Discards, Wealth Transfers, and Informal Wages

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Abstract

This paper examines if policy that subsidizes beverage container recycling through a deposit refund scheme improves birth outcomes in low income populations. A very simple economic model of recycling participation suggests that recycling for cash transfers wealth to low-wage earners. Between 1973 and 1990, ten states introduced deposit refund programs for beverage containers. This paper exploits idiosyncratic variation in the timing and location of policy implementation to measure for any reduction in the incidence of low birth weights associated with deposit refund programs. The results show deposit refund policy introductions are associated with a .3 percentage point reduction in the incidence of low birth weight on average among mothers with less than a high school education. I rely on the literature connecting wealth transfers and birth outcomes to argue this result extends a small empirical literature highlighting the progressive nature of beverage container deposit refund programs.

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

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1 Introduction

This paper suggests that waste collection and processing can create important and overlooked wealth transfers. One need not look further than Leviticus 19:9-10, for a historical reference to the waste stream's perceived importance in the social safety net:

> 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.

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 affectionately 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. 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 (Reloop, 2020). 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 deposition. Other policy instruments, such as Pigouvian taxes, create incentives to illegally dump waste (Fullerton & Wolverton, 2000). Beyond an efficient and effective waste policy, bottle bills possibly increase

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

the waste stream's wage potential.

The legislation creates a market around empty collection. Bottle bills require consumers to pay deposits on beverages at purchase. The programs return deposits to people who properly return empties. Importantly, the person who pays the tax does not need to be the same person to collect the return. If everyone does not collect their refunds, 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 conclusion, 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, 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. My analysis provides evidence that the waste stream transfers wealth to low income households. 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. Second, I employ birthweight as a proxy for economic wellbeing in low income populations. Together, these strategies allow me to test if an increase in the recycling wage improves economic wellbeing in low income populations.

In July 1971, Oregon passed the first bottle bill in the United States. Two years prior, 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 (ODEQ, 2022). 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 (RCC, 1978). Of the states that successfully implemented bottle bills, most initially failed to enact the legislation (Davis, 1982; Franchot, 1978; ODEQ, 2022; Peterson, 1976; RCC, 1978; Ross, 1982; White, 2018). In other words, the timing and location of bottle bill implementation was idiosyncratic.

I exploit the idiosyncratic timing and location of bottle bills' implementation to identify the law's impact on the incidence of low birthweight (low birth rate). Additionally, I employ treatment heterogeneity to provide suggestive evidence that wealth transfers play a role in the law's impact on the low birth rate. 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 months relative to winter months. I find bottle bills are associated with a .3 percentage point (pp) reduction in the incident of low birthweights for populations with less than a high school education. I find the effect lasts for more than ten years after policy implementation and the effect is stronger in non winter months relative to winter months. For context, after bottle bill introduction within the sample period, on average 30,000 low education mothers had live births in non winter months per year in New York state. During this time, the low birth rate was roughly 10% in non winter months for low education mothers. Thus, the finding that bottle bills are associated with a .3 pp reduction in the low birthweight among low education mothers suggests on average at least 90 new borns were positively impacted by bottle bills annually.

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

Second, my analysis adds to the relatively well developed literature on income and birthweight. WIC, SNAP, EITC, and OLO are all policies in North America that transfer wealth to low income mothers or people. These policies were all established to improve maternal and infant health and/or provide a social safety net. Effectively all programs transfer wealth to low income communities. Almond et al. (2011), Haeck and Lefebvre (2016), Hoynes et al. (2015), and Hoynes et al. (2011), Rossin-Slater (2013) find these policies improve birth outcomes in low income populations. My analysis extends this literature by studying the relationship between the low birth rate and bottle bills, another policy that effectively transfers wealth to low income communities. Unlike this literature, my work suggests that simply creating the potential for wealth transfers improves birth outcomes to a degree comparable to highly targeted policy. I find bottle bills reduce the incidence of low birthweight to a similar extent as SNAP and EITC. 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. Hoynes et al. (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.

At first glance, it's surprising that SNAP and EITC are associated with low birth rate reductions to a similar degree as bottle bills. Aggregate wealth transfers associated with EITC and SNAP far exceed that of the aggregate potential wealth transfers associated with bottle bills. New York state SNAP benefits in 2016 amounted to $4 billion distributed to 2.97 million people.[2] An unknown number of New York state residents participated in recycling 5.1 billion containers in 2016, amounting to $255 million in refunds. Moreover, 2.68 billion containers were never redeemed. In other words, $255 million were paid to a portion of the 19.64 million people living in the state.

When compared at the individual or household level, plausible wealth transfers associated with bottle bills are similar in magnitude to that of SNAP. Currently SNAP benefits are on average $200 per recipient household per month. Collecting six garbage bags of empties a week amounts to a monthly recycling income of $240. 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 SNAP and EITC drive low birth rate results for these programs, then the comparability of results from SNAP and EITC 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 low birth rate and the implementation of bottle bills. This discussion helps motivate my empirical approach. Second, I discuss the data used in the analysis. Third,

[2]These figures are available from the US Department of Agriculture.

I outline my empirical approach. Fourth, I share my results. Lastly, I test the integrity of the quasi-experimental design employed.

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 (RCC, 1977). Legislators established bottle bills as beverage consumption shifted from returnables to single use.[3] 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 deposition.

