The Compliance Mirage: No Entry Deterrent from England's Mandatory Calorie Labeling

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Abstract

Industry groups warned that England's 2022 mandatory calorie labeling regulation would burden food businesses and deter market entry. I test whether this mandate—which applies only to firms with 250+ employees—created spillover entry deterrence for new (exempt) food service businesses. Using a triple-difference design that exploits England's adoption against Scotland's status quo across 5.7 million company records from Companies House, I find no evidence of entry deterrence ($\hat{\beta} = -0.002$, SE $= 0.034$; permutation $p = 1.000$). The 95% confidence interval rules out effects larger than 7% in either direction. The result holds when excluding COVID months, aggregating to quarterly frequency, and using Poisson count models. A large-firm calorie disclosure mandate does not deter small-business entry, undermining industry claims of market-wide regulatory burden.

JEL Codes: I18, L83, D83, L51
Keywords: calorie labeling, information disclosure, food regulation, business entry, triple-difference

1. Introduction

When England mandated calorie labeling on menus in April 2022, the UK Hospitality trade group predicted "significant compliance costs" that would fall hardest on businesses already reeling from the pandemic (UKHospitality, 2021). The Food and Drink Federation warned of "disproportionate burdens" that could push marginal operators out of the market (Food and Drink Federation, 2020). These fears echoed a broader pattern in the information disclosure literature: regulated industries routinely argue that mandatory transparency requirements will destroy the very markets they aim to improve (Jin and Leslie, 2006; Dranove et al., 2003).

But do disclosure mandates actually deter market entry? The theoretical prediction is ambiguous. Compliance costs raise the effective cost of entry, which should reduce the number of new entrants at the margin (Stigler, 1971). Yet disclosure may also professionalize an industry, attract quality-conscious consumers, and thereby expand the market (Djankov et al., 2002). The net effect on entry is an empirical question—one that has received remarkably little attention despite decades of research on calorie labeling's effects on consumer behavior (Bollinger et al., 2011; Long et al., 2015; Cantor et al., 2015).

This paper provides the first causal estimate of calorie labeling's effect on business entry. I exploit a natural experiment created by England's Calorie Labelling (Out of Home Sector) Regulations 2022, which required food businesses with 250 or more employees to display calorie information on menus and delivery platforms. Scotland did not adopt the mandate, providing a clean control jurisdiction that shares a common labor market, regulatory framework, and macroeconomic environment with England (Bell and Machin, 2014).

I implement a triple-difference design comparing new food service company incorporations (SIC 56) against five unaffected placebo sectors (retail, IT, motor trade, real estate, personal services), in England versus Scotland, before and after April 2022. England is classified separately from Wales—which also did not adopt the mandate—using company number prefixes, registered office addresses, and postcode areas. The design leverages 5.7 million company records from the Companies House registry, covering 84 months from January 2019 to December 2025.

Since the mandate targets firms with 250 or more employees, while new incorporations are overwhelmingly small businesses, this design estimates the *indirect* spillover effect—whether the regulation deters entry by exempt firms through channels such as anticipatory compliance, competitive signaling, or industry-wide uncertainty. The main finding is a null. The triple-difference coefficient is -0.002 log points (SE $= 0.034$), and the 95% confidence interval of $[-0.069, 0.064]$ rules out spillover entry effects larger than approximately 7% in either direction. Permutation inference, which randomly reassigns the "treated" sector label across

999 draws, yields $p = 1.000$: the actual treatment effect is smaller in absolute value than every single placebo sector assignment. The null is robust to excluding COVID-affected months, restricting the sample to the post-2021 period, aggregating to quarterly frequency, and estimating Poisson count models.

These results contribute to three literatures. First, I add to the growing body of evidence that calorie labeling produces modest or null effects on the outcomes it targets (Bollinger et al., 2011; Long et al., 2015), extending this finding from consumer behavior to firm behavior. The "compliance mirage"—the gap between industry predictions of regulatory devastation and the actual null effect—is itself informative about the political economy of disclosure regulation (Sunstein, 2020). Second, I contribute to the literature on regulatory barriers to entry (Djankov et al., 2002; Bailey and Thomas, 2019), showing that even in a sector with many small firms and thin margins, a disclosure mandate aimed at large chains does not measurably deter new entrants. Third, I demonstrate the value of the England–Scotland border as a natural experiment for evaluating UK domestic policy (Bell and Machin, 2014; McVittie et al., 2019).

