The Importance of Earnings from Recycling to Families

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Abstract

A simple labor supply model implies that recycling–collecting discarded items and cashing in on their scrap value–channels money to marginalized populations. This paper evaluates the impact of bottle bills–a policy which substantially increased the scrap value of beverage containers–on US household food expenditures and birth outcomes. In the process, the analysis shows the general importance of earnings from recycling to US families and their prevalence. Bottle bills have a theoretically ambiguous impact on earnings from recycling. This paper begins by (i) documenting this ambiguity with a simple model, then (ii) illustrates bottle bills do in fact increase earnings by highlighting the policy increases low education households' food expenditures 10%, and finally (iii) shows this earnings increase improves birth outcomes to a similar extent as EITC, a widely studied welfare policy. Specifically, bottle bills reduced the incidence of low birth weight by .6-3.7% among mothers with less than a high school education. The surprising comparability of waste policy and EITC can be explained by differences in the populations reached by recycling earnings and EITC earnings from formal work.

Key words: Deposit Refund, Incidence of Low Birth Weight
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"Mr. Deane, a thin, bearded man who is 52 years old and unemployed,...headed for a recycling center that would pay him 21 cents a pound for...aluminum cans. [He] picks up 40 or 50 bucks a week, more on the weekends if the weather holds and there are lots of ball games. Last month, [Mr. Deane's] recycling center paid out more than $20,000, largely to the unemployed who brought boxes and carts filled with aluminum cans. Some make more than $300/week. One man hired three helpers and delivers his cans daily by the truckload. [Canners] go through the parking lot at Yankee Stadium after a game, social clubs, and delicatessens...'places where everybody drinks Bud'. [The recycling center manager suspects that] the new [bottle bill will] bring changes, although [canners] will still have plenty to collect.

– New York Times, the Bronx, 1983

"...a decade ago, Vermont lawmakers decided to take a stab at cleaning up littered highways. The result was a law that required deposits of at least 5 cents for certain drinks, to be reimbursed when the container is returned. As hoped for, roads are free of the once-discarded soda and beer bottles and cans. But an unexpected offshoot of the law are the people who scavenge for bottles and cans that others have thrown away, using the money to beef up their often meager incomes or welfare checks. [One couple] receives monthly welfare payments of slightly over $400 a month and say they'd be desperate without the [$165] they get each [month] from returning bottles and cans."

– The Associated Press, Montpelier, Vermont, 1982

Introduction

Informal recycling markets help fill gaps in the U.S. social safety net by redistributing part of the over $50 billion in annual scrap value generated by the US economy to marginalized populations.[1] These markets arise in part out of economic necessity, as many live in economic need. To mitigate financial hardship, some turn to recycling, collecting discarded items and cashing in on their scrap value. This paper evaluates the importance of this income source to families, in the process, highlighting the prevalence of informal recycling in the United States. Specifically, this paper focuses on earnings from recycling beverage containers, a category of recyclables with an annual scrap value over $4 billion.[2]

Anecdotal evidence suggests that recycling beverage containers serves as a vital source of income for many. For example, in Vermont in 1982, one household earned $165 per month recycling beverage containers (40% of their total income). In New York in 1983, one recycling center paid approximately $20,000 per month to *canners*–people who recycle other people's beverage containers for cash–some earning as much as $300 per week ($950 in 2024). A 2002 survey of 660 patrons at the 8 redemption sites in Santa Barbara revealed that 12% of households in the area who earn less than $10,000 annually derive 7% of their annual income from redemption, with Spanish-speaking households earning 9% (Ashenmiller 2009, Ashenmiller 2011). A 2023 survey of 257 canners, at 38 different redemption sites in NYC, found that, on average, canners support 2.4 dependents with average earnings around $119 per week, primarily using this income for food.[3] Many

[1] `https://pubs.usgs.gov/myb/vol1/2020/myb1-2020-recycling.pdf`

[2] `https://www.container-recycling.org/index.php/key-facts`

[3] This figure may underestimate top canners' earnings. A redemption center owner, in the NYC area, estimates 4,000 full-time canners earn $60,000 annually in the city, while there are also many part-timers who earn less. The former group was likely underrepresented in the survey, which focused on canners averaging 25 hours per week and using less efficient redemption sites, such as reverse vending machines.

canners also rely on formal welfare programs, with 50% enrolled in Medicaid/Medicare and 32% using food stamps.[4]

This paper systematically evaluates the importance of informal recycling incomes to families by estimating the impact of bottle bills–deposit refund programs for beverage containers–on household food consumption and birth outcomes. Bottle bills require consumers to pay refundable deposits on beverage containers at purchase, which anyone, not just the original depositor, can redeem. In effect, bottle bills substantially increase the scrap value of beverage containers, potentially increasing informal earnings from recycling. I estimate the impact of this increase on household food consumption and birth outcomes, two metrics of economic well-being in low-income households. Finding an effect, however, depends on bottle bills actually increasing recycling earnings and the prevalence of informal recycling income, both of which are unknown due to a lack of direct data on informal recycling earnings. Hence, this paper's empirical analysis simultaneously tests (i) whether bottle bills increased individuals' recycling earnings, (ii) the importance of this income source to families, and (iii) the prevalence of recycling earnings.

Leveraging bottle bill introductions to recover the importance of individuals' recycling earnings assumes that an increase in the scrap value of containers translates into an increase in individuals' earnings. This paper begins with a simple model of informal recycling markets that outlines the conditions under which this assumption holds. This framework motivates one of the paper's primary empirical questions: how do bottle bills impact informal recycling earnings? The paper proceeds by empirically asking this question with household level food expenditure data from the Panel Study of Income Dynamics (PSID).[5] With a difference-in-difference estimator, I estimate the impact of bottle bill introductions on food expenditures, finding bottle bills have no overall effect on food consumption. This is not surprising as bottle bills should only affect the earnings of the lowest income households–people who recycle for cash. I then look at heterogeneity in this average effect by education, where I use education to proxy for wage.[6] This heterogeneity analysis shows that bottle bills increase food consumption 10% among households who's head has no more than a 5th grade education and do not affect households with higher levels of educational attainment. These results show that bottle bills do in fact increase earnings from recycling for at least some households in the PSID.

With this evidence that bottle bills increase informal earnings from recycling, I then conduct a population wide assessment of bottle bills' impact on earnings, the importance of these earning to families, and the general prevalence of informal income from recycling with the universe of US birth outcomes. As previously discussed, birth outcomes, specifically birth weight, have been shown to be quite sensitive to small but meaningful cash transfers to low-income mothers (Almond, Hoynes and Schanzenbach 2011, Hoynes, Miller and Simon 2015). The increase in the aggregate scrap value caused by bottle bills leads to wage increases. Consequently, we would expect birth outcomes to improve in low-income or marginalized populations, to which canners likely belong, in response to this increase, if recycling serves as a significant income source for households.

I link birth outcomes to plausibly exogenous variation in the aggregate scrap value from bottle bills and temperature. To credibly identify the importance of informal recycling earnings, I isolate changes in the waste

[4] https://static1.squarespace.com/static/5647b691e4b0524134ec6b8c/t/65e1f1c13bbe880ffe2d9be9/1709306311996/REPORT_WEB+%281%29.pdf

[5] This analysis implicitly assumes that if bottle bills increase earnings from recycling then food expenditures will also increase. This assumption allows me to bypass the lack of data on recycling earnings.

[6] Even though the PSID data has household wage data, I use education to proxy for wage to be consistent with the birth outcomes analysis where I do not have household wage information but have data on education.

stream's value–the aggregate value of containers available for recycling–that are independent of variation in local economic conditions using: (i) bottle bill introductions and (ii) temperature-driven fluctuations in beverage consumption.[7] First, bottle bill introductions provide plausibly random variation in the scrap value of beverage containers. Legislative attempts to enact these bills often faced fierce lobbying, leading to frequent failures and delaying eventual successes. Many states and the federal government attempted but failed to implement bottle bills (Costle, Kreps, Andrus, Marshall, Blumenthal, Warren, Cutler, Cornell and Alm 1978). Of the eleven states that eventually passed bills, most experienced initial failures (Davis 1982, Franchot 1978, Peterson 1976, Ross 1982, White 2018, Costle et al. 1978). These delays suggest that the timing of bottle bill enactments was decoupled from broader economic or political conditions, providing a strong basis for treating bottle bills as a source of exogenous variation. Second, I show that temperature drives fluctuations in beverage consumption which in turn increases the number of containers available to be recycled. While this variation could be correlated with local economic conditions, I can effectively control for this potential confounder by combining temperature shocks with bottle bill implementations. Together, (i) bottle bills and (ii) temperature create plausibly random, spatially and temporally resolved fluctuations in the waste stream's value,[8] allowing me to fully leverage the spatial and temporal resolution of natality records and weaken the necessary identifying assumptions.

Leveraging bottle bills and temperature, I employ two distinct, but closely related, approaches to identify the effect of an increase in the waste stream's aggregate value. The first strategy–a triple difference (DiDiD)–compares (i) the years pre and post-bottle bills, (ii) the ten states that implemented bottle bills to the forty states that did not, and (iii) winter to non-winter months, when beverage consumption is generally higher due to higher temperatures. Identification assumes that in the absence of bottle bills, the outcome would not have changed differentially in bottle bill and non-bottle bill states, in winter versus non-winter months, during the sample period (Olden and Møen 2022). The second strategy–a difference in difference in temperature (DiDiT)–uses a triple interaction of temperature and bottle bill introductions. The estimator compares the temperature-outcome relationship in (i) the years pre and post-bottle bills, and (ii) the ten states that implemented bottle bills to the forty that did not (Colmer and Doleac 2023). Identification assumes that in the absence of bottle bills, the temperature-outcome relationship would not have changed differentially in bottle bill and non-bottle bill states.

Increases in recycling earnings due to bottle bills in non-winter, relative to winter months, reduce the incidence of low birth weight (ILBW) among mothers with low educational attainment as effectively as a $1,000 increase in after tax income from EITC[9] (Hoynes et al. 2015). Specifically, bottle bills reduce the ILBW by .6-3.7% among mothers with less than a high school education. This effect is driven by places with high income inequality, specifically, places with both a higher than average income per capita and proportion of the population eligible for federal transfers. The improvement in birth outcomes is observed just below

[7]The waste stream's value–the monetary value of all discarded materials–is likely procyclical; rising production and consumption both expand the waste stream and drive up scrap prices, e.g., scrap metal prices rise with demand for material (virgin or recycled) (Savov 2011).

