proxy for economic well-being in low income populations (Currie 2009, Almond, Hoynes and Schanzenbach 2011, Aizer, Hoynes and Lleras-Muney 2022). Together, these strategies allow me to test if an increase in the recycling wage improves economic well-being in low income populations.

In 1969 three Oregonian legislators tried to pass a bill banning the sale of beer in non-returnable containers. The legislation was struck down in the house on a 27-33 vote. In 1970, Washington tried to pass a BB. Polls suggested the bill would pass by a large margin. A publicity push by national beverage manufacturers killed the bill – 49% of the public voted for and 51% voted against the initiative. While the Oregon BB passed one year later, the legislation also faced intense lobbying.[4] After Oregon, ten other states proceeded to pass BBs starting with Vermont in 1972 and ending with Hawaii in 2002. Many states and the federal government attempted but failed to enact BBs during this period (Costle, Kreps, Andrus, Marshall, Blumenthal, Warren, Cutler, Cornell and Alm 1978). Of the states that successfully implemented BBs, most initially failed to enact the legislation (Davis 1982, Franchot 1978, Peterson 1976, Ross 1982, White 2018, Costle et al. 1978). Even if legislators' political preferences initially motivated BBs, most of these bills failed initially and were passed at a later date. Presumably this lag between bill initiation and enactment uncouples the timing of law introductions from confounders.

I exploit the idiosyncratic timing and location of BB implementations to identify the law's impact on the ILBW. Additionally, I employ treatment heterogeneity to provide suggestive evidence that wealth transfers play a role in the law's impact on the ILBW. In non-winter months, consumers produce more empties and empty collection is easier relative to winter months. Thus, through the wealth transfer channel BBs should induce the largest transfers in non-winter relative to winter months. Moreover, BBs should transfer the most wealth to the lowest wage earners, due to data constraints I use mother's education to proxy for wage. BBs are associated with a .6-3.7 percent reduction in the ILBW among mothers with less than a high school (low) education in non-winter relative to winter months. The association is strongest for low education mothers relative to mothers with more education. Lastly, after policy implementation, the association begins immediately and lasts for more than ten years. In New York, between 1983 and 2002, 30,000 low education mothers had live births annually and their ILBW was 10% in non-winter months. Thus, this paper suggests that BB wealth transfers positively impacted at least 57 New York residents annually on average.

[4] ODEQ. 2022. "Oregon's Evolving Bottle Bill." https://www.oregon.gov/deq/recycling/pages/bottle-bill.aspx.

First, my results extend the empirical literature documenting the progressive nature of BBs. Ashenmiller (2009, 2010, 2011) provide empirical evidence that BBs induce policy relevant wealth transfers through recycling. Specifically, Ashenmiller (2010) equates BBs to highly targeted earned income tax credits for very low wage earners and shows that BBs' wealth transfers reduce petty crime rates throughout the U.S. Ashenmiller (2011) finds 12% of households with an income less than $10,000 redeem empties for cash in Santa Barbara, California. This population received around 20% of all refunds in 2002, though the population only accounts for 1% of all Santa Barbara households. Moreover, recycling accounts for roughly 7% ($700) of these households' annual incomes. Extending these findings, I provide evidence that the wealth transfers documented in Santa Barbara, California by Ashenmiller (2009, 2011) are prevalent throughout the U.S. Moreover, this paper suggests that the wealth transfers documented in Ashenmiller (2010) have social welfare benefits beyond crime reduction. In short, this work contributes to a very small literature demonstrating that waste policy can have far reaching economic impacts.

Second, my analysis adds to the literature on U.S. welfare policy and the ILBW. As U.S. welfare program participants' income increases, the program's effect on the ILBW decreases (Hoynes, Page and Stevens 2011). For example, per dollar, WIC reduces the ILBW much more effectively than SNAP and EITC (Hoynes et al. 2011, Currie and Rajani 2015, Bitler and Currie 2005, Almond et al. 2011, Hoynes, Miller and Simon 2015). WIC, unlike EITC and SNAP, has no work or citizenship requirements and benefit access requires less documentation. Hence, WIC comes at lower administrative costs and likely reaches more marginalized populations. Obviously, EITC, WIC, and SNAP have very different goals. Nonetheless, another aspect of the differential between each program's effectiveness/$ may be that WIC is able to reach a more disadvantaged population than SNAP or EITC. BBs are not a social safety net policy, but the law's potential to transfer wealth to low wage earners lends itself to comparisons with some welfare programs. Similar to EITC and SNAP, BB wealth transfers are conditional on work. However, BB benefits require informal rather than formal labor. Moreover, BBs have no explicit eligibility requirements. Hence, benefits come at no administrative cost. A marginalized population unable to access EITC or SNAP may indeed be able to access BB wealth transfers, perhaps making BBs unexpectedly effective/$ at reducing the ILBW.

BBs reduce the ILBW among low education mothers as effectively as a $1,000 increase in after tax income from EITC. Hoynes, Miller and Simon (2015) finds a $1,000 increase in after tax income from EITC reduces the incidence of low birth weight by 1.6 to 2.9% among single mothers with a high school education or less. Initially, this similarity is surprising; BBs are not a welfare policy, yet they improve an important measure of welfare program efficacy as effectively as a staple welfare policy. Collecting two garbage bags of empties per week amounts to an annual recycling income of $1,040, roughly the $1,000 increase in after tax income from EITC found to improve the ILBW to a similar extent as BBs. New York State residents redeemed 5.1 billion containers in 2016, amounting to 25.5 million bags of empties and $255 million in refunds. In 2016, if 1 in every 100 New York resident redeemed empties, each returning equivalent amounts, then roughly 245,000 households would have earned $1,040 in refunds.[5] While EITC earnings from work far exceed aggregate potential BB wealth transfers, BBs may transfer relevant amounts to an entirely different subset of the population than that reached by EITC, likely those in incredible need. The comparability of EITC and BBs in their abilities to reduce the ILBW is plausible if the most desperate families participating in EITC drive

[5]Redemption rates are not constant across redeemers; more than 245,000 of the poorest and near poor households likely recycle for cash in New York State (Ashenmiller 2011). Some professional recyclers certainly earn far more than $1,040 per year and other households certainly earn far less.

associations between EITC and the ILBW.

The U.S. social safety net creates a patchwork of coverage through various programs such as Medicaid, SNAP, EITC, TANF, WIC, Head Start, and public housing. The safety net's architecture places a consequential administrative burden on participants (Currie 2006). Moreover, explicit and implicit requirements make take up of safety net programs low among needy families in the U.S. (Aizer et al. 2022). This paper's findings support the belief that the U.S. social safety net fails to reach many marginalized people, allowing for policies that impact the waste stream's value to have non-trivial welfare impacts.

2 Data

The analysis employs U.S. Vital Statistics Natality Data from 1969-2002, consisting of the full census of births from the National Center for Health Statistics (NCHS). I aggregate this data into county-education-season-year cells, employing data on birth month, year and weight as well as mother's education. I use Bailey, Clay, Fishback, Haines, Kantor, Severnini and Wentz (2018)'s infant health dataset to match NCHS county codes across years. This dataset does not include Alaska, so Alaska is excluded from the analysis. Additionally, I exclude all observations in which the mother's education is missing. Between 1979 and 1988, less than 2% of reported births in California and Texas included education information. Additionally, between 1978 and 1991, less than 3% of reported births in Washington included education information. The identification strategy should control for annual compositional changes in the data. Moreover, results are robust to removing Texas, California and Washington from the sample.

Identification relies on bottle bill (BB) introductions. Between 1969 and 2002, ten U.S. states implemented BBs. Figure A1 visualizes BB implementations in the U.S. during the sample period. In 1973, Oregon and Vermont implemented the first BBs. In 1978, Maine implemented a BB. Shortly after, Michigan and Iowa implemented in 1979. Then, Connecticut introduced the law in 1980. Three years later, in 1983, Delaware, Massachusetts and New York implemented bills. Lastly, California introduced the bill in 1987 (Ashenmiller 2010). Birth outcomes are assigned treatment based on when the pregnancy's third trimester began, as the third trimester is the most important in determining birth weight (Almond et al. 2011).

To isolate the effect of wealth transfers, I estimate treatment effect heterogeneity by season and education. In winter months beverage consumption decreases and empty collection is harder relative to non-winter months. Thus, in non-winter relative to winter months, BBs ought to induce larger wealth transfers, decreasing the ILBW more. I explore seasonal heterogeneity by assigning birth outcomes to seasons based on when the majority of a pregnancy's third trimester occurred. Babies born in January, February, March and April are designated as winter births. Babies born in all other months are designated as non-winter births. Moreover, as discussed in the introduction, lower wages earners are more likely than higher wage earners to receive BB wealth transfers, because lower wage earners have a relatively smaller opportunity cost. I use education to proxy for income because the natality data do not include information on wage. I explore educational heterogeneity by assigning birth outcomes to education groups using mother's education. I use three education groups: less than a high school (low) education, a high school (middle) education, and more than a high school (high) education.

Additionally, I include control variables covering income, government welfare spending, and weather. I use data from the Bureau of Economic Analysis (BEA), Regional Economic Information System for state level quarterly information on personal income, wages and salaries, farm wages and salaries, personal current transfer receipts, medicare benefits, state unemployment insurance compensation, and social security benefits. Throughout the analysis, I refer to these variables as *income* controls. The BEA only has sub-annual data during the sample period at the state level, given the analysis's focus on seasonality, I use this data despite its spatial coarseness. To assign BEA control variable values to birth outcomes, I find the variable averages during the third trimester. Weather controls were aggregated with population weights to the county by month level from ERA5-Land. Weather controls include cumulative exposure during the third trimester to cooling degree days (CDDs) and heating degree days (HDDs). The threshold used to construct both CDDs and HDDs is 20 degrees Celsius.

3 Empirical Strategy

3.1 Triple Difference

I use a triple difference (DiDiD) estimator to isolate the association between bottle bills (BBs) and the incidence of low birth weight (ILBW). Specifically, the estimator compares (i) the years pre and post-BBs, (ii) the ten states that implemented BBs to the forty states that did not, and (iii) winter to non-winter months, as BBs should transfer the most wealth during non-winter months. Lastly, I look at heterogeneity in this estimator by education, as BBs should transfer the most wealth to lower wage earners, due to data constraints education proxies for income.