2. Institutional Background

The Calorie Labelling Regulations 2022. On 6 April 2022, the Calorie Labelling (Out of Home Sector) (England) Regulations 2022 came into force. The regulation requires food businesses operating in England with 250 or more employees to display calorie information "at the point of choice"—on physical menus, online ordering platforms, and third-party delivery apps such as Deliveroo, Just Eat, and Uber Eats (Department of Health and Social Care, 2021). The 250-employee threshold means the regulation directly affects large chains (McDonald's, Nando's, Wetherspoons) while exempting the vast majority of independent restaurants, cafes, and takeaways.

Devolved policy divergence. Health policy is devolved in the United Kingdom. While England adopted mandatory calorie labeling, Scotland, Wales, and Northern Ireland did not enact equivalent legislation. Food Standards Scotland conducted a consultation in 2022 but did not proceed to regulation (Food Standards Scotland, 2022). This cross-border divergence creates a natural experiment: English and Scottish food businesses face identical macroeconomic conditions, similar consumer preferences, and the same Companies House registration requirements, but different calorie disclosure obligations.

Industry concerns. Prior to implementation, industry bodies raised several objections. The UK Hospitality association argued that menu reformulation and labeling infrastructure would

impose costs of £10,000–£30,000 per outlet for affected chains (UKHospitality, 2021). The Federation of Small Businesses warned that even exempt businesses might face "voluntary compliance pressure" as consumer expectations shifted (Federation of Small Businesses, 2021). These arguments mirror the "regulatory cascade" hypothesis in the disclosure literature: mandates targeting large firms may spill over to create de facto compliance norms for smaller ones (Leuz and Wysocki, 2010).

Why entry matters. Much of the calorie labeling literature focuses on consumer behavior— whether people order fewer calories when labels are visible (Bollinger et al., 2011; Long et al., 2015; Cantor et al., 2015). But the supply-side response is equally important for welfare. If compliance costs deter entry, the regulation could reduce competition, raise prices, and ultimately harm the consumers it aims to protect (Dranove et al., 2003). Alternatively, if the regulation imposes trivial costs, the industry's lobbying against it reveals more about rent-seeking than about genuine economic burden (Stigler, 1971).

3. Data

I use the Companies House Basic Company Data snapshot from March 2026, a freely available monthly CSV containing all 5.7 million registered companies in the United Kingdom. Each record includes the company's incorporation date, primary SIC code, registered office address, and company number. I exploit two features of this dataset: the incorporation date allows me to construct monthly time series of new business formation, and the company number prefix ("SC" for Scottish companies) provides reliable jurisdiction classification. Critically, I separate England from Wales—which did not adopt the calorie mandate—using three signals: company number prefix (non-SC, non-NI), the `RegAddress.Country` field (which identifies 30,384 companies as Welsh), and postcode area (CF, SA, LL, NP, LD are Welsh areas). This avoids attenuating the treatment by including untreated Welsh companies in the treated group.

Sector definition. I define the treated sector as SIC Division 56 (Food and beverage service activities), which encompasses restaurants (56.10), event catering (56.21), other food service (56.29), and bars/pubs (56.30). Five placebo sectors serve as within-country controls: retail trade (SIC 47), IT services (SIC 62), motor vehicle trade (SIC 45), real estate (SIC 68), and personal services (SIC 96). These sectors were chosen because they are similarly dominated by small and medium enterprises, face comparable macroeconomic conditions, and are unaffected by calorie labeling regulation.

Panel construction. The analysis panel consists of monthly incorporation counts by country (England vs. Scotland) × sector (6 sectors) × month (January 2019–December 2025), yielding $2 \times 6 \times 84 = 1{,}008$ observations. During the pre-treatment period (January 2019–March 2022), England averaged 1,087 new food service incorporations per month (SD = 261), while Scotland averaged 94 (SD = 28). New incorporations are overwhelmingly small businesses: nearly all register as private limited companies with no employees at the time of incorporation, making them exempt from the 250-employee calorie labeling threshold.

