No Reformulation Dividend for Dental Inequality: The UK Sugar Tax and Pre-Existing Convergence

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Abstract

Dental caries affects over two billion people worldwide, and sugar taxes are increasingly promoted as tools to reduce oral health inequalities. I test whether the UK's 2018 Soft Drinks Industry Levy—which triggered an 80% manufacturer reformulation rate—reduced dental decay differentially in more deprived English local authorities. Using a continuous treatment difference-in-differences design across 156 local authorities and seven biennial survey waves (2007/08–2023/24), I find no evidence of differential improvement: a one-standard-deviation increase in deprivation is associated with a statistically insignificant 0.21 percentage-point additional decline in childhood dental decay ($p = 0.49$). Event study estimates reveal pre-existing convergence in the deprivation gradient that predates the levy by a decade. The minimum detectable effect of 0.84 percentage points—3.1% of mean decay prevalence—bounds the scope for differential distributional gains through this channel.

JEL Codes: I18, H23, I14
Keywords: sugar tax, dental health, reformulation, health inequality, difference-in-differences

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1. Introduction

When the British government announced a two-tier levy on sugary soft drinks in March 2016, manufacturers didn't wait. Within two years, over 80% of the calorie reduction in the UK beverage market came not from consumers switching to water but from firms quietly reformulating their products below the tax thresholds (Bandy et al., 2020; Scarborough et al., 2020). By the time the Soft Drinks Industry Levy (SDIL) took effect in April 2018, only 18% of previously eligible drinks still exceeded 5g of sugar per 100ml, down from 52% (Pell et al., 2021). This supply-side transformation—what I call the *reformulation dividend*—has been credited with a 12% reduction in childhood hospital tooth extractions at the national level (Rogers et al., 2024; Sheringham et al., 2025).

A natural follow-up question is whether this dividend was shared equally. Dental caries is the world's most common chronic disease, affecting 2.3 billion people, with prevalence sharply graded by socioeconomic status (Vos et al., 2020; Bernabé et al., 2020). In England, five-year-olds in the most deprived areas are more than twice as likely to have observable dental decay as those in the least deprived (Marmot, 2010). If more deprived communities consumed more sugary drinks before the levy—as survey evidence consistently suggests—they should have received a larger "dose" of the reformulation shock. This reasoning underpins prominent claims that sugar taxes can reduce health inequalities (Briggs et al., 2013; Tsakos and Sheiham, 2015).

This paper tests that claim using a continuous treatment difference-in-differences design. I exploit cross-local-authority variation in the Index of Multiple Deprivation (IMD) as a proxy for pre-SDIL sugar exposure, interacted with the timing of the levy, to estimate whether dental decay converged differentially across deprivation groups after 2018. The design covers 156 English upper-tier local authorities observed across seven biennial waves of the National Dental Epidemiology Programme survey from 2007/08 to 2023/24—975 local-authority-wave observations spanning a 17-year window.

The main finding is a well-powered null. A one-standard-deviation increase in deprivation is associated with an additional 0.21 percentage-point decline in childhood dental decay after the SDIL—statistically indistinguishable from zero ($p = 0.49$) and small relative to the outcome's standard deviation of 8.5 percentage points (SDE $= -0.024$). At conventional power, the design can detect effects as small as 0.84 percentage points, or 3.1% of mean dental decay prevalence. The null is confirmed by permutation inference ($p = 0.53$ across 500 reassignments) and robust to excluding COVID-affected and transition-period survey waves.

More revealingly, the event study exposes the core identification challenge. The deprivation gradient in dental decay was already converging before the levy existed. Relative to 2007/08, more deprived areas had closed the gap by 0.88 percentage points by 2011/12 and 0.97 points by 2014/15—before the SDIL was even announced. Post-implementation coefficients (2018/19 through 2023/24) continue this trajectory without a visible break. When I add local-authority-specific linear trends to absorb this pre-existing convergence, the coefficient flips sign, suggesting that conditional on trend, more deprived areas may have experienced *less* improvement after the levy.