[8]Consider the following back of the envelope computation: let the temperature driven change in the number of containers available for recycling be $\%\Delta_T$ and the bottle bill price as $B\times$ the scrap value. Without a bottle bill, a temperature shock would increase the aggregate value by $\%\Delta_T$. With an active bottle bill, the value would increase by $\%\Delta_T \times B$. Therefore, with both a bottle bill and temperature shock, the waste stream's value would increase by $\%\Delta_T \times (B-1)$ relative to the case with no bottle bill.

[9]EITC (Earned Income Tax Credit) is a refundable tax credit aimed at providing financial assistance to low- and moderate-income workers, particularly those with children.

the low birth weight threshold. The wage increases likely benefit able-bodied households living just below the margin of "okay." To this end, nutritional deficits in these households are likely relatively minor, meaning that recycling wage increases are sufficient to push birth outcomes in this population from just below the "okay" threshold to above.

At first glance, the comparability of bottle bill wage increases to EITC in their efficacy at reducing the ILBW is striking, especially given that EITC is a formal welfare policy with earnings from work far exceeding aggregate potential increases in recycling earnings due to bottle bills. However, the comparability becomes plausible after recognizing (i) differences in the population reached by the two programs and (ii) the aggregate value of recycling earnings annually. EITC and informal recycling earnings likely reach different populations due to differences in the screening mechanism of each program. In a sense, the waste stream's screening mechanism is more inclusive than that of EITC, as canners are not required to engage in somewhat formal forms of work or navigate administrative barriers, as is required with EITC. On the other hand, the waste stream's mechanism is more restrictive; members of a household must be able to perform physically demanding tasks outdoors and be willing to sift through trash. In effect, the waste stream screens for both households' need and able-bodiedness, yielding a relatively healthy set of households with correctable nutritional deficits. Moreover, the potential earnings in the waste stream are also significant. For example, collecting two garbage bags of empties per week amounts to an annual recycling income of $1,040, roughly the $1,000 increase in after tax income from EITC found to improve the ILBW to a similar extent as bottle bills. New York State residents redeemed 5.1 billion containers in 2016, amounting to 25.5 million bags of empties and $255 million in refunds. In 2016, if 1 in every 100 New York resident redeemed empties, each returning equivalent amounts, then roughly 245,000 households would have earned $1,040 in refunds.[10]

The comparability of EITC and bottle bills underscores a broader pattern across U.S. welfare programs: the design of screening mechanisms significantly impacts policies' effects and efficacy. As U.S. welfare program participants' income increases, the efficacy of programs in reducing the ILBW decreases (Hoynes, Page and Stevens 2011). For example, per dollar, WIC[11] reduces the ILBW much more effectively than SNAP[12] and EITC (Hoynes et al. 2011, Currie and Rajani 2015, Bitler and Currie 2005, Almond et al. 2011, Hoynes et al. 2015). WIC's screening mechanisms are far more inclusive than those of EITC and SNAP–including no work or citizenship requirements and minimal documentation needed for access. Hence, WIC comes at lower administrative costs and likely reaches more marginalized populations. Although EITC, WIC, and SNAP have different goals, WIC's screening mechanism likely makes it more effective per dollar by allowing the policy to reach a more disadvantaged population. Similarly, bottle bills demonstrate greater per-dollar effectiveness than EITC due to their ability to reach a distinct population, one in economic need but capable of engaging in informal, physically demanding work. Bottle bills' effectiveness in reducing the ILBW is illustrative of weakness in the U.S. social safety net. The system's patchwork design and burdensome requirements (both explicit and implicit) screen out people and limit services to individuals who would likely benefit greatly from inclusion or additional support, leaving room for informal markets like those affected

[10]Wages vary across canners and redemption rates vary across consumers; more than 245,000 of the poorest and near poor households likely recycle for cash in New York State (Ashenmiller 2011). Some canners certainly earn far more than $1,040 per year, while others earn far less.

[11]WIC (Women, Infants, and Children) is a targeted program designed to provide nutritional support to low-income pregnant women, new mothers, and young children.

[12]SNAP (Supplemental Nutrition Assistance Program) is a federal program that provides financial assistance to low-income individuals and families to help them purchase food.

by bottle bills to have non-trivial welfare impacts (Currie 2006, Aizer, Hoynes and Lleras-Muney 2022).

1 Model

To begin, I develop a simple model that illustrates the theory behind how informal recycling markets redistribute money and how bottle bills may interact with these markets. This discussion establishes the basic assumptions underlying the empirical analysis, motivates one of the paper's primary empirical questions–how do bottle bills impact recycling earnings?, and creates a helpful framework for interpreting reduced form results.

Consider an economy with a unit mass of individuals, who may differ in their formal market wage $\overline{w}$ distributed according to the cumulative distribution function $F(\overline{w})$. Each individual chooses labor type Θ_i. The labor type denotes if (i) the individual works in the formal market but does not recycle their own containers ($\Theta_i = f_o$), (ii) works in the formal market and recycles their own containers ($\Theta_i = f_r$), or (iii) does not work in the formal market, recycles their own containers and earns money recycling other people's containers ($\Theta_i = p$). Individuals who choose to recycle either their own containers ($\Theta_i = f_r$) or both their containers and other people's ($\Theta_i = p$) receive d per container recycled for a fixed cost r. r can be thought of as the amount of time it takes to bring recyclables to a redemption center. Individuals of the type $\Theta_i = f_o$ contribute to the empties reservoir–a common pool resource of containers. These containers can be collected and recycled by individuals of the type $\Theta_i = p$ (Ashenmiller 2010, Ashenmiller 2011).

Define $w_i(\Theta)$ as individual i's wage if they select the labor type Θ:

$$w_i(\Theta) = \begin{cases} \overline{w}_i, \text{ if } \Theta \in \{f_o, f_r\} \\ w_r, \text{ if } \Theta = p \end{cases}$$

Individuals of the type $\Theta_i \in \{f_o, f_r\}$ earn at their formal market wage $\overline{w}_i$ whereas professional reycler or canners–individuals of the type $\Theta_i = p$–face a common wage w_r, the recycling wage, which is determined by both the size of the empties reservoir N and the total recycling effort E–a relationship that will be further developed later in this section.

All individuals share the same utility maximization problem, determining their optimal level of labor supply l and beverage consumption n, given a chosen labor type Θ, the associated wage $w(\Theta)$, the value of an empty container d, and the fixed cost of recycling r. As is common, the utility function is quasi-concave, decreasing with labor and increasing with consumption.

$$l\left(\Theta, w(\Theta), d, r\right), n\left(\Theta, w(\Theta), d, r\right) = \arg\max_{l,n} \quad U\left(l, n | \Theta, w(\Theta), d, r\right) \text{ s.t. constraints}$$

$l(\Theta, w(\Theta), d, r)$, $n(\Theta, w(\Theta), d, r)$ differ within labor types only due to variation in wages $w(\Theta)$ across individuals. Hence, for all individuals of the type $\Theta_i = p$ the optimal labor supply T_p is constant across individuals, as the recycling wage does not vary across individuals. In what follows, as a shorthand, I suppress $w(\Theta)$, d, and r, denoting $l(\Theta, w(\Theta), d, r)$, $n(\Theta, w(\Theta), d, r)$ as $l_i(\Theta), n_i(\Theta)$, where $l_i(\Theta) = l(\Theta, w_i(\Theta), d, r)$.

The indirect utility for individual i of type Θ is then:

$$v_i(\Theta) = \quad U(l_i(\Theta), n_i(\Theta)|\Theta, w_i(\Theta), d, r)$$

and realized indirect utility is:

$$v_i(\Theta_i^*) = \max\{v_i(f_o), v_i(f_r), v_i(p)\}$$

As is common, assume that as $w_i(\Theta)$ increases, $l_i(\Theta)$ decreases and $n_i(\Theta)$ increases, increasing $v_i(\Theta)$. This set up induces a sorting equilibrium, in which individuals sort into labor types based on their formal market wage $\overline{w}$. Individuals with a $\overline{w}$ below the recycling wage w_r will optimally choose to professionally recycle–the labor type p. Individuals with a wage above w_r will work in the formal market and optimally choose to recycle their own beverages if the opportunity cost of not recycling d relative to the opportunity cost of recycling–proportional to r–exceeds their marginal value of time $\overline{w}$. More formally, Θ^*, can be interpreted as a mapping from market wages $\overline{w}$ to labor types Θ:

$$\Theta^*(\overline{w}_i) = \begin{cases} p \text{ if } \overline{w}_i \leq w_r \\ f_r \text{ if } w_r < \overline{w}_i \leq \alpha\frac{d}{r} \text{ where } \alpha \text{ is a constant} \\ f_o \text{ if } \alpha\frac{d}{r} < \overline{w}_i \end{cases}$$

Now let the recycling wage $w_r = \frac{d\ N}{E}$, where the size of the empties reservoir

$$N = \int_{\overline{w}:\Theta^*(\overline{w})=f_o} n(f_o, \overline{w}, d, r)\, \partial F(\overline{w}) = \int_{\overline{w}>\alpha\frac{d}{r}} n(f_o, \overline{w}, d, r)\, \partial F(\overline{w})$$

and the total recycling effort $E = P \times T_p(dN, P, r)$, where the total number of recyclers

$$P = \int_{\overline{w}:\Theta^*(\overline{w})=p} \partial F(\overline{w}) = F(w_r)$$

and T_p is the mapping from the scrap value of the empties reservoir, the number of recyclers and the fixed cost of recycling to the optimal recycling labor supply, which, as previously discussed, is constant across all professional recyclers. In summary,

$$w_r = \frac{d\ N}{T_p\ P} = \frac{d\ \int_{\overline{w}>\alpha\frac{d}{r}} n(f_o, \overline{w}, d, r)\, \partial F(\overline{w})}{T_p\,(d\ N, F(w_r), r)\ \ F(w_r)} \tag{1}$$

Bottle bills have the potential to change both the *aggregate scrap value* available to professional recyclers ($d\ N$)–the size of the pie–and *the supply of recycling labor* ($T_p\ P$)–who gets pie and how much they get. Changes in equation (1)'s numerator alter the size of the pie–the aggregate scrap value available to professional recyclers. Changes in the denominator govern how any changes in the size of the pie are distributed to professional recyclers, i.e., who gets more or less due to a change in the aggregate scrap value available. If there is an increase in the aggregate scrap value, the recycling wage will increase leading to new recyclers–an increase in P–and a reduction in recyclers' optimal labor supply–a decrease in T_p. This intensive margin labor response will attenuate the total earnings increase recyclers' experience from the

higher recycling wage, though the increase will still be positive.

