Sugar Tax Without Sticker Shock: Reformulation, Deprivation, and the Persistent Health Gradient

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Abstract

The UK Soft Drinks Industry Levy (SDIL), announced in 2016 and implemented in 2018, achieved a 46% sugar reduction in soft drinks—80% through pre-implementation reformulation. We test whether this supply-side success translated into differential health gains for disadvantaged communities. Using a continuous treatment intensity difference-in-differences design across 348 English local authorities, we find that the SDIL did not close the deprivation gradient in childhood dental decay or obesity. All deprivation quintiles saw dental decay decline by 4–6 percentage points, but the Q5–Q1 gap remained at 14 points. For obesity, the deprivation gradient *continued to widen* after 2016, though pre-trend analysis reveals this trajectory preceded the SDIL. These findings suggest that product reformulation—the dominant mechanism behind the SDIL's sugar reduction—operates uniformly across the income distribution, failing to narrow health inequalities.

JEL Codes: I12, I14, H23, I18

Keywords: sugar tax, soft drinks industry levy, reformulation, health inequality, deprivation, dental decay, childhood obesity

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1. Introduction

In April 2018, the UK government began collecting a tax that had already done most of its work. By the time the Soft Drinks Industry Levy (SDIL) took legal effect, manufacturers had quietly reformulated half of all previously liable products below the tax threshold, cutting aggregate sugar content in soft drinks by 46% (Bandy et al., 2020). Coca-Cola shrank Fanta's sugar from 6.9 to 4.6 grams per 100ml. Ribena dropped from 10 to 4.6 grams. Lucozade fell from 13 to 4.5 grams. The tax raised less revenue than projected precisely because reformulation was so effective (HM Revenue & Customs, 2023). Public health advocates hailed this as "the single most successful public health policy of the decade" (Briggs et al., 2017). But for whom?

Sugar taxes are motivated partly by equity. The most deprived communities in England consume 2.3 times more sugary drinks than the least deprived (Public Health England, 2020), suffer three times the rate of childhood tooth extractions under general anaesthetic, and bear a disproportionate burden of diet-related disease (Marmot et al., 2020). If the SDIL's reformulation success reduced sugar intake, the health gains should have been largest in the communities that consumed the most sugar. This paper tests that prediction.

We construct a panel of 348 English local authorities (LAs) over 2006–2024, linking childhood health outcomes from Public Health England's Fingertips database with the Index of Multiple Deprivation (IMD) 2019. Our identification strategy exploits cross-LA variation in baseline deprivation as a continuous measure of treatment intensity: more-deprived areas had higher pre-SDIL sugary drink consumption and thus greater potential exposure to reformulation. We estimate a difference-in-differences model with LA and year fixed effects, testing whether health outcomes improved differentially in more-deprived areas following the SDIL announcement (March 2016) and implementation (April 2018).

Our headline finding is a null. The SDIL did not close the deprivation gradient in childhood dental decay. Between the pre-SDIL period (2007–2014) and the post-period (2018–2023), dental decay in 5-year-olds fell across all deprivation quintiles—by 4.7 percentage points in the least deprived and 4.8 points in the most deprived. But the Q5–Q1 gap barely moved, from 14.0 to 13.9 points. The interaction of post-reform timing with standardized deprivation is 0.016 (SE = 0.241), indistinguishable from zero. For childhood obesity, the picture is worse: the deprivation gradient *widened* after 2016, with Q4 and Q5 areas seeing obesity increase while Q1–Q3 areas saw small declines. However, event study analysis reveals this widening was a pre-existing trend that predated the SDIL.

These results pass a clean placebo test. COPD emergency admissions—which should not respond to sugar content in soft drinks—show no differential trend by deprivation around either the announcement or implementation date (pre-COVID). The dental event study shows flat pre-period coefficients, confirming that our null is not an artifact of violated parallel trends.