This null has three implications. First, it challenges the claim that supply-side reformulation reduces dental health inequalities. The SDIL's reformulation operated through a national product market: Coca-Cola reformulated Fanta for all of England simultaneously, not differentially for Blackpool versus Bath. Without geographic variation in the supply-side shock, the "dose" that more deprived areas receive is primarily determined by demand composition—and the demand margin was small relative to the supply-side channel (Pell et al., 2021; Scarborough et al., 2020). Second, it illustrates a broader methodological point about using area deprivation as a continuous treatment. When secular convergence in health outcomes is occurring—as it has across English local authorities in dental health, child obesity, and life expectancy (Marmot, 2010)—deprivation-intensity designs will attribute trend to treatment. Researchers employing similar designs for sugar taxes in other countries should present long pre-treatment event studies. Third, the finding that the SDIL's dental effects may be real but *distributionally uniform* is itself policy-relevant: the levy appears to have improved dental health for everyone, without closing the gap between rich and poor.

This paper contributes to the economics of sin taxes (Allcott et al., 2019; Cawley et al., 2019), the evaluation of sugar-sweetened beverage taxes (Colchero et al., 2016; Seiler et al., 2021; Cawley et al., 2021), and the literature on health inequalities and place-based policy (Macintyre et al., 2002). While several public health studies have evaluated the SDIL's dental effects using interrupted time series at the national level (Rogers et al., 2024; Sheringham et al., 2025), this is the first to exploit cross-sectional variation in treatment intensity using an economics-standard identification strategy. The null result complements national ITS findings by showing that the aggregate dental improvement—if real—is not differentially concentrated where sugar consumption was highest.

2. Institutional Background

The Soft Drinks Industry Levy. The SDIL was announced in the March 2016 Budget and implemented on April 6, 2018. It imposes a volumetric levy on manufacturers and importers of soft drinks containing added sugar: 18p per litre for drinks with 5–8g of sugar per 100ml, and 24p per litre for drinks exceeding 8g (HM Revenue and Customs, 2023). Pure fruit juices, milk-based drinks, and drinks produced by small manufacturers are exempt. The levy is paid by producers, not consumers at point of sale, distinguishing it from excise-style soda taxes in Philadelphia, Mexico, and Berkeley (Cawley et al., 2021; Colchero et al., 2016; Seiler et al., 2021).

The reformulation response. The two-year gap between announcement and implementation was deliberate—and consequential. By September 2017, six months before the levy took effect, manufacturers had already reformulated a majority of products below the lower threshold. Bandy et al. (2020) document that the sugar content of soft drinks fell by 29% between 2015 and 2018, with the vast majority of this reduction coming from recipe changes rather than consumer substitution. Pell et al. (2021) estimate that over 80% of the calorie reduction in British households' soft drink purchases was attributable to reformulation. For the dental channel, this supply-side mechanism is critical: reduced sugar in the product itself means reduced cariogenic exposure regardless of whether consumers change their purchasing behavior.

Sugar and dental decay. The biological pathway from sugar consumption to dental caries is well established (Moynihan and Kelly, 2016). Oral bacteria metabolize dietary sugars—particularly sucrose and glucose—into acids that demineralize tooth enamel. Frequent exposure accelerates this process, making sugary drink consumption a proximate cause of childhood caries. In England, caries in five-year-olds is surveyed biennially by the National Dental Epidemiology Programme, providing a direct measure of cumulative sugar exposure during early childhood.

The deprivation gradient. Children in more deprived areas have substantially higher rates of dental decay. In 2014/15, the last survey wave before the SDIL announcement, decay prevalence ranged from 14% in the least deprived local authorities to over 55% in the most deprived. This gradient reflects both higher sugary drink consumption and worse access to preventive dental care in deprived communities. If the SDIL's reformulation reduced sugar content uniformly across products, and if children in deprived areas consumed more of these products, then the deprivation gradient should narrow after the levy—the hypothesis this paper tests.

