Bottle Redemption, Wealth Transfers, and Informal Wages

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Abstract

This paper suggests 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 bottles. Policy introductions are associated with a .6-3.7 percent 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. These results indicate that job opportunities created by deposit refund programs alleviated gaps in welfare policy during the study period.

Key words: Deposit Refund, Infant Health
JEL Codes: Q52, Q53

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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 suggests 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 One need not look further than the Old Testament, for a historical reference to the waste stream's perceived importance in the social safety net.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 2% of people find employment in informal recycling. In the global north, scavenging through waste is predominantly illegal.

Deposit refund programs for containers, known as "bottle bills", 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 private redemption center servicing the region processed 5 million containers per week in 2021, amounting to $250 thousand in refunds weekly and $13 million annually. This redemption center is one of 40 operating in the area. Private redemption centers purchase empties from container collectors on the streets of New York City as well as building supers/managers. An estimated 4,000 individuals work full time as container collectors in New York City making roughly $60,000 per person annually. Many other laborers collect cans throughout the city, earning far less each year.[1]

Bottle bills are increasingly popular. In 2005, bottle bills covered 250 million people globally. In 2025, they will cover 500 million people in fifty nine different countries, states and provinces.[2] The motivation for bottle bills 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 the social costs of waste disposal. The policy taxes waste production and incentivizes proper disposal. Other policy instruments, such as Pigouvian taxes, create incentives to illegally dump waste (Fullerton and Wolverton 2000).

Beyond an efficient and effective waste policy, bottle bills possibly increase the waste stream's wage potential, creating a market around empty collection. Bottle bills require consumers to pay deposits on beverages at purchase and then 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

[1] This information is from an informational interview with a redemption center owner and industry expert.

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

collect their refunds, bottle bills create a common pool resource, one that can be exploited through recycling. Consider the following simple model for empty collection. Let the recycling wage be the number of discarded empties divided by the number of collectors on a given day. An earner who's market wage is lower than 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. In other words, very simple theory suggests that the market transfers wealth from high to low wage earners.

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. On the other hand, these people may value their time collecting empties differently than their time in the labor market. If a person receives positive utility from recycling, then they may choose to recycle even if the recycling wage falls below their market wage (Ashenmiller 2009). Alternatively, if a person receives negative utility from collecting empties, 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, the income distribution of redeemers is significantly skewed left relative to the local population (Ashenmiller 2009, Ashenmiller 2011).

In this paper, I present suggestive evidence that bottle bills do in fact 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 bottle bills, 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 the implementation of bottle bills as a proxy for shocks in the recycling wage (Ashenmiller 2010). Second, I employ the incidence of low birth weight as a 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 bottle bill. Polls suggested the bill would pass by a large margin. A publicity push by national beverage and container manufacturers killed the bill – 49% of the public voted for and 51% voted against the initiative. While the Oregon bottle bill passed one year later, the legislation also faced intense lobbying.[3] After Oregon, ten other states proceeded to pass bottle bills starting with Vermont in 1972 and ending with Hawaii in 2002. Many states and the federal government attempted but failed to enact bottle bills during this period (Costle, Kreps, Andrus, Marshall, Blumenthal, Warren, Cutler, Cornell and Alm 1978). Of the states that successfully implemented bottle bills, 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 bottle bills, 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 bottle bills' implementation to identify the law's impact on the incidence of low birth weight. Additionally, I employ treatment heterogeneity to provide suggestive evidence that wealth transfers play a role in the law's impact on the incidence of low birth weight. In non-winter months, consumers produce more empties and empty collection is easier relative to winter months. Thus, through the wealth transfer channel bottle bills should induce the largest wealth transfers in non-winter relative to winter months. I find bottle bills are associated with a .6-3.7 percent reduction in the incident of low birth weight among mothers with less than a high school education. I find the association lasts for more than ten years after policy implementation and the effect is stronger in non-winter relative to winter months.

First, my results extend the empirical literature documenting the progressive nature of bottle bills. Ashenmiller (2009, 2010, 2011) provide empirical evidence that bottle bills induce policy relevant wealth transfers through recycling. Ashenmiller (2010) finds the introduction of bottle bills reduces petty crime rates throughout the U.S. The author attributes the association to wealth transfers. Specifically, the author equates bottle bills to highly targeted earned income tax credits for very low wage earners. 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 one percent of all households in Santa Barbara. Moreover, recycling accounts for roughly 7% ($700) of these households' annual incomes. My work extends Ashenmiller (2009, 2010, 2011)'s 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

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

very small literature demonstrating that waste policy can have far reaching economic impacts.

Second, my analysis adds to the relatively well developed literature on U.S. welfare policy and the incidence of low birth weight. Today, one critique of the U.S. social safety net is that it fails to aid the poorest and most marginalized populations. 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, formal work and citizenship requirements restrict access to a number of programs including EITC and SNAP (Aizer et al. 2022, Bitler and Hoynes 2011). Bottle bills 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 U.S. welfare programs. Similar to EITC and SNAP, wealth transfers associated with bottle bills are conditional on work in most cases. However, bottle bill benefits require informal rather than formal labor. Bottle bills are unlike all U.S. welfare policies in that they have no eligibility requirements for participants and thus benefits come at no administrative cost to beneficiaries. Work requirements exclude the poorest households from welfare benefits in the U.S. (Hoynes and Schanzenbach 2018). Moreover, immigration status and administrative hurdles further restrict the population who can employ the formal U.S. social safety net. Bottle bills are exclusionary in that they require beneficiaries to be capable of sifting through discards and/or redeeming empties. However, a marginalized population unable to gain access to EITC or SNAP may indeed be able to gain access to wealth transfers associated with bottle bills.

WIC, unlike EITC and SNAP, has no work or immigration status requirements. Additionally, benefit access requires less documentation relative to EITC and SNAP. Therefore, the welfare policy also comes at lower administrative costs to participants. Researchers find WIC participation reduces the incidence of low birth weight among participants by 6-33% depending on the exact sub-population; reductions are larger in more disadvantaged populations (Hoynes, Page and Stevens 2011, Currie and Rajani 2015, Bitler and Currie 2005). Almond et al. (2011) finds the introduction of food stamps is associated with a 7-8% reduction in the incidence of low birth weight among white program participants and a 5-12% reduction among black program participants. Notably, food stamps transfer an average of $200 per month to households, and WIC transfers an average of $40 per month to expecting or new mothers. 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. Per dollar spent, WIC reduces the incidence of low birth weight much more effectively than SNAP and EITC. Obviously, the welfare policies have very different goals. WIC is specifically meant to improve pre- and post-natal health for both mothers and infants. Nonetheless, another aspect of the differential may be that WIC is able to reach a more disadvantaged population than SNAP or EITC.[4]

At first glance, it is surprising that EITC is associated with reductions in the incidence of low birth weight to a similar degree as bottle bills. Earnings from work associated with EITC far exceed that of the aggregate potential wealth transfers associated with bottle bills. However, when compared at the individual or household level, plausible wealth transfers associated with bottle bills are comparable to the $1,000 increase in after tax income from EITC found to improve birth outcomes to a similar extent as bottle bills. Collecting two garbage bags of empties per week amounts to an annual recycling income of $1,040. New York state residents redeemed 5.1 billion containers in 2016, amounting to 25.5 million bags of empties, $255 million in refunds, or an annual wealth transfer of $1,040 to 245,000 households.[5] Explicit and implicit requirements make take up of safety net programs low among needy families in the U.S. (Aizer et al. 2022). Bottle bill wealth transfers have no requirements and occur through informal means. The transfers may reach an entirely different subset of the population than formal safety net programs, likely those in incredible need. If the most needy families participating in EITC drive incidence of low birth weight effects, then the comparability of EITC results to that of bottle bills is more plausible.