Figure 1a depicts bottle bills' impact on the aggregate scrap value. The solid black line depicts individuals' contribution to the aggregate scrap value dN, assuming (i) a uniform distribution of individuals across formal market wages and (ii) beverage consumption $n(\overline{w})$ does not change with wage $\overline{w}$. Up until a wage $\overline{w} = \alpha \frac{d}{r}$, the solid black line remains at zero; individuals recycle their own containers, i.e., they do not contribute to the empties reservoir, and the solid black line cont. After a wage $\overline{w}$ of $\alpha \frac{d}{r}$, the solid black line jumps to a positive value and remains constant; these individuals contribute all their empty containers to the reservoir. The area under contribution profile represents the aggregate value of the empties reservoir. The dashed black line depicts how the contribution profile changes with the introduction of a bottle bill. As in the absence of a bottle bill, at lower wages individuals make no contribution. However, bottle bills increase the range of wages that do not contribute from all wages below $\alpha \frac{d}{r}$ to $\alpha \frac{d_b}{r}$. This reduction in the population of contributors is bottle bill's *indirect behavioral effect* I, which reduces the original area under the contribution profile. This reduction can be seen in the lower bound on equation (1)'s numerator integral, which increases by $\frac{d_b}{d}$. Above $\alpha \frac{d_b}{r}$, the contribution is now $\frac{d_b}{d}$ larger. This increase in the contribution is bottle bill's *direct price effect* D, which increases the original area under the contribution profile for individuals with wages above $\alpha \frac{d_b}{r}$. Within the scope of this model, bottle bills increase the aggregate scrap value if D the *direct price effect* exceeds I the *indirect behavioral effect*.

Figure 1b depicts bottle bills' impact on individuals' earnings. The solid black line depicts the mapping from an individuals' formal market wage $\overline{w}$ to their income in the absence of informal recycling markets, i.e., people with lower wages cannot choose to informal work in recycling over formal work. The brown dashed line depicts the mapping with informal recycling markets, i.e., individuals with a $\overline{w}$ below the recycling wage w_r choose to professionally recycle receiving a wage of $w_r > \overline{w}$, reaping the rents R depicted in the brown shaded region. The blue dotted line depicts the mapping with an active bottle bill, assuming the bottle bill increases the aggregate scrap value, i.e., $D > I$. w_r^b denotes the new recycling wage and R_b the additional rents from recycling due to the bottle bill. R_b is made up of a rectangle and a triangle. The rectangle denotes the effect on earnings at the *intensive margin*–the increase in recycling earnings for individuals who professionally recycled pre-bottle bill. The triangle denotes the effect on the *extensive margin*–the increase in earnings for people who did not professionally recycle pre-bottle bill.

This discussion demonstrates that bottle bills' effect on earnings is ambiguous, motivating this paper's first empirical question: did bottle bills increase earnings from recycling other people's containers? Moreover, the simple model highlights bottle bill impacts on earnings will affect individuals at the intensive and extensive margins. Incumbent canners will experience a wage increase and entrant canners will also experience a wage increase though by changing jobs.

2 Bottle Bill Introductions

Answering this paper's primary empirical questions relies on bottle bill (BB) introductions. Between 1969 and 2002, ten U.S. states implemented BBs. Figure A1 visualizes BB implementations in the U.S. during the sample period. In 1973, Oregon and Vermont implemented the first BBs. In 1978, Maine implemented a BB. Shortly after, Michigan and Iowa implemented in 1979. Then, Connecticut introduced the law in 1980. Three years later, in 1983, Delaware, Massachusetts and New York implemented bills. Lastly, California introduced

the bill in 1987 (Ashenmiller 2010). As previously discussed, due to legislative delays and fierce lobbying the timing of bottle bill implementations was plausibly random. I employ a balance test to asses if any controls confound identification; specifically, I regress active BBs in non-winter months–the paper's primary identification strategy–on the *income* and *weather* controls. Table A1 displays the balance test results; point estimates are small, as the test regresses large covariates on a dummy variable. Only the association between BBs in non-winter months with farm salaries and wages is statistically significant, suggesting only seasonal differences in payments to hired labor on farms varied systematically across treated and untreated units.

3 Impact of Bottle Bills on Household Food Consumption

First, I evaluate the impact of bottle bills on earnings with survey data on household food consumption. I cannot directly observe informal recycling earnings, so I test for an impact on food consumption with the implicit assumption that an increase in recycling earnings will increase food consumption. This analysis serves two purposes: (i) it resolves the theoretical ambiguity around if bottle bills increased or decreased earnings, and (ii) helps motivate the bottle bill birth outcome analysis.

3.1 Data

I use household food consumption data from the Panel Study of Income Dynamics (PSID) from 1969 to 1993 to examine the impact of bottle bills on food consumption and to implicitly recover the impact on recycling earnings. The PSID began in 1968, tracking ∼5,000 households and all members (+ descendants) in subsequent years. The sample is designed to over represent low-income and minority households–an advantage for this paper's analysis, as bottle bills should only impact lower income households' food consumption. The PSID sample period 1969-1993 was chosen to cover the ten bottle bill implementations between 1969 and 2000, while also minimizing survey differences across years. The sample also excludes 1973–food consumption variables were not included in this survey (Hoynes and Schanzenbach 2009).

Specifically, I use PSID to recover families' total food expenditures and expenditures on food consumed at home. Additionally, I use family demographic data on the family heads educational attainment, if a family has children, family's earnings from work, and the family's state of residence. There is some ambiguity in the time frame that food consumption metrics pertain to. The survey, which takes place in the spring, asks households about their "typical food consumption." Their response is then annualized and applied to the previous year. Following previous work with this data, I assume food expenditure apply to the survey year, rather than the previous year (Hoynes and Schanzenbach 2009).

3.2 Empirical Strategy

To recover the impact of bottle bills on food consumption, I employ a difference-in-difference model (DiD), where the treatment is at the state level, with controls for family and year fixed effects. The family level fixed effects are more restrictive than necessary for the DiD. This more restrictive model helps recover more precise estimates. In this model, identification requires that no state-level trends are correlated with both

bottle-bill introductions and family food consumption. Specifically, I estimate the following model:

$$\log(\text{food}_{jit}) = \beta \mathbb{1}(\text{BB})_{it} + \alpha_j + \delta_t + \epsilon_{ijt}$$

where $\log(\text{food}_{jit})$ is the food expenditure variable–either total expenditure on food or expenditure on food consumed at home–for family j in state i during year t. $\mathbb{1}(\text{BB})_{it}$ is an indicator variable equal to one if state i in year t has an active bottle bill. α_j is a family specific fixed effect controlling for all factors common to each family. δ_t is a year fixed effect, accounting for common factors in each year.

The DiD estimator is β, capturing variation in food expenditures specific to bottle bill states relative to non-bottle bill states, when a bottle bill was active relative to inactive. Goodman-Bacon (2021) highlights potential concerns with this interpretation of a DiD estimator. Given only ten states are treated and forty are not, the issues raised by Goodman-Bacon (2021) are of little consequence because a very small weight is placed on concerning comparisons. In a future iteration of this work, I will also show result sensitivity to alternate estimators.

3.3 Results

Table 1 reports the impact of bottle bills on household food consumption as reported in the PSID. I regress bottle bill implementations on the log of total household food expenditures (columns 1-2) and food expenditures for at home consumption (columns 3-4). Each column reports estimates from a different regression. Column (1) regresses bottle bill implementation on total food expenditures, recovering the overall average treatment effect for the whole population of people surveyed. The estimated effect is a precise zero. In the overall population I do not find any evidence to suggest that bottle bills increased total food consumption. This is to be expected, as bottle bills should only impact the lowest income households. The subsequent columns explore heterogeneity in the estimated effect by education, using education as a proxy for income, to test if bottle bills do in fact affect food expenditures, but just in a marginalized population.

Column (2) regresses bottle bill implementations on total food expenditures allowing for heterogeneity in the estimated bottle bill effect by education group. Specifically, the regression replicates that of column (1) but is fully stacked in education group, i.e., the estimate of interest–the effect of bottle bills–as well as all controls are interacted with an indicator for a households' education group. Column (2) reports the effect of bottle bills for each education group, where the estimates for each education group are reported sequentially in the second, fourth, fifth..., and eight rows, where the household head's educational attainment increases with the rows. The estimated effects are precise zeroes for all education groups but the lowest. Bottle bills increased the lowest education group–households who's head has a maximum education of fifth grade or less–total food expenditures by 9%. This pattern supports the hypothesis that bottle bills increased recycling earnings, thereby increasing food consumption, for canners–people who face the lowest market wages.

Total food expenditures include expenditures with food stamps, expenditures for at home food consumption not with food stamps, and expenditures on food consumed away from home. Additional earnings from bottle bills should be reflected in expenditures not from food stamps. Column (3) replicates column (2) for at home food expenditures. This analysis ensures the estimated increase in total food expenditures for the lowest education group is driven by an increase in earning from bottle bills rather than an increase in expenditures with food stamps. Reassuringly, the second row of column (3) shows the lowest education group's 9% increase

in total food consumption is driven by a 14% increase in their at home food consumption. Surprisingly, the eighth row of column (3) also reports that bottle bills increased at home food expenditures by 3% among households in the highest education group–households who's head has at least a college eduction (estimates are statistically significant at the 10% level). One plausible explanation for this increase is that children in these households start recycling their parents empty containers for cash in response to the increase in the scrap value associated with bottle bills. If this is the case, we would only expect to see a positive effect of bottle bills on the highest education households with children and no effect on households without children. In contrast, the effect on the lowest education households should not reflect a within household cash transfer, and should be seen in families with and without children.

Column (4) explores if the estimated increase in at home food expenditures for both the highest and lowest education households is driven by families with or without children. The second row of column (4) reports the estimated effect for the lowest education households without children and the third row reports the estimated difference in the effect for households with children relative to those without. The eighth and ninth rows of column (4) report the same estimates, but for the hightest education group households. This analysis shows that bottle bills increase at home food consumption among all households in the lowest education group on average. The estimate in the third row of column (4) is not statistically significant, showing there is no statistical difference in the treatment effect for households with and without children in the lowest education group. In contrast, bottle bills only increase at home food consumption for the highest education households who have children and have no estimated effect on households without children (see the ninth and eighth row of column 4, respectively).