Bottle bills curb litter and reduce waste; their connection to birth outcomes is not first order (RCC, 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[4] 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[5] 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 birthweight. In the next section, I argue all three of these mechanisms do not fully explain the long run (> 3 years) impact of bottle bills on the seasonal difference in the low birth rate. Then, I argue a fourth mechanism, wealth transfers, helps to more fully explain the association.

[3] 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 (RCC, 1978). With an 8% growth rate, beverage containers compromised roughly 7% of municipal solid waste in 1977 (RCC, 1977).

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

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

2.2 Potential Mechanisms

Four potential mechanisms likely drive the association between bottle bills and birthweight. 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 birthweight. 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).[6] As a consequence, beer and soda consumption decreased overall. This reduction possibly improved birth outcomes, leading to an association between bottle bill implementation and the incidence of low birthweight. In all states where price shocks occurred, sales are thought to have recovered after a couple years.[7] I find the association between bottle bills and the low birth rate 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 birthweight 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

[6] Michigan saw the most severe price shock, where prices rose 10-16% (Porter, 1983). While Oregon experienced negative direct price shocks (RCC, 1977).

[7] 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.

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 birthweight. Though, Chay and Greenstone (2003) find little association between reductions in TSP and the incidence of low birthweight 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 birthweight.[8]

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 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. Thus, bottle bills induce the largest wealth transfers in non winter months 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.

[8]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.

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 birthweight 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. 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, Massachusetts and New York implemented the bill. 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 is likely largest in non winter months relative to winter months. In winter months beverage consumption decreases and empty collection is harder. Thus, bottle bills 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 low birth rates 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 1, find the raw trends in the U.S. low birth rate during the sample period. The plot demonstrates heterogeneity in the low birth rate by season and education group during the sample period. There are significant within year across season and across education group differences in low birth rate that we cannot assume are constant across states. Thus, an empirical strategy relying on cross sectional differences in seasonal or educational differences in the low birth rate 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 bottle bills on the low birth rate.

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 weights to the county by month level from ERA5-Land. Weather controls include cumulative exposure during the third trimester to cooling degree days (CDDs) and heating degree day (HDDs). The threshold used to construct both CDDs and HDDs is 20 degrees celsius.

4 Econometric Model

I use a differences-in-differences-in-differences (DiDiD) estimator to identify the impact of bottle bills on the incidence of low birthweight. 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 10 states that implemented bottle bills to the 40 states that did not. Third, I compare winter to non-winter months as the wealth transfer is much larger in non winter relative to winter months.

Specifically, I estimate the following model:

$$Y_{ctes} = \beta \left[\mathbb{1}(\text{BB Implemented})_{it} * \mathbb{1}_{s=\text{not winter}}\right] + \alpha_{i,s} + \mu_{i,e} + \delta_{i,t} + \gamma_{s,t} + \lambda_{e,s} + \epsilon_{ctes} \quad (1)$$

Y_{ctes} is the incidence of low birthweight in county c in year t for education group e in season s. $\mathbb{1}(\text{BB Implemented})_{it} = 1$ if a bottle bill is implemented in state i in year t. $\mathbb{1}_{s=\text{not winter}} = 1$ if 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. Birth outcomes are assigned to seasons based on when the majority of a pregnancy's third trimester occurred. I assign birth outcomes to education groups using the mother's education. I use three education groups: less than a high school education, a high school education, and more than a high school education. To operationalize the DiDiD estimator, I include three sets of two way fixed effects. $\delta_{i,t}$ 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. For consistency across specifications, I include two more sets of two way fixed effects. $\mu_{i,e}$ is a state by education group fixed effect; this fixed effect allows for education group by state differences between states that implement and never enact bottle bills 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.

This specification estimates the difference in the low birth rate's seasonality between a world with bottle bills versus a world without bottle bills. The identifying assumption is that the seasonal difference in the low birth rate 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 low birth rate 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, and in non winter months relative to winter months. This only leaves variation at the level of bottle bill implementations.

Separate measures of bottle bills' effect in each year before and after implementation provide useful information particularly in testing the integrity of the DiDiD estimator's identifying assumptions. Hence I also estimate the following model:

$$Y_{ctes} = \sum_{j=-10}^{10} \left(\eta_j * \mathbb{1}_{(t,i)\in j} * \mathbb{1}_{s=\text{not winter}}\right) + \alpha_{i,s} + \mu_{i,e} + \delta_{i,t} + \gamma_{s,t} + \lambda_{e,s} + \epsilon_{ctes} \tag{2}$$

$\mathbb{1}_{(t,i)\in j} = 1$ 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. I plot η_j to provide visual evidence that equation (1)'s identifying assumptions are not invalid. Namely, the event study graphs provide an opportunity to asses if there are pre-bottle bill trends.