3. Data

Dental decay outcomes. I use the National Dental Epidemiology Programme (NDEP) survey, accessed via the Office for Health Improvement and Disparities (OHID) Fingertips

API (indicator 93563). The survey measures the percentage of five-year-olds with "experience of visually obvious dental decay" (dentinal caries, $d_3mft > 0$) in each local authority. The NDEP has been conducted biennially since the mid-1980s; I use seven waves from 2007/08 to 2023/24 for which consistent local-authority-level data are available. An alternative outcome would be hospital tooth extractions under general anaesthesia (Hospital Episode Statistics), which prior ITS studies have used (Rogers et al., 2024; Sheringham et al., 2025). I use decay prevalence instead for two reasons: first, it provides a longer pre-treatment baseline (seven waves spanning 17 years versus the HES panel starting in 2015/16), which is critical for assessing pre-trends in a deprivation-intensity design; second, decay prevalence is a more direct measure of the sugar-caries biological pathway, whereas hospital extractions also reflect variation in surgical capacity, waiting lists, and access to secondary care—factors that changed differentially across deprivation groups during COVID-19 and NHS winter pressures.

Treatment intensity. I measure pre-SDIL sugar exposure using the Index of Multiple Deprivation 2019 (IMD 2019) from OHID Fingertips (indicator 93553). The IMD is a composite score combining income, employment, education, health, crime, housing, and living environment deprivation at the local authority level (Ministry of Housing, Communities and Local Government, 2019). Scores range from 5.8 (least deprived) to 45.0 (most deprived) across 158 English upper-tier local authorities. Since deprived areas have higher baseline SSB consumption—the National Diet and Nutrition Survey shows that children in the lowest income quintile consume 30–50% more sugar from soft drinks than those in the highest—the IMD proxies for the intensity of pre-SDIL sugar exposure and thus the magnitude of the reformulation "dose." A limitation of this proxy is that IMD captures many dimensions of deprivation beyond SSB consumption (income, education, housing), so the interaction term may also reflect differential trends in dental care access, fluoridation, or other health inputs that covary with deprivation.

Controls. I include the most recent pre-SDIL childhood obesity rate (Year 6 prevalence from the National Child Measurement Programme, OHID indicator 20601) as a control for baseline health endowments correlated with both deprivation and caries risk.

Sample construction. I restrict to upper-tier local authorities (counties and unitary authorities) with non-missing values for both dental decay and IMD, yielding a panel of 156 local authorities across 7 survey waves (975 observations). The panel is unbalanced: coverage ranges from 128 local authorities in 2023/24 to 150 in 2014/15.

Table 1: Summary Statistics

	Full Sample		Pre-SDIL		Post-SDIL	
	Mean	SD	Mean	SD	Mean	SD
Dental decay (%)	27.2	8.5	28.6	8.7	25.1	7.7
IMD score	23.2	8.1				
IMD (standardized)	-0.00	1.00				
Obesity (%, pre-SDIL)	22.6	2.9				
Observations	975		577		398	
Local authorities	156		150		150	
Survey waves	7		4		3	

Notes: Dental decay is the percentage of 5-year-olds with visually observable dentinal decay, from the National Dental Epidemiology Programme (NDEP) survey. IMD is the Index of Multiple Deprivation 2019 score (higher = more deprived). Obesity is Year 6 prevalence from the National Child Measurement Programme (NCMP), measured in the most recent pre-SDIL year available. Pre-SDIL covers survey waves 2007/08 through 2016/17; post-SDIL covers 2018/19 through 2023/24. The SDIL was announced March 2016 and implemented April 2018.

4. Empirical Strategy

4.1 Identification

I estimate a continuous treatment difference-in-differences model:

$$\text{Decay}_{it} = \alpha_i + \gamma_t + \beta \cdot (\text{Post}_t \times \text{IMD}_i^*) + \mathbf{X}_{it}'\delta + \varepsilon_{it} \tag{1}$$

where Decay_{it} is the percentage of five-year-olds with visible dental decay in local authority i at survey wave t; α_i and γ_t are local authority and wave fixed effects; $\text{Post}_t = \mathbb{I}[t \geq 2018/19]$; $\text{IMD}_i^* = (\text{IMD}_i - \overline{\text{IMD}})/\text{SD}(\text{IMD})$ is the standardized deprivation score; and $\mathbf{X}_{it}$ includes controls interacted with Post. The coefficient β captures the differential change in decay prevalence after the SDIL per standard deviation of deprivation. Standard errors are clustered at the local authority level.

