Brazilian Battery Boom: Tax Breaks as Anti-Pollution Policy

Mikey Jarrell   Nathan Lazarus   David L. Vargas

June 17, 2025

Abstract

In 2005, Brazil began an ambitious attempt at regulating a high-polluting industry: lead-acid battery recycling. They pursued policies of tax breaks for formal (less-polluting) recycling firms, and drove their informal competitors out of business with verification requirements. This led to a centralization of lead battery recycling in a few municipalities that were home to formal recycling factories, increasing their exposure to lead emissions from these factories, which is associated with adverse consequences for children's cognition. Our preliminary results show that the tax break caused a sharp divergence in elementary school student performance between municipalities that did have these factories and those that did not. This supports the hypothesis that lead pollution fell across the country as informal recycling disappeared, while pollution increased near formal firms as those expanded. We are currently incorporating matched employer–employee data from the Relação Anual de Informações Sociais (RAIS), which allows us to estimate a first stage (how much recycling firms grew) and provide some of the first evidence on the effects of lead exposure as a child or young adult on labor market outcomes. Additionally, we are in the process of incorporating data on birth weights, which will give us a more direct measure of health impacts. We are hopeful that these additional results will paint a complete picture of the effects of this policy change and allow us to judge whether the template created by Brazil should be copied by the many other countries facing this challenge.

1 Introduction

Scientists have been aware of the negative consequences of being exposed to lead for centuries, and economists have added to that understanding over the past few decades (Aizer et al., 2018). Developed countries have, with occasional notable exceptions, largely eliminated lead as a serious public health hazard (Reyes, 2007). However, the battle is far from complete in developing countries, where lead exposure is thought to be robbing people of millions of life-years and hundreds of millions of IQ points each year (Larsen and Sánchez-Triana, 2023). One vehicle for lead exposure in poor countries is the pollution from the recycling of lead-acid batteries (Ericson et al., 2016). Living near battery-recycling plants has been found to have a negative causal relationship with health outcomes (Tanaka et al., 2022). It is thought that when this recycling is done informally, as is the case in many poor countries, the pollution rates — and, therefore, the health consequences — are even worse (Mahzab et al., 2024).

This was the (ostensible) rationale for a series of policy changes in Brazil in 2005–19 (Smith, 2024). Brazil is home to a substantial lead-acid battery manufacturing and recycling industry that was, in their telling, struggling to compete against the tax-evading and regulation-flouting informal recycling sector. Beginning in the early 2000s, Brazilian regulators and formal sector allies began to implement a series of policies to help Brazil's formal lead-acid battery recycling sector out-compete informal recyclers. The crown jewels of this lobbying effort was an exception to the value-added tax (VAT) beginning in 2005.[1] By leveling the tax playing field and by at least partially internalizing the environmental externality of lead pollution, Brazil appears to have eliminated the informal lead-acid battery industry and enabled its formal counterpart to flourish. They remain the only example of a low- or middle-income country (LMIC) to have done so. It is important to understand the effects of this policy change in order to judge whether it ought to be emulated in the many other countries facing a similar challenge.

This paper aims to estimate those effects: the causal effects of this policy change on health outcomes. We take a difference-in-differences approach to the data, comparing municipalities that were home to formal lead-acid battery firms (we will sometimes refer to these "treated" municipalities as "battery" municipalities) to those that were not ("control"), before and after the implementation of the tax break. The outcome measure on which we compare these municipalities is the one to which we were able to acquire access at the appropriate level of geographic granularity: education. This is a reasonable choice, since education outcomes have been found to be sensitive to small changes in lead exposure even within of one year of a policy change (Hollingsworth et al., 2022).

After the tax break, we find that the fraction of fourth graders in battery municipalities dropped by 14 percentage points compared to non-battery municipalities — a 20% decrease.

[1] Hereafter, for the sake of brevity and concreteness, we will occasionally refer to the policy changes as simply a "tax break." In reality, we should think of these as a bundle of policy changes aimed at eliminating informal players within the lead-acid battery industry, of which the tax break is but one example.

We find qualitatively similar results for the fraction who fail, drop out, or are too old for their grade level. The extent to which the reader believes that test scores in battery municipalities would have followed the same trends as test scores in the rest of the country had it not been for the policy change is an upper bound on the extent to which the reader should interpret our results as causal effects. Whether this is a good assumption is *ex ante* ambiguous.

The reasons for this ambiguity are several, but we will briefly mention one here: the geographic distribution of informal recyclers. To illustrate, assume that informal recycling causes at least as much pollution per battery recycled than formal recycling (Kinally et al., 2024). Further, assume that the tax break caused the formal recycling firms to out-compete and entirely eliminate the informal recycling sector. If there is no correlation between the location of informal recyclers and the location of formal recyclers, then, unless there are other time-varying confounders, our difference-in-differences results represent the causal effect of the tax break for formal recyclers on test scores for nearby students: a profoundly harmful policy change. However, if informal recycler location is correlated with formal recycler location, then spillover effects could bias our estimated treatment effects in either direction; see Section 3.1 for a more detailed discussion. Absent knowledge of the locations of informal recyclers, our results can be conservatively interpreted as quantifying a distributional effect of concentrating pollution in a small number of municipalities; we leave estimation of the net welfare effect of the policy change to future work.