Finally, I report results for equation (1) and (2) with heterogeneity by education group. Specifically, I estimate the following DiDiD estimators:

$$\begin{aligned}
Y_{ctes} = {} & \beta_1(\mathbb{1}(\text{BB Implemented})_{it} * \mathbb{1}_{e<HS} * \mathbb{1}_{s=\text{not winter}}) \\
& + \beta_2(\mathbb{1}(\text{BB Implemented})_{it} * \mathbb{1}_{e=HS} * \mathbb{1}_{s=\text{not winter}}) \\
& + \theta_1(\mathbb{1}(\text{BB Implemented})_{it} * \mathbb{1}_{e<HS}) \\
& + \theta_2(\mathbb{1}(\text{BB Implemented})_{it} * \mathbb{1}_{e=HS}) \\
& + \lambda_{e,s} + \alpha_{i,s} + \mu_{i,e} + \delta_{i,t} + \gamma_{s,t} + \epsilon_{ctes}
\end{aligned} \tag{3}$$

The subscripts have the same meaning as in equations (1) and (2) and the equation includes the same fixed effects as equation (1). This specification estimates the difference in the low birth rate's seasonality between a world with bottle bills versus a world without bottle bills for the education group with a high school education and with less than a high school education. The identifying assumption is the same as before; the seasonal difference in the low birth rate did not differentially change in bottle bill states versus non bottle bill states through a channel other than bottle bills during the sample period for the education group of interest.

The parameters of interest are β_1 and β_2. After adjusting for fixed effects, β_1 captures the variation in the low birth rate 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 non winter months relative to winter months, and for mother's with less than a high school education relative to mothers with more than high school education. β_2 captures the same variation as β_1 except for mother's with less than a high school education relative to mothers with more than a high school education.

As before, separate measures of bottle bills' effect in each year before and after implementation provide useful information particularly in testing the integrity of the DiDiD estimator's identifying assumptions. Hence I also estimate the following model:

$$\begin{aligned} Y_{ctes} = \sum_{j=-10}^{10} \phi_j \left(\mathbb{1}_{(t,i)\in j} * \mathbb{1}_{s=\text{not winter}}\right) + \sum_{e} \theta_{j,e} \left(\mathbb{1}_{(t,i)\in j} * \mathbb{1}_{e}\right) + \eta_{j,e} \left(\mathbb{1}_{(t,i)\in j} * \mathbb{1}_{e} * \mathbb{1}_{s=\text{not winter}}\right) \\ + \mu_{i,e} + \lambda_{e,s} + \alpha_{i,s} + \delta_{i,t} + \gamma_{s,t} + \epsilon_{ctes} \qquad (4) \end{aligned}$$

As before, $\mathbb{1}_{(t,i)\in j} = 1$ 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. In the summations over education groups, the excluded education group is mother's with more than a high school education. I plot η_j for the less than high school education group to visually test the identifying assumptions associated with β_1 from equation (3). Similarly, I plot η_j for the high school education group to visually test the identifying assumptions associated with β_2 from equation (3). Lastly, I plot ϕ_j to asses whether patterns in η_j are driven by variation in the pre or post trend for the more than high school education group.

Bottle bill implementations provide identifying variation at the state level. I use a DiDiD estimator to rely on within state within year identifying variation. To explore the robustness of the DiDiD estimator, I also employ a differences in differences (DiD) estimator. The estimator compares treated versus never treated states in the pre versus post period.

Specifically, I estimate the following model:

$$Y_{ctes} = \beta * \mathbb{1}(\text{BB Implemented})_{it} + \alpha_{i,s} + \mu_{i,e} + \gamma_t + \epsilon_{ctes} \tag{5}$$

As before, $\mathbb{1}(\text{BB Implemented})_{it} = 1$ if a bottle bill is implemented in state i during year t. Additionally, Y_{ctes} is the low birth rate 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 low birth rate between a world with bottle bills versus a world without bottle bills. The identifying assumption is that the low birth rate 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 low birth rate 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.

Goodman-Bacon (2021) highlights potential issues with the above interpretation of a DiD estimator. Given only ten units are ever treated during the sample period, the issues raised by Goodman-Bacon (2021) are of little consequence in this context because such a small weight is placed on comparisons of concern.

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

$$
\begin{aligned}
Y_{ctes} = \beta_1 * \mathbb{1}(\text{BB Implemented})_{it} + \beta_2 \left(\mathbb{1}_{s=\text{not winter}} * \mathbb{1}(\text{BB Implemented})_{it}\right) \\
+ \alpha_{i,s} + \mu_{i,e} + \gamma_t + \epsilon_{ites} \qquad (6)
\end{aligned}
$$

$$
\begin{aligned}
Y_{ites} = \beta_1 * \mathbb{1}(\text{BB Implemented})_{it} + \beta_2 \left(\mathbb{1}_{e=HS} * \mathbb{1}(\text{BB Implemented})_{it}\right) \\
+ \beta_3 \left(\mathbb{1}_{e<HS} * \mathbb{1}(\text{BB Implemented})_{it}\right) + \alpha_{i,s} + \mu_{i,e} + \gamma_t + \epsilon_{ctes} \qquad (7)
\end{aligned}
$$

In equation (6), β_1 captures the variation in the low birth rate 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 additional variation in the low birth rate in non winter months relative to 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 (7), β_1 captures the variation in high education mothers' low birth rate 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' low birth rates 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.

Additionally, for estimation equations (1-7) I include controls to test result robustness. Specifically, I include the following controls: personal income, wages and salaries, farm wages and salaries, personal current transfer receipts, medicare benefits, state unemployment insurance compensation, social security benefits, cooling degree days (CDDs), and heating degree days (HDDs). Each variable has been aggregated to approximate each birth's *exposure* during the third trimester. For each estimating equation listed above, I estimate the equation with no controls, temperature controls, income controls, and both temperature and income controls. Temperature controls include CDDs and HDDs. All other controls are designated as income controls. To understand how different sets of controls affect the results, I vary the controls included across robustness tests.

