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Temporary VAT changes and consumer price pass-through: evidence from UK catering

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ABSTRACT

This paper estimates the pass-through of temporary VAT changes to consumer prices and examines the price-setting mechanism behind it. The United Kingdom cut hospitality VAT from 20% to 5% in July 2020, raised it to 12.5% in October 2021, and restored it to 20% in April 2022, a rare reduction-and-restoration cycle. Using 543,335 Office for National Statistics price quotes underlying the Consumer Prices Index, we estimate event-study pass-through for treated catering relative to on-trade alcohol, which was excluded from the relief and serves as a within-sector control. Transmission is incomplete at every event. The 2020 cut passed through weakly; its estimate is small and positive but imprecise and not cleanly identified, because pre-trends are not flat, whereas the two restorations reached about 28% and 26% of the statutory change. Beneath these averages, incomplete transmission operates through the incidence and direction of repricing, that is, whether and in which direction posted prices move, rather than the size of adjustment among prices that do move; pass-through is also higher for chain outlets than for independents. Tax incidence thus depends less on the statutory rate than on whether, and how, prices adjust.

KEYWORDS

Value-added tax; tax pass-through; tax incidence; repricing and menu costs; extensive-margin repricing; temporary tax policy; Hospitality

JEL CLASSIFICATION

H22; H25; E31; L83

1. Introduction

On 15 July 2020, the United Kingdom reduced the value-added tax (VAT) on hospitality from the standard 20% to 5%. The reduction was unusually large, introduced as an emergency support measure for a sector brought to a near standstill by lockdown (HM Treasury 2020). The relief covered food and non-alcoholic drinks for on-premises consumption, hot takeaway food, holiday accommodation, and admission to attractions, excluding alcoholic drinks (HM Revenue and Customs 2020). It was then unwound in two steps, to 12.5% on 1 October 2021 and back to 20% on 1 April 2022. The episode has become a touchstone in the ongoing debate over a permanent rate reduction, with industry bodies largely asserting that the temporary cut lowered consumer prices. This study asks how much of these VAT changes was reflected in the prices consumers paid, and whether the cut and its later reversals passed through to the same extent.

The extent to which the reduction in consumer prices can be attributed to the tax cut remains

uncertain, as VAT adjustments often result in only partial pass-through to consumer prices, if any. The standard competitive model predicts a one-to-one transmission of tax changes into tax-inclusive prices; however, this ideal is rarely observed in practice. In imperfectly competitive markets, the pass-through rate, rather than statutory incidence, determines how the tax burden is shared between consumers and producers, and there is no inherent reason for this rate to equal unity (Weyl and Fabinger 2013). Empirical evidence indicates that pass-through is typically partial, highly variable, and may respond asymmetrically to price increases and decreases (Benedek et al. 2020; Benzarti et al. 2020; Carbonnier 2007; Kosonen 2015). Consequently, assessing the actual proportion of a temporary, sector-specific tax reduction that is passed on to consumers requires empirical measurement rather than assumptions. This issue gained particular significance during the implementation of such cuts by European governments amid the COVID-19

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pandemic and subsequent inflationary pressures during 2022 and 2023.

Three strands of the modern pass-through literature inform our expectations and frame this study's contribution. The first concerns magnitude. Existing evidence shows that VAT reductions in labour-intensive services are generally passed through only partially; however, the degree of transmission varies substantially across firms. Evidence from Finnish services and restaurants indicates pass-through rates ranging from nearly zero to about one-half, with chains adjusting prices much more than independent businesses (Harju, Kosonen, and Nordström Skans 2018; Kosonen 2015). Evidence from the 2009 French restaurant reform, the closest precedent to the UK policy, suggests even weaker pass-through to consumer prices. Only a small share of the tax cut was reflected in prices, while much of the benefit accrued to owners and employees (Benzarti and Carloni 2019). Cross-country evidence suggests that these outcomes are systematic rather than exceptional, as reduced-rate reforms pass through considerably less than changes to standard VAT rates (Benedek et al. 2020). Taken together, the literature indicates that incomplete transmission is a characteristic feature of VAT reductions in heterogeneous, labour-intensive service sectors, making limited consumer-price effects a plausible expectation for the UK hospitality reform.

A second strand concerns temporariness itself. A temporary change has to be priced in and then reversed, so menu costs are incurred twice, and it invites consumers to shift purchases across time. Both features can make its price effect differ from that of a permanent reform (Blundell 2009; Crossley, Low, and Wakefield 2009; D'acunto, Hoang, and Weber 2018). Britain's 2008–09 cut is a case in point, passing through at first and then fading before the statutory reversal arrived (Crossley, Low, and Sleeman 2014). The COVID-era German cut generated a similar picture, with incomplete pass-through in fuel (Montag, Sagimuldina, and Schnitzer 2020) and supermarket retail (Fuest, Neumeier, and Stöhlker 2025), alongside a measurable demand response (Bachmann et al. 2021). The lesson is that a temporary cut's effect on prices can be partial and short-lived, which matters for any policy sold on the promise of lasting relief.

The third strand, and the one where the literature is most openly divided, concerns asymmetry. The influential benchmark comes from Benzarti et al. (2020), who find that prices respond roughly twice as strongly to VAT increases as to decreases. This asymmetry raises equilibrium markups, persists for years, and recurs across EU reforms. On that reading, prices are sticky downward, and a cut is a poor way to help consumers. Yet a pointed counterexample has just appeared. Studying a temporary cut in Portuguese food prices using daily online prices, Bernardino et al. (2025) find the opposite: full, persistent, and symmetric pass-through, with prices dropping cleanly on impact, holding low for the policy's life, and reversing just as cleanly. They attribute the difference to unusually high salience in a high-inflation setting, echoing Chetty, Looney, and Kroft (2009) on consumers under-reacting to taxes not built into the posted price. The field is thus left with two incompatible templates, asymmetric-and-incomplete versus symmetric-and-full, and no clear account of when each applies. This study does not seek to settle that debate. Instead, the UK sequence permits examining transmission in both directions within a single institutional setting and asking which feature of that setting might reconcile such divergent findings.

The study most closely related to ours is Onnis et al. (2025), who examine the UK hospitality VAT reduction using web-scraped Booking.com prices for hotels in London and Edinburgh, with continental European destinations serving as controls. They estimate contemporaneous pass-through of approximately 20 to 50%, although the response is discretionary, concentrated in near-term bookings, and largely dissipates within two months. Their findings therefore provide evidence of limited and selective transmission within the UK accommodation sector. However, the analysis does not consider the two subsequent VAT restorations, does not extend to the wider catering sector, and cannot directly assess whether pass-through differs between tax reductions and increases. In addition, the use of cross-country controls may not fully capture UK-specific shocks.

Against this background, the study makes two principal contributions, together with two

supporting features of the design. The first principal contribution is to examine the complete policy cycle. To our knowledge, this is the first analysis of the UK hospitality VAT episode to use the Office for National Statistics price-quote microdata that underpin the Consumer Prices Index (CPI), and to study not only the 2020 cut but also both subsequent restorations within the same institutional setting. This extends the existing UK evidence beyond hotel accommodation and beyond the initial reduction. CPI microdata have been used to study VAT pass-through elsewhere, notably by Benkovskis and Fadejeva (2014) for Latvia, but not, to our knowledge, for the UK hospitality episode or across a full cut-and-restoration cycle.