4.2 Identifying assumption

The key assumption is that, absent the SDIL, the *difference* in dental decay between high- and low-deprivation local authorities would have remained constant over time. I assess this with an event study:

$$\text{Decay}_{it} = \alpha_i + \gamma_t + \sum_{k \neq -3} \beta_k \cdot (\mathbb{I}[t = k] \times \text{IMD}_i^*) + \varepsilon_{it} \tag{2}$$

where k indexes event time relative to the SDIL ($k = -3$ for 2007/08 through $k = +3$ for 2023/24), and the omitted category is $k = -3$.

4.3 Threats to validity

Three concerns warrant discussion. First, the IMD captures many dimensions of deprivation, not just SSB consumption. While the correlation between deprivation and sugary drink intake is well documented, other channels—differential access to dental care, fluoridation coverage, dietary composition beyond beverages—may also vary with IMD and change over time. The design attributes any post-2018 convergence in the IMD gradient to the SDIL, which would be a threat if other health policies also differentially targeted deprived areas during this period. Second, only four pre-treatment survey waves are available (2007/08, 2011/12, 2014/15, 2016/17), and the biennial frequency means each pre-treatment "period" spans two years. While these four waves cover an 11-year pre-treatment window, the limited number of observation points constrains the power of formal pre-trend tests. In the spirit of Rambachan and Roth (2023), I present the pre-trend coefficients transparently and report a specification with local-authority-specific trends to assess sensitivity to convergence assumptions, rather than relying solely on a pre-trend F-test. Third, the 2021/22 wave coincided with COVID-19 recovery, which disrupted dental services unequally across the deprivation spectrum. I address this by reporting results both with and without the COVID-affected wave.

5. Results

5.1 Main Results

Table 2 reports the continuous treatment DiD estimates. Across all five specifications, the interaction of Post with standardized IMD is negative but statistically insignificant. In the baseline model (column 1), a one-standard-deviation increase in deprivation is associated with an additional 0.207 percentage-point decline in dental decay after the SDIL (SE = 0.301, $p = 0.494$). This point estimate is small: it represents 2.4% of the outcome's standard deviation, placing it in the "small negative" range by conventional standardized effect-size benchmarks. Adding a quadratic in deprivation (column 2), controlling for pre-SDIL obesity (column 3), combining both (column 4), and excluding the COVID-affected 2021/22 wave (column 5) all yield similarly small and insignificant estimates.

Table 2: Effect of the SDIL on Childhood Dental Decay: Continuous Treatment DiD

	(1)	(2)	(3)	(4)	(5)
Post × IMD (std.)	-0.207	-0.157	-0.184	-0.119	-0.121
	(0.301)	(0.329)	(0.456)	(0.479)	(0.303)
Post × IMD^2		-0.134		-0.137	
		(0.245)		(0.244)	
Post × Obesity			Yes	Yes	
LA FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Excl. COVID (2021/22)					Yes
Observations	975	975	975	975	843
R^2 (within)	0.001	0.001	0.001	0.001	0.000
Mean dep. var.	27.2	27.2	27.2	27.2	27.5

Notes: The dependent variable is the percentage of 5-year-olds with visually observable dental decay. Post equals one for survey waves 2018/19 onward. IMD (std.) is the standardized Index of Multiple Deprivation 2019 score. Column (2) adds a quadratic in IMD interacted with Post. Column (3) controls for the pre-SDIL obesity rate interacted with Post. Column (5) excludes the COVID-affected 2021/22 survey wave. Standard errors clustered at the local authority level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The minimum detectable effect at 80% power is 0.84 percentage points, or 3.1% of mean dental decay prevalence. Effects larger than this threshold—which would represent approximately one-third of the across-wave decline observed over the full panel—can be ruled out at conventional significance.

5.2 Event Study

Table 3 reports the event study coefficients. The pattern reveals that the deprivation gradient in dental decay was already converging before the SDIL. Relative to 2007/08, more deprived local authorities had closed the gap by 0.88 percentage points by 2011/12 and 0.97 points by 2014/15 (marginally significant at the 10% level). The pre-trend F-test for joint significance of the $t-2$ and $t-1$ coefficients yields $p = 0.145$—not decisive, but the point estimates are economically meaningful and larger than the main DiD coefficient. Post-implementation coefficients continue the same trajectory: 0.52 at $t+1$, 1.03 at $t+2$, and 1.10 at $t+3$, with no visible break at the SDIL implementation date.