4 Impact of Bottle Bills on Birth Outcomes

4.1 Establishing Seasonality in Recycling Earnings

Beyond bottle bill introductions, identification relies on seasonality in recycling earnings. The use of seasonality in earnings relaxes the identification assumptions necessary to recover a causal estimate of bottle bills' effect on birth outcomes. This section establishes that aggregate earnings from recycling are highly seasonal and the majority of this seasonality is from temperature driven fluctuations in beverage consumption. To establish these relations, I leverage (i) monthly data on the refund value paid out at redemption centers in forty California counties, (ii) monthly grocery store scanner data from across the US, and (iii) daily weather data aggregated to the county by month level. I show that seasonality in aggregate recycling earnings–refunds–is predominantly driven by temperature fluctuations. I then show the primary cause of these fluctuations is variation in the size of the empties reservoir due to temperature driven variation in beverage consumption.

Data To establish the seasonality of earnings, temperature's important role, and the contribution of temperature driven fluctuations in beverage consumption, I combine *weather* data with data on beverage container *refunds* and *deposits*. I obtained county level monthly refund data from California through public record request. Uniquely[13], CalRecycle, California's Department of Resources Recycling and Recovery,

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

tracks refunds paid out by redemption centers and to curbside collection companies within the state, making county level data publicly available.[14] I construct beverage deposit data, using the Homescan (HMS) Nielsen data between 2005 and 2022. The HMS data tracks 40,000-60,000 US households' retail purchases. To approximate the deposits paid by a household, I curated a list containerized beverage products that likely require deposits upon sale in BB states (see table A5). I scale household deposits by HMS market weights to estimate the aggregate amount of deposits paid within a market for a given month. To construct a county by month panel of realized temperature and precipitation, I aggregate PRISM weather data with population weights.

Empirical Strategy I examine seasonality in recycling earnings using monthly data on aggregate recycling earnings in California–the refund value paid out at redemption centers in forty California counties. Refunds paid out at redemption centers in California go to individuals and predominantly to people who did not originally consume the beverage, hence seasonality in these refunds indicates seasonality in aggregate effective cash transfers (Ashenmiller 2009, Ashenmiller 2011). Additionally, I use data on deposits paid by households and refunds paid to curbside recycling companies to explore mechanisms driving the bottle bill transfer seasonality.

To begin I estimate weather dose response functions for redemption center refunds, curbside recycling refunds and household deposits. I flexibly model refunds/deposits as a function of daily temperature and precipitation within a month, following the climate econometrics literature (Hsiang 2016). Specifically, I estimate the following equation:

$$\log\left(y_{m(t)c}\right) = \boldsymbol{\theta}\mathbf{P}_{m(t)c} + \boldsymbol{\beta}\mathbf{T}_{m(t)c} + \delta_{ct} + \gamma_{cs(m)} + \nu_{s(m(t))} + \epsilon_{cm(t)} \tag{2}$$

Let $y_{m(t)c}$ denote the refund value (\$) paid out at redemption centers, to curbside recycling companies, or deposits paid by households in county c in month m during year t. $\mathbf{T}_{m(t)c}$ represents a fourth order polynomial in temperature, i.e., the k^{th} element denotes $\sum_{j\in c}\sum_{d(t)\in m(t)} T^k_{d(t)j}$, where $T_{d(t)j}$ is the maximum temperature at grid cell j on day d in year t. Similarly, $\mathbf{P}_{m(t)c}$ represents a fourth order polynomial in precipitation. δ_{ct} is a county by year fixed effect, $\gamma_{cs(m)}$ county by season, and $\nu_{s(m(t))}$ season by year. The response is identified from plausibly random year-to-year variation in weather within a county and season.

Finally, I estimate overall seasonality in redemption center refunds and deposits. Specifically, I estimate the following equation to calculate overall seasonality:

$$\log\left(y_{m(t)q}\right) = \beta_s \mathbf{1}_{s(m)} + \delta_{qt} + \epsilon_{m(t)q} \tag{3}$$

As before, $y_{m(t)q}$ reflects redemption center refunds or deposits in month m during year t in Nielsen market q. $\mathbf{1}_s$ is an indicator variable for season. Winter is dropped for normalization. δ_{qt} is a market by year fixed effect restricting identifying variation to within a market and year. The coefficient of interest β_s reflects the effect of season s on the outcome in percent, i.e., the outcome is $\beta_s \times 100\%$ higher in season s relative to winter.

[14] Curbside collection companies in California can claim refunds they collect through curbside residential recycling.

Results Figure 2 shows both refunds (green, yellow) and deposits (orange) increase in non-winter relative to winter months. The vast majority of this seasonality can be explained by seasonality in temperature, inducing fluctuations in the number of containers available for recycling, i.e., the size of the empties reservoir. Figure 2a, column 1 shows deposits increase by 15% and refunds by 20% in non-winter relative to winter months, for deposits paid by households and refunds paid to individuals at redemption centers. Figure 2a, column 2(3) shows the majority of this seasonality (no seasonality) can be predicted by temperature (precipitation). Figure 2b plots the temperature response of refunds paid to individuals at redemption centers (column 1, green), paid to curbside collection companies (column 2, yellow) and deposits paid by households (column 3, orange). Dotted lines in column 2-3 reflect the redemption center refund response estimated using the same estimation sample as that used by a given column's primary response. The temperature sensitivity of redemption center refunds could be driven by (i) beverage consumption, i.e., fluctuations in the size of the extractable common pool resource (the empties reservoir), or (ii) the recycling labor supply, i.e., recyclers redeem less when the weather is worse. The consistency in the response across refund types and deposits suggests that (i) fluctuations in empties reservoir size–the number of containers available to be recycled–is the primary cause of temperature related seasonality in aggregate earnings from recycling, i.e., refunds paid to individuals.

4.2 Data for Birth Outcomes Analysis

To recover the impact of bottle bills on birth outcomes, the analysis employs U.S. Vital Statistics Natality Data from 1969-2002, consisting of the full census of births from the National Center for Health Statistics (NCHS). To isolate the effect of recycling earnings, I estimate treatment effect heterogeneity by education. As previously discussed in the introduction, lower wage earners are more likely than higher wage earners to earn income recycling and thus experience income benefits from bottle bills. I use education to proxy for income because the natality data do not include information on wage. I explore educational heterogeneity by assigning birth outcomes to education groups using mother's education. Specifically, 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.

To conduct the analysis, I aggregate the natality data into county-education-month-year cells, employing data on birth month, year and weight as well as mother's education. I exclude all observations in which the mother's education is missing. Birth outcomes are assigned treatment based on when the pregnancy's third trimester began, as the third trimester is the most important in determining birth weight (Almond et al. 2011). I use Bailey, Clay, Fishback, Haines, Kantor, Severnini and Wentz (2018)'s infant health dataset to match NCHS county codes across years.[15] Table A3 and A4 examine if metrics of data quality and missingness in mother's education systematically vary with the treatment–bottle bill introductions and/or the interaction of implementations and non-winter months. Table A3 does not find that bottle bills are correlated with any measure of data integrity. Table A4 shows that missingness in mothers' education does not systematically vary with the treatment in the estimation sample.

Additionally, I include control variables covering income, government welfare spending, and weather.

[15]This dataset does not include Alaska, so Alaska is excluded from the analysis. Additionally, I employ the Horan-Hargis IDs in Bailey et al. (2018) to construct geographic units that do not change over the duration of the sample period. The Horan-Hargis ID groups counties that merged or split during the sample period into a single geographic unit (Horan and Hargis 1995). The final estimation sample is aggregated to the Horan-Hargis ID (HHID) level.

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

4.3 Empirical Strategy for Identifying the Bottle Bills' Impact on Births

DiDiD I use a triple difference (DiDiD) estimator to isolate the association between bottle bills (BBs) and birth outcomes. Specifically, the estimator compares (i) the years pre and post-BBs, (ii) the ten states that implemented BBs to the forty states that did not, and (iii) winter to non-winter months, as BB related earnings increases should be largest during non-winter months. Lastly, I look at heterogeneity in this estimator by education, as BBs should only impact individuals participating in informal recycling, likely lower wage earners, due to data constraints education proxies for income.

This paper's main regression, a DiDiD estimator *with* heterogeneity by education, simultaneously tests whether BB subsidies effectively transfer cash to relatively lower wage earners, evaluates if the transfer is larger in non-winter relative to winter months and measures the effect of BB subsidies on birth outcomes. The regression equation for the DiDiD estimator *without* heterogeneity is:

$$Y_{ctem} = \phi \left[\mathbb{1}(\text{BB})_{it} * \mathbb{1}(\text{not winter})_s\right] + \alpha_{c,s(m)} + \delta_{c,t} + \gamma_{s(m),t} + \epsilon_{ctem} \tag{4}$$

Y_{ctem} is a given birth metric in county c in year t for education group e in month m. $\mathbb{1}(\text{BB})_{it}$ denoting if county c in state i has an active BB. $\mathbb{1}(\text{not winter})_s$ equals one when season s of month m is not winter.[16] To operationalize the DiDiD estimator, I include three sets of two way fixed effects: $\delta_{c,t}$, $\gamma_{c,t}$, and $\alpha_{c,s}$. $\delta_{c,t}$ is a county by year fixed effect, accounting for all factors common to a county 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. $\alpha_{c,s}$ is a county by season fixed effect; this fixed effect allows for differences between counties for each season.

The DiDiD estimator is ϕ. ϕ is associated with the interaction: $\mathbb{1}(\text{BB})_{it} * \mathbb{1}(\text{not winter})_s$, equaling one for states with active BBs in non-winter months. ϕ captures variation in a birth outcome specific to BB states, relative to non-BB states, when a BB was active relative to inactive, and in non-winter relative to winter months. To identify ϕ the following assumption must hold: in the absence of BBs, birth outcomes would not have changed differentially in BB and non-BB states, in winter versus non-winter months, during the sample period (Olden and Møen 2022). Goodman-Bacon (2021) highlights potential concerns with this interpretation of a differences estimator. Given only ten units are treated and forty units are never treated, the issues raised by Goodman-Bacon (2021) are of little consequence because a very small weight is placed

[16] Season is assigned to birth outcome based on when the majority of the third trimester occurred, following the literature on when birth weights are most sensitive to cash transfers (Almond et al. 2011).

on concerning comparisons.