Our estimated treatment effects are quite large, although not entirely out of line with existing literature on the effect of lead exposure on education outcomes. Billings and Schnepel (2018) find that an intervention to reduce lead exposure in North Carolina caused a 0.12 standard deviation increase in test scores among 3–5th graders (pooled across reading and math). Aizer et al. (2018) find that a one µg/dL reduction in blood–lead among children in Rhode Island decreases the probability of being "substantially below proficient" in reading by about one percentage point (an 8% decrease) and in math by 0.8 percentage points (a 5% decrease). Hollingsworth et al. (2022), using data from Florida, found that NASCAR's 2007 switch from leaded to unleaded gasoline caused test scores among 3–5th graders living near the racetracks to increase by 0.08 standard deviations. In a meta-analysis of observational studies, Crawfurd et al. (2024) find that lead exposure is responsible for one-fifth of the gap in test scores between rich and poor countries. More related to our work, Ipapa (2023) finds that the introduction of 26 new lead-acid battery recycling facilities in Kenya in 2007 reduced test scores by 0.05 standard deviations, while Litzow et al. (2024) find that a 2009 policy change in the US that increased battery recycling in Mexico (the same shock as in Tanaka et al. (2022)) decreased test scores by 0.05–0.09 standard deviations.

In future drafts of this paper, we will push our results further. We will run synthetic control regressions in an attempt to resolve our parallel trends problem. We will attempt to re-clean and extend the education data that was originally compiled by the Inter-American Development Bank, because we have access to the original data and will be able to rule out any funny business stemming from the cleaning process. We will use health variables as outcomes in the same regressions run here, and we will find a boatload of zeros. We will run regressions using a more specific treatment variable which should capture changes in size of the battery firms — under the pretense that the only municipalities that should be affected by the policy change are those in which the battery firms grew and therefore processed more batteries and therefore created more lead pollution — and find small but still statistically significant results in the expected directions. We will attempt to get even more granular with our results, running regressions at the level of the zipcode (of which there are 900,000) units municipality — of which there are 5,000.

The above additions will address the most proximate flaws with the paper: we don't necessarily believe the results. This is an internal validity question. More imporantly — and, in my optinion, more importantly — we need to address the impact evaluation question: what is the net effect of the policy change on Brazil as a whole? To do that, we need to be able to say something about the treatment effect *on the control group* — something that our DiD regressions are fundamentally incapable of doing. So what do we do instead? There are two possible roads to take: which one we take depends on how the informal recyclers were distributed geographically prior to the policy change.

The first road is to assume that the informal recyclers were uniformly distributed across the country. In this case, we are relegated to the land of time series: a land in which I am hopeless lost, but where luckily UCSD has very capable tour guides. The second road is to identify places that we home to informal recyclers, and then define a second treatment group: places in which informal recycling decreased. In this places, treatment should have the opposite affect as we find in this paper: pollution should have decrease, and health and education should have improved. But where are these places? We don't know yet, but we're on the hunt.

2 Data

2.1 Educational Data

Our primary source of educational data is the Brazilian Education Panel Database (BEPD), compiled by the Inter-American Development Bank. This panel integrates various official sources that provide educational and socio-economic metrics across Brazil. In particular, it combines data from Brazil's School Census (Censo Escolar), the National Basic Education Assessment System (SAEB), and the Brazilian Institute of Geography and Statistics (IBGE).

Our primary outcomes include BEPD failure, passing, and dropout rates. These outcomes are reported at both the school and municipality levels, but we focus specifically on the municipality level indicators. Table 1 presents some basic summary statistics of the education outcomes in the relevant sample.

Table 1: LEARNING OUTCOMES

Percent of students	Mean	SD
Pass	73.59	13.52
Fail	13.15	8.20
Drop out	7.20	7.15
Transfer	7.35	6.42
Are overage for grade	29.13	19.30

Notes: Average outcomes for of year four students for all (5,570) municipalities 2002–15

The failure rate represents the proportion of students who do not meet the academic criteria to advance to the next grade level by the end of their fourth year. In contrast, the passing rate indicates the proportion of students who successfully fulfill the academic requirements for advancement. Additionally, some students may fully drop out of school, which is neither categorized as failing nor passing. The dropout rate reflects the proportion of students who leave the school system or fail to re-enroll without completing the academic year or advancing to the next grade. This is better summarized by the following set of equations:

$$\begin{aligned}
\text{Passing Rate} &= \frac{\text{Number of students passing by the end of the year}}{\text{Total number of students at the beginning of the year}}\\
\text{Failing Rate} &= \frac{\text{Number of students failing by the end of the year}}{\text{Total number of students at the beginning of the year}}\\
\text{Dropout Rate} &= \frac{\text{Number of students dropping out during the year}}{\text{Total number of students at the beginning of the year}}\\
\text{Transfer Rate} &= \frac{\text{Number of students transferring to other school}}{\text{Total number of students at the beginning of the year}}
\end{aligned} \tag{1}$$

As this is reported at the school level, and later summarized at the municipality. Schools only identify if the student transferred, but not their end outcome in the new school. Hence, the total array of options is characterized by passing, filling, dropout, and transfers.

2.1.1 Labor Market Data

In the labor market data from RAIS, we can see the location of individuals' first jobs, and use that as a proxy for where they attended school. This will then allow us to examine the effects of lead pollution from ULAB recycling on wages, occupational choice, years of schooling, and migration. We can also calculate a measure of worker skill from AKM person effects (Abowd et al., 1999).

2.2 Firm Data

In addition to educational data, we employ information on industry association membership from the Brazilian Institute of Recyclable Energy (IBER) associate list. IBER is what is sometimes known as a "producer responsibility organization": it serves as the liaison between the government and the industry. All legal lead-acid battery recyclers and manufacturers must register with IBER and comply with its standards. IBER provides a comprehensive list of the legal firms in the sector, so we scraped their website to create a detailed dataset with company names, tax IDs, locations, and types of businesses (recycler, manufacturer, retailer, etc.) *as of 2023.*[2].

As this is a post-treatment measure, we use the firms' tax IDs to track their history. Using the firms history, we restrict our sample to those firms that first opened and registered before 2005, when the law took place. Although this measure is still imperfect, given that the tax break heavily favors legally constituted firms, is unlikely that firms closed after the law took place. However, given our current data limitations, this hypothesis is left untested, so completely at the reader's discretion.