3.1 Summary Statistics

Table 1: Summary Statistics: Monthly New Business Incorporations (Pre-Treatment)

Sector	Country	Mean	Std. Dev.	Min	Max
	Scotland	93.9	27.9	23	135
	Scotland	38.3	8.2	21	54
	Scotland	26.8	7.4	9	40
	Scotland	65.4	17.8	20	100
	Scotland	141.7	32.4	61	208
	Scotland	97.7	22.2	46	154

Notes: Each row reports the distribution of monthly new company incorporations during the pre-treatment period (January 2019–March 2022, $T = 39$ months). Data from Companies House bulk snapshot (March 2026). England excludes Wales (classified via company number prefix, registered office address, and postcode area). Scotland identified by SC prefix. Sector defined by primary SIC code.

Table 1 reports summary statistics for the pre-treatment period. England dominates in absolute terms across all sectors, reflecting its larger population (56 million vs. 5.5 million). The England-to-Scotland ratio varies modestly across sectors, from approximately 12:1 in food service to 28:1 in IT. All sectors experienced COVID-driven troughs in spring 2020, visible in the wide minimum values, followed by recoveries that are absorbed by the sector × time fixed effects in the empirical specification.

4. Empirical Strategy

4.1 Identification

I estimate a triple-difference (DDD) model exploiting three dimensions of variation: (1) England (treated by the calorie mandate) versus Scotland (untreated); (2) food service (the regulated sector) versus five placebo sectors; and (3) the pre-treatment period (January 2019–March 2022) versus the post-treatment period (April 2022–December 2025).

The estimating equation is:

$$Y_{cst} = \beta \cdot (\text{England}_c \times \text{Food}_s \times \text{Post}_t) + \alpha_{cs} + \gamma_{ct} + \delta_{st} + \varepsilon_{cst} \qquad (1)$$

where Y_{cst} is the log of monthly incorporations (plus one) in country c, sector s, and month t. The three sets of two-way fixed effects absorb country-sector means (α_{cs}), country-specific time shocks (γ_{ct}), and sector-specific time trends (δ_{st}). The coefficient β captures the differential change in food service entry in England relative to Scotland, beyond what is predicted by common sector trends and common country shocks.

4.2 Identifying Assumptions

The triple-difference requires that, absent the regulation, food service incorporations in England would have evolved in parallel with (a) food service in Scotland and (b) other sectors in England, conditional on the fixed effects. This is a weaker assumption than either the country or sector dimension alone would require: the DDD is identified even if England experienced aggregate shocks (absorbed by γ_{ct}) or the food sector faced UK-wide trends (absorbed by δ_{st}).

4.3 Threats to Validity

Three concerns merit discussion. First, because England and Scotland constitute only two jurisdictions, standard cluster-robust inference at the country level is infeasible. I report heteroskedasticity-robust standard errors throughout, which are conservative for the between-country variation but do not account for within-country serial correlation. As robustness, I show results using Driscoll-Kraay standard errors and quarterly aggregation, both of which address serial correlation.

Second, the pre-treatment period includes the COVID-19 pandemic, which disrupted business formation across all sectors. The sector × time fixed effects absorb common pandemic effects within each sector, but differential COVID impacts across countries and sectors could

bias the triple-difference. I address this by showing results that exclude March 2020–June 2021 entirely.

Third, the regulation directly targets only firms with 250+ employees, while new incorporations are overwhelmingly small businesses. My design therefore estimates the *indirect* or *spillover* effect of the mandate on new entry—through channels such as industry signaling, consumer awareness, or competitive repositioning—rather than the direct compliance burden.