This paper's main regression, a DiDiD estimator *with* heterogeneity by education, simultaneously tests whether BBs transfer wealth to relatively lower wage earners, evaluates if the wealth transfer is larger in non-winter relative to winter months and measures the effect of wealth transfers associated with BBs on the ILBW. Notably, results can only provide suggestive evidence that BBs transfer wealth to the lowest wage earners. In a first step, I describe the DiDiD estimator *without* heterogeneity. Then, I outline how the paper's main specification modifies this regression to isolate heterogeneity by education. The regression equation for the DiDiD estimator *without* heterogeneity is:

$$Y_{ctes} = \phi \left[\mathbb{1}(\text{BB})_{it} * \mathbb{1}(\text{not winter})_s\right] + \alpha_{i,s} + \delta_{i,t} + \gamma_{s,t} + \epsilon_{ctes} \tag{1}$$

Y_{ctes} is the ILBW in county c in year t for education group e in season s. $\mathbb{1}(\text{BB})_{it}$ is a dummy for states with active BBs. $\mathbb{1}(\text{not winter})_s$ is a dummy for non-winter months. To operationalize the DiDiD estimator, I include three sets of two way fixed effects: $\delta_{i,t}$, $\gamma_{s,t}$, and $\alpha_{i,s}$. $\delta_{i,t}$ is a state by year fixed effect, accounting for all factors common to a state in a given year. $\gamma_{s,t}$ is a season by year fixed effect, controlling for common factors in a season during a specific year. $\alpha_{i,s}$ is a state by season fixed effect; this fixed effect allows for differences between states for each season during the sample period.

The DiDiD estimator is ϕ. ϕ is associated with the interaction: $\mathbb{1}(\text{BB})_{it} * \mathbb{1}(\text{not winter})_s$. This interaction equals one for states with active BBs in non-winter months. ϕ captures variation in the ILBW specific to BB states, relative to non-BB states, when a BB was active relative to inactive, and in non-winter relative to winter months. Goodman-Bacon (2021) highlights potential concerns with this interpretation of a differences estimator. Given only ten units are treated and forty units are never treated, the issues raised by Goodman-Bacon (2021) are of little consequence because a very small weight is placed on concerning comparisons.

BBs are expected to transfer more wealth to less educated mothers relative to more educated mothers. To identify heterogeneity in the DiDiD estimator by education, I interact the estimator with both a dummy variable for the less than high school and high school education groups, allowing mothers with more than a high school education (high education) to serve as the base group. The regression equation for the DiDiD estimator *with* heterogeneity is:

$$\begin{aligned} Y_{ctes} = {} & \beta_1 \left[\mathbb{1}(\text{BB})_{it} * \mathbb{1}(\text{<HS})_e * \mathbb{1}(\text{not winter})_s\right] + \beta_2 \left[\mathbb{1}(\text{BB})_{it} * \mathbb{1}(\text{HS})_e * \mathbb{1}(\text{not winter})_s\right] \\ & + \theta_1 \left[\mathbb{1}(\text{BB})_{it} * \mathbb{1}(\text{<HS})_e\right] + \theta_2 \left[\mathbb{1}(\text{BB})_{it} * \mathbb{1}(\text{HS})_e\right] \\ & + \phi \left[\mathbb{1}(\text{BB})_{it} * \mathbb{1}(\text{not winter})_s\right] + \lambda_{e,s} + \alpha_{i,s} + \mu_{i,e} + \delta_{i,t} + \gamma_{s,t} + \epsilon_{ctes} \end{aligned} \tag{2}$$

$\mathbb{1}(\text{<HS})_e$ is a dummy for mothers with less than a high school education (low education). $\mathbb{1}(\text{HS})_e$ is a dummy for mothers with a high school education (middle education). The interaction introduces two additional sets of fixed effects to control for cross sectional differences across states and seasons for each education group. $\mu_{i,e}$ is a state by education group fixed effect; this fixed effect allows for differences between states for each education group during the sample period. Lastly, $\lambda_{e,s}$ is a season by education group fixed effect; this fixed effect controls for all factors common to an education group and season. Standard errors are clustered at the state level, the level of treatment assignment. The clustering is at the state rather than state-year level to account for serial correlation across years (Bertrand, Duflo and Mullainathan 2004). Additionally, I weight the regression by the number of births in each education x season x county x year cell. Lastly, I estimate regression equation (2) with no controls, weather controls, income controls, and both weather and income controls to test result robustness. Moreover, I employ a balance test to asses if any controls confound identification; specifically, I regress active BBs in non-winter months on the *income* and *weather* controls (see Section A.1).

The parameter of interest is $\beta_1 + \phi$. ϕ now captures the association between BB implementations and high education mothers' ILBW in non-winter versus winter months. β_1 (β_2) captures the difference between this association for low (middle) and high education mothers. Therefore, $\beta_1(\beta_2) + \phi$ captures the association between BB implementations and low (middle) education mothers' ILBW in non-winter versus winter months (see Section A.5). To identify the parameter of interest, $\beta_1 + \phi$, the following assumption must hold: the ILBW among low education mothers did not change differentially in BB and non-BB states, in winter versus non-winter months, during the sample period (Olden and Møen 2022). Differential trends among low education mothers in the pre-BB period would falsify this assumption. Moreover, trends among middle-high education mothers provide suggestive evidence in support or contradiction of this assumption. Before BB introductions, the parallel trends assumption should hold in BB and non-BB states for the non-winter relative to winter ILBW among middle-high education mothers. Pre-BB trends would raise concern that factors impacting middle-high education mothers pre-BBs also impacted low education mothers driving the paper's results. Additionally, after policy implementation, the pattern of reduction seen among low education mothers should not be seen among middle-high education mothers. If post-BB patterns are comparable among all education groups, then these trends suggest that BB wealth transfers did not cause the observed association between BBs and low education mothers' ILBW, as the association should be largest for low education mothers.

The DiDiD estimator with heterogeneity is the preferred estimation strategy in this paper. There are two obvious alternative estimators: a difference-in-differences (DiD) estimator and a quadruple difference

(DiDiDiD) estimator. The DiD estimator relies on two sources of variation: differences in pre- and post-BB years and in BB and non-BB states. The DiDiDiD estimator relies on four sources of variation: those used by the DiDiD estimator and differences between low and middle education mothers. I choose the DiDiD over the DiD estimator, due to concerns that the DiD estimator suffers from omitted variable bias to a greater extent than the DiDiD estimator (see section A.4). I choose the triple difference estimator with heterogeneity over the quadruple difference estimator, due to concerns that measurement error, from using education as a proxy for wage, attenuates the DiDiDiD estimator (see section A.3).[6] Notably, the quadruple difference estimates are neither statistically significant nor statistically different from the triple difference estimates with heterogeneity, consistent with concerns that measurement issues attenuate the estimator.

3.2 Event Study: Testing Identifying Assumptions

Separate measures of a BBs' effect in each year before and after implementation provide useful information for testing the identifying assumption stated in the previous section. Hence I estimate the following model:

$$\begin{aligned} Y_{ites} = & \sum_{j=-10}^{10} \phi_j \left[\mathbb{1}_{(t,i)\in j} * \mathbb{1}(\text{not winter})_s\right] + \sum_{e\in[l,m]} \theta_{j,e} \left[\mathbb{1}_{(t,i)\in j} * \mathbb{1}_e\right] + \eta_{j,e} \left[\mathbb{1}_{(t,i)\in j} * \mathbb{1}_e * \mathbb{1}(\text{not winter})_s\right] \\ & + \mu_{i,e} + \lambda_{e,s} + \alpha_{i,s} + \delta_{i,t} + \gamma_{s,t} + \epsilon_{ites} \end{aligned} \tag{3}$$

$\mathbb{1}_{(t,i)\in j}$ is a dummy for states that implemented a BB j years before or after year t. The excluded time category is j=-1. Never treated states and treated states in the year before treatment are assigned to this category. j=10 or -10 refers to the period 10+ years after or before a state implemented a BB. $\sum_{e\in[l,m]}$ sums over the low (l) and middle (m) education groups. I exclude the high education group.

From equation (3), I plot $\eta_{(j,e)} + \phi_j$ and ϕ_j in event study style figures to visually test the identifying assumption's validity. $\eta_{(j,\text{low})} + \phi_j$ represents the association between BB implementation j years after or before policy introduction and low education mothers' ILBW in non-winter relative to winter months.[7] The event study graph of $\eta_{(j,\text{low})} + \phi_j$, assesses whether there were pre-BB trends in the association. Pre-BB trends would violate the identifying assumption. Additionally, I formally test the pre-BB parallel trends assumption using placebo BB introductions. To conduct this exercise, I first subset the sample to all observations in BB states prior to policy implementation and in non-BB states. I then randomly assign BB implementation years to each BB state during each state's pre-treatment period. Lastly, I estimate the DiDiD estimator with heterogeneity using the subsetted sample and the placebo treatment years. I conduct the pre-treatment placebo test five times, with a different set of placebo treatment years assigned per test. If the parallel trends assumption is invalid, then I would expect to find statistically significant associations between placebo BB implementations and low education mothers' ILBW in non-winter relative to winter months.

Lastly, with ϕ_j and $\eta_{(j,\text{middle})} + \phi_j$, I assess for trends in pre- and post-BB trends among middle-high

[6] The quadruple difference estimator differences between two triple difference estimators, the triple difference estimator for low and middle education mothers, respectively (see section A.5). Due to poor measurement of the treated population, both estimators measure BBs' effect on a portion of the treated population. Therefore, the quadruple difference estimator measures the difference between two estimators that poorly measure the effect of interest, attenuating the estimates.

[7] ϕ_j represents the association between BB implementation j years after or before policy enactment and high education mothers' ILBW in non-winter relative to winter months. $\eta_{(j,\text{low})}$ represents the difference between this association for low and high education mothers. Thus, $\eta_{(j,\text{low})} + \phi_j$ represents the association just among low education mothers not relative to the association among high education mothers.

education mothers in BB states relative to non-BB states. ϕ_j ($\eta_{(j,\text{middle})} + \phi_j$) represents the association between BB implementation j years after or before policy enactment and the ILBW among high (middle) education mothers in non-winter relative to winter months. On average, I expect BBs to transfer the most wealth to the lowest wage earners. Therefore, proxying for income with education, I expect BBs to be associated with no reduction or smaller reductions in the ILBW among middle-high education mothers relative to low education mothers. Hence, event study style plots of ϕ_j and $\eta_{(j,\text{middle})} + \phi_j$ provide a useful placebo test. Pre-BB trends or post-bb patterns of reduction comparable to that of low education mothers would raise concern around the identification assumption's validity.