Supporting this, our identification relies on a design feature. We use on-trade alcoholic drinks as the control group because they were excluded from VAT relief but were sold in the same broad hospitality settings as the treated catering items, which allows a within-sector comparison rather than a cross-country or cross-sector one. We are transparent that the pre-trends between the two groups are not flat, and we build the paper's conclusions around that limitation.

The policy sequence also permits an informative within-setting comparison of upward and downward transmission. However, because the observable event windows differ across events, this asymmetry comparison is highly sensitive to counterfactual trend assumptions, so we treat it as exploratory rather than as a principal contribution.

The second contribution concerns the repricing mechanism. We move beyond average pass-through estimates to examine patterns of price adjustment and their variation across outlets. The model distinguishes the frequency and direction of repricing from the magnitude of price changes among outlets that repriced. During the VAT cut, upward and downward adjustments occurred with similar frequency, so substantial gross repricing produced little net movement. By contrast, the two restorations were characterized by a clear predominance of upward adjustments. The decomposition focuses primarily on whether and in which direction prices were revised, rather than on systematically smaller adjustments among policy-consistent movers. We then use the multiple-versus-

independent outlet classification in the microdata to show that pass-through is consistently higher for chain outlets than for independents, and significantly so at the October 2021 restoration. This is consistent with a repricing-cost channel and with the firmer pass-through reported for chains by Harju, Kosonen, and Nordström Skans (2018). The extensive-margin pattern remains when the sample is restricted to collection-cell-item pairs with a single quote at both endpoints, although the available identifiers do not allow us to verify that the same physical outlet is observed throughout.

Across every specification we examine, full pass-through is rejected at all three events. The cut was passed through weakly. The estimate is about 0.12, small and positive across economically valid control groups, but it is imprecise, with a confidence interval that includes zero. We show that this estimate is not cleanly identified because pre-trends between treated catering and the control are not flat; therefore, we do not treat the precise magnitude of the cut as a headline result. The two subsequent increases were transmitted more clearly, with estimated pass-through of about 28% in October 2021 and 26% in April 2022, the latter being the most precisely estimated effect. Even so, pass-through remained substantially incomplete in both cases. The larger point estimates for the increases are consistent with asymmetric pass-through, but the differences are neither statistically significant nor robust to adjustment for pre-existing trends, so we treat the evidence of asymmetry as suggestive rather than conclusive. Full pass-through can be rejected at every event across the specifications we examine. The raw point estimates are larger for the restorations than for the cut, but this difference is not statistically significant and does not persist after detrending, so we do not interpret it as evidence of directional asymmetry.

2. Economic framework and empirical implications

Who bears a VAT change is not determined by who hands the money to the tax authority. In a perfectly competitive market with constant

marginal cost, an ad valorem change is fully reflected in the tax-inclusive price, but that is a limiting case. Under imperfect competition, the relevant object is the pass-through rate, the change in consumer price per unit change in tax, and it is this, not statutory incidence, that determines how the burden divides. Its value depends on demand curvature, firms' conduct, and cost conditions, and it may be below or above one (Weyl and Fabinger 2013). The magnitude is thus an empirical matter, and it carries the policy content directly: a temporary hospitality cut lowers consumer prices only to the extent that prices actually move.

Three features of our setting sharpen this baseline. The relief targets a labour-intensive service sector, where reduced-rate pass-through is typically well below one (Benedek et al. 2020; Benzarti and Carloni 2019; Kosonen 2015). It is also temporary, so a firm that reprices pays the menu cost twice, on the way in and on the way out, which can discourage adjustment altogether and leave transmission partial and short-lived (Blundell 2009; Crossley, Low, and Wakefield 2009). The VAT here is folded into the posted price, so a consumer sees the final figure move but cannot easily separate a tax-driven change from one driven by costs, demand, or margins. That opacity may dull the pressure to pass a cut on, without making the posted price any less visible. Therefore, we treat tax attribution as a possible channel rather than lean on a strong salience claim (Chetty, Looney, and Kroft 2009). Together, these point to the importance of the aggregate pass-through of each change, and of the pattern of price changes, including their frequency, direction, and size, that produces it. Whether the cut and its reversal pass through symmetrically is a separate matter. Observing a cut and two restorations for the same items lets us examine the difference directly rather than infer it from separate episodes.

This framework yields three empirical questions. First, how much of each statutory VAT change was reflected in consumer prices? Second, did the two increases pass through more strongly than the initial reduction? Third, did incomplete transmission arise because relatively few prices changed, or

because the prices that did adjust moved by less than the statutory benchmark?

3. Data and empirical strategy

3.1. Source and structure

This study uses the price-quote microdata underlying the UK CPI, published monthly by the Office for National Statistics (Office for National Statistics 2024). Each record is an individual price observation collected from an outlet in a given month, including the item identifier and description, the collected and base prices, region, an outlet-type indicator (multiple or chain versus independent), a validity marker, collector indicator codes, an anonymized shop code, and stratum and shop weights. Around 180,000 quotes across roughly 730 representative items are collected each month, near a mid-month index day, from about 140 locations. We assemble the complete monthly files from January 2018 to December 2023, covering 30 months before the first rate change and 20 after the last. The microdata consist of locally collected quotes; centrally collected items, including most hotel accommodations sourced from booking platforms, are not included. Therefore, the quote-level analysis concerns catering, on-premises food and non-alcoholic drinks, and hot takeaway, for which local collection is dense.

3.2. Treatment mapping

We classify every item before examining any prices. Candidates are drawn from the ONS CPI classification, in which each item carries a COICOP code; division 11 covers catering, accommodation, and the alcohol items that form the control group. Each candidate is adjudicated by hand against the statutory scope of the relief (HM Revenue and Customs 2020), since COICOP position alone does not resolve borderline cases. The mapping yields treated catering (23 items: on-premises meals, non-alcoholic drinks, and hot takeaway) and the primary control, on-trade alcohol (14 items: beer, wine, and spirits in licenced premises), which is excluded from the relief but sold in the same venues. For robustness, it also yields untreated catering (2 standard-rated items), off-trade alcohol

(20 items), and a zero-rated placebo (a cold take-away sandwich, unaffected throughout). Ten genuinely ambiguous items, at the hot-food serving-temperature boundary or involving mixed bundles and exempt admissions, are excluded from the primary analysis. Appendix A reports the complete mapping with per-item justifications.

3.3. Panel construction and quality control

3.3.1. The outlet identifier

The released microdata identify a quote's outlet only by region and an anonymized shop code. ONS documentation states that shop codes may be duplicated across collection locations and that the variable required to uniquely identify a physical outlet is withheld for disclosure reasons. We therefore do not interpret region by shop code as a physical outlet; we call it an anonymized collection cell, and specifications using it are cell-level rather than venue-level. Cell labels are persistent (median observed lifespan 28 months (maximum 71), computed on collection cells at the raw-quote stage), though label persistence does not imply a constant physical composition. Consistent with cells sometimes pooling more than one price point, 11.3% of cell-item-month combinations contain multiple quotes, which hold 21.1% of quotes and are concentrated in on-trade alcohol; 88.7% of cell-item-months contain a single quote. The preferred item-fixed-effects regressions retain individual quotes; for the extensive-margin and cell-level specifications, multiple quotes within a cell-item-month are collapsed to their mean log price.