Table 3: Event Study: IMD Gradient by Survey Wave

Survey wave	Event time	Coefficient	SE
2007/08	$t-3$	[Reference]	
2011/12	$t-2$	-0.879*	(0.490)
2014/15	$t-1$	-0.967*	(0.539)
SDIL announced March 2016			
2016/17	$t=0$	-0.784	(0.503)
SDIL implemented April 2018			
2018/19	$t+1$	-0.516	(0.464)
2021/22	$t+2$	-1.032**	(0.513)
2023/24	$t+3$	-1.101**	(0.555)
Pre-trend F-test		$p = 0.145$	
Observations		975	
LA FE		Yes	
Year FE		Yes	

Notes: Each coefficient is the interaction of event-time dummies with the standardized IMD 2019 score. The reference period is 2007/08 ($t-3$). Pre-trend F-test reports the p-value for the joint significance of the $t-2$ and $t-1$ coefficients (H_0: no pre-existing convergence). Standard errors clustered at the local authority level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

This pattern has two interpretations. If the convergence reflects secular forces unrelated to the levy—oral health campaigns targeting deprived areas, improved water fluoridation, and the long-term decline in caries prevalence across developed countries—then the DiD cannot isolate the SDIL's incremental contribution. Alternatively, if the SDIL's two-year announcement period (March 2016 to April 2018) triggered early reformulation that affected the 2016/17 survey wave, then the "pre-trend" at $t = 0$ may partly reflect anticipation. However, this interpretation cannot explain the convergence at $t - 2$ (2011/12) and $t - 1$ (2014/15), which predate the policy by 4–7 years. The weight of evidence favors the secular convergence interpretation.

5.3 Robustness

Table 4 presents four robustness checks and the permutation inference results. Excluding the 2016/17 transition wave (column 1) and additionally dropping the COVID-affected 2021/22 wave (column 2) leave the estimate insignificant. The most informative specification adds local-authority-specific linear time trends (column 3): the coefficient flips to 0.708 (SE = 0.506, $p = 0.16$), suggesting that once pre-existing convergence is absorbed, more deprived areas may have experienced *less* improvement after the levy. The placebo test (column 4), which assigns a fake treatment at 2014/15 in the pre-period sample, yields an estimate of -0.567 ($p = 0.18$)—larger in magnitude than the real treatment effect, confirming that the convergence pattern predates the SDIL. Across 500 random permutations of IMD scores, the two-sided permutation p-value is 0.526.

Table 4: Robustness Checks

	(1) Excl. 2016	(2) Excl. 2016,21	(3) LA trends	(4) Placebo
Post × IMD (std.)	-0.264	-0.183	0.708	-0.567
	(0.352)	(0.355)	(0.506)	(0.418)
Permutation p-value	0.526 (500 permutations)			
LA FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
LA-specific linear trends			Yes	
Sample	2007–23	2007–23	2007–23	2007–14
	excl. 2016	excl. 16,21		
Observations	839	707	975	441

Notes: Column (1) excludes the 2016/17 transition wave (after SDIL announcement, before implementation). Column (2) additionally excludes the COVID-affected 2021/22 wave. Column (3) adds local-authority-specific linear time trends. Column (4) is a placebo test restricting the sample to 2007/08–2014/15 with a fake treatment date of 2014. Permutation p-value from 500 random reassignments of IMD scores across local authorities. Standard errors clustered at the local authority level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6. Discussion

The null result in this paper does not contradict the national-level evidence that the SDIL reduced dental decay. Rogers et al. (2024) and Sheringham et al. (2025) find aggregate declines using interrupted time series, comparing post-SDIL trends to projected counterfactuals. What the null says is that this improvement—if causal—was not differentially concentrated in more deprived areas.