3.3 Permutation Test

In a final effort to falsify the paper's findings, I compute the probability that the paper's main result was observed by chance. Following Abadie, Diamond and Hainmueller (2010), I construct the main estimotor's distribution by randomly assigning observations to the treatment and control group. First, I subset the sample to observations from non-BB states. Then, I randomly select ten states to be placebo BB states and assign implementation years to each. Lastly, I estimate the DiDiD estimator with placebo BB states and implementation years using the subsetted sample. I conduct the permutation test 1,500 times; each iteration selects a different set of placebo BB states and assigns different placebo implementation years.

4 Empirical Results

BB introductions are associated with a .05-.33 percentage point (pp) reduction in low education mothers' ILBW in non-winter relative to winter months (see Table 1, Panel A, Row 1). This finding is robust to the inclusion of both weather and income controls, to county instead of state specific fixed effects and to a sample eliminating states with incomplete data. The reduction in low education mothers' ILBW is immediate and persists for more than ten years after BB implementation (see Figure 1, pink markers). Moreover, the permutation test reports a 5% chance of randomly observing this result (see Figure 2).[8] Before BB introductions, no systematic difference appears between BB and non-BB states in low education mothers' ILBW in non-winter relative to winter months (see Figure 1, pink markers). The point estimates in the pre-BB period are rarely statistically significant and oscillate around zero. This result fails to reject the identifying assumption and is robust to the inclusion of weather and income controls (see Figure A2). Moreover, the pre-treatment placebo exercise supports this visual finding (see Table 2).

Table 1, Panel A reports each education groups' association between BB implementations and their ILBW in non-winter relative to winter months, computed using estimates from the DiDiD with heterogeneity estimator (see equation 2); Panel B reports all estimates from this regression. Panel B, Row 3 reports high education mothers' association and is equivalent to Panel A, Row 3. Panel B, Row 1(2) reports the difference between the association for low (middle) and high education mothers. Panel A, Row 1(2) is the sum of Panel B Row 1(2) and 3. In both panels, Columns 1-4 sequentially add income and weather controls, scaling from no controls in Column 1 to all controls in Column 4. Column 5 replaces state with county specific fixed

[8]Figure 2 visualizes the distribution of the association between placebo BB introductions and low education mothers' ILBW in non-winter relative to winter months. The blue dotted line plots the actual point estimate, -.19 pp; 82 out of 1,500 placebo estimates are less than the observed reduction.

effects. Lastly, Column 6 estimates the model with a sample eliminating states with incomplete education data. Table 1 documents this paper's main findings and their robustness.

Figure 1 visualizes how each education groups' association between BB implementations and the ILBW in non-winter versus winter months evolved in every year before and after BB implementations. Figure 1 plots the association among low education mothers in pink, middle in purple, and high in green. Figure 1, pink markers provide a visual test for the identifying assumption – the ILBW among low education mothers did not change differentially in BB and non-BB states, in winter versus non-winter months. Moreover, trends among high and middle education mothers in BB and non-BB states provide useful placebo tests. Differential trends pre-BB or post-BB patterns of reduction comparable to that of low education mothers would raise concern around the identification assumption's validity.

Before BB implementation, no systematic difference appears between BB and non-BB states among middle (high) education mothers' ILBW in non-winter versus winter months; pre-BB point estimates are rarely statistically significant and oscillate around zero (see Figure 1 purple (green) markers). Just after BB introductions, point estimates continue to oscillate around zero. Six years after implementation, point estimates increase slightly. Table 1, Panel A, Row 2(3) summarizes the increases in middle (high) education mothers' ILBW. BBs are on average not associated with middle education mother's ILBW in non-winter relative to winter months; the association is slightly positive though statistically insignificant. BBs are on average associated with a .1 pp increase in high education mothers' ILBW in non-winter relative to winter months. This relationship attenuates and becomes statistically insignificant once states with extensive missing data are eliminated from the sample. In conclusion, these results fail to challenge the identifying assumption's validity. For example, if an increase in air-conditioner penetration explains the paper's findings, patterns of reduction observed among low education mothers likely would exist among middle-high education mothers either pre or post-BBs. These patterns of reduction do not exist, thus the results fail to provide evidence that an increase in air-conditioner penetration in BB states, coinciding with BB implementations, explains the paper's main findings.

Table 2 presents the pre-treatment placebo estimates – the association between BB introductions and low education mothers' ILBW in non-winter relative to winter months, computed using only untreated observations and placebo BB implementations assigned during the pre-treatment period. Each column differs in the random assignment of treatment years to BB states. The exercise finds no statistically significant association, failing to reject the parallel trends assumption.

Finally, the balance test examines if potential confounders (income and weather controls) are associated with BB introductions in non-winter months, testing the identifying assumption's plausibility. If seasonal differences systematically varied across BB and non-BB states between the pre- and post-BB periods, then the identifying assumption would be compromised. For example, weather varies seasonally and affects the ILBW (Deschenes, Greenstone and Guryan 2009). If weather patterns systematically varied across BB and non-BB states between the pre- and post-BB periods in non-winter relative to winter months, then this paper's findings could reflect differences in weather patterns across space and time rather than BBs' impact. Table 3 displays the balance test results; point estimates are small, as the test regresses large covariates on a dummy variable. Only the association between BBs in non-winter months with farm salaries and wages is statistically significant. The balance test suggests seasonal differences in payments to hired labor on farms varied systematically across treated and untreated units, rejecting the identifying assumption and highlighting the importance of interpreting this paper's findings with caution.

5 Other Potential Mechanisms

BBs potentially impacted air quality, beverage prices and employment (Costle et al. 1978). Changes in each could all explain an association between BBs and the ILBW. BB introductions increased beer and soda's cost, decreasing overall consumption initially, though sales recovered after a couple years (Loube 1975, Wagenbach 1985). This reduction possibly improved birth outcomes, leading to an association between BBs and the ILBW. Seasonal associations from this mechanism should have dissipated after 1-3 years. This paper documents an association that lasts for more than ten years, suggesting changes in beverage prices do not fully explain the documented relationship. Additionally, BBs created a marginal number of non-seasonal jobs (Loube 1975). Higher employment increases spending on normal goods including maternal health, consequently improving birth outcomes. The formal employment mechanism was not seasonal, hence it does not explain the seasonal differential in the association between BBs and the ILBW documented in this paper. Lastly, BBs may have affected air pollution concentrations. Chay and Greenstone (2003) find little association between reductions in TSP and the ILBW in the U.S. during the early 1980s. Thus, any variation in air pollution associated with BB implementations likely did not cause the observed reductions in the ILBW. In conclusion, while BBs impacted other factors influencing birth outcomes, these factors cannot fully explain the association documented in this paper.

6 Conclusion

This paper provides evidence that there were gaps in U.S. welfare policy that the waste stream filled. Specifically, bottle bills (BBs) created comparable reductions in the incidence of low birth weight (ILBW) to that of EITC (Hoynes et al. 2015). BBs enlarge waste stream wealth transfers by increasing used material's price and decreasing transaction costs. Hence, BBs expand the aggregate amount that can be earned through recycling, potentially leading to highly targeted wealth transfers to especially low income households (Ashenmiller 2010). Ashenmiller (2009, 2011) uses survey evidence from Santa Barbara, California to demonstrate that BBs transfer wealth. Ashenmiller (2010) leverages BBs' idiosyncratic introductions to show that the programs reduce petty crime rates. Extending this work, this paper examines BBs' impact on the ILBW, a measure of economic well-being and an important determinant of a child's welfare (Almond, Currie and Duque 2018).

I use a triple difference estimator to identify BBs' impact on the ILBW plausibly through the wealth transfer channel. The estimator employs comparisons between: (1) the years pre- and post BB introductions, (2) BB and non-BB states, and (3) winter and non-winter months. The estimator relies on the idiosyncratic timing and location of BB implementations for identification and leverages seasonality in the recycling wage to isolate BB wealth transfers' effect. Lastly, I test for treatment effect heterogeneity by education to provide further suggestive evidence that wealth transfers play a role in the law's impact on the ILBW. BBs should transfer the most wealth to the lowest wage earners, due to data constraints I proxy for wage with education. I find that BBs reduce the ILBW .6-3.7% among mothers with less than a high school education. The reduction is immediate and lasts for more than ten years. Moreover, I do not find that BBs reduce the

ILBW among mothers with at least a high school education, supporting the hypothesis that BBs' estimated effect acts through the wealth transfer channel.

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Table 1: Estimated Impact of Bottle Bills on the Incidence of Low Birth Weight

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Bottle Bill's Impact on the Incidence of Low Birth Weight by Education						
Low Education	-0.214	-0.208	-0.195	-0.191	-0.215	-0.161
	(0.048)	(0.050)	(0.052)	(0.053)	(0.053)	(0.036)
Middle Education	0.073	0.077	0.090	0.093	0.065	0.059
	(0.038)	(0.041)	(0.047)	(0.049)	(0.042)	(0.047)
High Education	0.113	0.115	0.132	0.132	0.100	0.090
	(0.052)	(0.055)	(0.060)	(0.061)	(0.056)	(0.063)
Panel B: DiDiD with Heterogeneity by Education Estimates						
$\mathbb{1}(\text{BB}) * \mathbb{1}(<\text{HS}) * \mathbb{1}(\text{not winter})$	-0.327	-0.323	-0.327	-0.323	-0.315	-0.251
	(0.060)	(0.059)	(0.060)	(0.059)	(0.063)	(0.065)
$\mathbb{1}(\text{BB}) * \mathbb{1}(\text{HS}) * \mathbb{1}(\text{not winter})$	-0.041	-0.038	-0.042	-0.039	-0.036	-0.031
	(0.032)	(0.033)	(0.033)	(0.033)	(0.031)	(0.041)
$\mathbb{1}(\text{BB}) * \mathbb{1}(\text{not winter})$	0.113	0.115	0.132	0.132	0.100	0.090
	(0.052)	(0.055)	(0.060)	(0.061)	(0.056)	(0.063)
$\mathbb{1}(\text{BB}) * \mathbb{1}(<\text{HS})$	-0.273	-0.276	-0.275	-0.278	-0.383	-0.332
	(0.329)	(0.329)	(0.330)	(0.330)	(0.228)	(0.248)
$\mathbb{1}(\text{BB}) * \mathbb{1}(\text{HS})$	0.158	0.156	0.158	0.156	0.106	0.172
	(0.117)	(0.118)	(0.117)	(0.118)	(0.108)	(0.104)
Observations	2443332	2443332	2443332	2443332	2441210	2369253
within R-squared	.0002	.0005	.0002	.0005	.0006	.0006
State x Season Fixed Effect	x	x	x	x		
County x Season Fixed Effect					x	x
State x Educ. Fixed Effect	x	x	x	x		
County x Educ. Fixed Effect					x	x
State x Year Fixed Effect	x	x	x	x		
County x Year Fixed Effect					x	x
Season x Year Fixed Effect	x	x	x	x	x	x
Educ. x Season Fixed Effect	x	x	x	x	x	x
Weather Controls		x		x	x	x
Income Controls			x	x	x	x
Limited Sample						x