3.3.2. Quality control

Quotes are retained with ONS validity markers eligible for index construction (codes 3 and 4; codes 53 and 54 do not occur in these files). CPI quotes are observed prices used in index compilation rather than transaction records, so sale prices are retained, and a no-sale-price check is reported. Comparable substitutions continue a series; non-comparable ones start a new spell. Prices are logged, and any change is treated as zero within a tolerance of -6 to 10 . The preferred specification applies no winsorization, since within-item-month trimming would use the post-treatment distribution to define extreme values and could remove

a genuine response. In any case, one-month log changes exceeding 0.5 in absolute value are 3.3% of treated changes (and those exceeding 1.0 are 0.3%), and these are overwhelmingly genuine price movements rather than product substitutions or resets. Two pandemic features require handling. The hospitality collection was suspended or imputed during lockdowns; we drop imputed quotes and, if the count of valid treated-catering quotes falls below half the 2019 monthly mean, treat the month as thin and drop it from estimation. *Eat Out to Help Out* distorts August 2020, which we exclude throughout. The assembled panel contains 842,077 quotes overall and 543,335 in the primary comparison.

Figure 1 shows the monthly counts of valid quotes for the treated and control groups across the sample, along with the thin-month threshold. The pandemic-era collection gaps in 2020 and early 2021 are clearly visible, as are the three rate changes. Estimation uses only months when coverage exceeds the threshold, so the lockdown months with low coverage do not contribute.

Our primary specification uses item fixed effects. Because region-by-shop-code does not identify physical outlets, we retain the item-fixed-effects specification as preferred and treat cell-level fixed effects as a robustness check.

3.4. Design and estimand

Pass-through is estimated by comparing treated catering with on-trade alcohol in an event study. For event e in month te ,

$$\ln 1mup_{ict} = \alpha_i + \delta_{r(c),t} + \gamma_{s(c),t} + \sum_{k \neq k_0} \beta_k^e \times 1[t - t_e = k] \times D_i + \varepsilon_{ict}$$

where $\ln p_{ict}$ is the natural logarithm of the price of item i observed in collection cell c in month t . D_i indicates treated catering, α_i are item fixed effects, and δ and γ are region-by-month and outlet-type-by-month fixed effects. The preferred regressions use equal quote weights, which require no assumptions about the published stratum weights; the preferred estimand is the average response across the collected quotes. CPI item expenditure weighting, most relevant to the policy arithmetic of Section 5, is reported

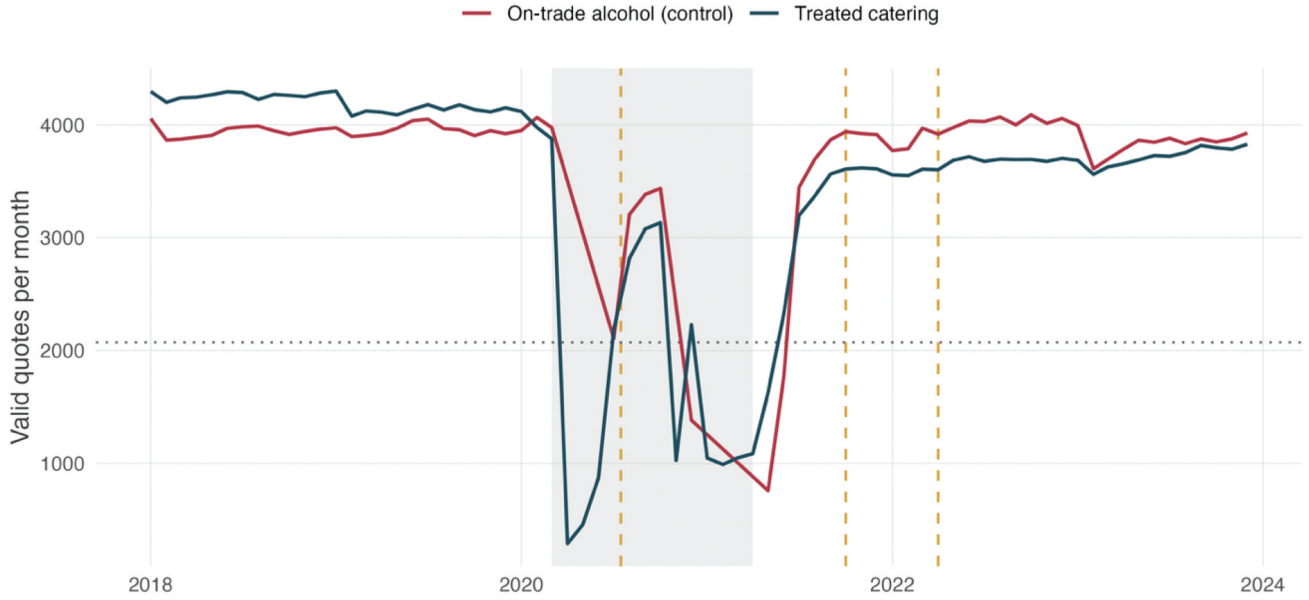


Figure 1. Data coverage and the pandemic collection gaps. *Note:* Monthly valid quotes for treated catering and on-trade alcohol; shaded regions mark lockdowns, gold vertical lines mark the rate changes, and the dashed line marks the thin-month threshold (half the 2019 treated mean). Estimation uses only months above the threshold.

alongside raw and normalised stratum weights as sensitivity checks. Item fixed effects remove permanent price-level differences; identification comes from differential movement in treated versus control prices, with the alcohol control and the time-varying fixed effects absorbing components of regional, establishment-type and macroeconomic shocks that are common to treated and control prices. They cannot absorb shocks that differentially affect catering and on-trade alcohol. We estimate a separate event study for each rate change. Let π_e be the log statutory price ratio, the full-pass-through benchmark (-0.1335 for the cut, $+0.0690$ and $+0.0645$ for the rises). The window-averaged treated coefficient and pass-through are

$$\theta_e = \frac{1}{|K_e^{post}|} \sum_{k \in K_e^{post}} \beta_k^e, \quad \Phi_e = \theta_e / \pi_e$$

so $\Phi_e = 1$ is full pass-through and $\Phi_e = 0$ none. The asymmetry parameter contrasts a rise with the cut, $\Delta_{e,e'} = \Phi_e - \Phi_{e'}$.

3.5. Estimation windows

The three changes are closely spaced, so both the sample and the averaged coefficients are bounded, and adjacent-policy months are

excluded from the estimation window. Pre-event months are not contaminated by an adjacent rate implementation but are not free of announcement effects: both increases were announced in the March 2021 Budget, so no post-announcement month is anticipation-free, and March 2021 is excluded. For the cut, collection at the July 2020 implementation is unusable, so the estimand is an average relative-price response across coverage-admissible reopening months (September 2020 to September 2021), not an immediate implementation effect. The October 2021 rise is estimated relative to September 2021 and identifies the incremental implementation effect; the April 2022 restoration has two clean pre-implementation months (January and February 2022). The exact windows are reported in Appendix B.