In our setting, we care about the presence of legal firms in a municipality. The first approach involves adding up the municipality's total number of associate firms. Table 2 illustrates the municipal level's average, maximum, and minimum number of firms. This shows that the average municipality has very few or most likely lacks member firms. This is especially true if we focus on recyclers and manufacturers. Complementing this, table 8 shows the total members count by type.

This raises further questions regarding the composition of the firms in this industry. For instance, the low representation might be linked to market concentration among a few companies. While it is difficult to verify this with our data, considering Brazil's vast geographical size, high market concentration could allow one firm to operate in multiple locations. However, Table 7 refutes this notion, revealing that out of the 569 firms, 397 are distinct companies. Furthermore, these companies are distributed across more than 250 municipalities.

[2]We believe this is sufficient for our purposes, i.e., that the location of a battery recycling plant in 2023 is noisy but unbiased estimate of the location of a battery recycling plant in 2005. Our hope is to validate this assumption with older data, but we are still waiting to hear back from IBER as to whether they can share records. With more time, we might explore alternative strategies, e.g., satellite data, tax records, etc.

Table 2: Municipality-Level Firm Presence by Type

Lead-acid battery firm type	Mean	SD	Min	Max
Assembler	0.002	0.045	0	2
Battery Manufacturer	0.005	0.106	0	4
Consumer	0.011	0.153	0	6
Dealership	0.000	0.014	0	1
Distribution Center	0.002	0.045	0	1
Distributor	0.053	0.451	0	13
Importer	0.004	0.068	0	2
Location	0.000	0.014	0	1
Logistics Operator	0.001	0.045	0	2
Recycler	0.002	0.047	0	1
Retailer	0.021	0.269	0	12

Sample period: 2002–2015

Notes: Average number of formal lead-acid battery firms per municipality broken down into types of firms. Averages calculated across 5,570 municipalities.

For the analysis, we focus only on manufacturing and recycling firms, as they are the ones with the capability to process the lead for recycling. The 1 shows the firm location across Brazil. There are few legally constituted firms, and they are heavily concentrated in the south of the country.

3 Empirical Strategy

Our identification strategy leverages the timing of the Brazilian government's tax break announcement in 2005, along with the spatial distribution of legally established battery recyclers and manufacturers. Specifically, we estimate the following differences-in-differences regression:

$$Y_{m,t} = \alpha_m + \lambda_t + \beta \text{Battery}_i \times \text{Post Tax Break}_t + \varepsilon_{m,t} \tag{2}$$

Where $Y_{m,t}$ represents various educational performance outcomes at the municipality level, including average failure, passing, and dropout rates. The variable Battery_m is a dummy variable that equals one if there is at least one legal recycling or manufacturing facility in municipality m, measuring the municipality's exposure to legal lead recycling. The variable Post Tax Break_t is a dummy that takes the value of one after the tax break

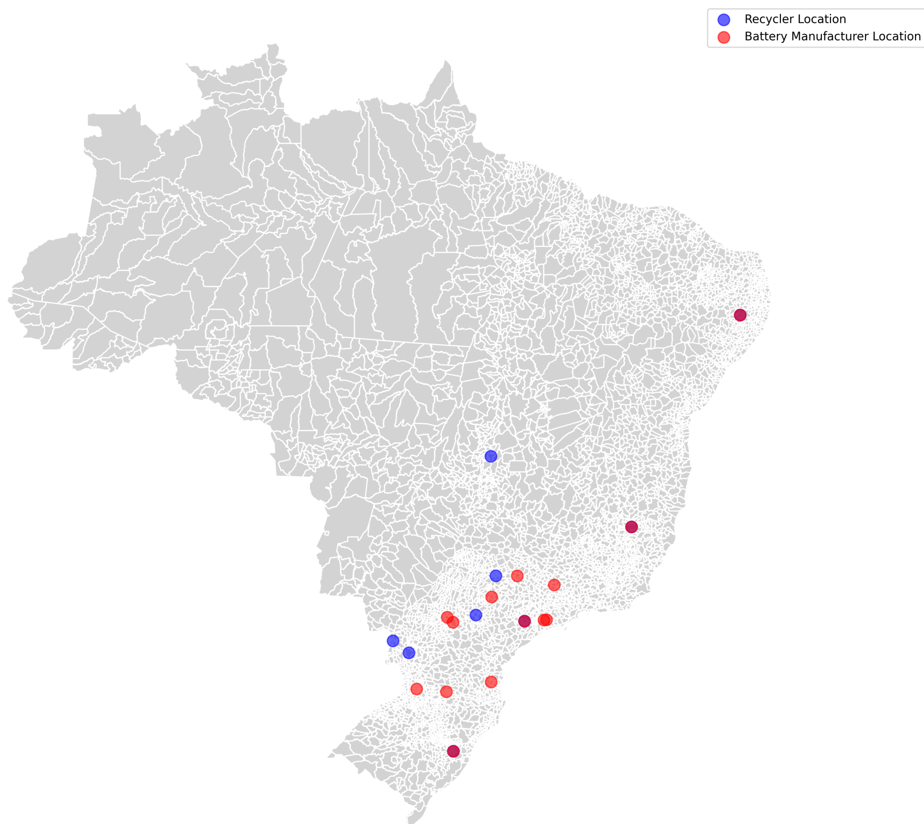

Figure 1: Figure 1: locations of battery firms

announcement on lead production.

We include municipality and time-fixed effects, represented by α_m and λ_t, respectively, which allow us to control for any observed and unobserved municipality-specific characteristics that remain constant over time, as well as national-level temporal shocks. Finally, $\varepsilon_{m,t}$ is the error term, with clustering at the municipal level.