The mechanism behind our null is intuitive once articulated: reformulation changes the *product*, not the *consumer*. When Fanta reduces its sugar content, everyone who buys Fanta—in Tower Hamlets or Surrey—gets the same lower-sugar product. Unlike a price-based mechanism, which operates through demand elasticity and disproportionately affects price-sensitive consumers, reformulation is supply-side and geographically uniform. The SDIL's signature success—achieving most of its sugar reduction through industry behavior change rather than consumer behavior change—may be precisely what limits its equity impact.

This paper contributes to three literatures. First, we provide the first local-authority-level causal analysis of the SDIL's health effects. Existing SDIL evaluations use national-level interrupted time series designs (Scarborough et al., 2021; Pell et al., 2021) or household panel data without geographic variation (Bandy et al., 2020). We are the first to exploit the cross-sectional variation that matters for equity assessment. Second, we speak to the optimal sin tax literature (Allcott et al., 2019; Dubois et al., 2020), which debates whether Pigouvian taxes should be designed to minimize regressivity or maximize health gains. Our finding that reformulation-driven taxes are equity-neutral adds a new margin: the distinction between supply-side and demand-side mechanisms matters for distributional incidence. Third, we contribute to the health inequality literature (Marmot et al., 2020; Bambra et al., 2020) by documenting that the deprivation gradient in childhood dental decay and obesity persisted through a period of dramatic sugar reduction—suggesting that closing health gaps requires interventions that target consumption behavior directly, not just product composition.

The remainder of the paper proceeds as follows. Section 2 describes the SDIL's design and the reformulation timeline. Section 3 presents our data and summary statistics. Section 4 details the identification strategy. Section 5 reports the main results, robustness checks, and placebo tests. Section 6 discusses mechanisms and policy implications.

2. Institutional Background

The Soft Drinks Industry Levy. The SDIL was announced in the March 2016 Budget and took effect on 6 April 2018. It imposes a two-tier tax on manufacturers and importers of soft drinks containing added sugar: 18 pence per litre for drinks with more than 5g of sugar per 100ml, and 24 pence per litre for drinks exceeding 8g per 100ml (HM Revenue & Customs, 2023). Pure fruit juices, milk-based drinks, and drinks with fewer than 5g per 100ml are exempt. The tax is levied on producers, not at the point of sale, giving manufacturers a direct financial incentive to reformulate below the threshold.

The reformulation response. The two-year gap between announcement and implementation was deliberate, designed to give industry time to reformulate (Briggs et al., 2017). The response exceeded expectations. By April 2018, the proportion of soft drinks exceeding the lower threshold had fallen from 52% to 15% (Bandy et al., 2020). Major brands reformulated aggressively: Lucozade Energy dropped from 13.0 to 4.5g sugar per 100ml; Ribena from 10.0 to 4.6g; Fanta from 6.9 to 4.6g; and Irn-Bru from 10.3 to 4.7g (Rogers et al., 2023). In aggregate, the sugar content of soft drinks fell by 46% between 2015 and 2019, with approximately 80% of this reduction occurring before the tax took legal effect. HMRC collected £336 million in SDIL revenue in 2022–23, well below initial forecasts, because the taxable base had shrunk through reformulation (HM Revenue & Customs, 2023).

Why deprivation matters. Sugar consumption follows a steep social gradient in England. Adults in the most deprived quintile consume 131% more sugar from soft drinks than those in the least deprived quintile (Public Health England, 2020). This consumption gradient maps directly onto health outcomes: dental decay prevalence in 5-year-olds is 34% in the most deprived areas versus 20% in the least deprived (Table 1). Childhood obesity shows a similar pattern. If reformulation reduced sugar in all soft drinks uniformly, the absolute reduction in sugar consumed should be larger in high-consumption (deprived) areas—and the health improvement should follow.

The equity hypothesis. The implicit equity case for the SDIL is that, by reducing sugar in the products consumed most heavily by disadvantaged groups, it would narrow health gaps. This paper tests whether that prediction held.

3. Data

We draw all outcome and exposure data from Public Health England's (now OHID) Fingertips API, which provides standardized public health indicators at the local authority level.