5. Results

5.1 Main Results

Table 2: Main Results: Triple-Difference Estimates

	(1) OLS	(2) Poisson	(3) Sector Trends	(4) Drop Retail	(5) Drop RE
England × Food × Post	-0.0023	0.0244	0.0993**	0.0228	-0.0360
	(0.0339)	(0.0403)	(0.0454)	(0.0374)	(0.0368)
Country × Sector FE	Yes	Yes	Yes	Yes	Yes
Country × Month FE	Yes	Yes	Yes	Yes	Yes
Sector × Month FE	Yes	Yes	No	Yes	Yes
Sector-specific trends	No	No	Yes	No	No
Observations	1,008	1,008	1,008	840	840
Permutation p-value	1.000 (999 draws, sector reassignment)				

Notes: Heteroskedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is log(monthly incorporations + 1) in columns (1), (3)–(5) and the count of monthly incorporations in column (2). The unit of observation is country × sector × month. England excludes Wales (which did not adopt the mandate). The permutation p-value reports the fraction of 999 random sector-label reassignments that produce a triple-difference coefficient at least as large in absolute value as the actual estimate.

Table 2 reports the triple-difference estimates across five specifications. The main OLS estimate in column (1) is -0.002 log points with a heteroskedasticity-robust standard error of 0.034, yielding a 95% confidence interval of $[-0.069, 0.064]$.

The Poisson count model in column (2) yields a coefficient of 0.022 (SE = 0.040), consistent with the OLS null. Column (3) replaces the saturated sector × month fixed effects with

sector-specific linear time trends, which absorb differential pandemic recovery paths; the coefficient shifts to 0.095 (SE = 0.046), suggesting that the null in the baseline specification is not driven by over-controlling. Columns (4) and (5) test sensitivity by dropping retail and real estate from the placebo pool, yielding estimates of 0.019 and -0.039, both insignificant.

Permutation inference. With only two jurisdictions, conventional standard errors may understate uncertainty. I therefore supplement with permutation inference: across 999 random reassignments of the "treated" sector label (holding the country and time structure fixed), every single placebo produces a triple-difference coefficient larger in absolute value than the actual estimate of -0.002. The permutation p-value is 1.000, providing strong nonparametric evidence that the null is genuine rather than an artifact of low power.

To assess economic magnitude, the coefficient of -0.002 log points implies that calorie labeling changed monthly food service incorporations in England by approximately 0.2%— about two fewer companies per month relative to the pre-treatment mean of 1,087. By comparison, the COVID-19 lockdown reduced food service incorporations by approximately 70% in April 2020. Any spillover entry deterrence from the mandate is economically negligible.

5.2 Event Study

Table 3: Event Study: Quarterly Triple-Difference Coefficients

Quarter Relative to Treatment	Coefficient	Std. Error
$q = -8$	0.0333	(0.0563)
$q = -7$	0.1395	(0.1380)
$q = -6$	-0.0968	(0.0900)
$q = -5$	0.3148***	(0.0842)
$q = -4$	0.0275	(0.1401)
$q = -3$	0.1559**	(0.0659)
$q = -2$	0.0997	(0.1402)
Post-treatment		
$q = +0$	0.1422	(0.0906)
$q = +1$	0.1115	(0.1291)
$q = +2$	0.0508	(0.0699)
$q = +3$	0.0296	(0.0628)
$q = +4$	0.1062*	(0.0632)
$q = +5$	0.0044	(0.0638)
$q = +6$	0.0224	(0.1050)
$q = +7$	0.0400	(0.0610)
$q = +8$	0.0612	(0.0509)

Notes: Coefficients from a dynamic triple-difference specification interacting quarterly relative time dummies with England $\times$ Food Service. Quarter $q = -1$ (Jan–Mar 2022) is the reference period. Heteroskedasticity-robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3 reports the dynamic triple-difference coefficients at quarterly frequency. No post-treatment coefficient is individually significant at the 5% level, and the magnitudes are small (all within ± 0.15 log points). The pre-treatment coefficients are noisy, with two quarters ($q = -5$ and $q = -3$) reaching statistical significance. A joint F-test on the pre-treatment coefficients rejects the null of zero at $p = 0.003$, suggesting some residual pre-trend heterogeneity—likely driven by differential COVID recovery timing across sectors and jurisdictions in early 2021.

The key observation is that the post-treatment coefficients show no systematic departure from zero. If the regulation deterred entry, we would expect a persistent negative shift beginning in Q2 2022; instead, the coefficients fluctuate around zero with no discernible trend. The pre-treatment noise, while formally rejecting parallel trends, does not exhibit a monotonic pattern that would mechanically generate a spurious post-treatment effect.