3.6. The pre-existing trend

Treated prices drift upwards relative to control before the policy. We estimate the drift in the pre-2020 sample as the coefficient λ on the treated-by-month-index interaction, yielding the estimate below (bootstrap CI 0.00002 to 0.00308):

$$\lambda = 0.0015 \text{ per month},$$

$$\theta_e(\text{detrended}) = \theta_e - \lambda \times h_e,$$

$$\Phi_e(\text{detrended}) = \theta_e(\text{detrended})/\pi_e$$

with h_e the mean post-window horizon from reference. Because whether this drift would have continued absent the policy is not identified, we report the raw and detrended pass-through as sensitivity scenarios under two counterfactual trend assumptions rather than as identified bounds, and foreground their difference in the asymmetry parameter. The detrending is consequential: it raises the cut estimate and reverses the sign of the rise-minus-cut contrast, which is central to our cautious reading of asymmetry. We also test the pre-trends formally. For each event, we jointly test the pre-period leads in the event study. The test rejects flat pre-trends for the cut and for the April 2022 restoration ($p < 0.001$) and does not reject for the October 2021 restoration ($p = 0.24$). Following Roth (2022), pre-tests have low power precisely when pre-trends matter, so passing the test would still be insufficient. For this reason, we do not rely on the pre-trend test. Instead, we report the detrended estimate across the full confidence interval for the drift, and show in Section 4.6 that the qualitative conclusion is robust to the choice of control group and the trend assumption. Across the confidence interval for the drift, the detrended cut ranges from about 0.12 to 0.43, while full pass-through is rejected at every value. We further find that the pre-2020 relative trend is well approximated as linear, since a quadratic term is not significant ($p = 0.18$), and we find no statistically detectable quadratic curvature in the pre-period, although this does not establish that linear extrapolation is correct. We therefore treat the detrended estimate as a scenario under an untestable trend assumption rather than as a correction.

3.7. Extensive margin

The aggregate price adjustment depends jointly on the incidence, direction, and magnitude of individual price changes, and we decompose it accordingly. For matched endpoints, the mean change decomposes exactly as

$$E[\Delta \ln p] = Pr(U) \times E[\Delta \ln p | U] + Pr(D) \times E[\Delta \ln p | D]$$

At the cut, treated up- and down-components nearly cancel, so a near-zero aggregate coexists with substantial gross repricing; at the restorations, upward repricing dominates. As distributional statistics, we report the median, the interquartile range, and the share of policy-consistent changes within 1 and 2 log points of the benchmark. Because a collection cell may pool multiple outlets, we refer to these as collection-cell-item price changes rather than outlet repricing. We verify that the pattern is not an artefact of multi-quote cells by repeating the analysis on matched endpoint pairs with a single underlying quote at both endpoints (92.1% of pairs); the shares, medians, and bands closely match those of the full sample.

3.8. Inference

Treatment varies across only 37 items, including 14 control items, and cluster sizes are unbalanced. Conventional cluster-robust inference may therefore remain unreliable (Cameron, Gelbach, and Miller 2008; Colin Cameron and Miller 2015), so we use a stratified item-cluster bootstrap as our primary inference procedure. The bootstrap addresses the unreliability of cluster-robust inference when clusters are few and unbalanced; it does not create identifying variation that the design does not contain. Because treatment is assigned at the food-versus-alcohol level, the effective comparison remains close to a two-group contrast, and the resampling should be read as correcting the inference rather than as conferring precision the design cannot support. This is a stratified pairs-cluster bootstrap that handles treated and control items separately, relabels duplicate items and cells, and re-estimates the full estimator, including the drift, in each of 4,999 draws. We summarize with 2.5th to 97.5th percentile intervals. We do not use a permutation test that reassigns the treated label within COICOP class 110101, because food and alcohol items are not fully exchangeable, which is the condition the permutation null requires; such a test would not have a valid interpretation. Cell dependence are assessed using two-way item-and-cell clustered errors and, where pre-trends are a concern,

report Rambachan and Roth (2023) relative-magnitude bounds for the cut, using an extended-window specification; the densely spaced increases yield too few clean pre-periods for these bounds to apply. Robustness to alternative control groups, weighting schemes, functional form, and placebo events and outcomes is reported in Section 4.6.

4. Results

4.1. Sample and descriptive patterns

The primary comparison comprises 543,335 price quotes for 37 items (23 treated catering items, 14 on-trade alcohol control items), observed monthly from 2018 to 2023. Table 1 sets out the two groups, the statutory rate they faced at each event, and summary characteristics of their price series. Two features of the data bear on identification. First, coverage is dense and stable outside the pandemic period, but collection was severely disrupted during the 2020 and early-2021 lockdowns; the coverage rule in Section 3 excludes the affected months from estimation. Second, a minority of collection cells contain multiple simultaneous quotes, and these are concentrated in the alcohol control; we return to their influence in Section 4.6.2.

4.2. Pass-through of the three rate changes

The central estimates are in Table 2, and they tell a consistent story. What we can decisively reject under every specification is full pass-through of the 2020 cut. The transmission of the cut was at most modest: the estimate is about 0.12 of the statutory reduction, small and positive across economically valid controls, but imprecise, so we cannot rule out that nothing was passed on, and we do not treat the precise figure as a headline result. The policy delivered to consumers only a small fraction of the price cut that a competitive, fully transmitting market would have produced.

The restorations look different. In the raw specification, both have larger point estimates than the cut and are distinguishable from zero, with roughly a quarter of each increase reflected in prices. The April 2022 return to 20% is the most precisely estimated of the three and the clearest sign that an upward adjustment genuinely occurred, if only in part. None of the three, though, comes close to full pass-through: in every case, most of the statutory change was not reflected in posted prices, exactly as the reduced-rate literature would lead one to expect for a labour-intensive service (Benedek et al. 2020; Benzarti and Carloni 2019; Kosonen 2015).

Figure 2 shows the full bootstrap distributions: the cut's mass is centred near zero and straddles it,

Table 1. Sample composition, statutory treatment, and price-series characteristics by group.

Group	Items	COICOP	Quotes	Multi-quote incidence	Jul 2020	Oct 2021	Apr 2022	Freq. change
<i>A. Primary comparison</i>								
Treated catering	23	110101–110103	269,278	low	20→5%	5→12.5%	12.5→20%	0.26
On-trade alcohol (control)	14	110102	274,057	high	20% (excl.)	20%	20%	0.36
<i>B. Robustness groups (excluded from the primary comparison)</i>								
Untreated catering	2	110103	—	—	20%	20%	20%	—
Off-trade alcohol	20	110201	—	—	20%	20%	20%	—
Zero-rated placebo	1	110103	—	—	0%	0%	0%	—

Notes: Statutory full pass-through corresponds to log price changes of -0.1335 (cut), $+0.0690$ (Oct 2021) and $+0.0645$ (Apr 2022). Quote counts are approximate group splits of the primary comparison; multi-quote cells hold 21.1% of quotes overall (computed at the raw-quote stage of panel construction). Dashes denote quantities not used in the primary analysis.

Table 2. Pass-through of each VAT change: full inference under the raw and detrended specifications.