3.1 Identifying assumption

The fundamental assumption in our design is the parallel-trends assumption. This means that, without the tax break, the educational outcomes in municipalities with legal lead battery recyclers and manufacturers would have been similar to those without them. We partially address this by estimating the following dynamic differences-in-differences regression:

$$Y_{m,t} = \alpha_m + \lambda_t + \sum_{j \in J} \beta_j \text{Battery}_i \times \delta_j + \varepsilon_{m,t} \tag{3}$$

Where J is a set that includes all years from 2002 to 2015, excluding the year immediately preceding the tax break announcement, 2004, and δ_j is a dummy equal to one when $t = j$. Thus, each β_j captures the differential educational gains or losses in municipalities with the presence of legal firms compared to those without any legal firms during year j, relative to the year prior to the announcement of the tax break law.

One challenge that we grapple with but ultimately ignore is the blatant stable unit treatment value assumption (SUTVA) violation inherent in our setting. We are explicitly measuring the spillover effect on formal firms of a policy directed at informal firms. If the policy had any effect on formal firms, it is by definition through its effect on their informal competitors. The distribution of informal firms across Battery and Non-Battery municipalities is unknown. If they are concentrated more in Battery municipalities, this will offset our estimated treatment effects, biasing them towards zero; if concentrated more in Non-Battery municipalities, the bias will inflate our estimated treatment affect.

4 Results and Discussion

We illustrate the main results of our difference-in-difference regression from Equation 3 in Figure 2. The y-axis measured learning (defined as the pass rate of fourth grade students) in municipalities that have formal lead-acid battery recyclers or manufacturers compared to municipalities that do not. We plot this difference for each year 2002–15, and normalize it such that the difference is 0 in 2004.

To make this more concrete, let's examine the 2007 results. The plot shows a treatment effect of -12 percentage points. How was this number calculated? In the figure, the "omitted" year is 2004, so that is our starting point. In 2004, the average treatment municipality saw 80% of its grade four students pass, compared to only 70% in control municipalities, a difference of 10 percentage points. By 2007, the results had flipped: in treatment municipalities, pass rates had fallen to 75%, while the control municipalities, pass rates had jumped up to 77%: a difference of -2 percentage points. Subtracting 10 from -2 produces our difference-in-difference estimate of -12 percentage points.

This example also makes clear the implications of the parallel trends assumption discussed earlier. If we believe that the 10 percentage point difference that we found in 2004 would have held through 2007 if it weren't for the policy, then we can interpret the -12 percentage points as the causal effect of the tax breaks, i.e., that the policy caused the pass rate to fall by 12 percentage points in municipalities where lead-acid battery recycling or manufacturing is present between 2004 and 2007.

Beginning in 2006, the year after the tax break announcement, we find treatment effects

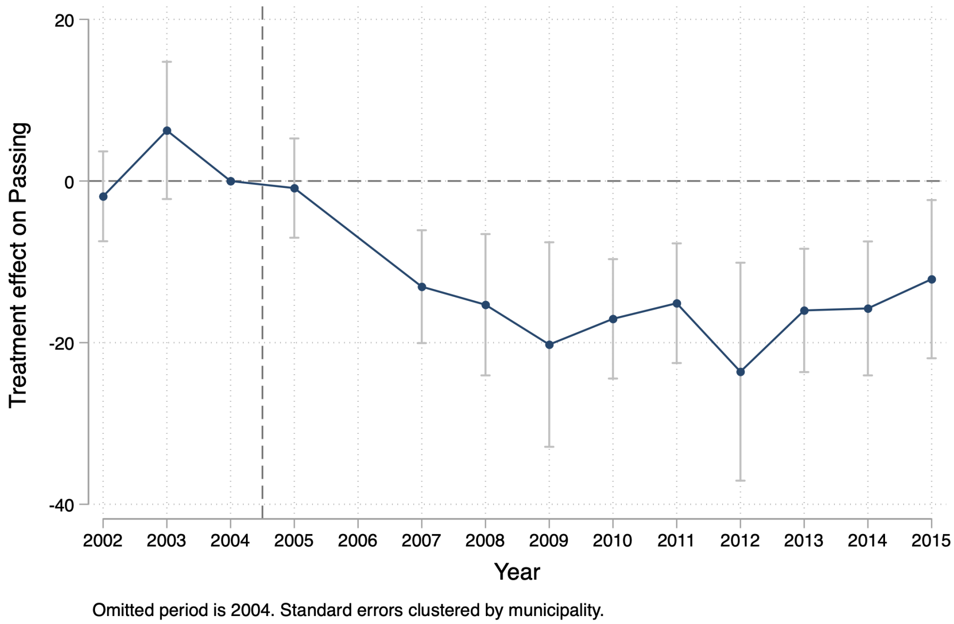

Figure 2: Estimated of the effect of the tax break on fourth grade pass rate

Notes: The y-axis plots coefficients from a difference-in-differences regression of the pass rate of fourth grade students on the presence of formal lead-acid battery manufacturers or recyclers at the municipality-level. The treatment effect is normalized such that 2004 is zero. Error bars reflect 95% confidence intervals.

that statistically significant. Qualitatively, they are large and negative[3], suggesting that the tax break caused a meaningful decline in learning in municipalities where battery firms are located — assuming the parallel trends requirement is satisfied. Although this assumption is unverifiable, the pre-trends, albeit limited in data availability, are reassuring: we see that the change between 2002 or 2003 and 2004 is statistically indistinguishable from zero, suggesting that before the tax break, the battery municipalities are on similar learning trends as the non-battery municipalities.