5 Results

Bottle bills should induce larger wealth transfers in non winter versus 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 relative to winter months. Estimating equation (1) seeks to test if bottle bills have a differential impact on the low birth rate in non winter months relative to winter months. Specifically, the equation measures differences between treated versus never treated states in the low birth rate's seasonality in the pre treatment versus post treatment periods. As reported in Table 1, estimates from equation (1) do not suggest that bottle bills impact the seasonal difference in the low birth rate on average.

With estimating equation (2), I test equation (1)'s identifying assumption: the seasonal difference in the low birth rate did not differentially change in bottle bill states versus non bottle bill states through a channel other than bottle bills during the sample period. As reported in Figure 2, estimates from equation (2) shows no pre-bottle bill trends in the low birth rate's seasonality, suggesting in the pre treatment period the identifying assumption holds. Specifically, Figure 2 displays estimates measuring differences between treated versus never treated states in the low birth rate's seasonality, separately by policy relative year, with the year prior to bottle bill implementation normalized to zero. Consistent with Table (1), Figure 2 shows no impact of bottle bills on the low birth rate's seasonality on average; bottle bills do not change the low birth rate in non winter months relative to winter months. Coefficients show no trends or systematic patterns. Coefficients oscillate around zero in the pre and post period and are rarely statically significant.

Bottle bills should transfer wealth to low wage earners. This socio-economic differential implies that if bottle bills improve birth outcomes through a wealth transfer channel, then the improvements should occur among the lowest wage earners. Moreover, the improvements should be largest in non winter months relative to winter months. Estimating equation (3) seeks to test if bottle bills have a differential impact on the low birth rate of low income populations in non winter months relative to winter months. Due to data constraints I use education as a proxy for wage; I look at the impact of bottle bills on populations with less than a high school education to approximate the policy's impact on low wage populations. More specifically, coefficients of interest from equation (3) measure differences between treated and never treated states in the low birth rate's seasonality in the pre treatment versus post treatment periods, with heterogeneity by educational attainment. As reported in Table (1), on average bottle bills are associated with a reduction in the low birth rate of low education mothers in non winter months relative to winter months. The estimates suggest bottle bills reduce the incidence of low birthweight among low education mothers by roughly .3 percentage points (pp).

With estimating equation (4), I test equation (3)'s identifying assumption: the seasonal difference in the low birth rate did not differentially change in bottle bill states versus non bottle bill states through a channel other than bottle bills during the sample period for each education group. As reported in Figure 3, estimates for equation (4) show no pre-bottle bill trends in the low birth rate's seasonality for each education group, suggesting the identifying assumption holds during the pre-bottle bill period. Specifically, Figure 3 displays estimates measuring the impact of bottle bills in policy relative time on the low birth rate's seasonality for all three education groups. I normalize the year prior to bottle bill implementation to zero. There is no pre-bottle bill trend in the estimates associated with low education mothers and there is a clear drop in the estimates in the post period. Bottle bills are associated with a reduction in the incidence of low birthweight for low education mothers in non winter months relative to winter months. Additionally, Figure 3 shows bottle bills have no clear impact on the low birth rate's seasonality for mothers with at least a high school education. For both high school (middle) and more than high school (high) education groups, coefficients show no obvious trends or systematic patterns. Coefficients oscillate around zero in the pre and post bottle bill period and are rarely statistically significant. I test the robustness of these conclusions by adding various sets of controls to equation (4). The robustness results for low education mother's are displayed in Figure 4. Including controls does not change the qualitative conclusion that there are no pre-bottle bill trends in the low birth rate's seasonality for low education mothers.

It is informative to compare these statistics to sample averages from New York state to understand the effect magnitude. After treatment within the sample period, there were on average 30,000 low education mothers who had live births in non winter months per year in New York state. During this time, the low birth rate was roughly 10% in non winter months for low education mothers. Thus, the finding that bottle bills are associated with a .3 pp reduction in the low birthweight among low education mothers suggests on average at least 90 new borns were positively impacted by bottle bills annually.

The effect of bottle bills on the low birth rate for low education mothers in non winter months fluctuates. In the post-bottle bill period, the association oscillate between a .5 pp reduction and a .1 pp reduction. Wealth transfers associated with bottle bills are a function of many different forces. Unemployment rates, beverage consumption, population density, laws around source separation, tourism, welfare spending, etc. jointly impact the number of recyclers and the recycling wage. In other words, wealth transfers associated with bottle bills likely fluctuate as local economic conditions and waste practices vary through time. Fluctuations in bottle bill wealth transfers could explain why associations between deposit refund programs and the low birth rate vary in the post period. Moreover, the complex process through which bottle bills transfer wealth to low income populations makes the introduction of controls potentially concerning. Weather, income and government spending likely impact birth outcomes both through bottle bill wealth transfers and other channels. Thus, I display all results with and without controls.