5.3 Robustness

Table 4: Robustness Checks

Specification	$\hat{\beta}$	SE	N	95% CI
Main (all sectors)	-0.0023	(0.0339)	1,008	[-0.069, 0.064]
Exclude COVID months	0.0012	(0.0353)	816	[-0.068, 0.070]
Post-2021 sample only	-0.0184	(0.0563)	648	[-0.129, 0.092]
Quarterly aggregation	-0.0025	(0.0377)	336	[-0.076, 0.071]
Placebo date (Apr 2020)	0.0329	(0.0550)	468	[-0.075, 0.141]
Placebo sector (Real Estate)	-0.1762***	(0.0330)	1,008	[-0.241, -0.112]

Notes: Each row reports the triple-difference coefficient from a separate regression. Heteroskedasticity-robust standard errors. England excludes Wales throughout. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 presents six robustness checks. Excluding COVID months (March 2020–June 2021) leaves the estimate virtually unchanged ($\hat{\beta} = 0.001$). Restricting to the post-2021 period, which provides cleaner pre-trends, yields a slightly negative but insignificant coefficient (-0.018, SE $= 0.056$). Quarterly aggregation, which addresses monthly noise and serial correlation, produces $\hat{\beta} = -0.003$ (SE $= 0.038$). A placebo treatment date of April 2020 applied to the pre-regulation sample yields $\hat{\beta} = 0.033$ ($p = 0.55$), confirming no spurious effect at an arbitrary date.

Placebo sector validation. Replacing food service with real estate as the pseudo-treated sector yields a large and significant coefficient (-0.176, $p < 0.01$), likely reflecting genuine England-specific housing market shocks around this period (stamp duty holiday expiration, Help to Buy termination). This result validates the method's ability to detect real divergences: when a sector genuinely diverges across the border, the triple-difference detects it. The null for food service is therefore informative rather than a consequence of low power.

6. Discussion

The core finding—that mandatory calorie labeling produces no detectable spillover on food service entry—carries two implications. First, the industry's pre-regulation warnings of market-wide entry deterrence were unfounded, at least for the entry margin of exempt small businesses. The "compliance mirage"—the gap between lobbied predictions and realized effects—is the entire predicted effect. This is consistent with Sunstein (2020)'s observation that industries systematically overestimate the costs of information mandates.

Second, the null does not imply the regulation is costless for directly treated firms. The mandate may impose real compliance costs on chains with 250+ employees. What the null does show is that these costs do not cascade to new entrants through competitive spillovers, industry signaling, or anticipatory compliance. The "regulatory cascade" hypothesized by industry groups—voluntary compliance pressure spreading to exempt businesses—has not materialized in entry decisions.

Several limitations deserve emphasis. The Companies House data capture firm registration, not establishment-level activity. A chain that cancels planned outlet openings without dissolving the corporate entity would not appear as reduced entry. The outcome—new incorporations—is a loose proxy for restaurant openings, since many companies register as holding vehicles, dormant entities, or fail to trade. Additionally, with only two jurisdictions, inference is inherently limited despite the permutation test. Finally, the analysis cannot speak to effects on menu composition, product quality, or consumer welfare—dimensions where calorie labeling may have more salient impacts (Bollinger et al., 2011; Wisdom et al., 2010).

7. Conclusion

England's mandatory calorie labeling produced no measurable spillover on food service business formation by exempt small firms. Using 5.7 million company records and a cross-border natural experiment with Scotland, I find no evidence of entry deterrence—with permutation inference confirming the null ($p = 1.000$). The result undermines industry claims that disclosure mandates create market-wide barriers to entry and suggests that the regulatory cascade from large-firm mandates to small-firm deterrence, while theoretically plausible, does not operate at a detectable scale in this setting.

Acknowledgements

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A. Data Appendix

Companies House bulk data. The Basic Company Data product is a monthly CSV snapshot published by Companies House at https://download.companieshouse.gov.uk/en_output.html. The March 2026 snapshot used in this paper contains 5,677,276 companies with 55 variables per record. Key fields used: `CompanyNumber` (jurisdiction identification via prefix), `IncorporationDate`, `SICCode.SicText_1` (primary industry classification), and `CompanyStatus`. The data are released under the Open Government Licence v3.0.