Notes: Panel A of Table 1 reports the association between bottle bills and the incidence of low birth weight in non-winter relative to winter months for each education group. Sub-population associations are computed using estimates from equation (2). All estimates from equation (2) can be found in Panel B. Specifically, the reported association in Panel A's first row is the sum of ϕ and β_1 from equation (2) or the first and third row of Panel B. The reported association in Panel A's second row is the sum of ϕ and β_2 from equation (2) or the second and third row of Panel B. The reported association in Panel A's third row is simply ϕ from equation (2) or the third row of Panel B. All reported parameters/estimates in a given column are from the same regression. Columns 1-4 sequentially add income and weather controls, scaling from no controls in Column 1 to both income and weather controls in Column 4. Column 5 replaces state specific fixed effects with county specific fixed effects. Lastly, Column 6 estimates the model with a data sample eliminating California, Texas, and Washington, states with extensive amounts of missing data. As discussed in the data section, between 1979 and 1988, California, Texas and Washington did not include data on mother's education for most observations. Each regression is weighted by the number of births in each education x season x county x year cell. The regressions are run at the county level to allow for county specific controls. The reported standard errors are clustered at the state level, i.e. the level of treatment, to allow for arbitrary dependence of ϵ_{ctes} across years within any given state.

Table 2: Pre-Treatment Placebo Test

	(1)	(2)	(3)	(4)	(5)
Low Education: $\mathbb{1}(\text{BB}) * \mathbb{1}(\text{not winter})$	-0.125	-0.060	-0.063	-0.032	-0.006
	(0.152)	(0.132)	(0.074)	(0.056)	(0.144)
Observations	2230300	2230300	2230300	2230300	2230300
within R-squared	0	0	0	0	0

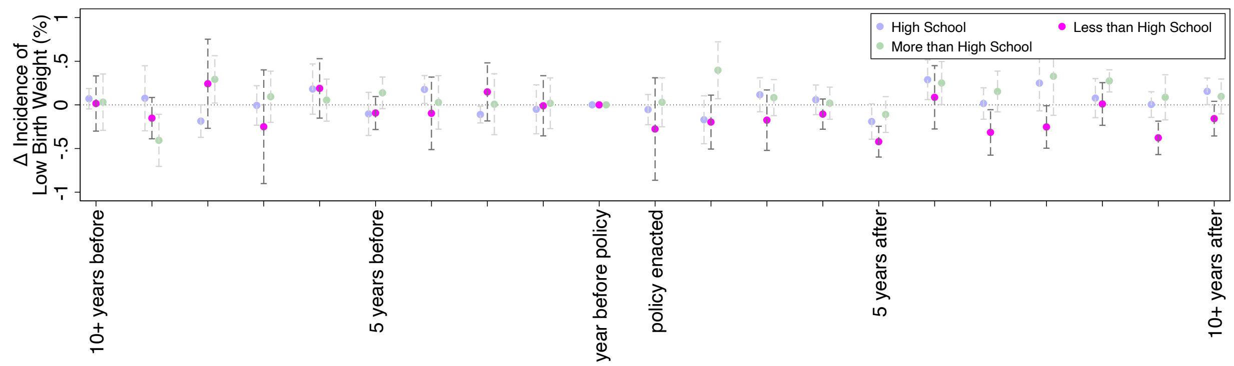
Notes: Table 2 reports pre-treatment placebo test associations between placebo bottle bill implementations in bottle bill states and the incidence of low birth weight in non-winter relative to winter months among low education mothers. Estimates are computed using equation (2) with income and weather controls on the sample of untreated observations. Columns differ in the random assignment of placebo treatment years i.e. a different set of placebo bottle bill implementation years are assigned to bottle bill states in each column. Each regression is weighted by the number of births in each education x season x county x year cell. The regressions are run at the county level to allow for county specific controls. The reported standard errors are clustered at the state level.

Table 3: Balance Test – Association between Potential Confounders and Active Bottle Bills in Non-Winter Months

	$\mathbb{1}(\text{BB}) * \mathbb{1}(\text{not winter})$
Weather (Cumulative °C)	
HDDs	0.000000203866
	(0.000001399792)
CDDs	0.000002302819
	(0.000003387573)
Income and Government Spending (Millions $2015)	
Personal Income	0.000010633737
	(0.000010665776)
Personal Current Transfer Reciepts	0.000005409612
	(0.000006988002)
State Unemployment Insurance Benefits	-0.000070316286
	(0.000080733917)
Medicare Benefits	-0.000111993138
	(0.000086417278)
Social Security Benefits	0.000061670310
	(0.000068007091)
Farm Wages and Salaries	0.000805451270
	(0.000312681951)
Wages and Salaries	-0.000027501019
	(0.000020945728)
Observations	2443332
within R-squared	.0029

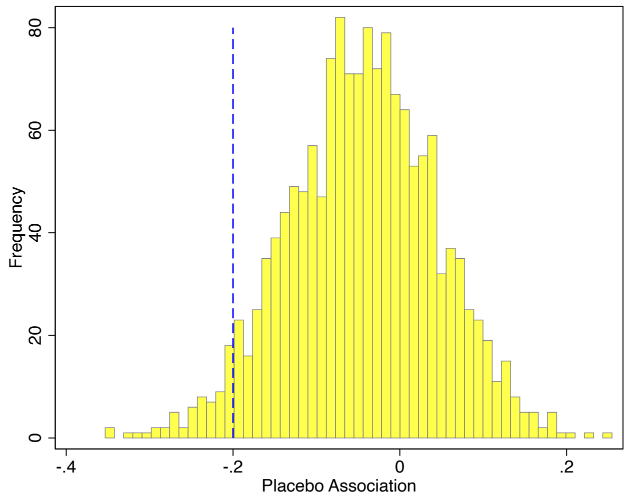
Notes: Table 3 displays the association between potential confounders and active bottle bills in non-winter months, the estimates from equation A4. Each row provides the association between a different confounder and the causal variable of interest. Point estimates are small, as the test regresses large covariates on the interaction of two dummy variables.. The balance test controls for state x season, season x year, and state x year fixed effects. The regression is weighted by the number of births in each education x season x county x year cell. The reported standard errors are clustered at the state level.

Figure 1: Association between Bottle Bills Before and After Implementation with the Incidence of Low Birth Weight by Education



Notes: Figure 1 plots the association between bottle bill implementations and the incidence of low birth weight in non-winter relative to winter months by education group in each year before and after the policy took effect. Each marker color corresponds to a different education group's association. The association is visualized among mothers with less than a high school education in pink, mothers with a high school education in purple, and mothers with more than a high school education in green. All associations are computed using estimates from equation (3), as described in Section 3.2. The association in the year prior to policy implementation is normalized to zero. Dashed lines represent 95% confidence intervals constructed using standard errors clustered at the state level. Additionally, the regression is weighted by the number of births in each education group by season by year by county cell.

Figure 2: Permutation Test



Notes: Figure 2 plots the distribution of the association between placebo bottle bills and the incidence of low birth weight among low education mothers in non-winter relative to winter months. The distribution is generated by estimating equation (2) on 1,500 different random assignments of bottle bill implementation years to ten randomly selected non-bottle bill states using the sample of observations from non-bottle bill states. The blue dashed line displays the actual association between bottle bill implementations and low education mothers' incidence of low birth weight in non-winter relative to winter months. Notably, only 82 out of 1,500 permutations estimate associations less than the actual association.

A Appendix

A.1 Balance Test

Omitted variable bias is the biggest concern with respect to a causal interpretation of the results emphasized in this paper. Researchers test for omitted variable bias in two ways, either with a balance or coefficient comparison test. In a balance test, researchers regress potential confounders on the causal variable of interest. If randomization or quasi-randomization is successful, then potential confounders and the causal variable of interest are not correlated. In a coefficient comparison test, researchers include potential confounders in a regression of the outcome variable on the causal variable. If the causal variable is in fact causal, then the inclusion of additional controls should not meaningfully change the causal estimate.

In the presence of measurement error, the balance test is superior to the coefficient comparison test (Pei, Pischke and Schwandt 2019). Moreover, the conclusion from a balance test can be recovered by regressing the causal variable of interest on confounders in a case with multiple confounding variables. In the context of this study, the causal variable of interest is the implementation of bottle bills in the U.S. in non-winter months conditional on fixed effects. Thus, I test for omitted variable bias by regressing the implementation of bottle bills in the U.S. in non winter months on a rich set of potential confounders. More specifically, I estimate the following regression:

$$\mathbb{1}(\text{BB})_{it} * \mathbb{1}(\text{not winter})_s = \sum_v \beta_v * v + \alpha_{i,s} + \delta_{i,t} + \gamma_{s,t} + \epsilon_{i,s,t} \tag{A4}$$

$\mathbb{1}(\text{BB})_{it} * \mathbb{1}(\text{not winter})_s$ is a dummy for states with active bottle bills in non-winter months. v is a potential confounder and $\sum_v$ sums over all controls included in previous robustness analyses.[9] $\delta_{i,t}$ is a state by year fixed effect, accounting for all factors common to a state in a given year t. $\gamma_{s,t}$ is a season by year fixed effect, controlling for common factors in a given season during a specific year. $\alpha_{i,s}$ is a state by season fixed effect; this fixed effect allows for season by state differences between states that implement and never enact bottle bills during the sample period.