Lastly, conclusions from the DiDiD results are mirrored in the DiD analysis with and without controls. Table 2 reports DiD results measuring differences between treated and never treated states in the low birth rate in the pre versus post bottle bill periods. Specifically, Table 2, column 1 reports the coefficient of interest from estimation equation (5). Table 2, Column 1, shows bottle bills are associated with a .3 pp reduction in the low birth rate. Table 2, column 5, highlights this association is largest for low education mothers. Table 2, column 9, demonstrates the association predominantly occurs in non winter months. Once income, government welfare spending, and weather controls are added the DiD estimates attenuate varying amounts. The largest attenuation appears after controlling for income and government welfare spending when estimating the raw association between bottle bills and the low birth rate. Without controls, bottle bills are associated with a .3 pp reduction in the low birth rate. With income and government spending on welfare controls, Table 2, Columns (3-4), bottle bills are associated with a .16 pp reduction in the low birth rate. The DiDiD results are 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.

6 Robustness

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 of interest. If the causal variable of interest 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 et al., 2019). Moreover, the conclusion from a balance test can be recovered by regressing the causal variable of interest on confounders in a case with multiple confounding variables. In the context of this study, the causal variable of interest is the implementation of bottle bills in the U.S. in non-winter months conditional on fixed effects. Thus, I test for omitted variable bias by regressing the implementation of bottle bills in the U.S. in non winter months on a rich set of potential confounders. More specifically, I estimate the following regression:

$$\mathbb{1}(\text{BB Implemented})_{it} * \mathbb{1}_{s=\text{not winter}} = \sum_{v} \beta_v * v + \alpha_{i,s} + \delta_{i,t} + \gamma_{s,t} + \epsilon_{i,s,t} \tag{8}$$

As before, $\mathbb{1}(\text{BB Implemented})_{it} = 1$ if a bottle bill is implemented in state i in year t. $\mathbb{1}_{s=\text{not winter}} = 1$ if s is not winter. $\delta_{i,t}$ 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. $\sum_v$ sums over all controls included in previous robustness analyses.

Table (3) displays the results from this robustness analysis. Notably, the association between farm salaries and wages with the causal variable of interest 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.

7 Conclusion

Ashenmiller (2009, 2010, 2011) shows bottle bills transfer wealth to low income households. 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 birthweight. Such policies create comparable reductions in the incidence of low birth rate to that of EITC and SNAP (Almond et al., 2011; Hoynes et al., 2015). Moreover, this work provides suggestive evidence that bottle bills transfer wealth to lower income households, adding to already existing results on the topic.

Specifically, I find that the introduction of bottle bills is associated with a large and persistent improvement in the incidence of low birth weights 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 this wealth transfer drives the association between bottle bill introductions and infant health documented in this paper.

Additionally, 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 causes these gaps and why is the waste stream able to fill them so effectively?

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Table 1: DiDiD – Impact of Bottle Bills on Low Birth Rate's Seasonality

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bottle Bill x Not Winter	0.039	0.043	0.058	0.059	0.113	0.115	0.132	0.132
	(0.037)	(0.040)	(0.045)	(0.047)	(0.052)	(0.055)	(0.060)	(0.061)
... x Less than HS					-0.273	-0.276	-0.275	-0.278
					(0.329)	(0.329)	(0.330)	(0.330)
... x HS					0.158	0.156	0.158	0.156
					(0.117)	(0.118)	(0.117)	(0.118)
... x Not Winter x Less than HS					-0.327	-0.323	-0.327	-0.323
					(0.060)	(0.059)	(0.060)	(0.059)
... x Not Winter x HS					-0.041	-0.038	-0.042	-0.039
					(0.032)	(0.033)	(0.033)	(0.033)
Observations	2445080	2445080	2445080	2445080	2445080	2445080	2445080	2445080
within R-squared	0	.0003	.0001	.0004	.0002	.0005	.0002	.0005
State x Season Fixed Effect	x	x	x	x	x	x	x	x
State x Educ. Fixed Effect	x	x	x	x	x	x	x	x
State x Year Fixed Effect	x	x	x	x	x	x	x	x
Season x Year Fixed Effect	x	x	x	x	x	x	x	x
Educ. x Season Fixed Effect	x	x	x	x	x	x	x	x
Weather Controls		x		x		x		x
Income Controls			x	x			x	x

Notes: The estimates in Table 1 are the coefficients from DiDiD estimators described in equation (1) and equation (3). The coefficients report the estimated impact of bottle bills on low birth rates in non winter months relative to winter months for different subsets of births. Each column includes coefficients from separate regressions. Column (1) reports the estimated coefficient of interest from equation (1). The column reports that bottle bills on average are not associated with the low birth rate's seasonality; bottle bill introductions are not associated with the low birth rate in non winter months relative to winter months. Columns (2-4) provide robustness for this finding; the estimating equations for these columns include weather and income controls. Column (5) looks at heterogeneity across education groups in the association between bottle bills and the low birth rate's seasonality. Column (5) reports the estimated coefficients of interest from equation (3). In Column (5), the coefficient associated with the *Bottle Bill x Not Winter* row is the association between bottle bills and the seasonal difference for mother's with more than a high school (high) education. Column (5), row (4) reports bottle bills are associated with a .3 percentage point (pp) reduction in the seasonal difference in the low birth rate for mothers with less than a high school (low) education relative to high education mothers. Column (5), row (5) reports bottle bills are associated with a .04 pp reduction in the seasonal difference in the low birth rate for mothers with a high school (middle) education relative to high education mothers. Columns (6-8) provide robustness for these findings. The estimating equations for these columns 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 reported standard errors are clustered at the state level. Weather controls include cumulative exposure to CDDs and HDDs during the third trimester. 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.