Event	Benchmark π	Coef. θ	Boot. SE of Φ	Φ (raw)	95% CI	p	Reject $\Phi=0?$	Reject $\Phi=1?$	Φ (detr.)	95% CI (detr.)
Cut (Jul 2020)	-0.1335	-0.0159	0.084	0.119	$[-0.038, 0.290]$	0.148	No	Yes	0.268	$[0.040, 0.561]$
Rise 1 (Oct 2021)	$+0.0690$	$+0.0190$	0.135	0.276	$[0.030, 0.564]$	0.024	Yes	Yes	0.232	$[-0.018, 0.525]$
Rise 2 (Apr 2022)	$+0.0645$	$+0.0171$	0.084	0.264	$[0.095, 0.428]$	0.003	Yes	Yes	0.170	$[-0.025, 0.350]$

Notes: p -values are two-sided bootstrap tail probabilities $p = 2 \times \min\{\Pr(\psi^* \leq 0), \Pr(\psi^* \geq 0)\}$ with the $(B+1)$ adjustment.

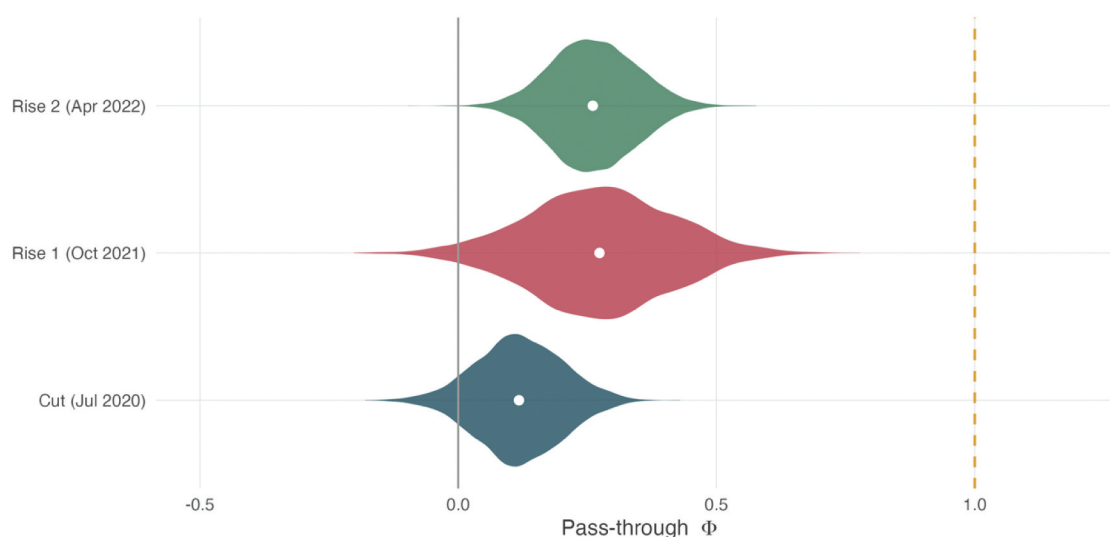


Figure 2. Pass-through of each VAT change into consumer prices. *Note:* Bootstrap distributions ($B = 4,999$); white points mark the point estimates and the gold dashed line marks full pass-through. Preferred item-fixed-effects, equal-quote specification.

the restorations are clearly positive and tighter, and none approaches unity.

4.3. Dynamics

The event-study paths show how the treated-control gap changed around each event. They are not uniformly flat beforehand, least of all around the tightly spaced restorations, which is precisely what motivates the trend-sensitivity analysis. After the cut, the gap wanders without ever approaching its

downward benchmark; after each restoration, it climbs slowly and settles short of the upward one. What is missing throughout is any sharp, complete jump in which prices respond at all; they do so only partially and over several months. Figure 3 presents the three paths.

4.4. Asymmetry and its sensitivity to the pre-trend

This section examines whether the increases pass through more readily than the VAT cut, as the

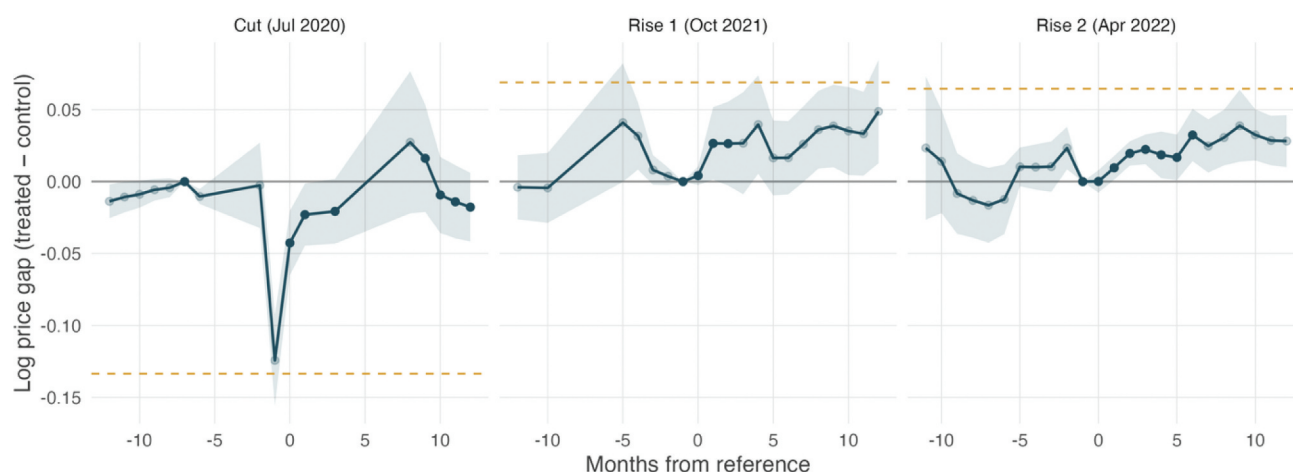


Figure 3. Event-study price paths: treated catering versus on-trade alcohol. *Note:* Treated minus control log price gap for each event; shaded bands are 95% confidence intervals, the gold dashed line the full-pass-through benchmark, and the reference period normalized to zero. Faded points mark thin-collection months that are excluded from the estimation. The figure is an extended descriptive event study displaying a symmetric window around each event; months excluded from the bounded estimation samples in Section 3.5 (thin-collection, adjacent-policy, and announcement-contaminated months) are shown in faded colours and are excluded from the preferred post-event averages.

Table 3. Asymmetry: rise-minus-cut pass-through contrasts under both specifications.

Contrast	Raw estimate	95% CI	p	Detrended estimate	95% CI	p
Rise 1 – Cut	0.163	[−0.095, 0.437]	0.244	−0.036	[−0.424, 0.341]	0.822
Rise 2 – Cut	0.151	[−0.116, 0.394]	0.237	−0.098	[−0.510, 0.251]	0.606

Note: Positive values indicate a rise passing through more than the cut. Bootstrap intervals and p -values, $B = 4,999$. All four intervals contain zero; the sign reversal between the raw and detrended specifications is the basis for treating asymmetry as suggestive rather than established.

asymmetry literature would predict (Benzarti et al. 2020). Table 3 presents the rise-minus-cut contrasts. At face value, the answer appears to be that both raw contrasts are positive, consistent with downward stickiness. But the evidence does not hold up for two reasons. The contrasts are imprecise, so neither is distinguishable from zero; more significantly, once we strip out the pre-existing trend in catering prices relative to alcohol, both turn slightly negative. Because the sign of the measured asymmetry depends on whether one adjusts for that trend, and because it never reaches significance in any case, we decline to interpret the results as establishing asymmetric pass-through. What they show instead is that any conclusion about asymmetry here rests on an identifying assumption, the counterfactual path of the treated-control gap, that the data alone cannot settle. That qualifies, without overturning, the benchmark set by Benzarti et al. (2020) and helps align it with the symmetry reported by Bernardino et al. (2025). Because the observable event windows differ across events, in horizon, pandemic state, and inflation environment, these contrasts should not be interpreted as a clean structural estimate of directional

asymmetry. We therefore treat the asymmetry comparison as informative but exploratory, not as a principal result.