BBs are expected to increase earnings for less educated mothers more than for more educated mothers. To identify heterogeneity in the DiDiD estimator by education, I interact each term in equation 4 with an education group specific dummy variable.[17] Point estimates from this regression are equivalent to estimating a DiDiD estimator for each education group; however, standard errors vary in that correlation in errors is permitted across education groups. The analysis also considers DiDiD estimators that rely on pooled estimation of fixed effects across education groups. My preferred DiDiD *with* heterogeneity estimator takes the following form:

$$Y_{ctem} = \phi_e \left[\mathbb{1}(\text{BB})_{it} * \mathbb{1}_e * \mathbb{1}(\text{not winter})_s\right] + \alpha_{c,s(m)} + \delta_{c,e,t} + \gamma_{s(m),e,t} + \epsilon_{ctem} \tag{5}$$

The specification is just short of a stacked DiDiD estimator in education, i.e., interacting $\alpha_{c,s}$ with education would make the estimator equivalent to a DiDiD estimator for each education group. Generally, DiDiD estimators are better than DiD estimators in that they relax identifying assumptions. However, the cost of this relaxation tends to be overfitting.[18] I employ a main specification slightly more relaxed than a DiDiD estimator for each education group to increase power and reduce the risk of overfitting, but asses the cost in terms of identifying assumptions every step of the way. Intuitively, this approach reckons that some information can be jointly estimated across education groups and does so to the extent that results do not qualitatively vary from the case with the most relaxed identifying assumptions.

DiDiT Building off the result that 70% of seasonality in aggregate recycling earnings is driven by temperature (discussed in section 4.1), I estimate a model examining the extent to which BB implementation modifies the relationship between birth outcomes and temperature in the final trimester. The DiDiD analysis examines the extent to which BBs induce policy relevant increases in income, relying on the fact that in most places in most years aggregate recycling earnings are larger in non-winter relative to winter months. The DiDiT analysis leverages temperature's role as a driver of recycling earnings seasonality, more precisely modeling fluctuations in earnings within a given year and place relative to the crude seasonal difference employed in the DiDiD analysis. Namely, I estimate the following model:

$$Y_{ctem} = \beta_e \left(T_{ctem} * \mathbb{1}(\text{BB})_{it} * \mathbb{1}_e\right) + \alpha_{cet} + \delta_{ce} * T_{ctem} + \gamma_{e,t} * T_{ctem} + \epsilon_{ctem} \tag{6}$$

T_{ctem} denotes maximum daily temperature over birth month m's third trimester as described in section 4.1. $\delta_{ce} * T_{ctem}$ reflects a county by education group specific temperature response and $\gamma_{et} * T_{ctem}$ a year by education specific response. The coefficient of interest β_e reflects the degree to which BBs modify the third trimester-temperature birth outcome relationship (Colmer and Doleac 2023). As a robustness check, β_e can be scaled by realized temperatures in non-winter and winter months to recreate the estimates from the DiDiD analysis with different identifying assumptions.

[17] Education groups include less than high school, high school, and more than high school.
[18] Fully saturated models have higher variance, as they are subject to overfitting (Angrist and Pischke 2009).

4.4 Results: Estimated Effect of Bottle Bills on Births

Table 2 outlines the effect of bottle bills on average birth weight (Panel A) and fertility (Panel B), estimated with the DiDiD estimator discussed in equation 4 and 5. Rows within each panel differ by education group. Row 1 (2) (3) depicts the estimated effect on low (middle) (high) education mothers. Columns differ in the fixed effect controls, increasing in stringency from left to right. The fifth column is equivalent to a DiDiD estimator fully stacked in education, i.e., estimating a separate DiDiD estimator for each education group. The fourth column, the preferred specification, arguably best balances the bias-variance tradeoff of DiDiD estimators. Bottle bills (BBs) increase low education average birth weights consistently across all specifications, though the effect's statistical significance dwindles with fixed effect stringency. Middle and high education mothers are not obviously affected by bottle bills, estimates flip signs and attenuate with fixed effect stringency, never reaching statistical significance. Finally, bottle bills do not impact low education mother fertility in non-winter relative to winter months, an important finding for supporting that BB birth weight effects likely act through an income channel rather than reflect compositional changes.

Figure 3 shows that the consistently positive effect of bottle bills on low education mothers' average birth weight is driven, at least in part, by a .1-.2 pp reduction in birth incidence between 2,000 and 2,500g. Figure 3a plots estimates from the DiDiT estimator detailed in equation 6 scaled by average temperature in winter (blue markers), non-winter (orange), and the difference (green), showing a clear reduction in the incidence of births just below the low birth weight threshold (between 2,000 and 2,500g). Figure 3b replicates this finding with estimates from the DiDiD estimator. Figure 3b plots the BBs' effect on low education births in non-winter relative to winter months throughout the birth weight distribution. Markers differ in fixed effects employed in estimation – red markers denote the preferred specification, purple denote fully stacked DiDiD estimates. Figure 3b shows BBs consistently reduce the incidence of births between 2,000 and 2,500g, unlike effects in other birth weight bins, assuaging concerns that stringent identifying assumption or overfitting drive this result. The event study estimates, plotted in Figure 3c, further support a causal interpretation of this effect. Estimated reductions in the incidence of births between 2,000 and 2,500g appear to be driven by a reduction in the incidence starting after bottle bill implementations, particularly in the policy relative period $[6, \infty)$, with policy relative time estimates in the pre-period tightly oscillating around zero. Finally, Figure 3d shows the effect of BBs on marginal births is concentrated among counties with above median average income and an above median population of people eligible for federal transfers, i.e., the most unequal places.

5 Other Potential Mechanisms

BBs potentially impacted air quality, beverage prices and employment (Costle et al. 1978). Changes in each could all explain an association between BBs and the ILBW. BB introductions increased beer and soda's cost, decreasing overall consumption initially, though sales recovered after a couple years (Loube 1975, Wagenbach 1985). This reduction possibly improved birth outcomes, leading to an association between BBs and the ILBW. Seasonal associations from this mechanism should have dissipated after 1-3 years. This paper documents an association that lasts for more than ten years, suggesting changes in beverage prices do not fully explain the documented relationship. Additionally, BBs created a marginal number of non-seasonal jobs (Loube 1975). Higher employment increases spending on normal goods including maternal

health, consequently improving birth outcomes. The formal employment mechanism was not seasonal, hence it does not explain the seasonal differential in the association between BBs and the ILBW documented in this paper. Lastly, BBs may have affected air pollution concentrations. Chay and Greenstone (2003) find little association between reductions in TSP and the ILBW in the U.S. during the early 1980s. Thus, any variation in air pollution associated with BB implementations likely did not cause the observed reductions in the ILBW. In conclusion, while BBs impacted other factors influencing birth outcomes, these factors cannot fully explain the association documented in this paper.

6 Conclusion

This paper provides evidence that there were gaps in U.S. welfare policy that the waste stream filled. Specifically, bottle bills (BBs) increased earnings from recycling, leading to comparable reductions in the incidence of low birth weight (ILBW) to that of EITC (Hoynes et al. 2015). BBs enlarge waste stream cash transfers by increasing used material's price and decreasing transaction costs. Hence, BBs expand the aggregate amount that can be earned through recycling, potentially leading to highly targeted cash transfers to especially low income households (Ashenmiller 2010). Ashenmiller (2009, 2011) uses survey evidence from Santa Barbara, California to demonstrate that BBs transfer cash. Ashenmiller (2010) leverages BBs' idiosyncratic introductions to show that the programs reduce petty crime rates.

Extending this work, this paper examines BBs' impact on household food expenditures and the ILBW, a measure of economic well-being and an important determinant of a child's welfare (Almond, Currie and Duque 2018). Employing a difference-in-difference analysis, I find bottle bills increased food expenditures among households who's head had a fifth grade education or less, showing bottle bills plausibly increased earnings from recycling. I do not find bottle bills had any effect on more educated household food expenditures. This null effect further supports the conclusion that the increase in expenditures is driven by an increase in earnings, as bottle bills should only affect earnings among the lowest income households–people who recycle for cash–and due to data constraints I proxy for wage with education. I use a triple difference (DiDiD) estimator to identify BBs' impact on the ILBW. The estimator employs comparisons between: (1) the years pre- and post BB introductions, (2) BB and non-BB states, and (3) winter and non-winter months. The estimator relies on the idiosyncratic timing and location of BB implementations for identification and leverages seasonality in the recycling wage to isolate BB effect the recycling income channel. I further test the robustness of the DiDiD estimator with a difference-in-difference-in-temperature estimator (DiDiT), leveraging temperatures' outsized role in recycling income's seasonality. The DiDiT estimator compares the temperature-birth outcome relationship in (i) the years pre and post-bottle bills and (ii) the ten states that implemented bottle bills to the forty that did not (Colmer and Doleac 2023). With both estimators, I find that BBs reduce the ILBW .6-3.7% among mothers with less than a high school education. The reduction is immediate and lasts for more than ten years. Moreover, I do not find that BBs reduce the ILBW among mothers with at least a high school education, supporting the hypothesis that BBs' estimated effect acts through the cash transfer channel.