The paper proceeds as follows. First, I highlight other potential mechanisms driving the correlation I find between the incidence of low birth weight and the implementation of bottle bills. This discussion helps motivate my empirical approach. Second, I discuss the data used in the analysis. Third, I outline my empirical approach, associated assumptions, and results. Fourth, I conclude.

2 Background

2.1 Bottle Bills

Four years after the first bottle bills took effect in the U.S., studies showed the bills reduced litter, decreased solid waste production, increased energy savings, and generated jobs (*Public Meeting of the Resource Conservation Committee on Beverage Container Deposit Legislation* 1977). Legislators established bottle

[4]Hoynes et al. (2011) notes that as the income of program participants increases, the programs' effect on the incidence of low birth weight decreases.

[5]If 1 in every 100 New York state residents returned the same number of redeemables per year, then roughly 245,000 households would earn $1,040 in refunds annually. Redemption rates are not constant across redeemers. Moreover, based on survey evidence from Ashenmiller (2011), more than 245,000 of the poorest and near poor households recycle for cash in New York State. Some professional recyclers certainly earn far more than $1,040 per year and other households certainly earn far less.

bills as beverage consumption shifted from returnables to single use.[6] The once private costs of returning and refilling bottles became social costs most visible in municipal waste budgets and as litter in the environment. Bottle bills corrected this cost shift by imposing a tax on consumption and an equal sized return on proper disposal.

Bottle bills curb litter and reduce waste; their connection to birth outcomes is not first order (Costle et al. 1978). The initial bills worked to preserve the use of refillable containers (Claussen 1973). As time passed and market forces drove refillables out of use, the bills incentivized recycling[7] and required producer responsibility. While the legislation's goal was waste oriented, bottle bill advocates and opponents in the 1970s and 1980s understood the law would have economic impacts far beyond the production of municipal solid waste. Opponents[8] argued the bills would hurt revenue and jobs. Beyond waste and litter reduction, advocates argued the laws would create jobs, lower beverage prices, reduce local air pollution, and save energy.

Changes in air quality, employment or beverage prices could all explain the association between bottle bills and birth weight. In the next section, I argue all three of these mechanisms do not fully explain the long run (> 3 years) association between bottle bills and the incidence of low birth weight among mothers with less than a high school education in non-winter relative to winter months. Then, I argue a fourth mechanism, wealth transfers, helps to more fully explain the association.

2.2 Potential Mechanisms

Four potential mechanisms likely drive the association between bottle bills and birth weight. I use my empirical analysis, stylized historical facts, and evidence from the literature to tease out the impact of one mechanism (wealth transfers) on the incidence of low birth weight. To begin, I will outline how the four potential mechanisms mentioned in the previous section are caused by bottle bills and why they might impact birth outcomes. Then, I argue why three of these mechanisms do not fully explain the paper's findings and why the fourth (wealth transfers) partially explains the results.

In many states, bottle bill introductions caused the cost of beer and soda in packaged containers to increase (Loube 1975, Wagenbach 1985).[9] As a consequence, beer and soda consumption decreased

[6] In 1950, people consumed over 90% of soda and beer fillings from returnables. In 1975, people consumed 34% of soda and 17% of beer from returnables (Costle et al. 1978). With an 8% growth rate, beverage containers compromised roughly 7% of municipal solid waste in 1977 (*Public Meeting of the Resource Conservation Committee on Beverage Container Deposit Legislation 1977).*

[7] Recycling refers to the collection of items deemed recyclable by the state rather than the use of used materials as an input to production.

[8] Bottle bill opponents included beverage and container manufacturers, grocery stores, and labor unions.

[9] Michigan saw the most severe price shock, where prices rose 10-16% (Porter 1983). While Oregon experienced negative

overall. This reduction possibly improved birth outcomes, leading to an association between bottle bill implementation and the incidence of low birth weight. In all states where price shocks occurred, sales are thought to have recovered after a couple years.[10] I find the association between bottle bills and the incidence of low birth weight lasts for more than ten years after implementation. Moreover, I find the association in non-winter months is stronger than that of winter months. If the reduction in beer and soda consumption is the only relevant mechanism, I would expect the seasonal association between bottle bills and birth weight to dissipate after one to three years. The association persists beyond three years and exhibits seasonality, suggesting the consumption mechanism does not fully explain the association.

Beyond reducing beverage consumption, bottle bills created jobs. The laws promoted industry around returnables and recycling (Loube 1975). In contrast, bottle bills hindered packaging manufacturing. The overall effect was a marginal increase in jobs; this could have improved birth outcomes. If employment levels are higher, people likely have more money and will spend more on normal goods. As a consequence, birth outcomes will improve because maternal health is considered a normal good. That said, the formal employment mechanism does not fully explain the association between bottle bills and birth outcomes because this channel would not cause seasonality. Employment in packaged beverage production and empty processing is not seasonal. Packaged beer and soda as well as empties are storable, thus firms can smooth production over the whole year.

As with any policy that impacts production and employment, bottle bills also affected air pollution concentrations. Opponents suggested the transportation of empties would increase the use of automobiles and trucks, decreasing air quality. Advocates suggested the laws reduced the amount of container manufacturing, improving air quality. I have not found any studies exploring this mechanism, so I cannot rule out the possibility that air pollution explains the association I find between bottle bills and birth weight. Though, Chay and Greenstone (2003) find little association between reductions in TSP and the incidence of low birth weight in the U.S. during the early 1980s. Thus, any variation in air pollution associated with the bottle bill implementations likely did not cause the observed reductions in the incidence of low birth weight.[11]

Wealth transfers is the final potential mechanism that could explain the long lasting seasonal association between bottle bills and birth outcomes. As discussed in the introduction, theory and empirical evidence suggest bottle bills improve birth outcomes via wealth transfers. Through the wealth transfer channel, I

direct price shocks (*Public Meeting of the Resource Conservation Committee on Beverage Container Deposit Legislation 1977*).

[10] After a couple years, sales in bottle bill states were comparable to the period before implementation (Wagenbach 1985). Similarly, sales in neighboring counties in bottle bill and non-bottle bill states were comparable a couple years after implementation (Loube 1975). Though, it's unclear to me how robust these findings are.

[11] Chay and Greenstone (2003) exploits a sharp reduction in average U.S. TSP levels between 1980 and 1982 caused by an economic recession. The bottle bills studied in this paper were implemented in the 1970s and 1980s.

expect bottle bills to have the strongest effect on birth outcomes in the population of low wage earners in non-winter months. In non-winter months, consumers produce more empties and empty collection is easier relative to winter months. The prediction that the treatment effect is seasonal and strongest for low wage earners is consistent with the paper's empirical results. In the next section, I outline the data I use in my analysis, then I discuss my empirical strategy, and finally I share results.

3 Data

In my analysis, I use U.S. Vital Statistics micro data from 1969-2002 aggregated to the county by education group by season by year level. I employ Vital Statistics data on birth month, mother's education, birth year, and birth weight to conduct my analysis.