Dental decay. Our primary outcome is the prevalence of visually obvious dental decay experience in 5-year-old children, from the National Dental Epidemiology Programme (NDEP; Indicator 93563). The NDEP conducts cross-sectional surveys of dental health in state primary school children at approximately three-year intervals. We observe seven waves: 2007/08, 2011/12, 2014/15, 2016/17, 2018/19, 2021/22, and 2023/24, covering 348 local authorities after merging with deprivation data. Dental decay is a particularly direct marker of sugar consumption: the WHO classifies free sugars as the primary dietary risk factor for dental caries (World Health Organization, 2015; Moynihan and Kelly, 2014), and the biological mechanism—bacterial fermentation of sugars producing enamel-dissolving acid—is well established.

Childhood obesity. Our secondary outcome is the percentage of Reception-year children (age 4–5) classified as overweight or obese (BMI at or above the 85th centile of the 1990 UK reference population), from the National Child Measurement Programme (NCMP; Indicator 20601). The NCMP measures virtually all children in state Reception classes annually. We observe 19 years (2006/07 through 2024/25) across 348 LAs, providing substantially more temporal variation than the dental data. We exclude the 2020/21 year, when COVID-19 disrupted data collection and only 20 LAs reported (NHS Digital, 2023).

Placebo outcome. We use COPD emergency hospital admissions per 100,000 (Indicator 92302) as a negative control. COPD is strongly correlated with deprivation but should not respond to changes in soft drink sugar content.

Deprivation. Treatment intensity is measured by the Index of Multiple Deprivation 2019 (Indicator 93553), which ranks every small area in England on seven domains including income, employment, health, and education (Noble et al., 2019). We use the LA-level aggregate score, standardized to mean zero and standard deviation one for regression analysis. IMD scores range from 5.5 (least deprived) to 45.0 (most deprived) across our 348 LAs.

Table 1 presents summary statistics. In the pre-SDIL period, dental decay averaged 26.2% nationally, with a 14.0 percentage point gap between the most and least deprived quintiles (Q5: 34.1% vs. Q1: 20.1%). Childhood obesity averaged 22.0%, with a 4.3 point deprivation gap. Table 5 shows pre-post changes by quintile.

4. Empirical Strategy

4.1 Identification

We exploit cross-LA variation in baseline deprivation as a continuous measure of SDIL treatment intensity. The identifying assumption is that, absent the SDIL, health outcomes would have evolved similarly across the deprivation distribution (conditional on LA and year fixed effects). Because deprivation is a pre-determined characteristic fixed at its 2019 value, this design avoids the endogeneity concerns that arise with time-varying treatment.

Our primary specification is:

$$Y_{it} = \alpha_i + \gamma_t + \beta_1(\text{PostReform}_t \times \text{Dep}_i) + \varepsilon_{it} \tag{1}$$

Table 1: Summary Statistics: Childhood Health Outcomes by Deprivation

Period	LAs	Obs	Mean	SD	Q1	Q5	Gap
Panel A: Dental Decay in 5-Year-Olds (%)							
Pre-SDIL (2007–2014)	339	985	26.3	8.8	20.1	34.1	14.0
Post-SDIL (2018–2023)	324	794	22.0	8.0	15.5	29.3	13.9
Panel B: Childhood Obesity, Reception Year (%)							
Pre-SDIL (2006–2015)	348	3,279	22.2	2.9	19.6	23.9	4.3
Post-SDIL (2016–2024, excl. 2020)	348	2,644	22.2	3.1	19.2	24.4	5.2

Notes: Dental decay is the percentage of 5-year-olds with experience of visually obvious dental decay, from the National Dental Epidemiology Programme (NDEP). Childhood obesity is the percentage of Reception-year children (age 4–5) classified as overweight or obese (BMI $\geq$ 85th centile), from the National Child Measurement Programme (NCMP). Q1 = least deprived IMD 2019 quintile; Q5 = most deprived. Gap = Q5 mean − Q1 mean. COVID year 2020/21 excluded from obesity post-period due to incomplete NCMP data collection.

where Y_{it} is the health outcome in LA i at time t; α_i and γ_t are LA and year fixed effects; PostReform_t equals one for survey waves from 2018/19 onward (when reformulation was essentially complete); and Dep_i is the standardized IMD 2019 score. The coefficient β_1 captures the differential change in health outcomes associated with a one-standard-deviation increase in deprivation, after the SDIL's reformulation took effect.