A.2 Another Event Study to Test Identifying Assumptions

As previously discussed, separate measures of a bottle bill's effect in each year before and after implementation provide useful information for testing one of the paper's main identifying assumptions – the incidence of low

[9]Controls include: personal income, wages and salaries, farm wages and salaries, personal current transfer receipts, medicare benefits, state unemployment insurance compensation, and social security benefits, cumulative cooling and heating degrees.

birth weight among mothers with less than a high school education did not change differentially in bottle bill states versus non-bottle bill states, in winter versus non-winter months, during the sample period. In the main text, I produce event study style graphs visualizing the association between bottle bill implementation and the incidence of low birth weight in non-winter relative to winter months for each education group in every year before and after the policy took effect (see Figure 1). For completeness, I also explore the association on average for all mothers. Specifically, I estimate the following model:

$$Y_{ites} = \sum_{j=-10}^{10} \left[\kappa_j * \mathbb{1}_{(t,i)\in j} * \mathbb{1}(\text{not winter})_s\right] + \alpha_{i,s} + \mu_{i,e} + \delta_{i,t} + \gamma_{s,t} + \lambda_{e,s} + \epsilon_{ites} \tag{A5}$$

$\mathbb{1}_{(t,i)\in j} = 1$ for state i in year t if state i implements a bottle bill j years before or after year t. The excluded time category is j=-1. Never treated states and treated states in the year before treatment are assigned to this category. j=10 or -10 refers to the period 10+ years after or before a state implemented a bottle bill. The regression is weighted by the number of births in each education x season x county x year cell.

From equation (A5), I plot κ_j in event study style figures to visually test the validity of the identifying assumption. With κ_j, I am able to assess whether there were pre and post-bottle bill trends in the estimator of interest among all mothers on average. κ_j represents the association between bottle bill implementation j years after or before policy enactment and the incidence of low birth weight among all mothers in non-winter relative to winter months.[10]

Figure A3, the event study style graph of κ_j, provides graphical evidence that the non-winter incidence of low birth weight relative to the winter rate evolved in parallel on average in bottle bill and non-bottle bill states before and after policy implementation. I expect a very small population of mothers to be impacted by bottle bills as wealth transfers associated with bottle bills rely on the majority of the population not recycling for cash. Therefore, Figure A3 both fails to reject the identifying assumption's validity and supports the hypothesis that bottle bills on average do not impact the incidence of low birth weight in non-winter versus winter months.

A.3 Alternative Estimator: Quadruple Difference

The DiDiDiD estimator exploits four sources of variation. First, I compare the years before and after bottle bills are implemented in ten states. Second, during the sample period, I compare the ten states that implemented bottle bills to the forty states that did not. Third, I compare winter to non-winter months.

[10]Notably, κ_j is a weighted average of $\eta_{(j,\text{low})} + \phi_j$, $\eta_{(j,\text{middle})} + \phi_j$, and ϕ_j.

Fourth, I compare low education mothers to middle education mothers. I choose to use the middle education group as the control group in the DiDiDiD estimator because the DiDiD analysis suggests middle education mothers' incidence of low birth weight in non-winter relative to winter months has a smaller association with bottle bill implementations than that of high education mothers (see Table 1).

Specifically, I estimate the following model:

$$\begin{aligned} Y_{ctes} = \beta_1 & \left[\mathbb{1}(\text{BB})_{it} * \mathbb{1}(<\text{HS})_e * \mathbb{1}(\text{not winter})_s\right] \\ & + \beta_2 \left[\mathbb{1}(\text{BB})_{it} * \mathbb{1}(>\text{HS})_e * \mathbb{1}(\text{not winter})_s\right] \\ & + \iota_{i,e,s} + \pi_{i,s,t} + \omega_{i,e,t} + \nu_{s,e,t} + \epsilon_{ctes} \end{aligned} \tag{A6}$$

Y_{ctes} is the incidence of low birth weight in county c in year t for education group e in season s. $\mathbb{1}(\text{BB})_{it} = 1$ if state i has an active deposit refund program in year t. $\mathbb{1}(\text{not winter})_s = 1$ if season s is not winter. $\mathbb{1}(<\text{HS})_e = 1$ if education group e is less than high school (low education). $\mathbb{1}(>\text{HS})_e = 1$ if education group e is more than high school (high education).

To operationalize the DiDiDiD estimator, I include four sets of three way fixed effects: $\iota_{i,e,s}$, $\pi_{i,s,t}$, $\omega_{i,e,t}$, and $\nu_{s,e,t}$. $\pi_{i,s,t}$ is a state by season by year fixed effect, accounting for all factors common to state i in season s in year t. $\omega_{i,e,t}$ is a state by education group by year fixed effect, accounting for all factors common to state i in education group e in year t. $\nu_{s,e,t}$ is a season by education group by year fixed effect, accounting for all factors common to season s in education group e in year t. Lastly, $\iota_{i,e,s}$ is a state by season by education group fixed effect; this fixed effect allows for differences between states for each season and each education group during the sample period.

The parameter of interest is β_1. β_1 is associated with the interaction: $\mathbb{1}(\text{BB})_{it} * \mathbb{1}(<\text{HS})_e * \mathbb{1}(\text{not winter})_s$. As previously discussed, this interaction of dummy variables takes the value of one for all states with active bottle bills for the low education group in non-winter months. After adjustments for fixed effects, β_1 captures variation in the incidence of low birth weight among low education mothers, relative to middle education mothers, in bottle bill states, relative to non-bottle bill states, in years when the bottle bill was in effect, relative to before implementation, and in non-winter months, relative to winter. The identifying assumption is that the incidence of low birth weight among mothers with less than a high school education relative to mothers with a high school education did not change differentially in bottle bill states versus non-bottle bill states, in winter versus non-winter months, during the sample period.

Notably, the DiDiDiD estimator nonparametrically controls for much more within state within year variation than the DiDiD estimator, making the DiDiDiD estimator relatively more robust to omitted variable bias. In each year, the DiDiDiD estimator accounts for factors common to a state and season, a state and education group, as well as a season and education group. In contrast, in each year, the DiDiD estimator only accounts for factors common to a state and factors common to a season. See section A.5 for a longer discussion comparing the DiDiDiD and DiDiD estimators.

During the sample period, trends in the incidence of low birth weight varied substantially by education group and season. The DiDiDiD estimator is able to non-parametrically control for variation in these subgroup trends at the state level. That said, concerns of omitted variable bias associated with the DiDiD estimator are not fully alleviated by the DiDiDiD estimator. For example, if non-winter work opportunities improved for only low education laborers differentially in bottle bill states relative to non-bottle bill states after policy implementation through a channel other than the bottle bill, then both the DiDiD estimator and the DiDiDiD estimator would suffer from omitted variable bias. The balance test conducted in the main text and described in depth in section A.1 explores concerns of omitted variable bias through this channel.

As just discussed, the DiDiDiD estimator does not alleviate the most prominent omitted variable bias concerns associated with the DiDiD estimator. Moreover, the difference between education groups, made to construct the quadruple difference estimator, likely further attenuates the estimated treatment effect and increases the estimator's variance. The DiDiD estimator differences between two groups that are treated unknown extents by bottle bills: non-winter and winter months. Anecdotal evidence suggests wealth transfers in non-winter months are twice the size of wealth transfers in winter months. Due to data constraints during the sample period, I cannot quantitatively verify this observation. To reduce the threat of omitted variable bias, I difference across seasons, acknowledging that this difference attenuates the treatment effect estimate by differencing between two treated groups. The seasonal difference allows me to compare a given education group to itself in a given year and state.

The DiDiDiD estimator builds off the DiDiD estimator by differencing between two additional groups that are treated unknown extents by bottle bills: low and middle education mothers. As discussed previously, I am not able to observe the exact population recycling for cash. The simple economic model sketched in the introduction and empirical evidence from the literature suggest on average participation among lower wage individuals is *much* higher than that of higher wage individuals. The birth weight data does not include mother's wage or household income. Therefore, I use mother's education to proxy for income. Measurement error in the use of educational attainment as a proxy for household income and household income as a proxy for populations that recycle for cash have the potential to attenuate the DiDiDiD estimator and increase its variance. The quadruple difference estimator differences between two triple difference estimators, the triple difference estimator for low and middle education mothers, respectively (see section A.5). Due to poor measurement of the population of people actually treated by the policy, both estimators likely measure the effect of bottle bills on some portion of the treated population in non-winter versus winter months. Therefore, the quadruple difference estimator measures the difference between two estimators that poorly measure the effect of interest, attenuating estimates.

With this context, its unsurprising that the DiDiDiD estimates are slightly smaller and noisier than the DiDiD with heterogeneity estimates (see Table A1), measurement error likely attenuates the estimator and increases its noise. The DiDiDiD estimates report that bottle bills are associated with a .15 pp reduction in the incidence of low birth weight among low education mothers relative to middle education mothers in non-winter months relative to winter months. The estimate is statistically significant at the 90% confidence level. Moreover, the DiDiDiD estimate is not statistically different from the DiDiD parameter of interest.

A.4 Alternative Estimator: Difference-in-Differences

The DiD estimator exploits two sources of variation. First, I compare the years before and after bottle bills are implemented in ten states. Second, during the sample period, I compare the ten states that implemented bottle bills to the forty states that did not. As with the DiDiD estimator, I look at heterogeneity in this estimator by education group. I also look at heterogeneity in this estimator by season. Notably, the DiD estimator only accounts for factors common to a given year. The DiDiD estimator accounts for all factors common to a state in a given year and all factors common to each season in a given year. To limit omitted variable bias, this paper's main results rely on a DiDiD estimator as the identification strategy employs within state within year variation. For completeness, I define and estimate a DiD estimator as well. Specifically, I estimate the regression equation:

$$Y_{ctes} = \beta * \mathbb{1}(\text{BB})_{it} + \alpha_{i,s} + \mu_{i,e} + \gamma_t + \epsilon_{ctes} \tag{A7}$$

As before, $\mathbb{1}(\text{BB})_{it} = 1$ if a bottle bill is implemented in state i during year t. Additionally, Y_{ctes} is the incidence of low birth weight in county c in year t for education group e in season s. I include state x season, state x education group, and year fixed effects. These fixed effects operationalize β as a DiD estimator. This specification estimates the difference in the incidence of low birth weight between a world with bottle bills versus a world without bottle bills. The identifying assumption is that the incidence of low birth weight did not differentially change in bottle bill states versus non bottle bill states through a channel other than bottle bills during the sample period. The parameter of interest is β. After adjusting for fixed effects, β captures the variation in the incidence of low birth weight for bottle bill states relative to non bottle bill states, in years when bottle bills operated in treated states, relative to before bottle bill implementations.