Table 2: DiD – Impact of Bottle Bills on the Incidence of Low Birthweight

	(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

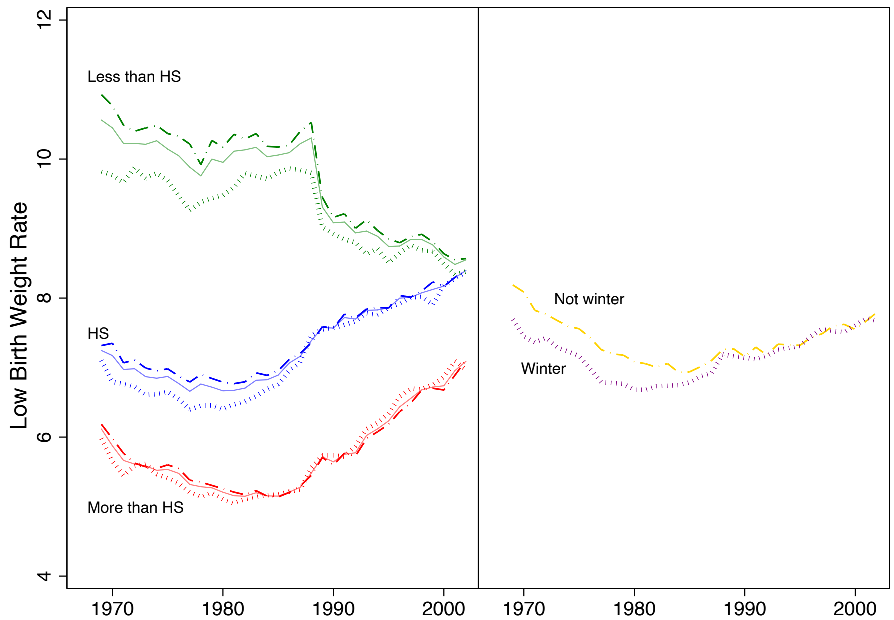
Notes: The estimates in Table 2 are the coefficients from DiD estimators defined in equations (5-7). The coefficients report the estimated impact of bottle bills on low birth rates for different subsets of births. Each column includes coefficients from separate regressions. Column (1) reports that bottle bills are associated with a .35 pp reduction in the incidence of low birthweight. This result attenuates, when income controls are added in Columns (3-4). Column (5) looks at heterogeneity in this association across education groups. In Column (5), *Bottle Bill* row provides estimates of the association between bottle bills and high education mothers' low birth rate. Column (5), row (2) reports estimates of the association between bottle bills and low education mothers' low birth rate. Columns (6-8) explore result robustness as income and weather controls are added to the estimating equation. Column (9) looks at heterogeneity in the association across seasons. In Column (9), the *Bottle Bill* row provides estimates of the association between bottle bills and the winter low birth rate. Column (3), row (2) reports the association between bottle bills and the non winter low birth rate. Columns (10-12) explore result robustness as income and weather controls are 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 reported standard errors are clustered at the state level. Weather controls include cumulative exposure to CDDs and HDDs during the third trimester. 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.

Table 3: Balance Test

	(1)
HDDs (degress C)	0.000000203866
	(0.000001399792)
CDDs (degrees C)	0.000002302818
	(0.000003387572)
Personal Income (2015 USD)	0.000000000001
	(0.000000000001)
Personal Current Transfer Reciepts (2015 USD)	0.000000000001
	(0.000000000001)
State Unemployment Insurance Benefits (2015 USD)	-0.000000000007
	(0.000000000008)
Medicare Benefits (2015 USD)	-0.000000000011
	(0.000000000009)
Social Security Benefits (2015 USD)	0.000000000006
	(0.000000000007)
Farm Wages and Salaries (2015 USD)	0.000000000081*
	(0.000000000031)
Wages and Salaries (2015 USD)	-0.000000000003
	(0.000000000002)
Observations	2445080
within R-squared	.0029

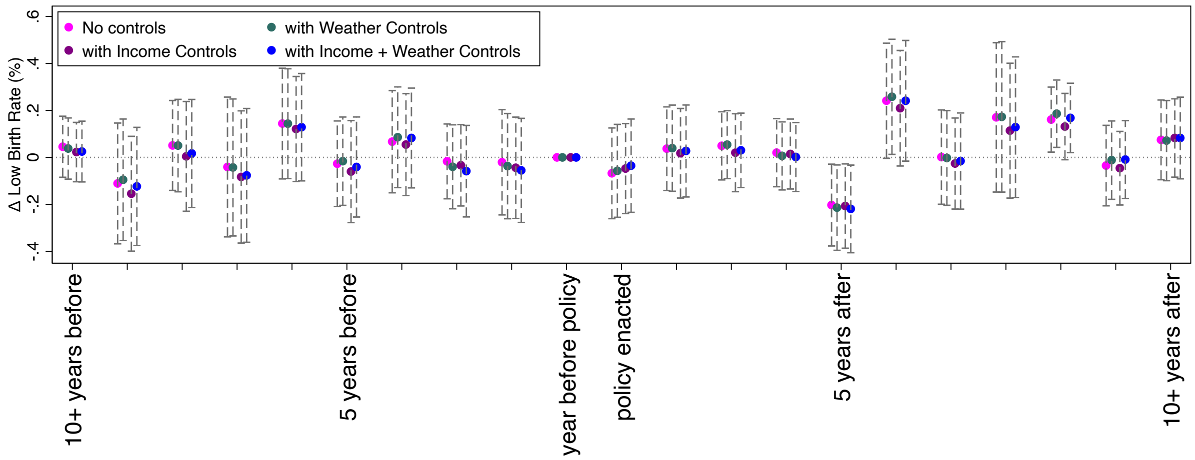
Notes: Table 3 displays the estimates of the coefficients of interest from the balance test defined in equation (8). Each row provides the association between a different confounder and the causal variable of interest. The balance test controls for state x season, season x year, and state x year fixed 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.