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Table 1: Estimated Impact of Bottle Bills on Household Food Consumption

	(1) log(food)	(2) log(food)	(3) log(food@home)	(4) log(food@home)
$\mathbb{1}$(BB)	0.01305			
	(0.02182)			
$\mathbb{1}$(BB) $\times$ $\mathbb{1}\{\text{Grade} \leq 5\}$		0.09353*	0.13591**	0.15174**
		(0.04718)	(0.06703)	(.05405)
... $\times$ $\mathbb{1}\{\text{Has Children}\}$				-0.06704
				(0.14645)
$\mathbb{1}$(BB) $\times$ $\mathbb{1}\{\text{Grade} \in [6,8]\}$		-0.03376	-0.03603	
		(0.02503)	(0.03266)	
$\mathbb{1}$(BB) $\times$ $\mathbb{1}\{\text{Grade} \in [9,11]\}$		-0.04143	-0.05603	
		(0.02820)	(0.05749)	
$\mathbb{1}$(BB) $\times$ $\mathbb{1}\{\text{Grade} = 12\}$		-0.00742	-0.01360	
		(0.01963)	(0.02670)	
$\mathbb{1}$(BB) $\times$ $\mathbb{1}\{\text{Grade} = \text{some college}\}$		0.00326	-0.00408	
		(0.01384)	(0.03140)	
$\mathbb{1}$(BB) $\times$ $\mathbb{1}\{\text{Grade} \geq \text{college}\}$		0.01675	0.02737*	0.00000
		(0.01306)	(0.01528)	(0.02399)
... $\times$ $\mathbb{1}\{\text{Has Children}\}$				0.066742**
				(0.02656)
Observations	141598	141598	137728	28681
within R^2	0	.000144	.000203	.00112

Notes: Table reports bottle bills' effect on household food expenditures. Columns differ in both the food expenditure metric and the dimension of heterogeneity explored. Column (1) reports the estimated effect of bottle bills on households log(total food expenditures)–the average effect across all households. Column (2) reports the effect broken out by education groups. The second row, column (2), documents the estimated treatment effect for households with a head who has no more than a fifth grade education. The fourth through eighth rows document estimated effects for families with heads who have sequentially more education. Column (3) replicates column (2) for households at home food expenditures. Again treatment effect estimates are broken out by education group. Finally, column (4) looks at heterogeneity by families with and without children for the lowest and highest education groups–the two groups for which there is a statistically significant effect of bottle bill on at home food consumption. The third row of column (4) documents the difference in the lowest education households' estimated treatment effect for families with children relative to those without. Similarly, the ninth row of column (4) documents this same difference for the highest education families. All regressions control semi-parametrically for family size, the number of children in a family, and household head age. Additionally, all regressions include family and year specific fixed effects. Columns (2-4) allow controls to vary by education group. Standard errors are clustered by state–the level of treatment.

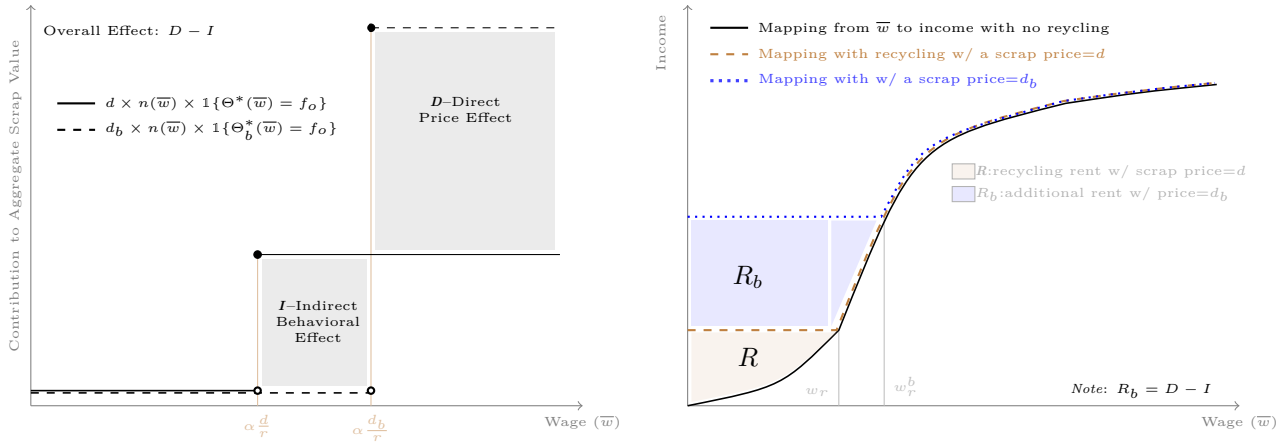
Table 2: The Effect of Bottle Bills on Birth Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Average Birth Weight						
$\mathbb{1}$(BB) × $\mathbb{1}$(not winter) × $\mathbb{1}$(<HS)	3.30*	4.02**	2.31*	3.39*	1.75	2.38
	(1.72)	(1.65)	(1.19)	(1.79)	(1.51)	(1.77)
$\mathbb{1}$(BB) × $\mathbb{1}$(not winter) × $\mathbb{1}$(HS)	0.86	0.80	0.42	0.12	-0.10	0.01
	(1.37)	(1.39)	(1.43)	(1.26)	(1.35)	(1.63)
$\mathbb{1}$(BB) × $\mathbb{1}$(not winter) × $\mathbb{1}$(>HS)	-1.45	-1.57	0.23	-0.99	0.73	
	(1.43)	(1.41)	(1.60)	(1.32)	(1.87)	
within R^2	0.210816	0.000746	0.000469	0.000007	0.000001	0.000001
Panel B: Number of Births						
$\mathbb{1}$(BB) × $\mathbb{1}$(not winter) × $\mathbb{1}$(<HS)	-0.44	-0.49	0.17	-0.30	0.04	-0.15
	(0.79)	(0.84)	(0.97)	(1.15)	(1.14)	(1.30)
$\mathbb{1}$(BB) × $\mathbb{1}$(not winter) × $\mathbb{1}$(HS)	-0.18	0.06	-0.25	-0.36	-1.15*	-1.10
	(0.53)	(0.60)	(0.55)	(0.58)	(0.68)	(0.71)
$\mathbb{1}$(BB) × $\mathbb{1}$(not winter) × $\mathbb{1}$(>HS)	-0.20	-0.57	-0.75	-0.20	0.06	
	(0.42)	(0.52)	(0.72)	(0.46)	(0.47)	
within R^2	0.049415	0.002953	0.002858	0.000002	0.000007	0.000006
Observations	2439765	2439765	2439765	2439765	2439765	2439765
County x Ed.		x				
County x Season	x	x		x		
County x Season x Ed			x		x	x
County x Season x Year						x
Season x Year	x	x	x			
County x Year	x	x	x			
Season x Ed x Year				x	x	x
County x Ed x Year				x	x	x

Notes: Table reports bottle bills' estimated effect on the average birth weight (Panel A) and number of births (Panel B) in non-winter relative to winter months. Specifically, table regresses the birth count on the following interaction $\mathbb{1}(\text{BB}) \times \mathbb{1}(\text{not winter})$, where $\mathbb{1}$(BB) is an indicator for counties with active bottle bills and $\mathbb{1}$(not winter) is an indicator for non-winter months. Estimates are broken out by education group, where $\mathbb{1}$(<HS) is an indicator for low education mothers, $\mathbb{1}$(HS) middle education mothers, and $\mathbb{1}$(>HS) high education mothers. Columns differ in the fixed effect included in estimation. The first column includes county x season, county x year, and season x year fixed effects, operationalizing the estimators with estimates reported in panel A, row 1 and panel B, row 1-3 as DiDiD estimators. Columns 2-4 include additional fixed effects, controlling for education specific characteristics in addition to those necessary to operationalize a DiDiD estimator. Column 5 includes fixed effects equivalent to those of column 1, but allowing for fixed effects to vary by education group, i.e., column 5 reports estimates from a fully stacked DiDiD estimator. Finally, column 6 reports estimates from a DiDiDiD estimator, where the fourth difference comes from comparing differences in the effect of bottle bills on low and middle education mothers in non-winter relative to winter months to that of high education mothers. Standard errors are clustered by state, the level of treatment.

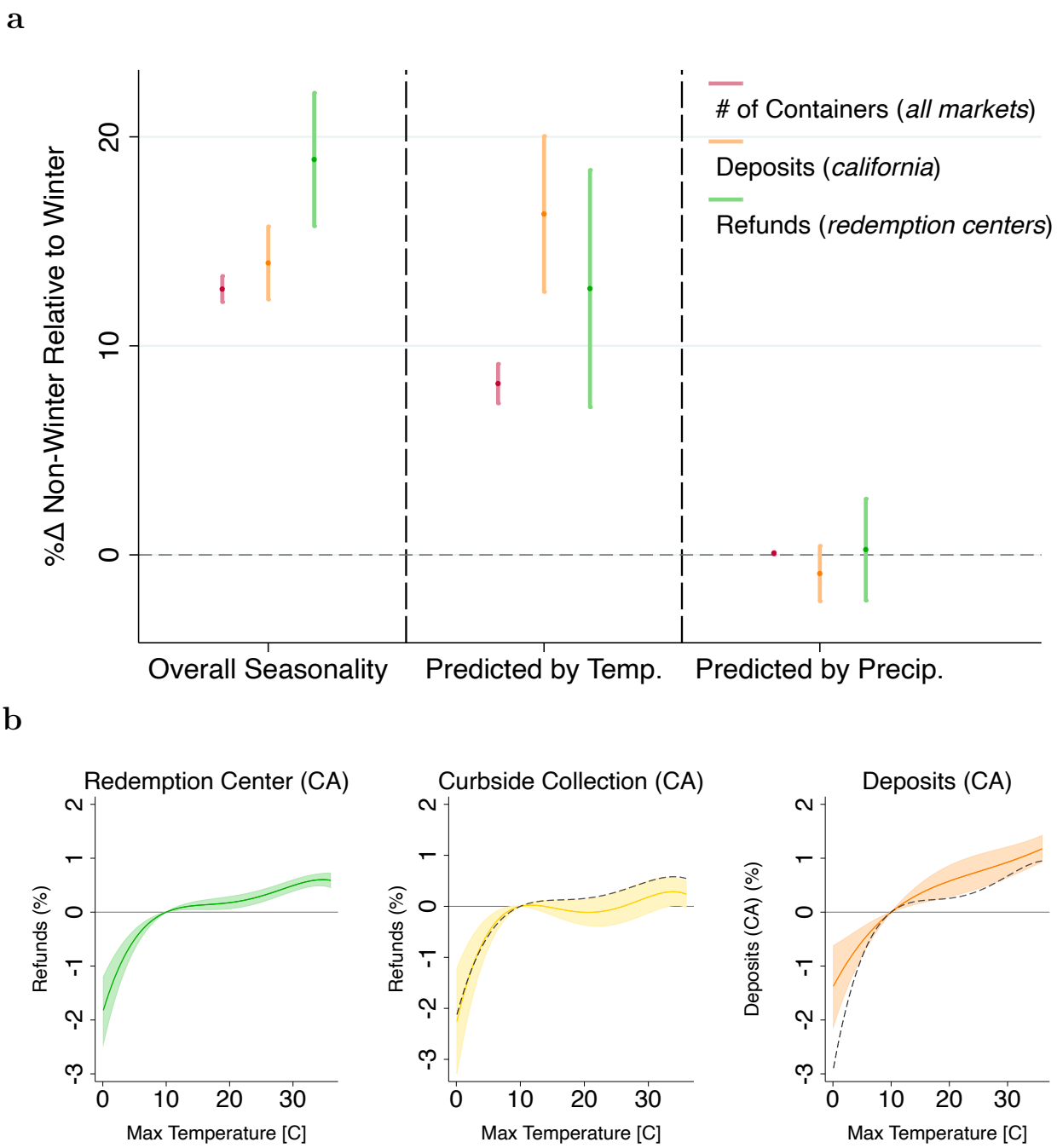
Figure 1: A Stylized Depiction of Bottle Bills' Effects