As with the DiDiD estimator, I also explore heterogeneity in the DiD estimator by season and education group. Specifically, I estimate the following two models:

$$\begin{aligned} Y_{ctes} &= \beta_1 * \mathbb{1}(\text{BB})_{it} + \beta_2 \left(\mathbb{1}(\text{not winter})_s * \mathbb{1}(\text{BB})_{it}\right) \\ &\quad + \alpha_{i,s} + \mu_{i,e} + \gamma_t + \epsilon_{ites} \qquad \text{(A8)} \\ Y_{ites} &= \beta_1 * \mathbb{1}(\text{BB})_{it} + \beta_2 \left(\mathbb{1}(\text{HS})_e * \mathbb{1}(\text{BB})_{it}\right) \\ &\quad + \beta_3 \left(\mathbb{1}(<\text{HS})_e * \mathbb{1}(\text{BB})_{it}\right) + \alpha_{i,s} + \mu_{i,e} + \gamma_t + \epsilon_{ctes} \qquad \text{(A9)} \end{aligned}$$

In equation (A8), β_1 captures the variation in the incidence of low birth weight in winter months for bottle bill states relative to non-bottle bill states, in years when bottle bills operated in treated states, relative to before bottle bill implementations. β_2 captures the difference in variation in the incidence of low birth weight between non-winter months and winter months for bottle bill states relative to non bottle bill states, in years when bottle bills operated in treated states, relative to before bottle bill implementations. In equation (A9), β_1 captures the variation in high education mothers' incidence of low birth weight for bottle bill states relative to non-bottle bill states, in years when bottle bills operated in treated states, relative to before bottle bill implementations. β_2 and β_3 capture additional variation in low and middle education mothers' incidence of low birth weight relative to high education mothers' for bottle bill states relative to non bottle bill states, in years when bottle bills operated in treated states, relative to before bottle bill implementations.

Results for estimation equations (A7,A8,A9) can be found in Table A2. Without controls, bottle bills are associated with a .35 pp reduction in the incidence of low birth weight among all mothers. This association attenuates to .16 pp, when income and weather are added. Notably, the DiDiD results are much more robust than the DiD results to the introduction of controls.

As discussed previously, bottle bills are only expected to transfer wealth to and thus improve birth outcomes among the lowest wage earners. The DiD estimates find significant associations between bottle bill implementations and the incidence of low birth weight among all mothers. Moreover, the association is more pronounced in non-winter months relative to winter months among the entire population. Through a wealth transfers channel, this finding is surprising, suggesting the DiD estimator either suffers from omitted variable bias or bottle bills are acting on the incidence of low birth weight through a channel other than wealth transfers. The sensitivity of DiD estimates to the inclusion of controls suggests the DiD estimates certainly suffer from omitted variable bias.

Importantly, the DiDiD estimator alleviates some of the concern around omitted variable bias that arises from the DiD estimates. Moreover, the DiDiD estimator does not find any association between bottle bill implementations and the incidence of low birth weight in non-winter relative to winter months among all mothers (see Figure A3).

A.5 The Difference between the DiDiD Estimator with Heterogeneity by Education Group and the DiDiDiD Estimator

In the main text, I describe both a DiDiD estimator and a DiDiD estimator with heterogeneity by education group. In the appendix, I also describe a DiDiDiD estimator that augments the DiDiD estimator with a difference between education groups. The regression equations associated with each estimator are clearly different. In this section, I further outline the differences between each estimator in a simpler context to help build intuition around the estimators employed in the main text. Intuition from this simpler framework can be directly applied to the estimators described in the main text. To begin, I define the triple difference estimator in terms of sample means with the framework laid out by Olden and Møen (2022). After defining the DiDiD estimator, I describe the other two estimators using this same framework. I detail the quadruple difference estimator before the triple difference estimator with heterogeneity by education group to help illustrate how the latter estimator exists between the DiDiD and DiDiDiD estimators.

DiDiD For the sake of exposition, assume a set of treated states (T) introduce bottle bills in the same year, while a set of control states (C) do not. Further, only the universe of births in non-winter months (N) are affected by bottle bills; winter births (W) are unaffected. Moreover, the population of each state can be subdivided into two groups, group L and group H. Bottle bills only affect group L, the low education population, i.e. group L is the group that can benefit from the policy. Lastly, there are two time periods pre- and post-bottle bill implementation.

To establish a counterfactual, I have a number of options. (1) I could compare group N and W within the treatment states. This approach would be invalid if bottle bills have within-state spillovers. Moreover, this approach would be invalid if trends in group N and W vary, regardless of the bottle bill. (2) Another option is to compare group N in the treatment states with group N in the control states. This approach would be invalid if the outcomes for group N in treatment and control states trended differently, regardless of the bottle bill. (3) Alternatively, I could compare the difference between group N and W outcomes in treatment states with the difference in control states. This approach would be valid if the general economic differences between treated and control states did not affect relative outcomes of group N and W. In that case, I could use the relative difference to estimate what would have happened to the relative outcomes of group N and W in the treated states in the absence of treatment (Olden and Møen 2022). In the context of the exposition above, I estimate the following regression equation to identify the effect of bottle bills.

$$\begin{aligned} Y_{ist} = \beta_0 + \beta_1 T + \beta_2 N + \beta_3 \text{Post} + \beta_4 T * N + \beta_5 T * \text{Post} + \beta_6 N * \text{Post} \\ + \beta_7 T * N * \text{Post} + \epsilon_{ist} \end{aligned} \tag{A10}$$

Y_{ist} is the outcome in state i in season s in year t. All variables from the basic setup outlined above are dummy variables. The conditional mean function of equation (A10) is $\mathbb{E}[Y_{ist}|T, N, \text{Post}]$. Under standard OLS assumptions and an additive effect, I can use $\mathbb{E}[\epsilon_{ist}|T, N, \text{Post}] = 0$ to show all eight expected values of $\mathbb{E}[Y_{ist}|T, N, \text{Post}]$ (Olden and Møen 2022). This is the next step in defining the triple difference estimator in terms of sample means.

$$\begin{aligned}
&\mathbb{E}[Y|T=0, N=0, \text{Post}=0] = \beta_0 \\
&\mathbb{E}[Y|T=1, N=0, \text{Post}=0] = \beta_0 + \beta_1 \\
&\mathbb{E}[Y|T=0, N=1, \text{Post}=0] = \beta_0 + \beta_2 \\
&\mathbb{E}[Y|T=0, N=0, \text{Post}=1] = \beta_0 + \beta_3 \\
&\mathbb{E}[Y|T=1, N=1, \text{Post}=0] = \beta_0 + \beta_1 + \beta_2 + \beta_4 \\
&\mathbb{E}[Y|T=1, N=0, \text{Post}=1] = \beta_0 + \beta_1 + \beta_3 + \beta_5 \\
&\mathbb{E}[Y|T=0, N=1, \text{Post}=1] = \beta_0 + \beta_2 + \beta_3 + \beta_6 \\
&\mathbb{E}[Y|T=1, N=1, \text{Post}=1] = \beta_0 + \beta_1 + \beta_2 + \beta_3 + \beta_4 + \beta_5 + \beta_6 + \beta_7
\end{aligned} \tag{A11}$$

Using the above equations and substituting expected values with their sample equivalents, I can show the DiDiD estimator (β_7) is equivalent to the following:

$$\hat{\beta}_7 = \Big[(\hat{Y}_{T=1,N=1,\text{Post}=1} - \hat{Y}_{T=1,N=1,\text{Post}=0}) - (\hat{Y}_{T=0,N=1,\text{Post}=1} - \hat{Y}_{T=0,N=1,\text{Post}=0})\Big]$$

$$- \left[(\hat{Y}_{T=1,N=0,\text{Post}=1} - \hat{Y}_{T=1,N=0,\text{Post}=0}) - (\hat{Y}_{T=0,N=0,\text{Post}=1} - \hat{Y}_{T=0,N=0,\text{Post}=0}) \right] \tag{A12}$$

Notably, the triple difference estimator is equivalent to the difference between two DiD estimators. As discussed in the main text, I expect $\hat{\beta}_7$ to equal zero. If bottle bills transfer wealth, I only expect a small portion of the population to benefit. Thus, I proceed with estimators that look for an effect in group L.

DiDiDiD A quadruple difference estimator allows me to look at the effect of bottle bills on group L. Through the same steps as just taken for the triple difference estimator, I define the quadruple difference estimator in terms of sample means. To begin, I specify the DiDiDiD regression equation.

$$\begin{aligned} Y_{ist} = &\beta_0 + \beta_1 T + \beta_2 N + \beta_3 L + \beta_4 \text{Post} \\ &+ \beta_5 T * N + \beta_6 T * L + \beta_7 T * \text{Post} \\ &+ \beta_8 N * L + \beta_9 N * \text{Post} + \beta_{10} L * \text{Post} \\ &+ \beta_{11} T * N * L + \beta_{12} T * N * \text{Post} + \beta_{13} T * L * \text{Post} + \beta_{14} N * L * \text{Post} \\ &+ \beta_{15} T * N * L * \text{Post} + \epsilon_{ist} \end{aligned} \tag{A13}$$

The conditional mean function of equation (A13) is $\mathbb{E}[Y_{ist}|T, N, L, \text{Post}]$. Under standard OLS assumptions and an additive effect, I can use $\mathbb{E}[\epsilon_{ist}|T, N, \text{Post}] = 0$ to show all sixteen expected values of $\mathbb{E}[Y_{ist}|T, N, L, \text{Post}]$ (Olden and Møen 2022). This is the next step in defining the quadruple difference estimator in terms of sample means.