Figure 1: Raw Trends in the Low Birth Rate



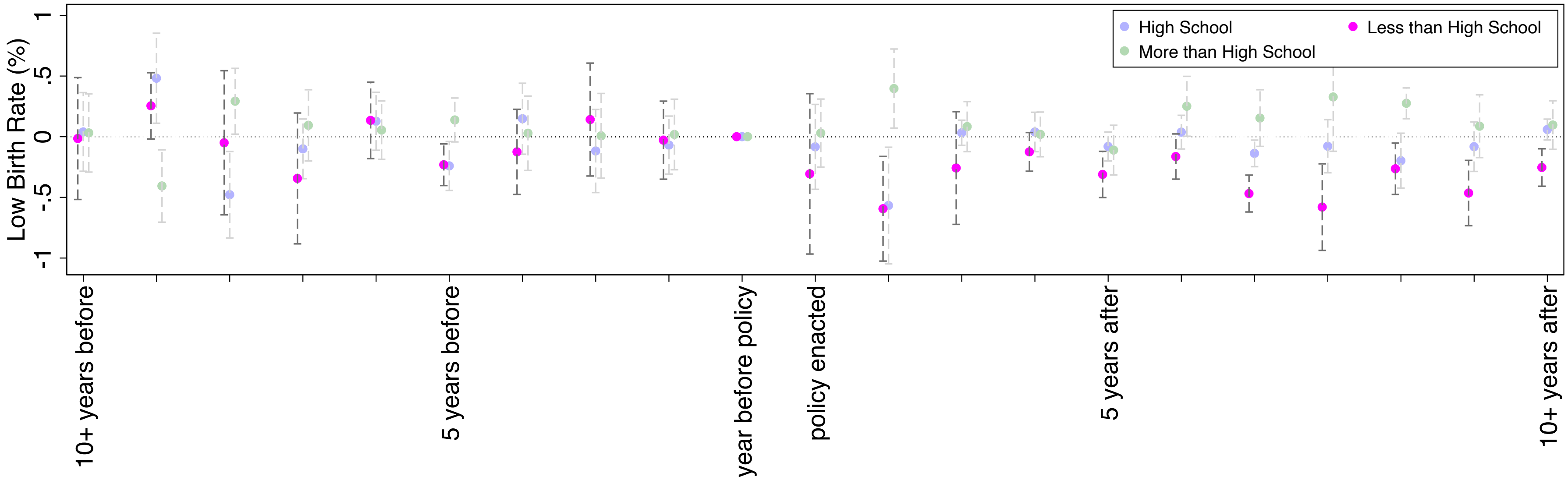
Notes: This figure plots the incidence of low birthweight in the U.S. between 1969 and 2004. The figure highlights how trends in the low birth rate vary by education group and season. I assign education groups to birth outcomes using the mother's educational attainment at the time of birth. I assign 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 2: Event Study for Low Birth Rate in Non Winter Relative to Winter Months