Figure 4 shows the contrast distributions under both specifications.

4.5. The mechanism of adjustment

Incomplete pass-through can arise two ways, from few prices moving or from movers moving little, and the two carry quite different meanings. Here the data point clearly to the first. At each event, a large share of treated prices does not budge at all, yet those that do move in the policy-consistent direction shift by close to the mechanical benchmark, the median change being essentially the benchmark itself. The evidence thus points primarily to the incidence and direction of repricing, rather than to systematically smaller adjustments among prices that move in the policy-consistent direction. Figure 5 shows the distributions of changes behind this reading. A similar pattern appears in Benkovskis and Fadejeva (2014), who, using Latvian CPI micro-data, find that the frequency of price changes

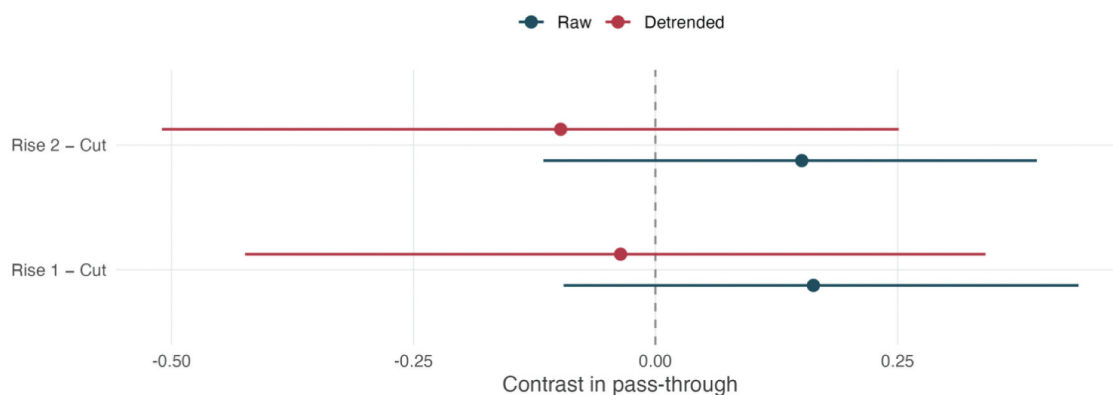


Figure 4. Asymmetry in pass-through and its sensitivity to the pre-trend. Note: Rise-minus-cut contrasts, raw versus detrended bootstrap distributions. Positive values indicate a rise passing through more than the cut. Both contrasts are positive in the raw specification but become negative after detrending, and in both cases the 95% interval includes zero. Because the sign depends on the trend adjustment and the difference is never statistically significant, we treat directional asymmetry as exploratory rather than as an established result.

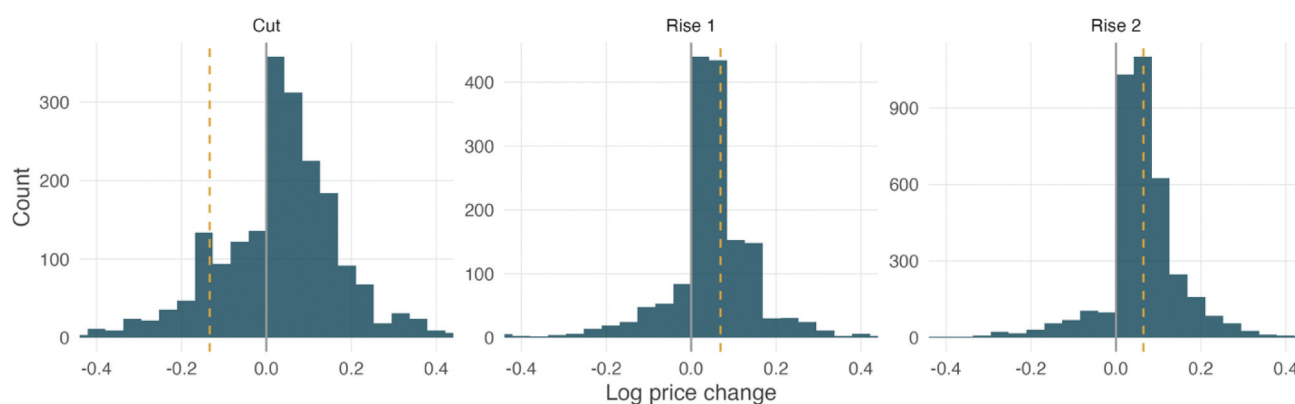


Figure 5. How prices adjusted: within-cell-item price changes. *Note:* Distribution of log price changes for treated catering at each event; the gold dashed line marks the full-pass-through benchmark. A large mass of observations shows no change, while policy-consistent changers concentrate near the benchmark. The cut uses matched-endpoint changes, whereas the restorations use one-month changes; the distributions should therefore be interpreted within events rather than as directly comparable frequencies across events.

Table 4. Extensive-margin decomposition of treated price changes, by event.

Event	P(increase)	P(decrease)	P(no change)	Median $\Delta \ln p$	Up contrib.	Down contrib.	Net mean
Cut	0.24	0.24	0.52	≈ 0.00	+0.025	-0.032	-0.007
Jul (2020)							
Rise 1	0.16	0.03	0.81	+0.07	+0.014	-0.004	+0.011
Oct (2021)							
Rise 2	0.28	0.02	0.70	+0.07	+0.023	-0.002	+0.021
Apr (2022)							

Notes: Descriptive statistics on treated catering items over each event window (matched endpoints for the cut; one-month changes for the restorations). The median column reports the median across all observed changes, which is near zero at the cut because upward and downward revisions offset. Conditional medians among policy-consistent changers, which lie close to the statutory benchmarks (-0.1335, +0.0690, +0.0645), are reported in Table 7. These statistics are descriptive and are not substituted into the decomposition identity in section 3.7. Because the cut uses matched endpoint changes whereas the restorations use one-month changes, these frequencies are most naturally interpreted within each event rather than as directly comparable event-to-event probabilities.

peaks at the moment of a VAT adjustment while the average size of revisions is partially offset, and that pass-through is stronger for increases than for reductions.

Table 4 quantifies this. The cut is distinctive: upward and downward revisions occur in almost equal measure, so substantial gross repricing nets to near zero. This is a cancellation of responses, not their absence. The restorations differ, with upward revisions predominating. The cut's weak aggregate transmission thus reflects offsetting movements at the level of individual series, which a summary coefficient would obscure.

Figure 6 shows that the cut's net is near zero between two large offsetting bars, whereas the restorations are up-margin-dominated.

The aggregate estimates may mask heterogeneity across outlet types, which the microdata allow us to examine through the multiple-versus-independent classification. We re-estimate pass-through separately for chain and

independent outlets. Pass-through is higher for chains at every event. At the October 2021 restoration, the difference is large and statistically significant, with chains at 0.51 and independents at about zero (difference 0.57, bootstrap $p = 0.002$). At the cut and the April 2022 restoration, the difference points the same way but is not significant (chains 0.24 versus 0.04 at the cut; 0.29 versus 0.17 at the second restoration). Table 5 reports these estimates with bootstrap intervals. These split-sample estimates use item and region-by-month fixed effects within each outlet type; they are not constrained to average to the pooled estimate of 0.119, which is identified from the full sample with the additional outlet-type-by-month effects.