One might worry that pass rates are an imperfect measure of learning. For example, suppose the policy changes truly did cause an increase in lead pollution in battery municipalities relative to non-battery municipalities. Consider a student for whom this additional lead exposure caused them to do so poorly in school that they drop out; absent the policy changes, they would merely have failed to pass. If this student disappears from the dataset rather than being included among the non-passing students, then the policy changes would

[3]These effects are larger than most prior studies on the effects of lead exposure on education outcomes, but these almost exclusively come from the United States, where lead exposure is substantially lower than it is in developing countries (Reyes, 2015; Billings and Schnepel, 2018; Aizer et al., 2018; Hollingsworth et al., 2022). The only examples in economics that study this effect in LMICs have found treatment effects around 0.05–0.12 standard deviations, which is substantially smaller than what we find (**?**Ipapa, 2023; Crawfurd et al., 2024).

appear to have caused learning to *increase*. This differential attrition across treatment and control would bias the results upwards.

Similarly, consider a student for whom this additional lead exposure caused them to do badly enough to be held back a grade. Imagine that absent the policy they would have moved on to the next grade and then failed, but with the policy change, they pass as a result of repeating the grade. Again, then our regression would show the treatment effect to be an *increase* in the pass rate despite a decrease in learning.

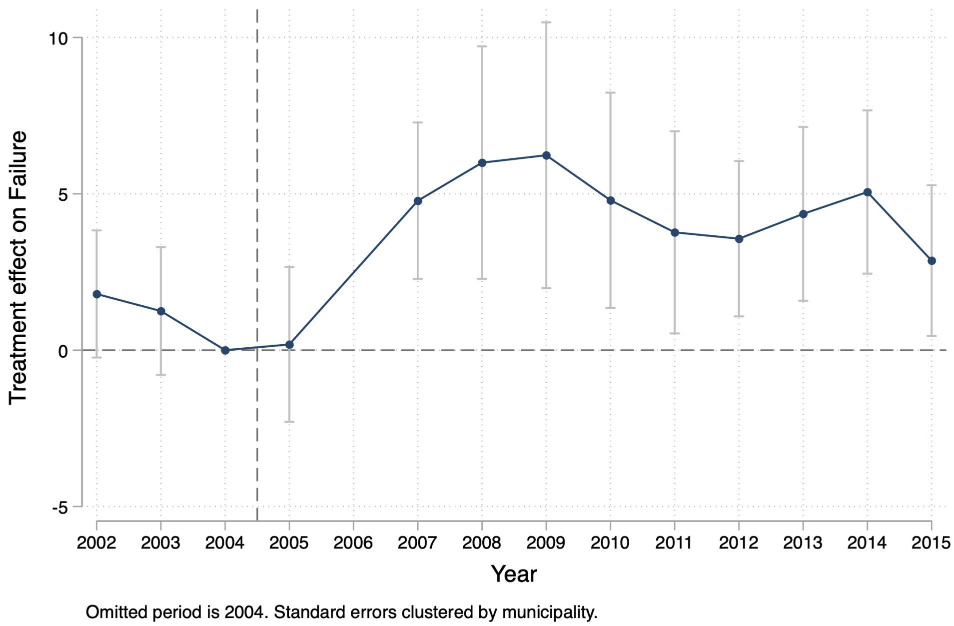

Figure 3: Estimated of the effect of the tax break on fourth grade failure rate

Notes: The y-axis plots coefficients from a difference-in-differences regression of fraction of fourth grade students who fail on the presence of formal lead-acid battery manufacturers or recyclers at the municipality level. The treatment effect is normalized such that 2004 is zero. Error bars reflect 95% confidence intervals.

To partially address these concerns, we report the results of the same specification but where we use alternative measures of learning. Instead of pass rate as the outcome of interest, Figure 3 uses failure rate, Figure 4 use dropout rate, and Figure 5 uses over-age rate (defined as the fraction of students who are older than the norm for their grade level). The directions and magnitudes of these results are in line with what one might expect given the pass rates results in Figure 2: after the policy change, the fraction of students who failed, dropped out, or were over-age all increased in battery municipalities.

Table 3 collapses the year-by-year non-parametric specification into the simple two-by-two (i.e., battery vs. non-battery, before vs. after) specification of Equation 2. The coefficients of

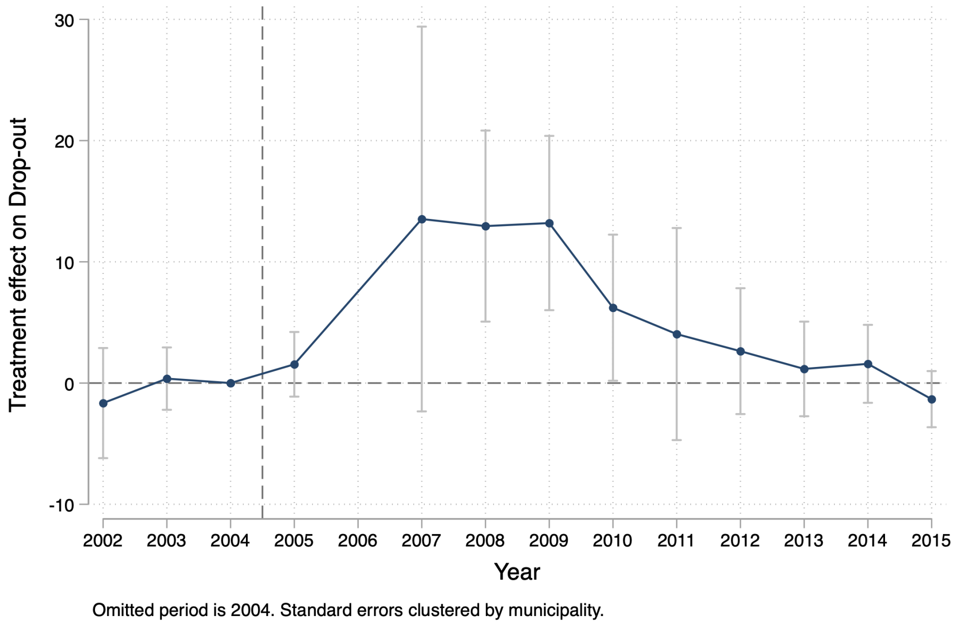

Figure 4: Estimated of the effect of the tax break on fourth grade dropout rate

Notes: The y-axis plots coefficients from a difference-in-differences regression of fraction of fourth grade students who drop out on the presence of formal lead-acid battery manufacturers or recyclers at the municipality level. The treatment effect is normalized such that 2004 is zero. Error bars reflect 95% confidence intervals.