4.2 Decomposition

A key feature of the SDIL timeline is the two-year announcement window. We decompose the total effect into announcement and implementation components:

$$Y_{it} = \alpha_i + \gamma_t + \beta_1(\text{Transition}_t \times \text{Dep}_i) + \beta_2(\text{Post}_t \times \text{Dep}_i) + \varepsilon_{it} \tag{2}$$

where Transition_t is an indicator for the 2016/17 wave (the announcement year, before implementation) and Post_t indicates 2018/19 onward. Here β_1 captures any differential effect during the announcement-reformulation window, while β_2 captures the full post-implementation effect. Note that β_2 reflects both reformulation (which continued into 2019–2020 as manufacturers optimized recipes) and any additional price channel; the decomposition $\beta_2 - \beta_1$ thus provides an upper bound on the marginal price effect rather than a clean separation.

4.3 Event study

For transparency about pre-trends, we estimate an event study specification:

$$Y_{it} = \alpha_i + \gamma_t + \sum_{t \neq t_0} \beta_t (\mathbb{I}[t] \times \text{Dep}_i) + \varepsilon_{it} \tag{3}$$

with the last pre-treatment period as reference (2014 for dental, 2015 for obesity). Flat pre-period coefficients validate the parallel trends assumption.

4.4 Inference

All standard errors are clustered at the local authority level to account for serial correlation within LAs (Callaway and Sant'Anna, 2021). Our 348 clusters are well above the threshold where cluster-robust inference is reliable.

5. Results

5.1 Dental Decay

Table 2 reports the main results for dental decay in 5-year-olds. Column (1) estimates the simple post-reform interaction: the coefficient on PostReform × Deprivation is 0.016 (SE = 0.241), economically negligible and statistically indistinguishable from zero. A one-standard-deviation increase in deprivation is associated with essentially no differential change in dental decay after the SDIL.

Column (2) decomposes into transition and post-implementation periods. Neither coefficient is significant: the transition interaction is 0.484 (SE = 0.313) and the post-implementation interaction is 0.133 (SE = 0.263). If anything, the positive signs—the opposite of what the equity hypothesis predicts—suggest that more-deprived areas saw slightly less improvement, though both estimates are imprecise. Column (3) restricts the post-period to 2021/22 onward, when children surveyed had near-complete lifetime exposure to reformulated products; the coefficient is −0.124 (SE = 0.260), still null. Column (4) adds a linear deprivation-specific time trend, yielding a coefficient of 0.277 (SE = 0.359).

The event study (Table 4, Panel B) confirms the null. Pre-period dental coefficients for 2007 and 2011 (relative to 2014) fluctuate around zero without systematic pattern, validating parallel trends. Post-period coefficients for 2018 and 2021 are similarly centered on zero.

Statistical power. With a standard error of 0.241 on the main dental coefficient, the minimum detectable effect (MDE) at 80% power and 5% significance is approximately

Table 2: Effect of the SDIL on Dental Decay in 5-Year-Olds

	(1)	(2)	(3)	(4)
Post-Reform × Deprivation	0.016			0.277
	(0.241)			(0.359)
Transition × Deprivation		0.484		
		(0.313)		
Post-Implementation × Dep.		0.133		
		(0.263)		
Full Exposure × Deprivation			-0.124	
			(0.260)	
Trend × Deprivation				-0.031
				(0.035)
Observations	2,104	2,104	2,104	2,104
Within R^2	0.0000	0.0016	0.0002	0.0005
LA FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: Dependent variable is dental decay prevalence (%) in 5-year-olds. Deprivation is the standardized IMD 2019 score (mean 0, SD 1). Post-Reform = 1 for 2018/19 onward (reformulation complete). Transition = 2016/17 only (announcement year). Full Exposure = 1 for 2021/22 onward (cohorts with near-complete lifetime exposure to reformulated products). Column (4) adds a linear deprivation-specific time trend. Standard errors clustered by local authority in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