Treatment effect identification relies on the introduction of bottle bills. Between 1969 and 2002, ten U.S. states implemented the law. Figure 1 visualizes bottle bill implementations in the U.S. during the sample period. As discussed previously, the timing of these bills was idiosyncratic due to heavy lobbying. Most states attempted multiple times and for multiple years before successfully enacting the law. Beyond these ten, many states tried and failed numerous times to pass bottle bills. In the U.S., Oregon and Vermont implemented the first bottle bills in 1973. After a five year hiatus, in 1978, Maine implemented a bottle bill. Shortly after, Michigan and Iowa implemented in 1979. Then, Connecticut implemented the law in 1980. Three years later, in 1983, Delaware, Massachusetts and New York implemented bills. Lastly, California implemented the bill in 1987 (Ashenmiller 2010). Birth outcomes are assigned treatment based on the month the pregnancy's third trimester began, as the third trimester is the most important in determining birth weight.

To isolate the effect of wealth transfers, I explore treatment effect heterogeneity by season and education. If bottle bills act on birth outcomes through the wealth transfer channel, the treatment effect should be largest in non-winter months relative to winter months. In winter months beverage consumption decreases and empty collection is harder relative to non-winter months. Thus, bottle bills ought to induce larger wealth transfers in non-winter months relative to winter months. I expect this differential in wealth transfer size to cause larger decreases in the incidence of low birth weight in non-winter months relative to winter months. Birth outcomes are assigned 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 of the year are designated as non winter births.

Moreover, if bottle bills act on birth outcomes through the wealth transfer channel, the treatment effect is likely largest for people with less than a high school education relative to those with more than a high school education. Individuals with less than a high school education generally have lower wage jobs relative to individuals with more than a high school education. As discussed in the previous section, lower wage earners are more likely than higher wage earners to receive wealth transfers from bottle bills. As, lower wage earners have a relatively smaller opportunity cost. I use education as a proxy for income because the birth outcome data does not include demographic information on wage. I assign birth outcomes to education groups using the 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.

In figure 2, find the raw trends in the U.S. incidence of low birth weight during the sample period. The plot demonstrates heterogeneity in the rate by season and education group during the sample period. There are significant within year across season and across education group differences in the incidence of low birth weight that I cannot assume are constant across states. Thus, an empirical strategy relying on cross sectional variation in seasonal or educational differences in the incidence of low birth weight could suffer from omitted variable bias. These trends emphasize the importance in the plausibly random timing of bottle bill implementation for the causal identification of a bottle bill's impact on the incidence of low birth weight.

Additionally, I include a rich set of 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. Given the analysis's focus on seasonality, I use state level data, because the BEA does not have sub-annual county level data during the study period. To assign BEA control variable values to birth outcomes, I find the variable average during the third trimester. Weather controls were aggregated with population weight 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 day (HDDs). The threshold used to construct both CDDs and HDDs is 20 degrees Celsius.

Table A4 shows the means and standard deviations for the main variables in the analysis – all control variables and the incidence of low birth weight. I break statistics down by season as well as bottle bill and non-bottle bill states. Moreover, I break the incidence of low birth weight summary statistics down by education group. As previously highlighted by figure 2, the summary statistics demonstrate the incidence of low birth weight is highest in non-winter months among the least educated mothers. There is little seasonal variation in the income controls employed by the paper, and there is a lot of seasonal variation in weather controls, obviously.

4 The Association Between Bottle Bills and the Incidence of Low Birth Weight

4.1 Identification Strategy: Differences in Differences in Differences

I use a triple difference (DiDiD) estimator to isolate the association between bottle bills and the incidence of low birth weight. Specifically, the estimator exploits three 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, as I expect bottle bills to tranfer more wealth during non-winter months. Lastly, I look at heterogeneity in this estimator by education group. I only expect relatively lower wage earners to receive a wealth transfer from bottle bills. As discussed before, the birth weight data does not include household income, so I use mother's education as a proxy for income.

The paper's main regression is a DiDiD estimator *with* heterogeneity by education group. In a first step, I describe the DiDiD estimator *without* heterogeneity. Then, I outline how I modify this regression equation to isolate heterogeneity in the DiDiD estimator by education group, defining the paper's main regression. The regression equation for the DiDiD estimator *without* heterogeneity is:

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

Y_{ctes} is the incidence of low birth weight in county c in year t for education group e in season s. $\mathbb{1}(\text{bottle bill})_{it} = 1$ 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. Birth outcomes are assigned treatment based on the month the pregnancy's third trimester began, as the third trimester is the most important in determining birth weight.

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: $\mathbb{1}(\text{bottle bill})_{it} * \mathbb{1}(\text{not winter})_s$. This interaction of dummy variables takes the value of one for all states with active bottle bills in non-winter months. After adjustments for fixed effects, ϕ captures variation in the incidence of low birth weight specific to 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 the winter. 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 during the sample period, the issues raised by Goodman-Bacon (2021) are of little consequence in this context because a very small weight is placed on comparisons of concern.

Bottle bills are expected to transfer more wealth to less educated mothers relative to more educated mothers. To isolate variation by education group, I interact the DiDiD estimator with a dummy variable for the less than high school and high school education groups. The regression equation for the DiDiD estimator with heterogeneity by education group is:

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

$\mathbb{1}(\text{<HS})_e$ and $\mathbb{1}(\text{HS})_e$ are dummy variables associated with the low and middle education groups. $\mathbb{1}(\text{<HS})_e = 1$ if education group e is less than high school (low education). $\mathbb{1}(\text{HS})_e = 1$ if education group e is high school (middle education). I assign birth outcomes to education groups using the mother's 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.

The parameter of interest is $\beta_1 + \phi$. After adjustments for fixed effects and other interactions, ϕ now captures variation in the incidence of low birth weight among *high education* mothers only. Specifically, ϕ

captures the association between bottle bill implementations and high education mothers' incidence of low birth weight in non-winter versus winter months. β_1 captures the difference between this association for low and high education mothers. β_2 captures the difference among middle and high education mothers. Therefore, $\beta_1 + \phi$ captures the association between bottle bill implementations and low education mothers' incidence of low birth weight in non-winter versus winter months. See section A.4 for a longer discussion about the interpretation of $\beta_1 + \phi$.

To identify the parameter of interest, $\beta_1 + \phi$, the following assumption must hold: the incidence of low 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 (Olden and Møen 2022). I provide graphical evidence that the non-winter incidence of low birth weight relative to the winter rate evolved in parallel among low education mothers in bottle bill and non-bottle bill states prior to policy implementation (see Figure 3). Moreover, I am able to provide graphical evidence that the non-winter incidence of low birth weight relative to the winter rate evolved in parallel on average among mothers with at least a high school education in bottle bill and non-bottle bill states before policy implementation (see Figure 4). After policy implementation, the pattern of reduction seen among low education mothers' incidence of low birth weight is not seen in the incidence of low birth weight among mothers with at least a high school education (see Figure 4). Thus, for the identifying assumption to be violated, there must have been a policy change or socio-economic shift that (a) coincided with bottle bill implementations, (b) predominantly affected mothers with less than a high school education in non-winter months relative to winter months, and (c) did not positively impact mothers with at least a high school education differentially across seasons.

I estimate regression equation (2) with no controls, weather controls, income controls, and both weather and income controls to test result robustness. To understand how different sets of controls affect the results, I vary the controls included across robustness regressions. I include the following *income* controls: personal income, wages and salaries, farm wages and salaries, personal current transfer receipts, medicare benefits, state unemployment insurance compensation, and social security benefits. I also include the following *weather* controls: cooling degree days (CDDs) and heating degree days (HDDs). As discussed in section 4.3, the association between bottle laws and the incidence of low birth weight among mothers with less than a high school education is robust to the inclusion of a wide range of controls and time-varying effects, making it less likely that the association is confounded by contemporaneous changes differentially affecting low education mothers in the treated versus control states in non-winter versus winter months.