$$\begin{aligned} &\mathbb{E}[Y|T = 0, N = 0, L = 0, \text{Post} = 0] = \beta_0 \\ &\mathbb{E}[Y|T = 0, N = 0, L = 0, \text{Post} = 1] = \beta_0 + \beta_4 \\ &\mathbb{E}[Y|T = 0, N = 0, L = 1, \text{Post} = 0] = \beta_0 + \beta_3 \\ &\mathbb{E}[Y|T = 0, N = 0, L = 1, \text{Post} = 1] = \beta_0 + \beta_3 + \beta_4 + \beta_{10} \\ &\mathbb{E}[Y|T = 0, N = 1, L = 0, \text{Post} = 0] = \beta_0 + \beta_2 \\ &\mathbb{E}[Y|T = 0, N = 1, L = 0, \text{Post} = 1] = \beta_0 + \beta_2 + \beta_4 + \beta_9 \\ &\mathbb{E}[Y|T = 1, N = 0, L = 0, \text{Post} = 0] = \beta_0 + \beta_1 \\ &\mathbb{E}[Y|T = 1, N = 0, L = 0, \text{Post} = 1] = \beta_0 + \beta_1 + \beta_4 + \beta_7 \\ &\mathbb{E}[Y|T = 0, N = 1, L = 1, \text{Post} = 0] = \beta_0 + \beta_2 + \beta_3 + \beta_8 \end{aligned}$$

$$\mathbb{E}[Y|T=0,N=1,L=1,\text{Post}=1] = \beta_0+\beta_2+\beta_3+\beta_4+\beta_8+\beta_9+\beta_{10}+\beta_{14}$$
$$\mathbb{E}[Y|T=1,N=0,L=1,\text{Post}=0] = \beta_0+\beta_1+\beta_3+\beta_6$$
$$\mathbb{E}[Y|T=1,N=0,L=1,\text{Post}=1] = \beta_0+\beta_1+\beta_3+\beta_4+\beta_6+\beta_7+\beta_{10}+\beta_{13}$$
$$\mathbb{E}[Y|T=1,N=1,L=0,\text{Post}=0] = \beta_0+\beta_1+\beta_2+\beta_5$$
$$\mathbb{E}[Y|T=1,N=1,L=0,\text{Post}=1] = \beta_0+\beta_1+\beta_2+\beta_4+\beta_5+\beta_7+\beta_9+\beta_{12}$$
$$\mathbb{E}[Y|T=1,N=1,L=1,\text{Post}=0] = \beta_0+\beta_1+\beta_2+\beta_3+\beta_5+\beta_6+\beta_8+\beta_{11}$$
$$\begin{aligned}\mathbb{E}[Y|T=1,N=1,L=1,\text{Post}=1] &= \beta_0+\beta_1+\beta_2+\beta_3+\beta_4+\beta_5+\beta_6+\beta_7\\ &+\beta_8+\beta_9+\beta_{10}+\beta_{11}+\beta_{12}+\beta_{13}+\beta_{14}+\beta_{15}\end{aligned} \tag{A14}$$

Using the above equations and substituting expected values with their sample equivalents, I can show the DiDiDiD estimator (β_{15}) is equivalent to the following:

$$\begin{aligned}\hat{\beta}_{15} = &\Big[(\hat{Y}_{T=1,N=1,L=1,\text{Post}=1}-\hat{Y}_{T=1,N=1,L=1,\text{Post}=0})-(\hat{Y}_{T=0,N=1,L=1,\text{Post}=1}-\hat{Y}_{T=0,N=1,L=1,\text{Post}=0})\Big]\\ &-\Big[(\hat{Y}_{T=1,N=0,L=1,\text{Post}=1}-\hat{Y}_{T=1,N=0,L=1,\text{Post}=0})-(\hat{Y}_{T=0,N=0,L=1,\text{Post}=1}-\hat{Y}_{T=0,N=0,L=1,\text{Post}=0})\Big]\\ &-\Big[(\hat{Y}_{T=1,N=1,L=0,\text{Post}=1}-\hat{Y}_{T=1,N=1,L=0,\text{Post}=0})-(\hat{Y}_{T=0,N=1,L=0,\text{Post}=1}-\hat{Y}_{T=0,N=1,L=0,\text{Post}=0})\Big]\\ &+\Big[(\hat{Y}_{T=1,N=0,L=0,\text{Post}=1}-\hat{Y}_{T=1,N=0,L=0,\text{Post}=0})-(\hat{Y}_{T=0,N=0,L=0,\text{Post}=1}-\hat{Y}_{T=0,N=0,L=0,\text{Post}=0})\Big]\end{aligned} \tag{A15}$$

Notably, like the triple difference estimator is equivalent to the difference between two DiD estimators, the quadruple difference estimator is equivalent to the difference between two triple difference estimators. As discussed in the main text, I do not have data on who recycles for cash. Using a basic economic model, I argue the lowest-wage earners in a population ought to be the people recycling for cash. Due to data constraints, I proxy for wage with education, introducing measurement error. The quadruple difference estimator differences between two triple difference estimators, the triple difference estimator for group L and group H, respectively. Due to poor measurement of the population of people actually treated by the policy, both estimators estimate the policy's effect on some portion of the treated population. As a consequence, the group H triple difference estimator serves as a poor baseline. Thus, I choose to rely on the DiDiD estimator with heterogeneity by education group instead.

DiDiD with Heterogeneity by Education Group To construct the DiDiD estimator with heterogeneity by education group, I interact $T * \text{Post} * N$ with L, creating a model that is fully saturated if I treat $T * \text{Post}$ as one variable. Through the same steps as just taken for the triple and quadruple difference estimators, I define the triple difference estimator with heterogeneity by education group in terms of sample means. As discussed in the main text, the goal of this estimator is to isolate the association between bottle bills and the relative outcome between N and W for group L. To begin, I specify the estimator's regression equation.

$$\begin{aligned} Y_{ist} = \beta_0 &+ \beta_1 T + \beta_2 N + \beta_3 L + \beta_4 \text{Post} \\ &+ \beta_5 T * N + \beta_6 T * L + \beta_7 T * \text{Post} \\ &+ \beta_8 N * L + \beta_9 N * \text{Post} \\ &+ \beta_{12} T * N * \text{Post} + \beta_{13} T * L * \text{Post} \\ &+ \beta_{15} T * N * L * \text{Post} + \epsilon_{ist} \end{aligned} \tag{A16}$$

As in the main text, the association of interest is the sum of two coefficients: $\beta_{12} + \beta_{15}$.[11] Below, I will show the sum of these coefficients is approximately the triple difference estimator for group L. The conditional mean function of equation (A16) is $\mathbb{E}[Y_{ist}|T, N, L, \text{Post}]$. Under standard OLS assumptions and an additive effect, I can use $\mathbb{E}[\epsilon_{ist}|T, N, \text{Post}] = 0$ to show all sixteen expected values of $\mathbb{E}[Y_{ist}|T, N, L, \text{Post}]$ (Olden and Møen 2022). This is the next step in writing the parameter of interest from regression equation (A16) in terms of sample means.

Notably, unlike the other two regression equations (A10, A13), equation (A16) is not fully saturated. I assume β_{10}, β_{11}, and β_{14} from equation (A13) are zero. In words, I am assuming in control states relative outcomes between N and W for group L and H did not change differentially between the pre- and post-treatment periods. Additionally, I assume relative outcomes between N and W do not vary for the L group relative to the H group between treated and control states in the pre-treatment period. Both assumptions align with the identification assumption, so they do not come at a high cost. Moreover, the assumptions reduce the variance associated with the estimator in the main text as a fully saturated model is subject to over fitting (Angrist and Pischke 2009). Below I define all sixteen expected values of $\mathbb{E}[Y_{ist}|T, N, L, \text{Post}]$.

$$\mathbb{E}[Y|T = 0, N = 0, L = 0, \text{Post} = 0] = \beta_0$$

[11] In the main text, the association of interest is the sum of ϕ and β_1. ϕ is analogous to β_{12}; both coefficients are associated with the interaction: $T * N * \text{Post}$. β_1 is analogous to β_{15}; both coefficients are associated with the interaction: $T * N * L * \text{Post}$.

$$
\begin{aligned}
&\mathbb{E}[Y|T=0,N=0,L=0,\text{Post}=1] = \beta_0+\beta_4\\
&\mathbb{E}[Y|T=0,N=0,L=1,\text{Post}=0] = \beta_0+\beta_3\\
&\mathbb{E}[Y|T=0,N=0,L=1,\text{Post}=1] = \beta_0+\beta_3+\beta_4\\
&\mathbb{E}[Y|T=0,N=1,L=0,\text{Post}=0] = \beta_0+\beta_2\\
&\mathbb{E}[Y|T=0,N=1,L=0,\text{Post}=1] = \beta_0+\beta_2+\beta_4+\beta_9\\
&\mathbb{E}[Y|T=1,N=0,L=0,\text{Post}=0] = \beta_0+\beta_1\\
&\mathbb{E}[Y|T=1,N=0,L=0,\text{Post}=1] = \beta_0+\beta_1+\beta_4+\beta_7\\
&\mathbb{E}[Y|T=0,N=1,L=1,\text{Post}=0] = \beta_0+\beta_2+\beta_3+\beta_8\\
&\mathbb{E}[Y|T=0,N=1,L=1,\text{Post}=1] = \beta_0+\beta_2+\beta_3+\beta_4+\beta_8+\beta_9\\
&\mathbb{E}[Y|T=1,N=0,L=1,\text{Post}=0] = \beta_0+\beta_1+\beta_3+\beta_6\\
&\mathbb{E}[Y|T=1,N=0,L=1,\text{Post}=1] = \beta_0+\beta_1+\beta_3+\beta_4+\beta_6+\beta_7+\beta_{13}\\
&\mathbb{E}[Y|T=1,N=1,L=0,\text{Post}=0] = \beta_0+\beta_1+\beta_2+\beta_5\\
&\mathbb{E}[Y|T=1,N=1,L=0,\text{Post}=1] = \beta_0+\beta_1+\beta_2+\beta_4+\beta_5+\beta_7+\beta_9+\beta_{12}\\
&\mathbb{E}[Y|T=1,N=1,L=1,\text{Post}=0] = \beta_0+\beta_1+\beta_2+\beta_3+\beta_5+\beta_6+\beta_8\\
&\mathbb{E}[Y|T=1,N=1,L=1,\text{Post}=1] = \beta_0+\beta_1+\beta_2+\beta_3+\beta_4+\beta_5+\beta_6+\beta_7\\
&\qquad+\beta_8+\beta_9+\beta_{12}+\beta_{13}+\beta_{15} \qquad \text{(A17)}
\end{aligned}
$$

Using the above equations and substituting expected values with their sample equivalents, I can show the parameter of interest, $\beta_{15}+\beta_{12}$, approximates the following:

$$
\begin{aligned}
\hat{\beta}_{15}+\hat{\beta}_{12} \approx &\Big[(\hat{Y}_{T=1,N=1,L=1,\text{Post}=1}-\hat{Y}_{T=1,N=1,L=1,\text{Post}=0})-(\hat{Y}_{T=0,N=1,L=1,\text{Post}=1}-\hat{Y}_{T=0,N=1,L=1,\text{Post}=0})\Big]\\
-&\Big[(\hat{Y}_{T=1,N=0,L=1,\text{Post}=1}-\hat{Y}_{T=1,N=0,L=1,\text{Post}=0})-(\hat{Y}_{T=0,N=0,L=1,\text{Post}=1}-\hat{Y}_{T=0,N=0,L=1,\text{Post}=0})\Big] \qquad \text{(A18)}
\end{aligned}
$$

As mentioned above, the parameter of interest approximates the triple difference estimator for group L. The assumptions mentioned previously must hold perfectly for the equation above to be equivalent.