$0.241 \times 2.8 \approx 0.67$ percentage points per standard deviation of IMD. Across the full deprivation range (approximately 4 SDs), this implies a minimum detectable differential change of roughly 2.7 percentage points between the most and least deprived areas. The actual Q5–Q1 gap is 14 points, so the design can detect effects equivalent to roughly 19% of the existing gap. This is a meaningful effect size, suggesting the null reflects a genuine absence of differential impact rather than simply low power. The low within-R^2 values (0.0001–0.002) are expected: LA and year fixed effects absorb most variation in health outcomes, which is precisely their purpose in a DiD design.

5.2 Childhood Obesity

Table 3 reveals a different pattern, though one that ultimately does not support a causal SDIL interpretation. Column (1) shows that the deprivation gradient in childhood obesity widened after the SDIL announcement: the Post-Announce × Deprivation coefficient is 0.421 (SE = 0.080, $p < 0.001$). However, the event study (Table 4, Panels B–C) reveals clear pre-trend violations: obesity pre-period coefficients are negative relative to 2015 (-0.50 in 2008, -0.42

Table 3: Effect of the SDIL on Childhood Obesity (Reception Year)

	(1)	(2)	(3)	(4)
Post-Announce × Deprivation	0.421***	0.328***	0.416***	0.255***
	(0.080)	(0.084)	(0.079)	(0.092)
Post-Implement × Deprivation		0.126		
		(0.078)		
Trend × Deprivation				0.018*
				(0.011)
Observations	5,943	5,943	5,923	5,943
Within R^2	0.0123	0.0127	0.0120	0.0130
Excl. COVID	No	No	Yes	No
LA FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: Dependent variable is the percentage of Reception-year children classified as overweight or obese. Deprivation is the standardized IMD 2019 score. Post-Announce = 1 for 2016/17 onward. Post-Implement = 1 for 2018/19 onward. Column (3) drops the 2020/21 COVID year. Column (4) adds a linear deprivation-specific time trend. The positive coefficients indicate the deprivation gradient in childhood obesity *widened* after the SDIL announcement. Standard errors clustered by LA in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

in 2010), indicating that the deprivation-obesity gradient was already steepening before the SDIL. Column (4) adds a linear deprivation-specific trend; the post-announcement coefficient falls to 0.255 (SE = 0.092) but remains significant, suggesting possible acceleration beyond the pre-trend.

Given the pre-trend violation, we interpret the obesity results as *descriptive* evidence that the deprivation gradient in childhood obesity continued to widen during the SDIL period, consistent with broader austerity-era dynamics (Marmot et al., 2020; Bambra et al., 2020). We cannot attribute the widening to the SDIL, nor can we claim the SDIL prevented further divergence. The dental decay analysis, with its flat pre-trends, carries the weight of the causal identification.

5.3 Placebo: COPD Admissions

Table 4, Panel A, reports the COPD placebo restricted to pre-COVID years (2010–2019). The Post-Announce × Deprivation coefficient is −0.43 (SE = 3.20) and the Post-Implement coefficient is −2.44 (SE = 2.66), both statistically insignificant. COPD admissions showed no differential trend by deprivation around the SDIL timeline, confirming that our design is not picking up spurious correlations between deprivation and general health trends.

5.4 Deprivation Quintile Analysis

Table 5 makes the equity result concrete. Every deprivation quintile saw dental decay decline between the pre- and post-SDIL periods. The magnitude is remarkably uniform: −4.7 points in Q1 (least deprived), −5.8 in Q2, −4.2 in Q3, −5.1 in Q4, and −4.8 in Q5 (most deprived). The Q5–Q1 gap narrowed by a trivial 0.1 percentage points (from 14.0 to 13.9). For obesity, Q1–Q3 improved modestly (−0.4 to −0.1 points) while Q4 and Q5 worsened (+0.4 to +0.6 points), widening the gap from 4.3 to 5.3 points.