This heterogeneity has a natural interpretation and connects the mechanism to market structure. In our data, chains reprice more often than independents, with a repricing frequency of about 0.13

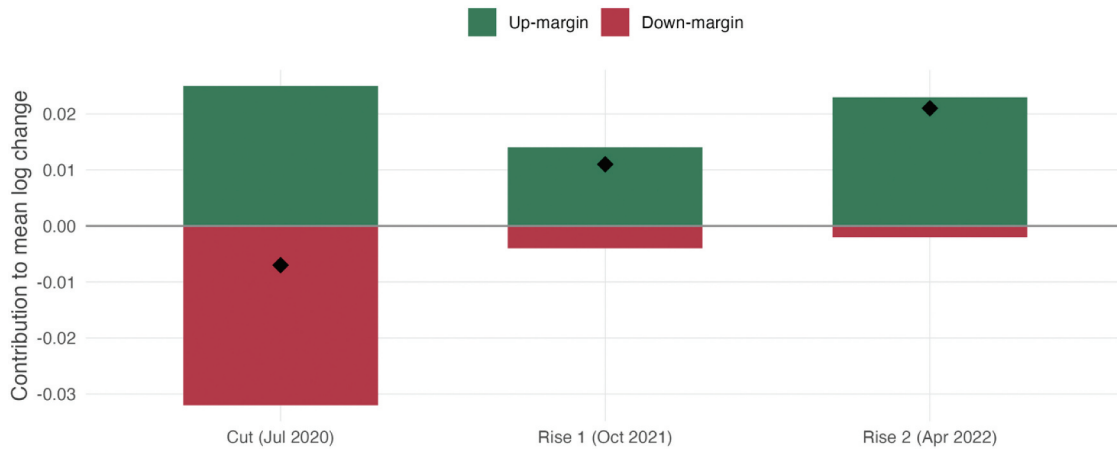


Figure 6. Why the cut nets to near zero: offsetting margins. *Note:* Up- and down-margin contributions to the mean treated change, with the net change shown as a diamond. The cut’s near-zero net masks large offsetting margins; the restorations are dominated by the up-margin.

Table 5. Pass-through by outlet type (chains versus independents).

Event	Chains Φ	Independents Φ	Difference	95% CI (diff.)	p (diff.)
Cut (Jul 2020)	0.24	0.04	+0.20	[−0.05, 0.58]	0.124
Rise 1 (Oct 2021)	0.51	−0.06	+0.57	[0.18, 1.04]	0.002
Rise 2 (Apr 2022)	0.29	0.17	+0.12	[−0.16, 0.39]	0.383

Notes: Pass-through by outlet type, estimated separately for multiple (chain) and independent outlets using the ONS outlet classification. Differences and intervals are from the stratified item-cluster bootstrap ($B = 4,999$). The chain-independent difference is significant only at the October 2021 restoration; the direction is consistent across events.

versus 0.09. Since incompleteness works through the extensive margin, outlets that adjust prices more readily transmit more of a statutory change. This is consistent with a lower-repricing-cost interpretation, though product mix, size, and location may also contribute, and the pattern echoes the firmer pass-through reported by Harju, Kosonen, and Nordström Skans (2018) for chains in the Finnish restaurant reform. It is also consistent with the role of market structure and competitive intensity emphasized by Dimitrakopoulou et al. (2024). We do not claim a uniform difference across events, since the contrast is significant only at the October 2021 restoration, but the direction is consistent, and the mechanism is coherent.

4.6. Robustness and sensitivity

4.6.1. Weighting

The choice of quote weighting does not affect the substantive conclusions. Table 6 reports pass-through under four schemes. The restoration estimates are stable across all four, always positive, and the cut remains small under equal, raw, and expenditure weights. The single exception is the normalized-stratum scheme, which raises the cut estimate but yields an interval so wide as to be uninformative; this scheme up-weights sparsely sampled cells, disproportionately the lockdown-period observations that the coverage rule otherwise handles, and we therefore do not treat it as the headline. Full pass-through is rejected under every scheme for every event.

Table 6. Pass-through by weighting scheme (item-FE).

Event	Equal quote	95% CI	Raw stratum	Normalised stratum	Expenditure	Reject $\Phi=1$?
Cut (Jul 2020)	0.12	[−0.04, 0.29]	0.10	0.30 †	0.08	Yes
Rise 1 (Oct 2021)	0.28	[0.03, 0.56]	0.27	0.35	0.31	Yes
Rise 2 (Apr 2022)	0.26	[0.10, 0.43]	0.25	0.23	0.38	Yes

Notes: 95% interval shown for the preferred equal-quote scheme. Point estimates; the equal-quote column matches Table 2 and carries its interval. † The normalised-stratum cut estimate is imprecise, with a 95% interval spanning zero, reflecting the up-weighting of sparse lockdown cells.

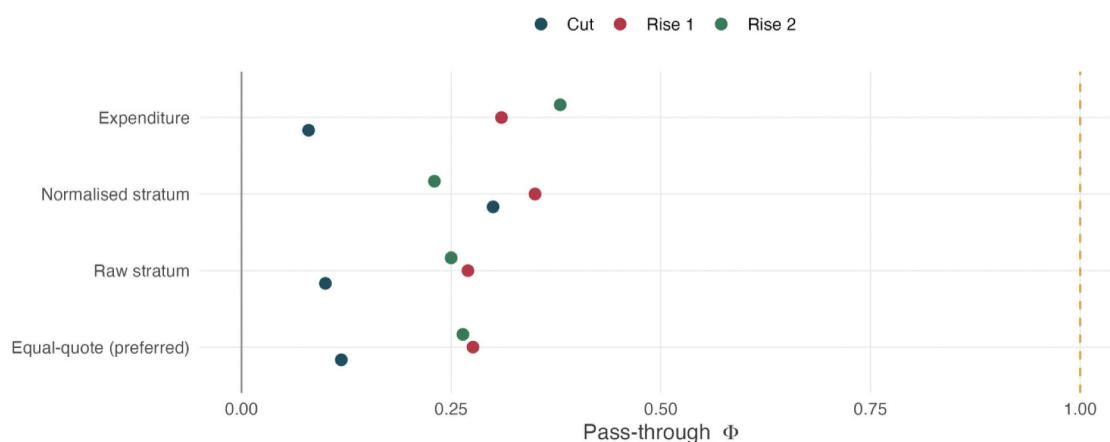


Figure 7. Pass-through is stable across weighting schemes. *Note:* Point estimates by event and weighting scheme; the gold dashed line marks full pass-through. The restoration estimates are relatively stable across weighting schemes, while the cut remains small except under the normalized-stratum scheme, which up-weights sparse lockdown cells.

Figure 7 plots the point estimates across weighting schemes.

4.6.2. The single-quote subsample

Because a collection cell may pool multiple physical price points, a change in a cell's recorded price need not reflect a single outlet's decision. To show that the mechanism in Section 4.5 does not arise from shifts in the number or mix of simultaneously observed quotes, we repeat the analysis on the subsample of matched pairs with a single quote at both endpoints, which comprises the large majority of the data. As Table 7 shows, the repricing shares and typical changes are almost identical between the full sample and the restricted subsample. This does not establish that the same physical outlet is observed throughout, which the released identifiers cannot support, but it removes the composition mechanism most likely to generate spurious price changes.