Table A1: Quadruple Difference Estimates of the Association between Bottle Bills and the Incidence of Low Birth Weight

	(1)	(2)	(3)	(4)	(5)
$\mathbb{1}(\text{BB}) * \mathbb{1}(<\text{HS}) * \mathbb{1}(\text{not winter})$	-0.148	-0.149	-0.147	-0.148	-0.108
	(0.103)	(0.103)	(0.103)	(0.103)	(0.100)
Observations	2443329	2443329	2443329	2443329	2434808
within R-squared	0	.0003	.0001	.0004	.0005
State x Season x Educ. Fixed Effect	x	x	x	x	
County x Season x Educ. Fixed Effect					x
State x Educ. x Year Fixed Effect	x	x	x	x	
County x Educ. x Year Fixed Effect					x
State x Season x Year Fixed Effect	x	x	x	x	
County x Season x Year Fixed Effect					x
Season x Educ. x Year Fixed Effect	x	x	x	x	x
Weather Controls		x		x	x
Income Controls			x	x	x

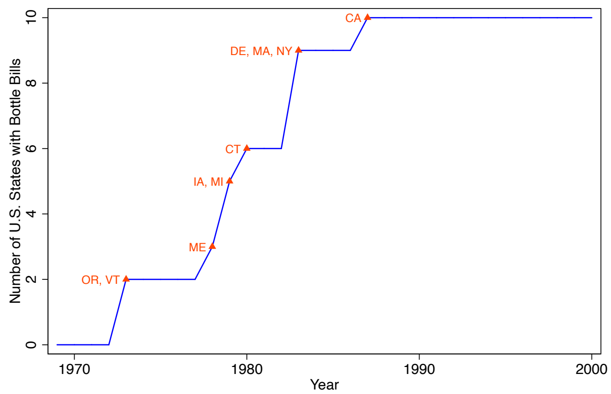
Notes: Table A1 reports DiDiDiD estimates of the association between bottle bills and the incidence of low birth weight in non-winter relative to winter months for low education mothers relative to middle education mothers. Estimates are computed using equation (A6). Columns differ by the controls added to estimating equation (A6). For example, Column (1) includes no controls and Column (4) includes both weather and income controls. Column (5) replaces state specific fixed effects with county specific fixed effects. Each regression is weighted by the number of births in each education x season x county x year cell. The regressions are run at the county level to allow for county specific controls. The reported standard errors are clustered at the state level.

Table A2: DiD Estimates of the Association between Bottle Bills and the Incidence of Low Birth Weight

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Bottle bill	-0.346	-0.350	-0.158	-0.163	-0.328	-0.333	-0.145	-0.150	-0.156	-0.176	0.034	0.014
	(0.162)	(0.161)	(0.164)	(0.164)	(0.119)	(0.117)	(0.179)	(0.179)	(0.171)	(0.169)	(0.165)	(0.163)
... x Less than HS					-0.457	-0.455	-0.429	-0.428				
					(0.347)	(0.346)	(0.344)	(0.343)				
... x HS					0.200	0.200	0.185	0.186				
					(0.113)	(0.114)	(0.109)	(0.109)				
... x Not Winter									-0.283	-0.260	-0.286	-0.262
									(0.025)	(0.027)	(0.024)	(0.028)
Observations	2443332	2443332	2443332	2443332	2443332	2443332	2443332	2443332	2443332	2443332	2443332	2443332
within R-squared	.0002	.0005	.0007	.001	.0003	.0007	.0008	.0011	.0002	.0006	.0007	.0011
State x Season Fixed Effect	x	x	x	x	x	x	x	x	x	x	x	x
State x Educ. Fixed Effect	x	x	x	x	x	x	x	x	x	x	x	x
Year Fixed Effect	x	x	x	x	x	x	x	x	x	x	x	x
Weather Controls		x		x		x		x		x		x
Income Controls			x	x			x	x			x	x

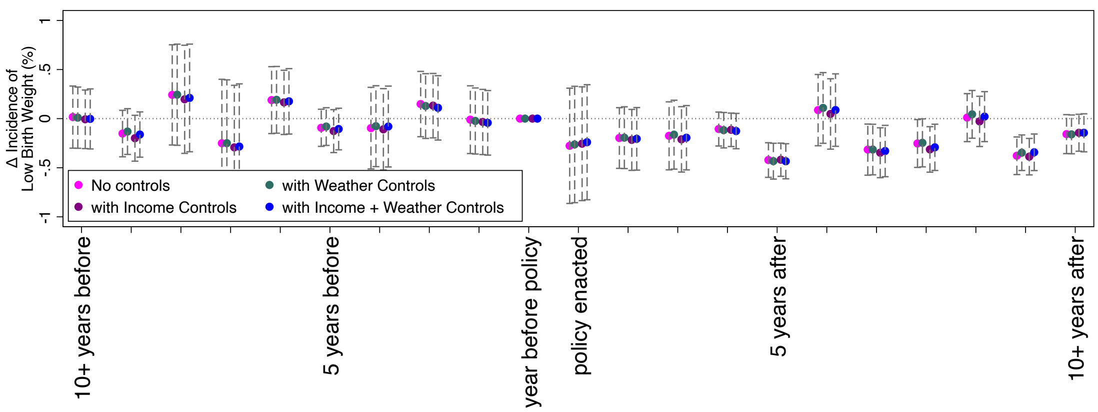
Notes: Table A2 reports DiD estimates from equations (A7, A8, A9). All reported estimates in a given column are from the same regression. Columns (1-4) report estimates from equation (A7), documenting the association between bottle bills and the incidence of low birth weight estimated with a DiD estimator. Columns differ by the controls added to the estimating equation. For example, Column (1) includes no controls and Column (4) includes both weather and income controls. Columns (5-8) report estimates from equation (A9), documenting how the DiD estimates vary by education group. In Columns (5-8), the first row documents the association between bottle bills and incidence of low birth weight among mothers with more than a HS education, and the third (second) row documents the difference between this association for mothers with a HS education (less than a HS education) and mothers with more than a HS education. As with the first four columns, Columns (5-8) differ by the controls added to the estimating equation. Columns (9-12) report estimates from equation (A8), documenting how the DiD estimates vary by season. In Columns (9-12), the first row documents the association between bottle bills and incidence of low birth weight in winter months, and the fourth row documents the difference between this association for non-winter births and winter births. As with the first four columns, Columns (9-12) differ by the controls added to the estimating equation. Each regression includes state x season, state x education group, and year fixed effects. Each regression is weighted by the number of births in each education x season x county x year cell. The regressions are run at the county level to allow for county specific controls. The reported standard errors are clustered at the state level.

Figure A1: U.S. Bottle Bill Introductions



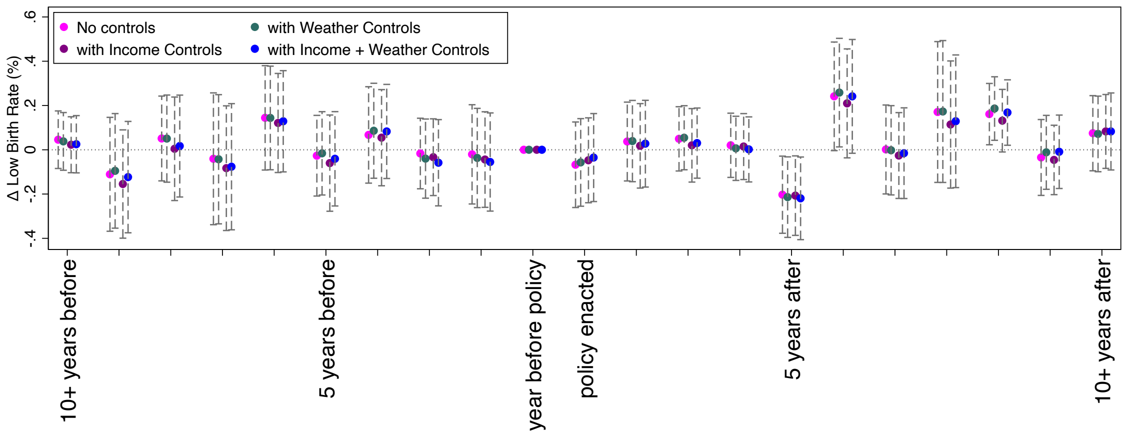
Notes: Figure A1 plots when each bottle bill state implemented bottle bills between 1969 and 2002. The blue line documents the total number of U.S. states with an active bottle bill in each year. The red triangle documents when each bottle bill state implemented the program. For example, in 1980, Connecticut implemented a bottle bill, making it the sixth U.S state to introduce a deposit refund program for containers.

Figure A2: Main Result Robustness – Association between Bottle Bills Before and After Implementation with the Incidence of Low Birth Weight among Mothers with Less than a HS Education



Notes: Figure A2 plots the association between bottle bill implementation and the incidence of low birth weight among mothers with less than a high school education in non-winter relative to winter months in each year before and after the policy took effect. These associations are computed using estimates from equation (3). The association in the year prior to policy implementation is normalized to zero. Each marker color corresponds to a different version of equation (3); the estimating equation for each color differs in the included controls. For example, pink markers correspond to no included controls and blue markers correspond to including both weather and income controls. Dashed lines represent 95% confidence intervals constructed using standard errors clustered at the state level. Additionally, the regression is weighted by the number of births in each education group by season by year by county cell.

Figure A3: Association between Bottle Bills and the Incidence of Low Birth Weight *without* Heterogeneity by Education



Notes: Figure A3 plots the association between bottle bill implementation and the incidence of low birth weight among *all* mothers in non-winter relative to winter months in each year before and after the policy took effect. These associations are estimated using regression equation (A5). The association in the year prior to policy implementation is normalized to zero. Each marker color corresponds to a different version of equation (A5); the estimating equations for each color differ in the included controls. For example, pink markers correspond to no included controls and blue markers correspond to including both weather and income controls. Dashed lines represent 95% confidence intervals constructed using standard errors clustered at the state level. Additionally, the regression is weighted by the number of births in each education group by season by year by county cell.