The DiDiD estimator with heterogeneity by education group 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. I choose the DiDiD estimator 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.3 for a longer discussion and DiD estimates. 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.[12] See section A.2 for a longer discussion and DiDiDiD estimates. Notably, the quadruple difference estimates are neither statistically significant nor statistically different from the triple difference estimates with heterogenity by education group, consistent with measurement issues.

4.2 Event Study: Testing Identifying Assumptions

As discussed in the previous section, separate measures of a bottle bill's effect in each year before and after implementation provide useful information for testing the identifying assumption. Hence I also estimate the following model:

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

$\mathbb{1}_{(t,i)\in j} = 1$ 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. $\sum_{e\in[l,m]}$ 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 validity of the identifying assumption. $\eta_{(j,\text{low})} + \phi_j$ represents the association between bottle bill implementation j years after or before policy enactment and the incidence of low birth weight among low education mothers in non-winter relative to winter months.[13] Importantly, Figure 3, the event study style graph of $\eta_{(j,\text{low})} + \phi_j$, provides

[12] The quadruple difference estimator differences between two triple difference estimators, the triple difference estimator for low and middle education mothers, respectively (see section A.4). 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 the estimates.

[13] $\eta_{(j,\text{low})}$ represents the association between bottle bill implementation j years after or before policy enactment and the incidence of low birth weight among low education mothers in non-winter relative to winter months relative to the association

an opportunity to assess whether there were pre-bottle bill trends in the association after nonparametric adjustments for all state by year, season by year, state by season, state by education, and season by education factors. As discussed in the previous section, Figure 3 fails to reject the assumption that the association evolved in parallel among low education mothers in bottle bill and non-bottle bill states prior to policy implementation. In section 4.5, I also test the pre-bottle bill parallel trends assumption using placebo bottle bill introductions. Results from this test also fail to reject the assumption.

Additionally, with ϕ_j and $\eta_{(j,\text{middle})} + \phi_j$, I am able to assess whether there were pre and post-bottle bill trends in the incidence of low birth weight among mothers with at least a high school education, in non-winter relative to winter months, in bottle bill states relative to non-bottle bill states. ϕ_j and $\eta_{(j,\text{middle})} + \phi_j$ represent the association between bottle bill implementation j years after or before policy enactment and the incidence of low birth weight among high and middle education mothers, respectively, in non-winter months relative to winter months. On average, I expect bottle bills to transfer more wealth to the lowest wage earners. Using mother education as a proxy for household income, I expect that bottle bills transfer more wealth to low education mothers relative to middle and high education mothers. Moreover, I expect bottle bills to be associated with no reduction or smaller reductions in the incidence of low birth weight among mothers with at least a high school education on average relative to mothers with less than a high school education. Thus, event study style plots of ϕ_j and $\eta_{(j,\text{middle})} + \phi_j$ provide a useful placebo test. Conditional on controls I do not expect to find a pattern of reduction comparable to that of low education mothers in post-bottle bill trends in the incidence of low birth weight among high and middle education mothers. Trends in the pre-bottle bill period or a comparable pattern of reduction in the post-bottle bill period would raise concern around the identification assumption's validity. I fail to reject the assumption with visual evidence from event study style graphs of ϕ_j and $\eta_{(j,\text{middle})} + \phi_j$ (see Figure 4).

4.3 Baseline Estimates of the Association between Bottle Bills and the Incidence of Low Birth Weight

Bottle bills should transfer more wealth in non-winter relative to winter months. This seasonal differential implies that if bottle bills improve birth outcomes through a wealth transfer channel, then the improvements should be larger in non-winter months. Also, bottle bills should transfer the most wealth to the lowest

among high education mothers. ϕ_j represents the association between bottle bill implementation j years after or before policy enactment and the incidence of low birth weight among high education mothers in non-winter relative to winter months. Thus, $\eta_{(j,\text{low})} + \phi_j$ represents the association just among low education mothers not relative to the association among high education mothers.

wage earners. This socio-economic differential implies that if bottle bills improve birth outcomes through a wealth transfer channel, then the improvements should be the most prominent among the lowest wage earners. Estimating equation (2) simultaneously tests whether bottle bills 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 bottle bills on the incidence of low birth weight. Notably, the estimates from this regression can only provide suggestive evidence that bottle bills transfer wealth to the lowest wage earners.

Table 1 reports the association between bottle bill implementations and low education mothers' incidence of low birth weight in non-winter versus winter months. Figure 3 visualizes how this association evolved on average in each year before and after bottle bill implementation. Before bottle bill implementation, no systematic difference appears between bottle bill and non-bottle bill states in low education mothers' incidence of low birth weight in non-winter relative to winter months; the point estimates in the pre-bottle bill period are rarely statistically significant and oscillate around zero.

Starting when bottle bills are implemented, the incidence of low birth weight drops substantially − by roughly .19 percentage points (pp) on average − among mothers with less than a high school education in non-winter relative to winter months. This reduction persists for more than ten years after bottle bill implementation (see Figure 3). This finding is consistent with the hypothesis that bottle bills transfer wealth to relatively lower wage earners and wealth transfers are largest in non-winter months. Overall, bottle bills are associated with a .05-.33 pp reduction in the incidence of low birth weight among mothers with less than a high school education (see Table 1). This finding is robust to the inclusion of both weather and income controls.[14]

It is informative to compare these statistics to sample averages from New York state to understand the association's magnitude. During the sample period, after bottle bill introduction, on average 30,000 low education mothers had live births in non-winter months per year in New York state. During this time, the incidence of low birth weight was roughly 10% in non-winter months for low education mothers. Thus, the finding that bottle bills are associated with a .19 pp reduction in the low education mothers' incidence of low birth weight suggests on average at least 57 newborns were positively impacted by bottle bills annually.

Importantly, this pattern of reduction is most prominent among mothers with less than a high school education. If bottle bills transfer wealth to a portion of the population, then I expect bottle bills on average

[14]Income 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. Weather controls include cumulative cooling and heating degrees. Further discussion around controls and their assignment to birth outcomes can be found in the Data section.

to transfer more wealth to the lowest wage earners. Using education as a proxy for household income, I expect bottle bills to have a much smaller effect on mothers with at least a high school education. Table 2 reports the association between bottle bill implementation and the incidence of low birth weight in non-winter versus winter months for each education group. The pattern of reduction only exists among low education mothers. In fact, small increases are detected among mothers with at least a high school education.

Figure 4 visualizes how these associations evolved in each year before and after policy implementation. Before bottle bill implementation, no systematic difference appears between bottle bill and non-bill states in middle-high education mothers' incidence of low birth weight in non-winter relative to winter months; the point estimates in the pre-bottle bill period are rarely statistically significant and oscillate around zero. Starting when bottle bills are implemented, the incidence of low birth weight among middle-high education mothers continues to oscillate around zero. Six years after implementation, the incidence of low birth weight increases slightly − by .05 to .1 pp on average − among mothers with at least a high school education in non-winter relative to winter months. The increase is larger for high education mothers than for middle education mothers.