6. Discussion

Why reformulation didn't close the gap. The SDIL's reformulation mechanism is supply-side: manufacturers changed their recipes before the tax took effect, reducing sugar content in all units of a given product regardless of where or by whom they are purchased (Bandy et al., 2020). This is fundamentally different from a demand-side (price) mechanism, where higher prices disproportionately reduce consumption among price-sensitive (typically lower-income) consumers (Allcott et al., 2019). The uniformity of reformulation across the income distribution—everyone gets the same lower-sugar Fanta—means the *proportional* reduction in sugar intake was similar across deprivation levels, even though *absolute* consumption differed.

This finding has direct implications for the optimal sin tax literature. Dubois et al. (2020) show that the targeting of soda taxes depends on whether high-sugar consumers are also high-externality consumers. Our results add a new dimension: the *mechanism* through which the tax operates matters for incidence. A tax that primarily works through reformulation rather than price is more efficient (lower deadweight loss, broader reach) but less progressive than textbook Pigouvian reasoning implies.

Substitution and behavioral persistence. One explanation for the persistent deprivation gradient is substitution. Smith et al. (2018) document that sweet snacks are more price-sensitive than beverages in the UK. If consumers in deprived areas substituted from reformulated soft drinks to confectionery, biscuits, or juice drinks (which are exempt from the SDIL), the sugar reduction from reformulation would be partially offset. The SDIL covers only 25% of UK sugar intake from beverages, leaving the majority of dietary sugar untouched (Swinburn et al., 2019).

Implications for health inequality policy. Our results suggest that reducing product sugar content—while effective at the population level—is insufficient to narrow health inequalities. Closing the deprivation gap in dental decay and childhood obesity may require interventions that differentially reach disadvantaged communities: targeted screening programs, water fluoridation in deprived areas, or restrictions on fast food outlet density near schools. The SDIL's reformulation success is real, but it is a tide that lifts all boats equally.

Limitations. Three limitations deserve note. First, our dental data comprise only seven irregularly-spaced survey waves, limiting statistical power for the event study. The obesity data, with 19 annual observations, provide stronger temporal variation but exhibit pre-trend violations that complicate causal interpretation. Second, we measure deprivation at the LA level using a composite index; within-LA variation in consumption patterns is not captured. Third, our design cannot identify the SDIL's national-level effect (absorbed by year fixed effects)—we identify only the differential effect across the deprivation distribution.

7. Conclusion

The UK Soft Drinks Industry Levy achieved something remarkable: it convinced an entire industry to reformulate its products before the tax even took effect. But reformulation, by its nature, is a blunt instrument for equity. It changes the product on every shelf in every shop, regardless of the neighborhood. Our evidence shows that this supply-side mechanism left the deprivation gradient in childhood health outcomes essentially unchanged. For policymakers designing the next generation of food and beverage taxes, the lesson is that efficiency and equity may pull in different directions. A tax that works through reformulation is efficient—low revenue, high behavior change, minimal consumer burden. But a tax that works through prices is more progressive, channeling health gains toward the communities that need them most. The SDIL achieved its stated objective of reducing sugar in the food supply. But reducing sugar uniformly across the income distribution is not the same as closing health gaps. Achieving the latter requires interventions that differentially reach the communities where the burden is greatest.

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Project Repository: https://github.com/SocialCatalystLab/ape-papers

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A. Data Appendix

Fingertips API. All health outcome data were retrieved from the OHID Fingertips API (https://fingertips.phe.org.uk/api/) using the `all_data/csv/by_indicator_id` endpoint with area type 401 (County & Unitary Authority). The API returns standardized indicators with area codes, time periods, values, confidence intervals, counts, and denominators. We filtered to English upper-tier local authorities (area codes matching E06, E07, E08, E09, E10) and the "Persons" sex category.

Dental decay (Indicator 93563). The National Dental Epidemiology Programme surveys dental health of 5-year-old children in state primary schools. Surveys are cross-sectional and conducted at approximately 3-year intervals. The outcome is the percentage of children with "experience of visually obvious dental decay." Participation is voluntary and consent rates vary across LAs. Seven waves are available: 2007/08, 2011/12, 2014/15, 2016/17, 2018/19, 2021/22, 2023/24. After filtering and merging with IMD data, our panel contains 2,104 LA-wave observations across 348 LAs.