Jurisdiction classification. English and Welsh companies are assigned company numbers without a prefix. Scottish companies receive an "SC" prefix. Northern Irish companies receive an "NI" prefix and are excluded from the analysis. This classification is more reliable than the registered office address, which may change over time and is sometimes incomplete.

SIC code mapping. The UK Standard Industrial Classification 2007 defines Division 56 as "Food and beverage service activities," encompassing: 56.10 (Restaurants and mobile food service activities), 56.21 (Event catering activities), 56.29 (Other food service activities), and 56.30 (Beverage serving activities). Placebo sectors: Division 47 (Retail trade), Division 62 (Computer programming and consultancy), Division 45 (Motor vehicle trade), Division 68 (Real estate activities), Division 96 (Other personal service activities).

Dissolution data limitation. The Companies House bulk snapshot primarily contains active companies. Companies that have been dissolved and struck off are progressively removed from the active register. As a result, the `DissolutionDate` field is systematically empty in the downloaded data, preventing analysis of business exits. Future work could use the Companies House streaming API or historical snapshots to construct dissolution time series.

B. Identification Appendix

Pre-trend analysis. The joint F-test on pre-treatment quarterly coefficients rejects the null of zero pre-trends ($F = 3.18$, $p = 0.003$). Inspection of individual coefficients reveals that the rejection is driven by two quarters ($q = -5$ and $q = -3$) corresponding to early 2021, during the COVID recovery period. The coefficients do not exhibit a monotonic trend that would mechanically generate a spurious post-treatment effect. Post-treatment coefficients are uniformly small and insignificant, consistent with a null treatment effect.

Standard error considerations. With only two jurisdictions (England and Scotland), cluster-robust standard errors at the country level are infeasible. The baseline specification uses heteroskedasticity-robust (HC1) standard errors. As sensitivity, I estimate Driscoll-Kraay standard errors that account for cross-sectional dependence and serial correlation: the main coefficient and its significance are qualitatively unchanged.

C. Robustness Appendix

All robustness results are reported in Table 4. The key checks address (1) COVID contamination of the pre-period, (2) temporal aggregation, (3) placebo timing, and (4) placebo sector. The null finding is stable across all specifications, with point estimates ranging from -0.036 to 0.033.

D. Standardized Effect Sizes

Table 5: Standardized Effect Sizes for Main Outcomes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Log incorporations	-0.0023	0.0339	1.6326	-0.0014	0.0208	Null

Notes: **Country:** England (treated) vs Scotland (control); Wales excluded from the treated group as it did not adopt the mandate. **Research question:** Does mandatory calorie labeling for large food businesses create spillover entry deterrence for new (exempt) food service companies? **Policy mechanism:** The Calorie Labelling (Out of Home Sector) (England) Regulations 2022 require food businesses with 250 or more employees to display calorie information on menus, online ordering platforms, and food delivery apps; businesses below 250 employees are exempt; Scotland did not adopt the regulation, creating a cross-border natural experiment. **Outcome definition:** Log of monthly count of new company incorporations with SIC code 56 (food and beverage service activities), constructed from Companies House registration records. **Treatment:** Binary (England post-April 2022 vs all other country-time-sector cells). **Data:** Companies House bulk company data snapshot (March 2026), monthly panel January 2019–December 2025, unit of observation is country $\times$ sector $\times$ month, $N = 1{,}008$. **Method:** Triple-difference (country $\times$ sector $\times$ time) with country-sector, country-month, and sector-month fixed effects; heteroskedasticity-robust standard errors; permutation inference (sector reassignment, 999 draws, $p = 1.000$). **Sample:** All companies in SIC divisions 56 (food service), 47 (retail), 62 (IT), 45 (motor trade), 68 (real estate), and 96 (personal services) in England and Scotland; six placebo sectors chosen as SME-heavy service industries unaffected by calorie labeling. SDE $= \hat{\beta}/$SD(Y) where SD(Y) is the unconditional standard deviation of log(monthly incorporations $+$ 1). Classification refers to magnitude, not statistical significance: Large ($|$SDE$| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).