4.4 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:

$$\mathbb{1}(\text{bottle bill})_{it} * \mathbb{1}(\text{not winter})_s = \sum_v \beta_v * v + \alpha_{i,s} + \delta_{i,t} + \gamma_{s,t} + \epsilon_{i,s,t} \tag{4}$$

$\mathbb{1}(\text{bottle bill})_{it} * \mathbb{1}(\text{not winter})_s$ is one in 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.[15] $\delta_{i,t}$ 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.

Weather varies seasonally and is known to affect the incidence of low birth weight (Deschenes, Greenstone and Guryan 2009). The causal variable is also seasonal. If weather patterns systematically vary across bottle bill and non-bottle bill states between the pre- and post-bottle bill periods in non-winter relative to winter months, then the results documented in this paper could reflect differences in weather patterns across space and time rather than the impact of bottle bills. In this case, quasi-randomization would clearly be unsuccessful. Table 3 displays the results from the balance test. The association between CDDs as well as HDDs and the causal variable is not statistically significant.

However, the association between farm salaries and wages with the causal variable is statistically significant. The DiDiD estimator relies on seasonal differences within states in a given year. If seasonal differences systematically varied across treated and untreated units, then the DiDiD estimator's identifying assumption is compromised. The balance test suggests seasonal differences in payments to hired labor on farms varied systematically across treated and untreated units during the sample period. This finding rejects the null hypothesis that the assumed quasi-randomization is successful. Thus, associations in this paper should be interpreted with caution as they are subject to an unknown amount of omitted variable bias.

4.5 Placebo Tests

I conduct two placebo tests to examine the robustness of this paper's main conclusions. In the first test, the pre-treatment placebo, I empirically asses the pre-treatment parallel trends assumption that was discussed and visually examined with event study style plots in Sections 4.2 and 4.3. In the second test, permutation, I compute the probability that I could have observed the reported results purely by chance.

[15]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.

Pre-Treatment Placebo This test provides an alternative avenue to falsify the parallel trends assumption in the pre-treatment period. To conduct this exercise, I first subset the sample to all untreated observations, i.e., all observations in bottle bill states prior to policy implementation and all observations in non-bottle bill states. I then randomly assign bottle bill implementation years to each bottle bill state during each state's pre-treatment period. Lastly, I estimate the DiDiD estimator with heterogeneity by education group using the subsetted sample and the placebo treatment years. If the parallel trends assumption is invalid, then I would expect to find statistically significant associations between placebo bottle bill implementations and low education mothers' incidence of low birth weight in non-winter relative to winter months.

I conduct the pre-treatment placebo test five times; in each test, a different set of placebo treatment years are assigned to the bottle bill states. Results can be found in Table 4. The test fails to reject the assumption that prior to bottle bill implementation the non-winter incidence of low birth weight relative to the winter rate evolved in parallel among low education mothers in bottle bill and non-bottle bill states. All placebo DiDiD estimates are statistically insignificant.

Permutation The classic framework for permutation inference computes a test statistic's distribution by randomly assigning observations to the treatment and control group (Abadie, Diamond and Hainmueller 2010). I apply this framework to the main identification strategy employed in this paper to compute the likelihood of randomly observing the paper's main result. First, I subset the sample to only observations from non-bottle bill states. Then, I randomly select ten states to be placebo bottle bill states and assign implementation years to each state. Lastly, I estimate the DiDiD estimator with placebo bottle bill states and implementation years on the sample of observations from non-bottle bill states.

I conduct the permutation test 1,500 times; in each test, a different set of placebo bottle bill states are selected and placebo implementation years are assigned. In Figure 5, find the distribution of the parameter of interest, the association between placebo bottle bills and the incidence of low birth weight among low education mothers in non-winter relative to winter months. Notably, only 61 of the 1,500 permutations estimate associations less than the actual association emphasised throughout this paper.

5 Conclusion

Ashenmiller (2009, 2010, 2011) shows bottle bills transfer wealth to the working poor. My work suggests that through this channel the laws improve birth outcomes as well, highlighting that waste policy can have far reaching economic impacts. Subsidizing markets around used material collection with deposit refund programs creates societal benefits through reductions in the incidence of low birth weight. Such policies create comparable reductions in the incidence of low birth weight to that of EITC (Hoynes et al. 2015). Moreover, this work adds suggestive evidence to a small literature documenting that bottle bills transfer wealth to the working poor.

Specifically, I find that the introduction of bottle bills is associated with a large and persistent improvement in the incidence of low birth weight among mothers with less than a high school education, particularly in non-winter months. As outlined in the introduction, bottle bills increase recycling wages, potentially leading to highly targeted wealth transfers to especially low income households. I argue that this wealth transfer drives the association between bottle bill introductions and infant health documented in this paper. This analysis suggests that there were clear gaps in welfare policy in the U.S. that the waste stream was able to fill during the sample period. This finding raises the following questions – what caused these gaps and why was the waste stream able to fill them so effectively? Moreover, my work suggests that simply creating the potential for wealth transfers improves birth outcomes to a degree comparable to highly targeted policy.

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Table 1: Association between Bottle Bills and the Incidence of Low Birth Weight: Main Results and Robustness

	(1)	(2)	(3)	(4)
Low Education: $\mathbb{1}$(bottle bill) $*$ $\mathbb{1}$(not winter)	-0.214	-0.208	-0.195	-0.191
	(0.048)	(0.050)	(0.052)	(0.053)
Observations	2445080	2445080	2445080	2445080
within R-squared	.0002	.0005	.0002	.0005
State x Season Fixed Effect	x	x	x	x
State x Educ. Fixed Effect	x	x	x	x
State x Year Fixed Effect	x	x	x	x
Season x Year Fixed Effect	x	x	x	x
Educ. x Season Fixed Effect	x	x	x	x
Weather Controls		x		x
Income Controls			x	x

Notes: Table 1 reports the association between bottle bills and the incidence of low birth weight in non winter relative to winter months among mothers with less than a high school education. This association is computed using estimates from equation (2). All estimates from equation (2) can be found in Table A1. Specifically, the reported association is the sum of ϕ and β_1 from equation (2) or the first and third row in Table A1. Columns differ by the controls added to estimating equation (2). Column (1) includes no controls. Columns (2-4) include weather and income controls. 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 2: Association between Bottle Bills and the Incidence of Low Birth Weight – Heterogeneity by Education Group

	(1)	(2)	(3)	(4)
Low Education: $\mathbb{1}$(bottle bill) $*$ $\mathbb{1}$(not winter)	-0.214	-0.208	-0.195	-0.191
	(0.048)	(0.050)	(0.052)	(0.053)
Middle Education: $\mathbb{1}$(bottle bill) $*$ $\mathbb{1}$(not winter)	0.073	0.077	0.090	0.093
	(0.038)	(0.041)	(0.047)	(0.049)
High Education: $\mathbb{1}$(bottle bill) $*$ $\mathbb{1}$(not winter)	0.113	0.115	0.132	0.132
	(0.052)	(0.055)	(0.060)	(0.061)
Observations	2445080	2445080	2445080	2445080
within R-squared	.0002	.0005	.0002	.0005
State x Season Fixed Effect	x	x	x	x
State x Educ. Fixed Effect	x	x	x	x
State x Year Fixed Effect	x	x	x	x
Season x Year Fixed Effect	x	x	x	x
Educ. x Season Fixed Effect	x	x	x	x
Weather Controls		x		x
Income Controls			x	x