Childhood obesity (Indicator 20601). The National Child Measurement Programme measures height and weight of virtually all children in Reception year (age 4–5) and Year 6 (age 10–11) in state schools. We use the Reception-year percentage classified as overweight or obese (BMI $\geq$ 85th centile of the 1990 UK reference). Annual data are available from 2006/07 through 2024/25. We exclude 2020/21, when only 20 LAs reported due to COVID-19 school closures (NHS Digital, 2023). Our panel contains 5,943 LA-year observations across 348 LAs.

COPD admissions (Indicator 92302). Emergency hospital admissions for chronic obstructive pulmonary disease per 100,000 population, directly standardized. Annual data from 2010/11 through 2023/24. For the placebo test, we restrict to the pre-COVID period (2010/11–2019/20). The pre-COVID panel contains 3,458 observations.

IMD 2019 (Indicator 93553). The Index of Multiple Deprivation 2019 is a composite measure of relative deprivation published by MHCLG (Noble et al., 2019). We retrieve LA-level aggregate scores from Fingertips. After filtering, 350 English LAs have IMD scores; 348 match our outcome data. We standardize to mean 0, SD 1 for regression analysis, and create quintile groups using the sample distribution. IMD scores range from 5.54 (Hart, Hampshire) to 45.04 (Blackpool).

Geographic harmonization. English local authority boundaries changed during our sample period (e.g., Bournemouth, Christchurch and Poole were merged into a unitary authority in

2019). The Fingertips API reports data using the area codes current at each survey date. We merge on area codes as reported, which means that our panel is unbalanced: some LAs appear in all waves while others appear only in subsets. This is standard practice for Fingertips data and does not introduce systematic bias, as boundary changes affected a small number of areas.

B. Identification Appendix

Biological lag in dental outcomes. Dental decay in 5-year-olds reflects cumulative sugar exposure over the child's lifetime. Children surveyed in 2016/17 were born around 2011/12 and had almost all their sugar exposure before the SDIL announcement. Children surveyed in 2021/22, born around 2016/17, had most of their early years during the reformulation period. We classify 2016/17 as the "transition" period and 2018/19 onward as "post-reform" for our main analysis. The "full exposure" specification (Column 3 of Table 2) restricts to 2021/22 onward, capturing children with near-complete lifetime exposure to reformulated products.

Pre-trends for dental decay. The dental event study coefficients for 2007 and 2011 (relative to 2014) are 0.483 and −0.296, neither statistically significant. This supports the parallel trends assumption, though we note that with only two pre-period points (given the irregular survey spacing), pre-trend tests have limited power.

Pre-trends for childhood obesity. The obesity event study reveals a systematic pre-trend: coefficients relative to 2015 are negative and significant for 2006–2012, indicating that the deprivation-obesity gradient was already steepening before the SDIL. This pattern is consistent with the broader literature documenting widening health inequalities in England during the austerity period (Marmot et al., 2020; Bambra et al., 2020). We include a linear trend-control specification (Column 4 of Table 3) to assess sensitivity.

C. Robustness Appendix

Alternative treatment timing. Given the biological lag in dental outcomes, we test a "full exposure" specification that defines treatment as 2021/22 onward. The coefficient is −0.124 (SE = 0.260), still null. This confirms that even for children whose entire early life occurred during the reformulation period, there is no differential effect by deprivation.

Excluding COVID year. Dropping the 2020/21 COVID year from the obesity panel barely changes the results: the post-announcement coefficient is 0.416 (SE = 0.079), nearly identical to the full-sample estimate of 0.421.

Linear pre-trend control. Adding a deprivation-specific linear time trend to the dental specification yields a coefficient of 0.277 (SE = 0.359), still null. For obesity, the trend-controlled estimate is 0.255 (SE = 0.092), smaller than the baseline but still positive and significant—consistent with acceleration beyond the pre-trend, though we interpret this cautiously.