Why the reformulation dividend may be distributionally uniform. The SDIL's dominant mechanism was supply-side reformulation: manufacturers changed recipes for the national product market. When Lucozade reduced its sugar content from 13g to 4.5g per 100ml, it did so for every bottle sold in every shop in England. The "dose" of reformulation is therefore determined by the product market, not by local deprivation. For deprivation to generate differential exposure, the demand channel must operate: more deprived areas must disproportionately consume the *products that reformulated the most*. But the evidence suggests that reformulation was near-universal among levy-liable drinks (Bandy et al., 2020), attenuating this differential. In contrast, demand-side sugar taxes—like the per-ounce excise in Philadelphia (Cawley et al., 2021)—create price variation that maps more directly onto consumption differences, and may be better suited to generating distributional effects.

Outcome choice and limitations. This paper uses dental decay prevalence rather than hospital tooth extractions, which prior ITS studies have linked to the SDIL. If the levy's dental effects operate primarily through reduced severity of caries (fewer extractions) rather than reduced prevalence (fewer children with any decay), the prevalence outcome would miss the relevant margin. Future work with local-authority-level HES extraction data—which would require direct NHS Digital data access rather than the public Fingertips API used here—could test whether the distributional null extends to the extraction margin. Additionally, the biennial survey frequency and limited pre-treatment waves mean that the pre-trend assessment, while transparently reported, cannot definitively distinguish slow-moving secular convergence from parallel trends.

Implications for deprivation-intensity designs. This paper illustrates a general identification challenge. When health outcomes are converging across deprivation groups due to secular forces—as is the case for childhood dental decay, obesity, and life expectancy in England—interacting deprivation with a post-treatment indicator will conflate trend with treatment. The three pre-treatment waves available here are sufficient to detect the convergence but insufficient to distinguish SDIL-specific acceleration from continuation of the trend. Future work exploiting product-level reformulation data matched to local purchasing patterns, or geographic variation in baseline product mix, could provide sharper identification.

What the null does and does not say. The null bounds the *distributional* effect of the SDIL on dental health, not the *aggregate* effect. The minimum detectable effect of 0.84 percentage points bounds the scope for differential improvements through this channel: effects large enough to meaningfully narrow the deprivation gap would have been detectable. But the levy may still have reduced decay uniformly across all areas—a benefit that would appear only in national time series, not in a design that differences out common trends. From a welfare perspective, a uniform reduction in caries is still valuable; it simply does not address inequality.

7. Conclusion

Britain's sugar tax triggered one of the most dramatic industry reformulations in public health history, but this supply-side transformation did not produce a reformulation dividend for dental inequality. The deprivation gradient in childhood dental decay was converging before the levy was announced and continued converging afterward at the same rate. A well-powered null, confirmed by permutation inference and robust to alternative specifications, rules out differential effects that would be visible at the local-authority level.

The broader lesson is about mechanism specificity. Supply-side interventions that change products for the entire market may improve population health without altering its distribution. Whether sugar taxes can reduce health inequalities likely depends on whether the operative channel—price, reformulation, or substitution—maps onto the existing consumption gradient. Reformulation, the SDIL's dominant channel, does not.

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A. Data Appendix

Dental decay data. The primary outcome is from the National Dental Epidemiology Programme (NDEP), accessed via the OHID Fingertips API (indicator 93563, area type 402). The survey uses visual examination (no X-rays) to assess the proportion of five-year-old children with at least one decayed, missing, or filled tooth at the dentinal level ($d_3mft > 0$). Each local authority participates by sending examination teams to a representative sample of schools. Coverage varies by wave: 150 local authorities in 2014/15, declining to 128 in 2023/24 due to local authority reorganization and non-participation. Data were downloaded on 2026-03-29 from `fingertips.phe.org.uk`.

Deprivation data. The Index of Multiple Deprivation 2019 (IMD 2019, Fingertips indicator 93553) provides a single deprivation score for each local authority, combining seven domains. I use the score level rather than rank or decile to preserve the continuous treatment intensity interpretation. The score is time-invariant by design (a single 2019 release), so it captures the cross-sectional distribution of deprivation rather than changes over time.

Obesity data. Year 6 obesity prevalence from the National Child Measurement Programme (Fingertips indicator 20601) serves as a control for baseline child health endowments. I use the most recent pre-SDIL measurement for each local authority (typically 2016/17).

Sample restrictions. I restrict to upper-tier local authorities (counties and unitary authorities) because the NDEP reports at this level. I drop observations with missing dental decay or IMD values. No winsorization or top-coding is applied.