Table 3: Effect of Tax Breaks

VARIABLES	(1) Passing	(2) Failure	(3) Drop-out	(4) Transfer Rate	(5) Over-age
Presence × Post Regulation	-14.02***	2.484**	4.894***	0.503	14.94***
	(2.891)	(1.090)	(1.768)	(3.488)	(2.489)
Observations	52,491	52,543	52,543	23,202	55,914
R-squared	0.520	0.468	0.318	0.333	0.494
Year FE	YES	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES	YES
Mean DV	69.79	11.57	6.479	13.35	31.71
SD DV	15.66	7.563	6.958	11.79	20.39

interest tell us how much the outcome of interest changed in battery municipalities relative to non-battery municipalities after the policy change. For example, Column (1) found that the tax break caused a reduction in the fraction of fourth grade students who passed to decrease by 12 percentage points. Meanwhile, the fraction of students who failed increased

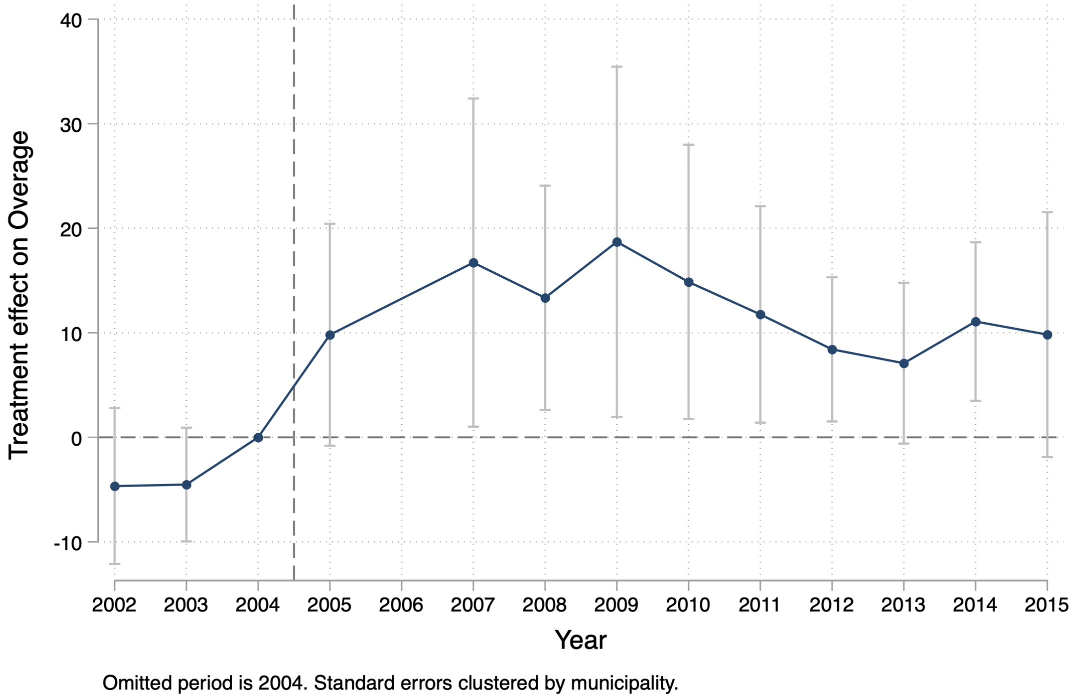

Figure 5: ESTIMATED OF THE EFFECT OF THE TAX BREAK ON FOURTH GRADE OVER-AGE RATE

Notes: The y-axis plots coefficients from a difference-in-differences regression of fraction of fourth grade students who are older than the typical fourth-grade age on the presence of formal lead-acid battery manufacturers or recyclers at the municipality level. The treatment effect is normalized such that 2004 is zero. Error bars reflect 95% confidence intervals.

by 2 percentage points; this is not mechanically identical to the change in fraction who passed because the tax break also caused the fraction who dropped out to increase by 4 percentage points and the fraction who were held back to increase by 15 percentage points. The fraction of students who transfer, which we might think of as outcome which is unlikely to be affected by the policy change, is approximately zero. In the appendix, as a robustness check, we report results for the subset of schools that are public as opposed to private. These are similar to the main results but noisier.

4.1 Balance and matching

To further asses the validity of our results we check for the balance on covariates between the control and treatment municipalities. Table 4 shows the mean for the treatment and control units, and their difference.

There is a strong imbalance across units. Mainly, the treatment units are richer and

	Treatment	Control	Difference
Population	216,522	23,694	192,828
	(290,599)	(52,439)	[0.00]
GDP per capita (BRL)	11,054	6,422	4,632
	(4,393)	(7,507)	[0.01]
Gini coefficient	0.54	0.55	−0.01
	(0.04)	(0.07)	[0.82]
Mortality per 1,000	5.54	4.46	1.08
	(1.66)	(2.54)	[0.06]
Infant mortality per 1,000 births	15.86	19.80	−3.94
	(8.52)	(16.84)	[0.31]
Human Development Index	0.66	0.52	0.14
	(0.06)	(0.10)	[0.00]
Observations	19	5,514	

Table 4: Brazilian municipalities in 2003. Parentheses denote standard deviations, brackets denote p-values. Human Development Index is a composite score of education, longevity, and income.

more populous compared to the rest of the country. To address this issue we take advantage of the methods developed by Abadie and Imbens (2006) to match on observables. We first estimate a propensity score using by a logistic regression, following the equation 4. Then, each treated unit is matched to up to 100 control units with the closest propensity scores.[4]

$$\begin{gathered} P(T=1 \mid X) = \tfrac{\exp(X\beta)}{1+\exp(X\beta)} \\ T=1 : \text{Treated unit} \quad X : \text{Observed covariates.} \end{gathered} \tag{4}$$

Once we have the matched control municipalities. Control units matched multiple times are assigned fractional weights, following 5, which then used to re-estimate the main regression 3

$$w_c = \frac{\text{Number of times } c \text{ is matched}}{\text{Total treated units matched to } c} \tag{5}$$

As expected this approach helped us overcome the comparability issue we were facing. Table 5 shows the mean for treatment and controls in the matched sample. The new differences are statistically equal to zero, hence we end up with a more comparable set of controls after matching.