COPD pre-COVID. Restricting the COPD placebo to 2010–2019 (eliminating COVID-era admission drops) yields null coefficients for both post-announcement (−0.43, SE = 3.20) and post-implementation (−2.44, SE = 2.66), confirming the placebo.

D. Standardized Effect Sizes

Table 4: Placebo and Pre-Trend Diagnostics

	Dental Decay	Childhood Obesity
Panel A: COPD Emergency Admissions Placebo (per 100,000; 2010–2019)		
Post-Announce × Dep.	-0.43	
	(3.20)	
Post-Implement × Dep.	-2.44	
	(2.66)	
Panel B: Pre-Period Event Study Coefficients ($\beta_t \times$ IMD)		
Reference year:	2014	2015
2007	0.483	
	(0.402)	
2011	-0.296	
	(0.370)	
2008		-0.497***
		(0.174)
2010		-0.423**
		(0.168)
2012		-0.347**
		(0.154)
2014		-0.310**
		(0.136)
Panel C: Post-SDIL Event Study Coefficients		
2018	0.244	0.137
	(0.376)	(0.123)
2021	-0.056	0.037
	(0.346)	(0.116)
2023		-0.104
		(0.106)
LA FE	Yes	Yes
Year FE	Yes	Yes

Notes: Panel A reports coefficients from regressing COPD emergency admissions (per 100,000) on Post-Announce and Post-Implement interacted with standardized IMD, limited to pre-COVID years (2010–2019). Neither coefficient is statistically significant, confirming the placebo. Panels B and C report event study coefficients ($\beta_t \times$ IMD) with 2014 (dental) and 2015 (obesity) as reference years. Dental pre-period coefficients fluctuate around zero. Obesity pre-period coefficients are negative and significant, indicating a pre-existing trend toward *narrowing* of the deprivation gradient prior to the SDIL. Standard errors clustered by LA in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Pre-Post Changes in Health Outcomes by Deprivation Quintile

	Dental Decay (%)			Obesity (%)		
Quintile	Pre	Post	Δ	Pre	Post	Δ
Q1 (Least deprived)	20.1	15.5	-4.7	19.6	19.2	-0.4
Q2	23.4	17.6	-5.8	21.5	21.1	-0.3
Q3	25.4	21.2	-4.2	22.5	22.3	-0.2
Q4	28.1	23.1	-5.1	23.5	23.8	0.3
Q5 (Most deprived)	34.1	29.3	-4.8	23.9	24.4	0.6
Q5 − Q1 Gap	14.0	13.9	-0.1	4.3	5.2	1.0

Notes: Pre-SDIL averages: dental decay 2007–2014 (3 waves), obesity 2006–2015 (10 years). Post-SDIL averages: dental decay 2018–2023 (3 waves), obesity 2016–2024 excluding 2020/21 (8 years). Quintiles based on IMD 2019 deprivation scores. Δ = Post − Pre. The Q5−Q1 gap measures the absolute health inequality between most and least deprived quintiles. For dental decay, the gap narrowed from 14.0 to 13.9 percentage points. For obesity, the gap widened from 4.3 to 5.3 percentage points.

Table 6: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Dental Decay	0.016	0.241	8.75	0.0018	0.0275	Null
Childhood Obesity	0.421	0.080	3.00	0.1402	0.0267	Moderate positive

Notes: SDE = $\hat{\beta}$ / SD(Y). Treatment intensity is a 1-SD increase in the IMD 2019 deprivation index. **Research question:** Whether the UK Soft Drinks Industry Levy differentially improved childhood health in more-deprived local authorities. **Data:** PHE/OHID Fingertips; dental decay from the NDEP (348 LAs, 7 waves, 2007–2023); obesity from the NCMP (348 LAs, 19 years, 2006–2024). **Method:** Continuous treatment intensity DiD with LA and year FE; SEs clustered by LA. **Sample:** N = 2,104 (dental); 5,943 (obesity). Classification labels refer to the magnitude of the standardized point estimate, not to statistical significance. "Null" denotes a near-zero effect size ($|\text{SDE}| < 0.005$), not a failure to reject a null hypothesis.