Notes: Table 2 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 Table A1. Specifically, the reported association in the first row is the sum of ϕ and β_1 from equation (2) or the first and third row in Table A1. The reported association in the second row is the sum of ϕ and β_2 from equation (2) or the second and third row in Table A1. All reported parameters in a given column are from the same regression. The reported association in the third row is simply ϕ from equation (2) or the third row in Table A1. Columns differ by the controls added to estimating equation (2). Column (1) includes no controls. Columns (2-4) include weather and income controls. 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

	$\mathbb{1}$(bottle bill) $*$ $\mathbb{1}$(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
	(0.000080733916)
Medicare Benefits	-0.000111993138
	(0.000086417277)
Social Security Benefits	0.000061670310
	(0.000068007090)
Farm Wages and Salaries	0.000805451270
	(0.000312681947)
Wages and Salaries	-0.000027501019
	(0.000020945728)
Observations	2445080
within R-squared	.0029

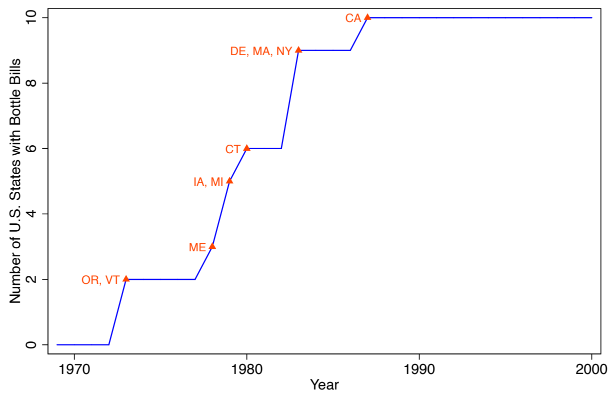
Notes: Table 3 displays the association between potential confounders and active bottle bills in non-winter months, the coefficients of interest from equation 4. Each row provides the association between a different confounder and the causal variable of interest. The balance test controls for state x 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.

Table 4: Pre-Treatment Placebo Test

	(1)	(2)	(3)	(4)	(5)
Low Education: $\mathbb{1}$(bottle bill) $*$ $\mathbb{1}$(not winter)	0.045	-0.186	-0.037	-0.070	-0.170
	(0.073)	(0.119)	(0.169)	(0.155)	(0.160)
Observations	2232048	2232048	2232048	2232048	2232048
within R-squared	.0003	.0003	.0003	.0003	.0003

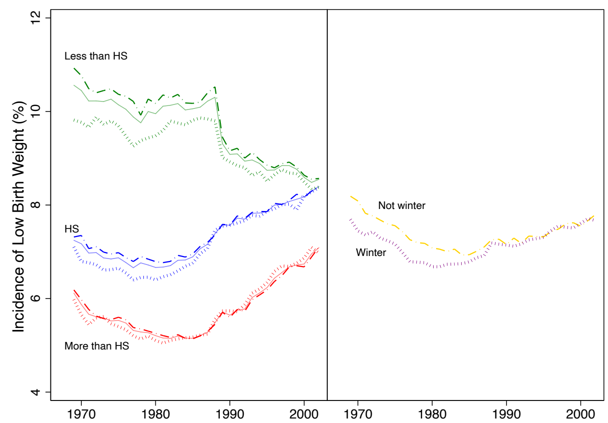
Notes: Table 4 reports pre-treatment placebo test associations between placebo bottle bill implementations in bottle bill states and the incidence of low birth weight in non-winter relative to winter months among low education mothers. Estimates are computed using equation (2) on the sample of untreated observations. Columns differ in the random assignment of placebo treatment years. For example, a different set of placebo bottle bill implementation years are assigned to bottle bill states in Column 1 and Column 5. 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 1: U.S. Bottle Bill Introductions



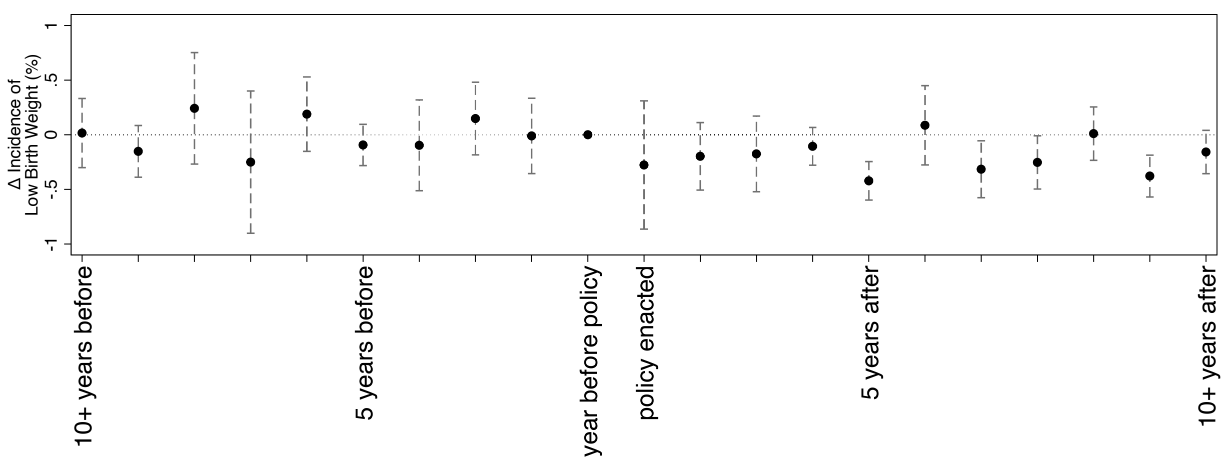
Notes: Figure 1 plots when each bottle bill state implemented bottle bills between 1969 and 2002. The blue line documents the total number of U.S. states with an active bottle bill in each year. The red triangle documents when each bottle bill state implemented the program. For example, in 1980, Connecticut implemented a bottle bill, making it the sixth U.S state to introduce a deposit refund program for containers.

Figure 2: Raw Trends in the Incidence of Low Birth Weight



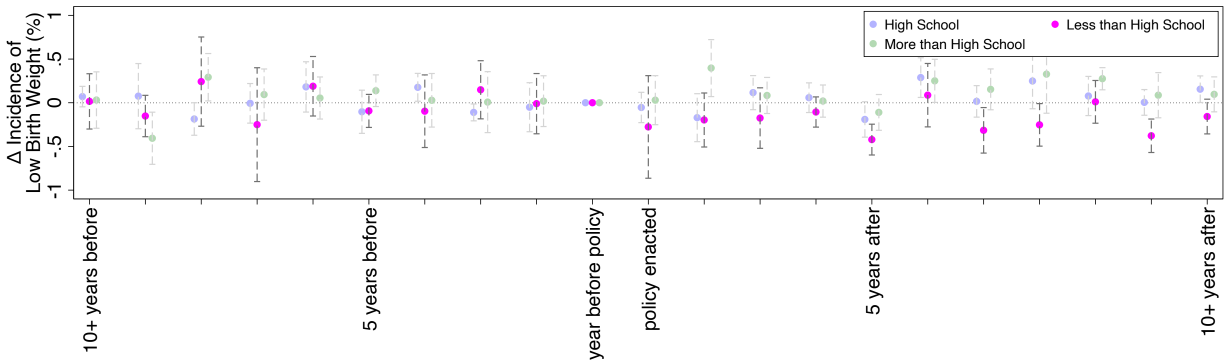
Notes: This figure plots the incidence of low birth weight in the U.S. between 1969 and 2002. The figure highlights how trends in the incidence of low birth weight varied by education group and season. I assign education groups to birth outcomes using the mother's educational attainment at the time of birth. I assign seasons to birth outcomes based on if the majority of the third trimester occurred in winter months or not. Dot dash lines represent rates in non-winter months. Dotted lines represent rates in winter months. Solid lines represent the rate for birth cohorts born in any season. I constructed these time series using U.S. Vital Statistics micro data from 1969-2002.