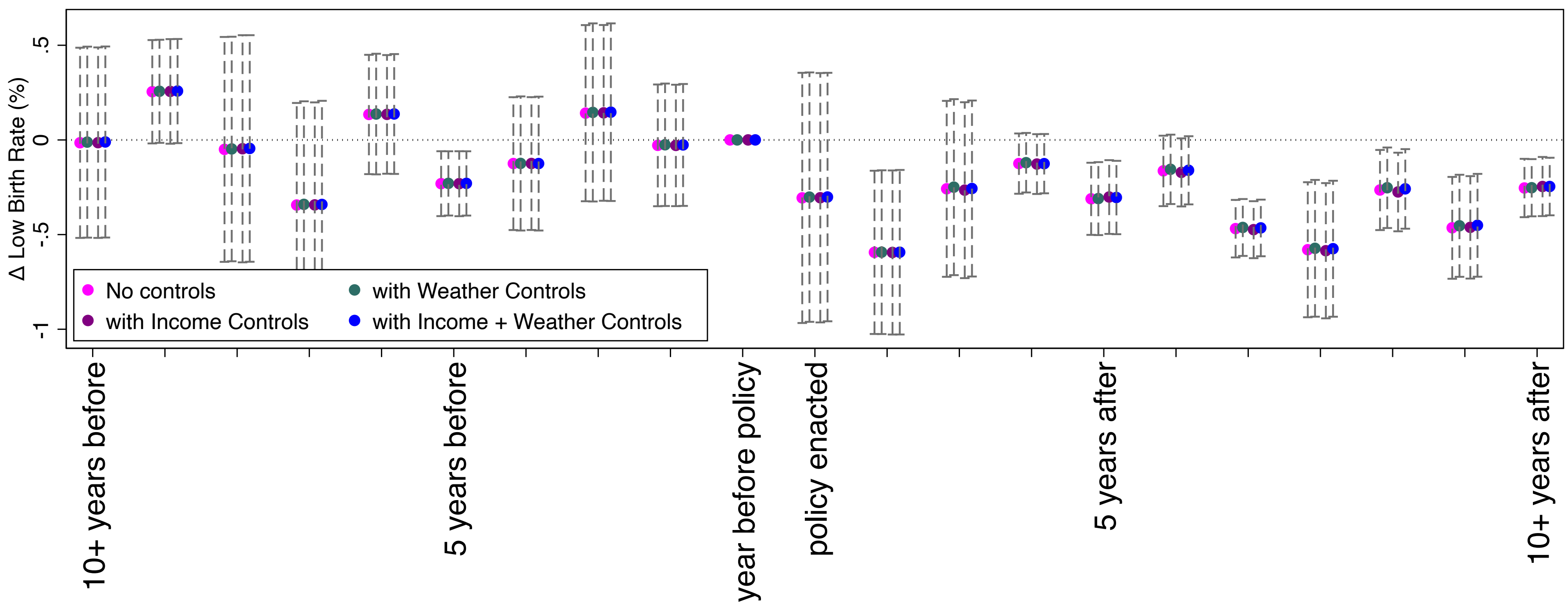
Notes: The estimates in Figure 2 plot the policy relative time path for estimates from equation (2). For each policy relative year there are four estimates plotted. Each coefficient is associated with a different estimating equation; the estimating equations differ in the controls included. Weather controls include cumulative exposure to CDDs and HDDs during the third trimester. 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. The coefficients associated with *5 years before* represent the association between bottle bill implementation five years before policy enactment and the low birth rate's seasonality. The association between the year prior to policy implementation and the low birth rate's seasonality is normalized to zero. Dashed lines represent 95% confidence intervals constructed using standard errors clustered at the state level. Additionally, the regression is weighted by the number of births in each education group by season by year by county cell. Estimates displayed in this plot test the validity of identifying assumptions required for a causal interpretation of estimates in column (1) of Table 1. This figure does not suggest the identifying assumption for equation (1) is invalid; there is no pre-bottle bill trend in the treatment effect.

Figure 3: Event Study for Low Birth Rate's Seasonality with Heterogenity by Education Group



Notes: The estimates in Figure 3 plot the policy relative time path for estimates from equation (4). The specific coefficients plotted in this figure highlight the association between the year relative to bottle bill enactment and the seasonal difference in the low birth rate for a specific education group. Pink markers represent the association between a policy relative year and the seasonal difference in the low birth rate for mother's with less than a high school (low) education relative to mothers with more than a high school education. Coefficients plotted in pink correspond to η_j in equation (4) for the low education group. Purple markers represent the association between a policy relative year and the seasonal difference in the low birth rate for mother's with a high school (middle) education relative to mothers with more than a high school education. Coefficients plotted in purple correspond to η_j in equation (4) for the middle education group. Green markers represent the association between a policy relative year and the seasonal difference in the low birth rate for mother's with more than a high school (high) education. Coefficients plotted in green correspond to ϕ_j in equation (4). 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 grouping. I've bolded the pink coefficients as I expect the association of bottle bills and the low birth rate's seasonality to be most pronounced for mother's with a low education. For low and middle education mothers, estimates are relative to those of high education mothers. Thus, this plot both highlights the impact of bottle bills on low birth rate's seasonality for three education groups and demonstrates the extent to which variation in the base group (high education mothers) drives variation in the two other education groups. This figure suggests the identifying assumption for equation (3) is valid; there are no pre-bottle bill trend in the treatment effect for any education group. Estimates displayed in this plot test the validity of identifying assumptions required for a causal interpretation of estimates in column (5) of Table 1. *Note*, a Figure 2 estimate in a given period is not a weighted sum of Figure 3 estimates for that period. As, Figure 3 estimates for low and middle education mothers are relative to those of high education mothers.

Figure 4: Event Study Robustness for Seasonality in Low Education Mother's Low Birth Rate



Notes: The estimates in Figure 4 plot the policy relative time path for the estimates from equation (4) that identify the association between the policy relative year and the seasonal difference in the low birth rate for mother's with less than a high school (low) education. For each policy relative year there are four estimates plotted. Each coefficient is associated with a different version of estimating equation (4); the estimating equations differ in the controls included. Weather controls include cumulative exposure to CDDs and HDDs during the third trimester. 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. Pink markers represent the estimate of interest for estimating equation (4) with no control variables. These estimates are identical to the pink markers displayed in Figure 3. Coefficients plotted in pink correspond to η_j in equation (4) for the low education group. 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 grouping. This figure supports the identifying assumption for equation (3) with controls; there are no strong pre-bottle bill trend in the treatment effect for the low education group. Estimates displayed in this plot test the validity of identifying assumptions required for a causal interpretation of estimates in row (4), column (5-8) of Table 1.