a Direct and Indirect Effects on Total Scrap **b** Effect on Individuals' Income



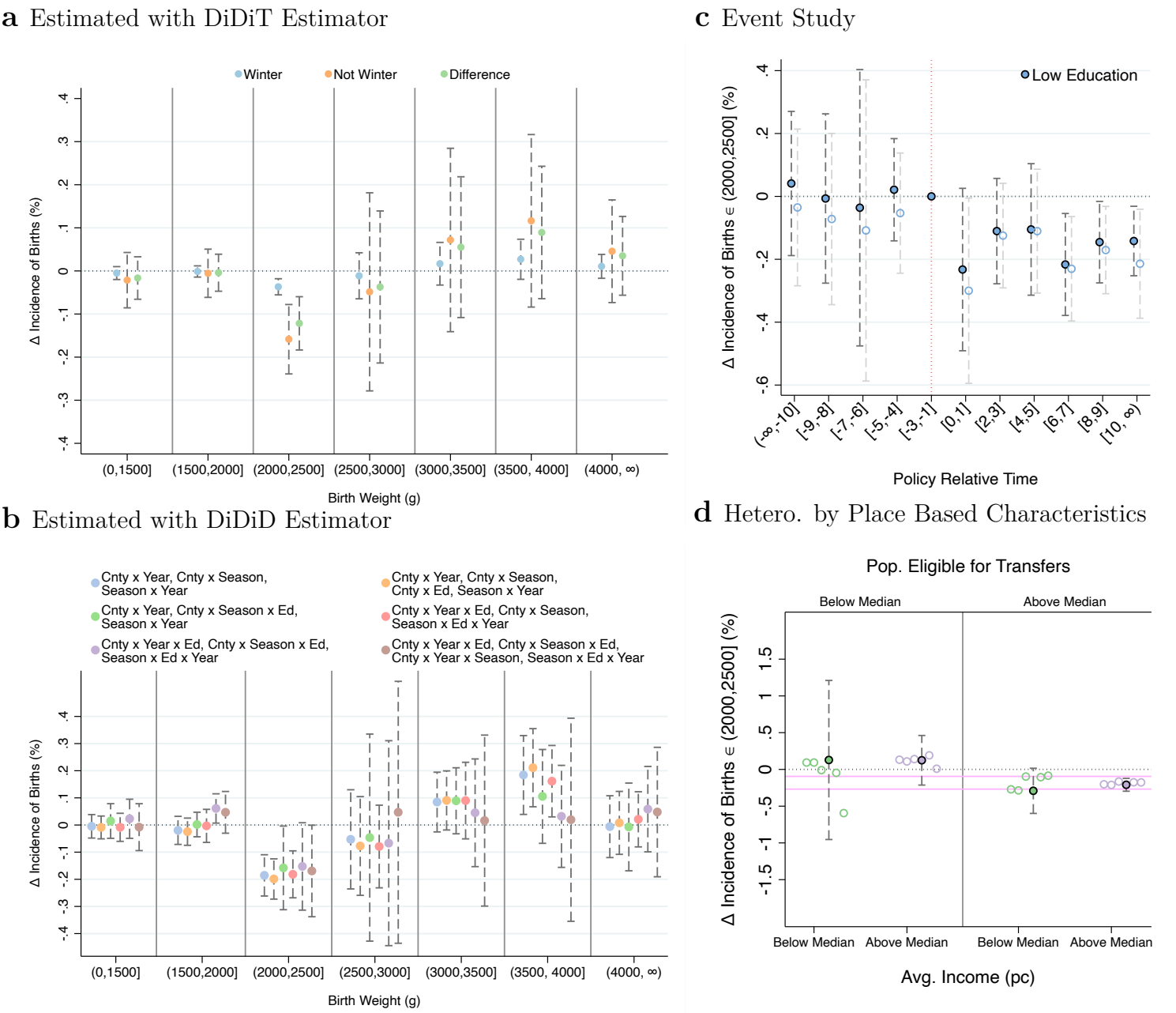
Notes: Figure depicts bottle bills' impact on the aggregate scrap value (Panel A) and on individuals' income from recycling (Panel B). **Panel A**'s solid black line depicts individuals' contribution to the aggregate scrap value dN, assuming (i) a uniform distribution of individuals across formal market wages and (ii) beverage consumption $n(\overline{w})$ does not change with wage $\overline{w}$. The gray area under contribution profile represents the aggregate value of the empties reservoir. The dashed black line depicts how the contribution profile changes with the introduction of a bottle bill. The reduction in the population of people who do not recycle their own containers–bottle bill's *indirect behavioral effect*–is denoted with the gray box and the I annotation. The increase in the contribution due to the higher scrap value–bottle bill's *direct price effect*–is denoted with the gray box and the D annotation. **Panel B**'s solid black line depicts the mapping from an individuals' formal market wage $\overline{w}$ to their income in the absence of informal recycling markets. The brown dashed line depicts the mapping with informal recycling markets, i.e., individuals with a $\overline{w}$ below the recycling wage w_r professionally recycle receiving a wage of $w_r > \overline{w}$, reaping the rents R depicted in the brown shaded region. The blue dotted line depicts the mapping with an active bottle bill, assuming the bottle bill increases the aggregate scrap value, i.e., $D > I$. w_r^b denotes the new recycling wage and R_b the additional rents from recycling due to the bottle bill. R_b is made up of a rectangle and a triangle. The rectangle denotes the effect on earnings at the *intensive margin*–the increase in recycling earnings for individuals who professionally recycled pre-bottle bill. The triangle denotes the effect on the *extensive margin*–the increase in earnings for people who did not professionally recycle pre-bottle bill.

Figure 2: Seasonality in Recycling Earnings – Evidence from Deposits and Refunds



Notes: Figure plots seasonality in the aggregate scrap value available (deposits) and aggregate earnings from recycling (refunds). **Panel A**, column 1 plots the overall increase in non-winter relative to winter months for deposits (orange) and refunds paid to individuals at redemption centers (green) in California (see estimation equation 3). For example, a coefficient of 25 implies that on average monthly deposits/refunds are 25% higher in non-winter relative to winter. Dots mark point estimates and shading depict 95% confidence intervals. Panel A, column 2(3) plots the relative increase as predicted by temperature (precipitation), where the temperature (precipitation) prediction is computed from the estimated refund/deposit dose response (see estimation equation 2). **Panel B** documents the results when we regress log(Refunds) or log(Deposits) on f(Weather) (see equation (2)), specifically depicting additional refunds/deposits (%) in response to maximum daily temperature, relative to 10°C. Solid lines represent point estimates and shading depicts 95% confidence intervals. Column 1 (2) plots weather responses for refunds paid out at redemption centers (to curbside collection companies). Column 3 plots responses for deposits paid at grocery stores. Columns differ in the estimation data sample. Column 1 uses the full data sample, Column 2 the sample with refund data on both curbside collection and redemption centers, and Column 3 the sample with refund and deposit data. Dashed lines in Columns 2 and 3 depict redemption center refund responses estimated using the each columns' respective sample, i.e., the dashed line in Column 2 plots the redemption center refund response estimated using counties with redemption center and curbside refund data. Columns 1-2 regressions control for county by year, county by season and season by year fixed effects. Errors are clustered by county. Column 3 includes the same fixed effects as Columns 1-2 except county specific fixed effects are instead market specific. Column 2 standard errors are robust to heteroskedasticity.

Figure 3: Bottle Bills' Effect on Low Education Mother Birth Weights



Notes: Figure plots bottle bills' estimated effect on low education mother birth outcomes. **Panel A and B** plot the effect throughout the birth weight distribution. The first panel of both column, denoted by $(0, 1500]$, plots the effect on the incidence of birth incidence less than 1,500 grams. Columns 2-7 plot birth incidence effects in bins sequentially increasing in birth weight, where the x-axis denotes the bin specification. **Panel A** plots estimates from a DiDiT estimator as described in equation 6, scaled by average maximum temperature in bottle bill states in winter months (blue markers), non-winter months (orange) as well as the difference between non-winter and winter months (green). **Panel B** plots estimates from a DiDiD estimator as described in equation 4 and 5. Panel B marker colors denotes the fixed effect specification used to estimate each coefficient, increasing in stringency from left to right within each column. Red markers denote the preferred specification (detailed in eq. 5), purple markers are equivalent to separately estimating DiDiD estimators for each education group, and brown markers denote a DiDiDiD estimator where the fourth difference is between low education mothers and high education mothers. **Panel C** plots event study estimates from a DiDiD style regression on the incidence of births between 2000 and 2500g, where year zero denotes the bottle bill implementation year. Specifically, the effect is plotted for every two years after 10 years before implementation and before 10 years after implementation. The effect in the three years before bottle bill implementation is normalized to zero. Solid markers denote estimates from a regression that controls for county x season, county x education x year, and season x education x year fixed effects (equivalent to red markers in Panel B). Hollow markers denote estimates from a regression that controls for county x season x education, county x education x year, and season x education x year fixed effects (purple markers in Panel B). **Panel D** plots heterogeneity in the effect on the incidence of births between 2000 and 2500g by place based characteristics. Solid markers denote the preferred fixed effect specification equivalent to red markers in Panel B or solid markers in Panel C. Hollow markers denote all other fixed effect specifications plotted in Panel B, where ordering corresponds to Panel B. Pink lines denote the overall effects' 95% confidence intervals. Finally, standard errors are clustered by state, the level of treatment. Dashed vertical grey lines denote the 95% confidence intervals for each estimate.