B. Identification Appendix

Pre-trend analysis. The event study in Table 3 constitutes the primary pre-trend diagnostic. The marginal significance of pre-treatment coefficients at $t-2$ and $t-1$ suggests ongoing convergence in the deprivation gradient. The joint F-test ($p = 0.145$) does not formally reject parallel trends at conventional levels, but the point estimates are economically large relative to the DiD coefficient.

Sensitivity to convergence rate. Adding local-authority-specific linear trends (Table 4, column 3) absorbs the pre-existing convergence but also absorbs any treatment effect that operates gradually. The sign reversal (from -0.21 to $+0.71$) indicates that the baseline DiD estimate is dominated by the pre-trend. This is consistent with the approach of Rambachan and Roth (2023), who argue that sensitivity to trend assumptions should be reported transparently rather than resolved with a single specification.

Permutation inference. I conduct a permutation test by randomly reassigning IMD scores across local authorities 500 times and re-estimating the baseline DiD. The two-sided permutation p-value of 0.526 confirms that the observed coefficient is well within the distribution expected under the null of no treatment effect.

C. Robustness Appendix

Excluding transition and COVID waves. The SDIL was announced in March 2016, so the 2016/17 survey wave may capture anticipation effects. I report estimates excluding this wave (unchanged: $\beta = -0.264$, $p = 0.45$) and additionally excluding the COVID-affected 2021/22 wave ($\beta = -0.183$, $p = 0.61$). Neither exclusion alters the null.

Alternative treatment definitions. Using IMD in levels rather than standardized produces a coefficient of -0.026 per IMD point (SE $= 0.037$, $p = 0.50$), equivalent to -0.207 per standard deviation, confirming the baseline. Using IMD terciles rather than continuous treatment yields insignificant estimates for both the high-deprivation (-0.53, SE $= 0.80$) and middle-deprivation (-0.53, SE $= 0.85$) groups relative to the low-deprivation reference.

D. Standardized Effect Sizes

Table 5: Standardized Effect Sizes for Main Outcomes

Outcome	Specification	$\hat{\beta}$	SD(X)	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled							
Dental decay	Baseline DiD	−0.207	—	8.45	−0.0245	0.0356	Small negative
Panel B: Heterogeneous (sample splits)							
Dental decay	High deprivation	−0.165	—	7.94	−0.0208	0.0718	Small negative
Dental decay	Low deprivation	0.499	—	6.57	0.0760	0.1726	Moderate positive

Notes: **Country:** United Kingdom (England). **Research question:** Does the UK Soft Drinks Industry Levy (SDIL) reduce childhood dental decay differentially in more deprived local authorities? **Policy mechanism:** The SDIL imposes a two-tier volumetric levy on soft drinks manufacturers (18p/litre for 5–8g sugar per 100ml, 24p/litre above 8g), announced March 2016 and implemented April 2018; over 80% of calorie reduction came from manufacturer reformulation rather than consumer switching, creating a national supply-side sugar reduction in beverages. **Outcome definition:** Percentage of 5-year-olds with visually observable dentinal caries (d3mft > 0) from the National Dental Epidemiology Programme biennial survey. **Treatment:** Continuous; standardized Index of Multiple Deprivation (IMD) 2019 score interacted with a post-SDIL indicator, capturing differential exposure through higher baseline sugary drink consumption in more deprived areas. **Data:** Office for Health Improvement and Disparities Fingertips (indicator 93563), 7 survey waves 2007/08–2023/24, upper-tier local authority level, 975 LA-wave observations. **Method:** Two-way fixed effects (LA + wave) with continuous treatment intensity, standard errors clustered at the local authority level. **Sample:** 156 English upper-tier local authorities with non-missing dental survey data and IMD scores; COVID-affected 2021/22 wave retained in main specification, dropped in robustness. SDE $= \hat{\beta} \times \mathrm{SD}(X)/\mathrm{SD}(Y)$ where $\mathrm{SD}(X) = 1$ (treatment already standardized) and $\mathrm{SD}(Y)$ is the unconditional standard deviation of dental decay prevalence. Classification refers to magnitude, not statistical significance: Large ($|\mathrm{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).