Figure 3: Association between Bottle Bills and the Incidence of Low Birth Weight – Main Result



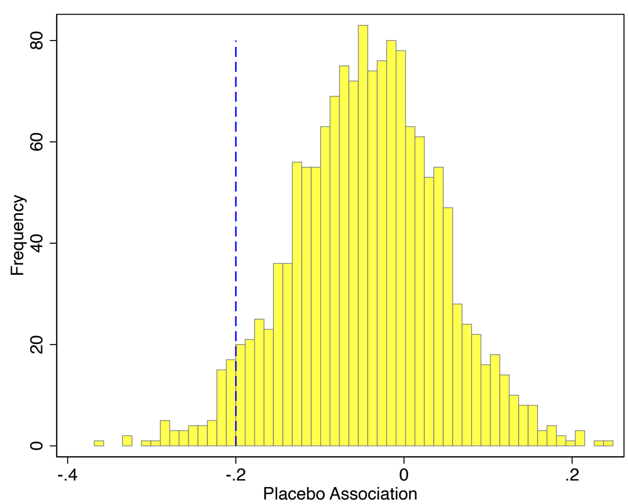
Notes: Figure 3 plots the association between bottle bill implementation and the incidence of low birth weight among mothers with less than a high school education in non-winter relative to winter months in each year before and after the policy took effect. These associations are computed using estimates from equation (3). The association in the year prior to policy implementation is normalized to zero. Figure A1 plots the associations visualized here with robustness to 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 4: Association between Bottle Bills and the Incidence of Low Birth Weight – Heterogeneity by Education Group



Notes: Figure 4 plots the association between bottle bill implementation 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. All associations are computed using estimates from equation (3). The association in the year prior to policy implementation is normalized to zero. The pink markers in this figure correspond to the associations displayed in Figure 3. 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 5: Permutation Test



Notes: Figure 5 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 61 out of 1,500 permutations estimate associations less than the actual association.

A Appendix

A.1 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 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 Figures 3 and 4). 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.

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 (see Figure A2). 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.[16]

Figure A2, 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 A2 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.

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

A.2 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. 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 2).

Specifically, I estimate the following model:

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

Y_{ctes} is the incidence of low birth weight in county c in year t for education group e in season s. $\mathbb{1}(\text{bottle bill})_{it} = 1$ 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: $\mathbb{1}(\text{bottle bill})_{it} * \mathbb{1}(\text{<HS})_e * \mathbb{1}(\text{not winter})_s$. 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.4 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. In section 4.4, I explore and discuss 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.4). 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 estimates (see Table A2), 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.3 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. Find the DiD estimator specifics in equations (A7, A8, A9). 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, I use a DiDiD estimator as the identification strategy relies on within state within year variation. For completeness, I define and estimate a DiD estimator as well. Specifically, I estimate the regression equation:

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

As before, $\mathbb{1}(\text{bottle bill})_{it} = 1$ 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:

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

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 A3. 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, weather, and government spending controls are added. Notably, the DiDiD results are much more robust than the DiD results to the introduction of controls. The DiDiD estimators rely on variation within a state within a year, whereas the DiD estimators rely on variation at the state level.

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. 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 A2).

A.4 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, a DiDiD estimator with heterogeneity by education group and 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.

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

$$\mathbb{E}[Y|T = 1, N = 0, \text{Post} = 0] = \beta_0 + \beta_1$$

$$\mathbb{E}[Y|T = 0, N = 1, \text{Post} = 0] = \beta_0 + \beta_2$$

$$\mathbb{E}[Y|T = 0, N = 0, \text{Post} = 1] = \beta_0 + \beta_3$$

$$\mathbb{E}[Y|T = 1, N = 1, \text{Post} = 0] = \beta_0 + \beta_1 + \beta_2 + \beta_4$$

$$\mathbb{E}[Y|T = 1, N = 0, \text{Post} = 1] = \beta_0 + \beta_1 + \beta_3 + \beta_5$$

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

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:

$$\begin{aligned}\hat{\beta}_7 = &\Big[(\hat{Y}_{T=1,N=1,\text{Post}=1} - \hat{Y}_{T=1,N=1,\text{Post}=0}) - (\hat{Y}_{T=0,N=1,\text{Post}=1} - \hat{Y}_{T=0,N=1,\text{Post}=0})\Big] \\ &- \Big[(\hat{Y}_{T=1,N=0,\text{Post}=1} - \hat{Y}_{T=1,N=0,\text{Post}=0}) - (\hat{Y}_{T=0,N=0,\text{Post}=1} - \hat{Y}_{T=0,N=0,\text{Post}=0})\Big]\end{aligned} \tag{A12}$$

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.

$$\mathbb{E}[Y|T=0,N=0,L=0,\text{Post}=0] = \beta_0$$
$$\mathbb{E}[Y|T=0,N=0,L=0,\text{Post}=1] = \beta_0 + \beta_4$$
$$\mathbb{E}[Y|T=0,N=0,L=1,\text{Post}=0] = \beta_0 + \beta_3$$

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

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:

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

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, its not entirely clear how well the group H triple difference estimator serves as a 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: $\beta_{12} + \beta_{15}$.[17] 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

[17] 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: $T * N * L * \text{Post}$.

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 $\mathbb{E}[Y_{ist}|T, N, L, \text{Post}]$.

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

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:

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

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: DiDiD Estimates

	(1)	(2)	(3)	(4)
$\mathbb{1}$(bottle bill) $*$ $\mathbb{1}$(<HS) $*$ $\mathbb{1}$(not winter)	-0.327	-0.323	-0.327	-0.323
	(0.060)	(0.059)	(0.060)	(0.059)
$\mathbb{1}$(bottle bill) $*$ $\mathbb{1}$(HS) $*$ $\mathbb{1}$(not winter)	-0.041	-0.038	-0.042	-0.039
	(0.032)	(0.033)	(0.033)	(0.033)
$\mathbb{1}$(bottle bill) $*$ $\mathbb{1}$(not winter)	0.113	0.115	0.132	0.132
	(0.052)	(0.055)	(0.060)	(0.061)
$\mathbb{1}$(bottle bill) $*$ $\mathbb{1}$(<HS)	-0.273	-0.276	-0.275	-0.278
	(0.329)	(0.329)	(0.330)	(0.330)
$\mathbb{1}$(bottle bill) $*$ $\mathbb{1}$(HS)	0.158	0.156	0.158	0.156
	(0.117)	(0.118)	(0.117)	(0.118)
Observations	2445080	2445080	2445080	2445080
within R-squared	.0002	.0005	.0002	.0005
State x Season Fixed Effect	x	x	x	x
State x Educ. Fixed Effect	x	x	x	x
State x Year Fixed Effect	x	x	x	x
Season x Year Fixed Effect	x	x	x	x
Educ. x Season Fixed Effect	x	x	x	x
Weather Controls		x		x
Income Controls			x	x

Notes: Table A1 reports all estimates from equation (2). All reported estimates in a given column are from the same regression. Columns differ by the controls added to estimating equation (2). Column (1) includes no controls. Columns (2-4) include weather and income controls. Each regression includes state x season, state x education group, season x education group, state x year, and season x 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.