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Table A1: Balance Test

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

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

Table A2: DiD Estimates of Bottle Bills' Impact on the Incidence of Low Birth Weight

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

Notes: Table reports difference-in-difference estimates of the impact of bottle bills on the incidence of low birth weight ILBW. All reported estimates in a given column are from the same regression. Columns (1-4) report estimates from regressing bottle bill implementations $\mathbb{1}(\text{BB})$ on the ILBW, documenting bottle bills' impact on the overall population. Columns differ by the controls included in the estimating equation. For example, Column (1) includes no controls and Column (4) includes both weather and income controls. Columns (5-8) report heterogeneity in the DiD estimates by education group. In Columns (5-8), the first row documents bottle bills' impact on the ILBW among mothers with more than a HS education, and the third (second) row documents the difference between this association for mothers with a HS education (less than a HS education) and mothers with more than a HS education. As with the first four columns, Columns (5-8) differ in the included controls. Columns (9-12) report heterogeneity in the DiD estimates by season. In Columns (9-12), the first row documents bottle bills' impact on the ILBW in winter months, and the fourth row documents the difference between this effect for non-winter births and winter births. Again, Columns (9-12) differ in the controls included. 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 by state–the level of treatment.

Table A3: The Effect of Bottle Bills on Data Quality

	# Records (%Δ)	Records Dropped (%)				ILBW (%Δ)			
𝟙(BB)	-0.04	0.05	0.05	0.02	0.02	-0.01	-0.01	-0.01	-0.01
	(0.04)	(0.07)	(0.07)	(0.06)	(0.06)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	67657	67657	67067	66397	65777	67078	66500	65835	65215
within R^2	0.0009	0.0003	0.0003	0.0001	0.0001	0.0005	0.0003	0.0006	0.0006
County FE	x	x	x	x	x	x	x	x	x
Year FE	x	x	x	x	x	x	x	x	x
No Missing Birth Records			x	x	x		x	x	x
Max Dropped Record Rate < p99				x	x			x	x
Max %Δ ILBW < p99					x				x

Notes: Table regresses various measures of data integrity on bottle bill implementations denoted as 𝟙(BB). Column 1 reports the effect of implementations on %Δ in birth counts between Bailey et al. (2018) and the estimation sample. Columns 2-5 document bottle bills' effect on the percent of observations dropped (record dropping rate) due to missing birth weight data or the mother's age falling below 15. Columns differ in the estimation sample. Column 2 includes the entire sample, column 3 restricts the sample to counties with no missing birth records, column 4 additionally restricts the sample to counties with a maximum record dropping rate below the 99th percentile, and column 5 further restricts the sample to counties with a maximum %Δ ILBW below the 99th percentile. Columns 6-9 document the effect of bottle bills on the %Δ in ILBW between Bailey et al. (2018) and the estimation sample. As with Columns 2-5, columns 6-9 differ in the estimation sample. Column 6 employing the same sample as column 2, column 7 as column 3 and so forth. Standard errors are clustered at the state level, or the level of treatment.

Table A4: The Effect of Bottle Bills on (%) Birth Records Missing Mother's Education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\mathbb{1}$(BB)	-18.92	4.24					
	(23.25)	(12.95)					
$\mathbb{1}$(BB) $\times$ $\mathbb{1}$(not winter)			2.66	2.73	0.36	0.50	0.47
			(1.63)	(1.68)	(0.70)	(0.66)	(0.66)
Observations	796450	796450	796403	796403	667230	667230	651425
within R^2	0.034174	0.212887	0.0026	0.0103	0.0001	0.0042	0.0041
Weather Controls		x		x		x	x
Income Controls		x		x		x	x
County FE	x	x					
Year FE	x	x					
County x Season FE			x	x	x	x	x
County x Year FE			x	x	x	x	x
Season x Year FE			x	x	x	x	x
Sample Excludes TX, CA, WA, NM, ID, AR					x	x	x
No Missing Birth Records							x
Max Dropped Record Rate < p99							x
Max %Δ ILBW < p99							x

Notes: Table regresses the percent of birth records missing mother's education on bottle bill implementations $\mathbb{1}$(BB) and bottle bill implementations in non-winter months $\mathbb{1}$(BB) $\times$ $\mathbb{1}$(not winter). Column 1-2 report the effect of implementations on missingness. Columns differ in the controls included. Column 1 includes no controls beyond those needed to operationalize the DiD estimator, county and year fixed effects. Column 2 additionally includes weather and income controls. Columns 3-7 report the effect of bottle bill implementations in non-winter relative to winter months on missingness. Columns differ in the controls included and the estimation sample. All columns include county x season, county x year, and season x year specific fixed effects to operationalize the DiDiD estimator. Columns 3-4 report estimates from the full sample. Columns 5-6 exclude states with median missingness rates exceeding the mean. Column 7 further restricts the sample to counties without missing birth records (i.e., Bailey et al. (2018) birth counts match the estimation sample), counties with maximum record dropping rates below the 99th percentile, and counties with maximum %Δ ILBW below the 99th percentile. Additionally, columns 4, 6 and 7 include weather and income controls. Standard errors are clustered at the state level, or the level of treatment.

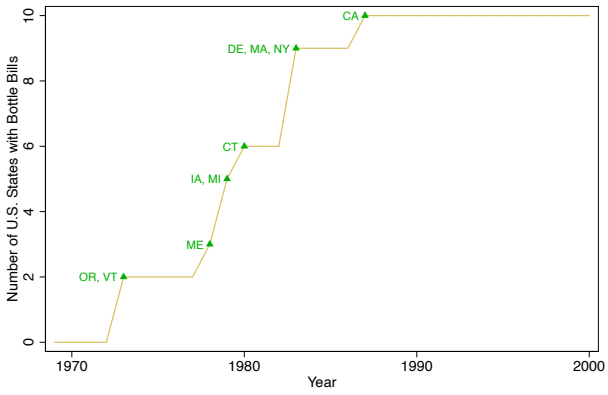
Table A5: Nielsen Product Modules Used to Assign Deposits

Product Module Code	Product Module Description	Product Group Description
452	REFERENCE CARD FOUNTAIN BEVERAGE	CARBONATED BEVERAGES
1030	FRUIT DRINKS & JUICES-CRANBERRY	JUICE, DRINKS - CANNED, BOTTLED
1031	CIDER	JUICE, DRINKS - CANNED, BOTTLED
1032	FRUIT JUICE - GRAPEFRUIT - OTHER CONTAINERS	JUICE, DRINKS - CANNED, BOTTLED
1033	FRUIT JUICE - APPLE	JUICE, DRINKS - CANNED, BOTTLED
1034	FRUIT JUICE - GRAPE	JUICE, DRINKS - CANNED, BOTTLED
1035	FRUIT JUICE-GRAPEFRUIT-CANNED	JUICE, DRINKS - CANNED, BOTTLED
1036	FRUIT JUICE - LEMON/LIME	JUICE, DRINKS - CANNED, BOTTLED
1037	FRUIT JUICE-ORANGE-CANNED	JUICE, DRINKS - CANNED, BOTTLED
1038	FRUIT JUICE - PINEAPPLE	JUICE, DRINKS - CANNED, BOTTLED
1039	FRUIT JUICE-PRUNE	JUICE, DRINKS - CANNED, BOTTLED
1040	FRUIT JUICE - ORANGE - OTHER CONTAINER	JUICE, DRINKS - CANNED, BOTTLED
1041	FRUIT DRINKS-CANNED	JUICE, DRINKS - CANNED, BOTTLED
1042	FRUIT DRINKS-OTHER CONTAINER	JUICE, DRINKS - CANNED, BOTTLED
1044	FRUIT JUICE-REMAINING	JUICE, DRINKS - CANNED, BOTTLED
1045	FRUIT JUICE-NECTARS	JUICE, DRINKS - CANNED, BOTTLED
1049	REMAINING DRINKS & SHAKES-NON REFRIGERATED	SOFT DRINKS-NON-CARBONATED
1054	VEGETABLE JUICE - TOMATO	JUICE, DRINKS - CANNED, BOTTLED
1055	VEGETABLE JUICE AND DRINK REMAINING	JUICE, DRINKS - CANNED, BOTTLED
1189	COOKING WINE & SHERRY	CONDIMENTS, GRAVIES, AND SAUCES
1484	SOFT DRINKS - CARBONATED	CARBONATED BEVERAGES
1487	WATER-BOTTLED	SOFT DRINKS-NON-CARBONATED
1553	SOFT DRINKS - LOW CALORIE	CARBONATED BEVERAGES
1635	PERISHABLE JUICE AND DRINK	JUICE, DRINKS - CANNED, BOTTLED
1636	PERISHABLE LIQUID COFFEE AND COFFEE DRINK	COFFEE
1637	PERISHABLE LIQUID TEA AND TEA DRINK	TEA
1638	PERISHABLE SOFT DRINK AND WATER	SOFT DRINKS-NON-CARBONATED
5000	BEER	BEER
5001	NEAR BEER/MALT BEVERAGE	BEER
5010	LIGHT BEER (LOW CALORIE/ALCOHOL)	BEER
5020	MALT LIQUOR	BEER
5040	ALCOHOLIC COCKTAILS	LIQUOR
5041	WINE-VERMOUTH	WINE
5049	WINE-APERITIFS	WINE
5050	WINE-DOMESTIC DRY TABLE	WINE
5052	WINE-IMPORTED DRY TABLE	WINE
5053	WINE-FLAVORED/REFRESHMENT	WINE
5054	WINE-KOSHER TABLE	WINE
5055	WINE-SAKE	WINE
5056	WINE-SANGRIA	WINE
5057	WINE-SPARKLING	WINE
5058	WINE-SWEET DESSERT-DOMESTIC	WINE
5059	WINE-SWEET DESSERT-IMPORTED	WINE
5060	WINE - NON ALCOHOLIC	WINE
6074	RBC BEER	BEER
6079	RBC JUICE AND DRINK	JUICE, DRINKS - CANNED, BOTTLED
6086	RBC SOFT DRINK AND WATER	SOFT DRINKS-NON-CARBONATED
6088	RBC WINE	WINE

Notes: Table lists product modules with products for which consumers likely paid deposits. The first column gives the Nielsen product module code, the second the product module description, and the third an example product with the product module. Deposits are assigned based on the UPC (i.e., product) size and the California legislation in the purchase year. During the sample period California changed deposit size in 2007 from 4 cents to 5 cents for containers less than 24 ounces and 8 cents to 10 cents for containers more than 24 ounces.[19] Container purchases prior to 2007 are assigned a deposit of 4 (8) cents for containers smaller (larger) than 24 ounces. After 2007, containers smaller (larger) than 24 ounces are assigned 5 (10) cent deposits.

[19] https://www.bottlebill.org/index.php/current-and-proposed-laws/usa/california

Figure A1: U.S. Bottle Bill Introductions



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