Furthermore, our main results hold. Table 6 shows our main specification using the matched sample and weights. Although we lose statistical significance in the failure rates, all other results hold. There is some attenuation on the effects, however, all effects on passing, dropout, and overage remain large and statistically different from zero.

[4] All of this within the common support, please see 6 in the appendix

	Treatment	Control	Difference
Population	208,387	211,457	$-3,070$
	(308,398)	(265,371)	[0.95]
GDP per capita (BRL)	11,456	10,726	730
	(4,560)	(6,004)	[0.43]
Gini coefficient	0.54	0.54	0.00
	(0.05)	(0.06)	[0.59]
Crude mortality per 1,000	5.61	5.57	0.04
	(1.80)	(2.11)	[0.91]
Infant mortality per 1,000 births	15.92	14.62	1.30
	(9.26)	(7.60)	[0.38]
Municipal Human Development Index	0.66	0.66	0.00
	(0.06)	(0.06)	[0.96]
Observations	16	114	

Table 5: Brazilian municipalities in 2003. Parentheses denote standard deviations, brackets denote p-values. Human Development Index is a composite score of education, longevity, and income.

Dependent Variable	(1) Pass	(2) Fail	(3) Drop out	(4) Over-age	(5) Transfer
$\text{Batteries}_m \times \text{Post}_t$	-8.16**	1.19	4.35**	10.84***	-0.03
	(3.27)	(1.13)	(1.83)	(3.30)	(3.40)
Observations	1,685	1,688	1,688	1,408	1,072
R^2	0.43	0.49	0.32	0.34	0.25
Mean of Dep. Var.	80.56	6.73	4.68	17.60	12.91

Table 6: Each column reports the coefficient (and standard errors) on the interaction term from a separate weighted DiD regression. All regressions follow the form (municipality and year fixed effects, interaction between treatment dummy batteries_m and timing dummy post_t) but changes the dependent variable. The dependent variables are measured in percentage points.

5 Conclusion

We find evidence that series of policy changes which began in 2005 in Brazil that were aimed at eliminating the informal firms within the lead-acid battery industry caused a decline in learning outcomes for people living near formal firms within the same industry. This is not, however, an indictment of the policy change because this is but one component its effects on overall welfare. Future work will need to concern itself with the estimation of the components we leave out, such as the positive impacts on formal firm productivity, and reduction in pollution for people living in areas previously occupied by informal firms.

References

Abadie, A. and Imbens, G. W. (2006). Large Sample Properties of Matching Estimators for Average Treatment Effects. *Econometrica*, 74(1):235–267.

Abowd, J. M., Kramarz, F., and Margolis, D. N. (1999). High Wage Workers and High Wage Firms. *Econometrica*, 67(2):251–333.

Aizer, A., Currie, J., Simon, P., and Vivier, P. (2018). Do Low Levels of Blood Lead Reduce Children's Future Test Scores? *American Economic Journal: Applied Economics*, 10(1):307–341.

Billings, S. B. and Schnepel, K. T. (2018). Life after Lead: Effects of Early Interventions for Children Exposed to Lead. *American Economic Journal: Applied Economics*, 10(3):315–344.

Crawfurd, L., Todd, R., Hares, S., Sandefur, J., and Bonnifield, R. S. (2024). The Effect of Lead Exposure on Children's Learning in the Developing World: A Meta-Analysis. *The World Bank Research Observer*, page lkae010.

Ericson, B., Landrigan, P., Taylor, M. P., Frostad, J., Caravanos, J., Keith, J., and Fuller, R. (2016). The Global Burden of Lead Toxicity Attributable to Informal Used Lead-Acid Battery Sites. *Annals of Global Health*, 82(5):686–699.

Hollingsworth, A., Huang, J. M., Rudik, I., and Sanders, N. J. (2022). A Thousand Cuts: Cumulative Lead Exposure Reduces Academic Achievement. *Journal of Human Resources*.

Ipapa, G. (2023). The Hidden Costs of Recycling: Lead Exposure and Student Learning. *Working Paper*.

Kinally, C., Antonanzas-Torres, F., Podd, F., and Gallego-Schmid, A. (2024). Life cycle assessment of solar home system informal waste management practices in Malawi. *Applied Energy*, 364:123190.

Larsen, B. and Sánchez-Triana, E. (2023). Global health burden and cost of lead exposure in children and adults: A health impact and economic modelling analysis. *The Lancet Planetary Health*, 7(11):e831 – e840.

Litzow, E., Cecato, B., Zarate-Barrera, T., and Romero, M. (2024). Toxic Recycling: The Cost of Used Lead-Acid Battery Processing in Mexico.

Mahzab, M., Kundu, A., and Plambeck, E. (2024). Lead Poisoning from Relocation of Lead-Acid Battery Production for Electric Vehicles from China to Bangladesh. *Working Paper*.

Reyes, J. W. (2007). Environmental Policy as Social Policy? The Impact of Childhood Lead Exposure on Crime. *The B.E. Journal of Economic Analysis & Policy*, 7(1).