Table A2: Association between Bottle Bills and the Incidence of Low Birth Weight – Quadruple Difference Estimator

	(1)	(2)	(3)	(4)
$\mathbb{1}$(bottle bill) $*$ $\mathbb{1}$(<HS) $*$ $\mathbb{1}$(not winter)	-0.148	-0.149	-0.147	-0.148
	(0.103)	(0.103)	(0.103)	(0.103)
Observations	2445077	2445077	2445077	2445077
within R-squared	0	.0003	.0001	.0004
State x Season x Educ. Fixed Effect	x	x	x	x
State x Educ. x Year Fixed Effect	x	x	x	x
State x Season x Year Fixed Effect	x	x	x	x
Season x Educ. x Year Fixed Effect	x	x	x	x
Weather Controls		x		x
Income Controls			x	x

Notes: Table A2 reports 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). Column (1) includes no controls. Columns (2-4) include weather and income controls. 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 A3: DiD Estimates

	(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	2445080	2445080	2445080	2445080	2445080	2445080	2445080	2445080	2445080	2445080	2445080	2445080
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

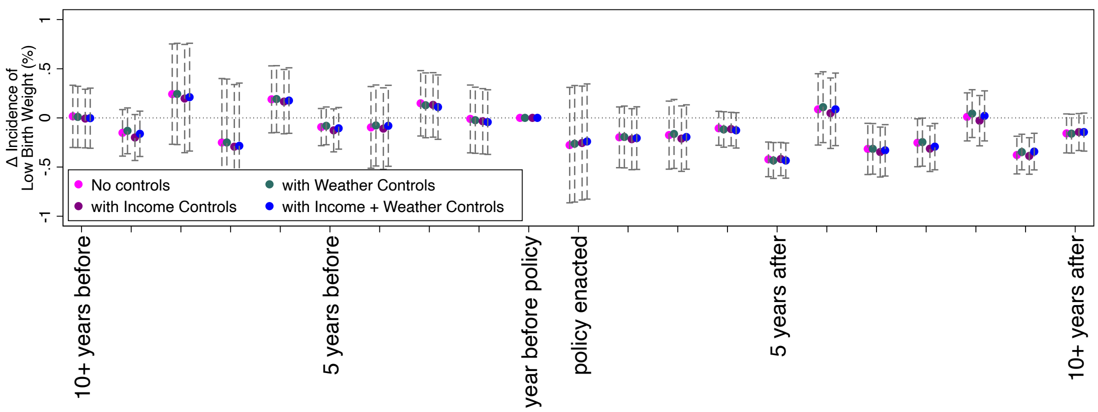
Notes: Table A3 reports all estimates from equations (A7, A9, A8). All reported estimates in a given column are from the same regression. Columns (1-4) report estimates from equation (A7); columns differ by the controls added to the estimating equation. Column (1) includes no controls. Columns (2-4) include weather and income controls. Columns (5-8) report estimates from equation (A9); 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); 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.

Table A4: Incidence of Low Birth Weight and Control Variable Means by Season and State Type

	Non-Bottle Bill States		Bottle Bill States	
	Non-Winter	Winter	Non-Winter	Winter
Incidence of Low Birth Weight (%)				
Low Education Mothers	10.27	9.65	8.69	8.52
	(6.79)	(6.61)	(4.51)	(4.44)
Middle Education Mothers	7.43	7.15	7.01	6.90
	(4.76)	(4.73)	(3.07)	(3.05)
High Education Mothers	5.88	5.84	5.66	5.74
	(5.40)	(5.40)	(3.47)	(3.52)
Income Controls (Millions $2015)				
Personal Income	21751.43	21477.66	58774.80	58218.10
	(16743.00)	(16517.98)	(44028.97)	(43569.10)
Personal Current Transfer Reciepts	2,705.47	2,663.02	7,461.92	7,355.78
	(2,238.68)	(2,217.71)	(5,378.60)	(5,318.60)
State Unemployment Insurance Benefits	95.44	94.36	280.45	278.79
	(95.33)	(94.55)	(205.57)	(202.58)
Medicare Benefits	499.29	489.04	1,282.15	1,261.31
	(538.59)	(531.99)	(1,068.68)	(1,057.47)
Social Security Benefits	1,130.96	1,113.51	2,471.97	2,442.34
	(883.20)	(875.78)	(1,578.92)	(1,567.81)
Farm Wages and Salaries	43.90	43.69	159.21	157.79
	(28.67)	(28.34)	(193.58)	(191.40)
Wages and Salaries	12196.04	12062.15	33347.40	33090.08
	(9,094.08)	(8,983.14)	(23803.73)	(23587.53)
Weather Controls (Cumulative $^\circ C$)				
HDDs	418.70	1,380.07	537.46	1,474.99
	(437.15)	(602.76)	(446.67)	(549.23)
CDDs	240.16	19.36	131.55	7.68
	(216.20)	(47.73)	(122.99)	(19.45)

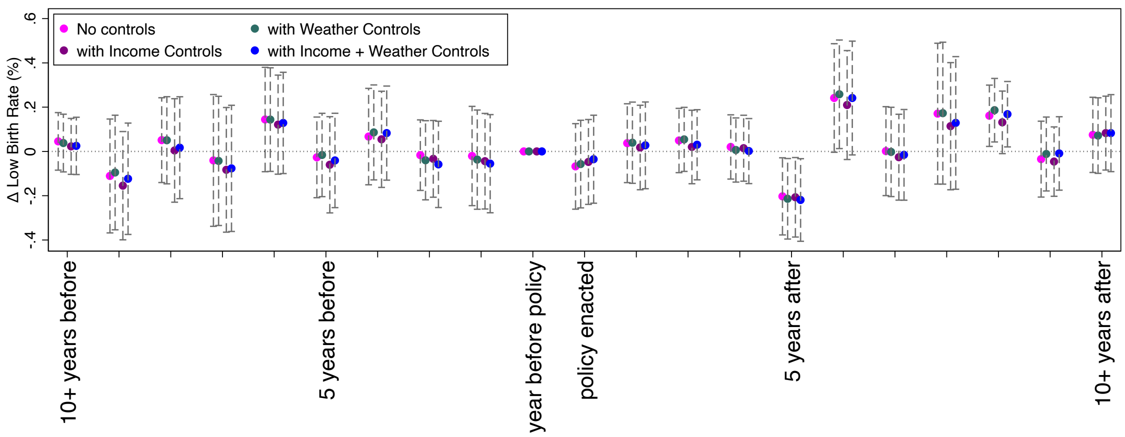
Notes: Table A4 reports means for the incidence of low birth weight and control variables employed in the paper. The standard deviation for each variable is reported in parenthesis under the weighted average. The first two columns report means for non-bottle bill states. The second two columns report means for bottle bill states. The first and third column report means for non-winter months, and the second and fourth column report means for winter months. For example, the first row in the last column reports the incidence of low birth weight in bottle bill states in winter months for low education mothers is 8.52 %. The first three rows document incidence of low birth weight statistics for each education group. The rest of the rows document means for control variables. I construct incidence of low birth weight summary statistics using U.S. Vital Statistics micro data from 1969-2002. I construct income control summary statistics for the same time period using data from the BEA, Regional Economic Information System. Lastly, I use ERA5-Land to construct weather controls.

Figure A1: Association between Bottle Bills and the Incidence of Low Birth Weight – Main Result Robustness



Notes: Figure A1 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. 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 A2: Average Association between Bottle Bills and the Incidence of Low Birth Weight



Notes: Figure A2 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. 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.