Reyes, J. W. (2015). Lead Policy and Academic Performance: Insights from Massachusetts. *Harvard Educational Review*, 85(1):75–107.

Smith, H. (2024). How Brazil Solved Its Lead-Acid Battery Problem.

Tanaka, S., Teshima, K., and Verhoogen, E. (2022). North-South Displacement Effects of Environmental Regulation: The Case of Battery Recycling. *American Economic Review: Insights*, 4(3):271–288.

A Appendix

Table 7: Firm Concentratedness

	Count
Unique Companies	397
Unique Cities	277

Notes: A count of the unique number of formal lead-acid battery firms in our data and the number of municipalities containing at least one such firm.

Table 8: Count of Firms by Type

Type	Count
Assembler	9
Battery Manufacturer	29
Consumer	61
Dealership	2
Distribution Center	13
Distributor	298
Importer	21
Location	1
Logistics Operator	7
Recycler	12
Retailer	114

Notes: A count of the number of formal lead-acid battery firms in our data, broken down by firm type.

Table 9: Learning in Public Schools

	Mean	SD	Min	Max
Passing (public)	77.268	14.783	0	100
Failure (public)	11.675	7.784	0	100
Drop-out (public)	7.821	10.910	0	100
Overage (public)	28.776	25.966	0	100
Transfer (public)	10.038	10.154	0	100

Sample period: 2002–2015

Table 10: Effect of Tax Breaks (Public Schools)

	(1) Passing	(2) Failure	(3) Drop-out	(4) Transfer	(5) Over-age
Presence × Post Tax-break	-5.614**	0.964	4.268***	-0.890	6.845***
	(2.255)	(0.896)	(1.502)	(1.665)	(2.450)
Observations	50,147	50,193	50,193	28,568	52,792
Mean DV	73.62	12.66	10.46	9.275	33.47
SD DV	15.88	8.581	13.92	9.663	33.94
Year FE	YES	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES	YES

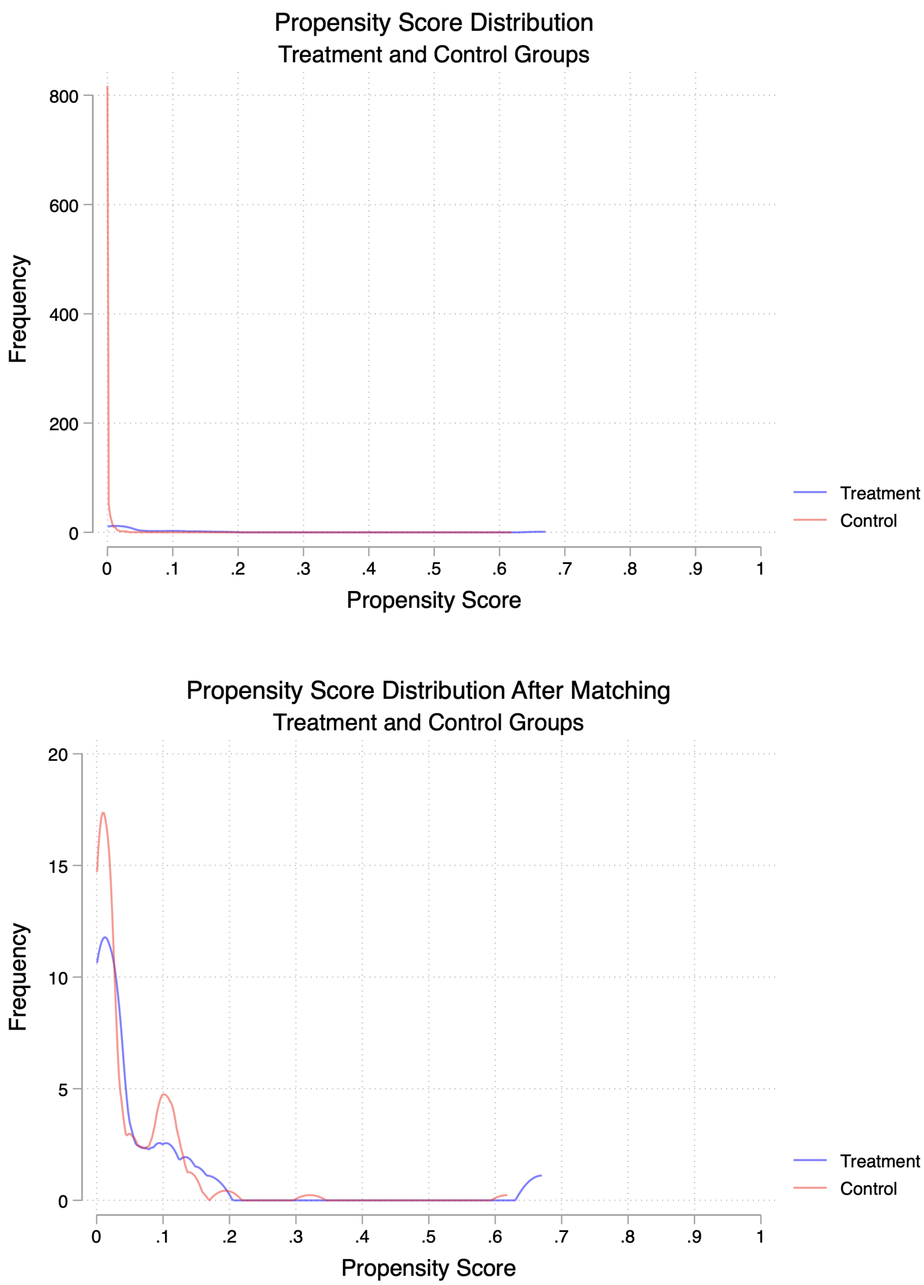

Figure 6: Support Matching