Figure 8 shows the two samples as paired bars; they are visually indistinguishable across all three quantities.

4.6.3. Alternative controls and further checks

The findings are robust across the remaining checks, reported in full in Appendix C. We re-estimate pass-through under each alternative control group. The cut is small and positive across all three controls (0.12 with on-trade alcohol, 0.16 with off-trade alcohol, and 0.18 with untreated catering), and the two restorations are positive under all controls. The point estimates vary in magnitude, but the cut never turns negative and full pass-through is rejected throughout, so the results do not reverse across controls. We retain on-trade alcohol as the baseline because it is the control that best shares the pandemic demand shock: its collection volume during the first lockdown fell to near zero, as treated catering fell to about 13% of its 2019 level, whereas off-trade retail alcohol held at about 90% and so did not experience the same shock. Table 8 reports pass-through under each control alongside this lockdown-volume diagnostic. Two-way item-and-cell clustering widens the intervals but does not alter the signs or the rejection of full pass-through. Relative-magnitude bounds on the cut do not overturn that

Table 7. Extensive margin: full sample versus single-quote subsample.

Event	P(incr.) full	P(incr.) single	P(decr.) full	P(decr.) single	Median full	Median single
Cut (Jul 2020)	0.24	0.22	0.26	0.22	−0.13	−0.13
Rise 1 (Oct 2021)	0.16	0.15	0.04	0.03	+0.07	+0.07
Rise 2 (Apr 2022)	0.29	0.28	0.03	0.02	+0.07	+0.07

Notes: Single-quote subsample: matched endpoint pairs with exactly one underlying quote at both endpoints (92.1% of pairs). The near-identical values indicate the mechanism is not driven by multi-quote cell composition. Median columns report the median change among price series that moved in the policy-consistent direction (hence the cut median is near the statutory benchmark of −0.1335, not zero); this differs from Table 4, whose P (decrease) is the matched-endpoint share and whose median is taken over all changes. The full-sample probabilities here are computed on one-month changes and therefore differ slightly from the matched-endpoint decomposition shares in Table 4; both are correct for their respective bases.

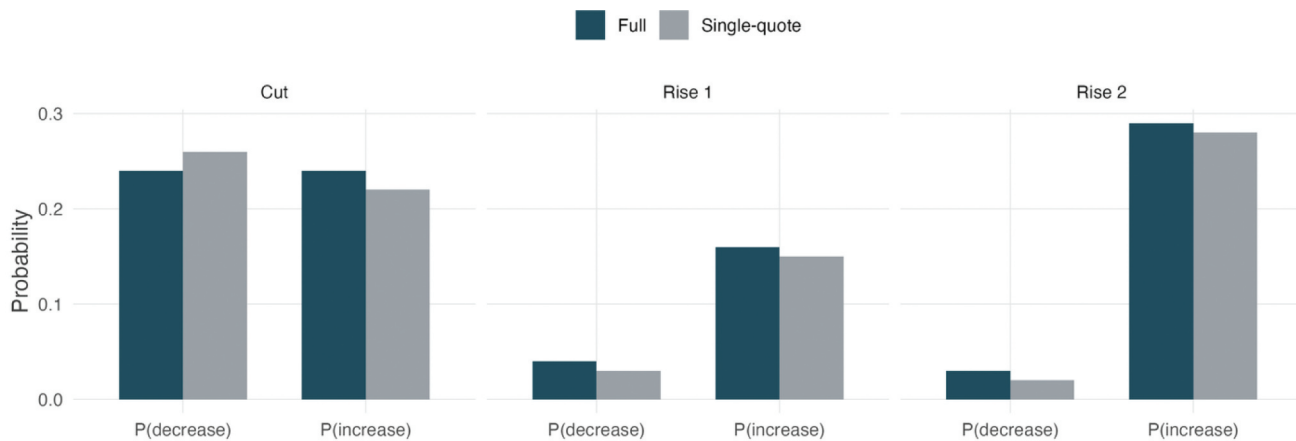


Figure 8. Extensive-margin patterns in the single-quote subsample. *Note:* Full sample versus single-quote subsample for the probabilities of a price increase and a price decrease, by event. The close correspondence suggests that the extensive-margin pattern is not driven by the composition of multi-quote cells.

rejection. A specification that collapses the data to a single observation per collection-cell-item-month, weighting each cell once, yields very similar estimates (cut 0.10, restorations 0.27 and 0.24), indicating that changes in quote composition do not drive the price paths. Excluding sale prices and applying the composite stratum-by-shop weight each leaves the estimates essentially unchanged.

A specific concern is that shocks to the on-trade alcohol control, rather than to treated catering, could move the estimated pass-through. Three candidates arise within our window: the 2021 carbon dioxide shortage, which affected carbonated drinks and draught beer; disruptions to alcohol distribution; and changes to alcohol duty. Each would bias pass-through only if it moved control prices differentially around a VAT event, and two features of the design limit that risk. First, the region-by-month and outlet-type-by-month fixed effects absorb any shock common to a period, so only a shock to on-trade alcohol that occurs precisely on a VAT date and is not captured by these effects could contaminate the estimate. Second, and more

directly, the results do not reverse when the alcohol control is replaced with untreated standard-rated catering, which is not exposed to any alcohol-specific shock: the cut remains small and positive (0.18), and full pass-through is still rejected. An alcohol-specific confounder large enough to drive the findings would have to leave this alcohol-free control unaffected, which we regard as unlikely. Alcohol duty is, moreover, unlikely to be the driver: duty rates were frozen throughout the study window, and the reform of the alcohol-duty system took effect only in August 2023, after the last event window closed.

A related threat arises from the shared-venue design itself. Because food and alcohol are often priced by the same establishment, a firm that cross-subsidizes across its menu could move alcohol (control) prices in response to the food VAT change, violating the stable-unit-treatment assumption and contaminating the control. The sign of any resulting bias is not determined a priori, as it depends on how establishments adjust alcohol prices relative to food prices. We therefore

Table 8. Pass-through by control group, and control validity.

Control group	Cut Φ	Rise 1 Φ	Rise 2 Φ	Lockdown volume ratio	Shares shock?
On-trade alcohol (baseline)	0.119	0.276	0.264	0.00	Yes (preferred)
Off-trade alcohol	0.163	0.476	0.472	0.91	No
Untreated catering	0.180	0.170	0.187	0.25	Partly

Notes: Pass-through for each event on the canonical specification, by control group. The lockdown volume ratio is each group's collection volume during the first lockdown (April to June 2020) relative to its 2019 level; treated catering fell to 0.13. Off-trade alcohol did not share the hospitality demand shock, so on-trade alcohol, which shares that shock, is the preferred control. Under every control, the cut is small and positive, and full pass-through is rejected throughout.

do not rely on a directional argument. Instead, the untreated standard-rated catering control, which shares no menu items with the treated items and is therefore not subject to cross-menu repricing, provides the cleaner check. It yields the same qualitative conclusion, which a cross-subsidization story specific to alcohol cannot explain.

We also address the concern that the results might depend on the choice of control group or trend specification by performing two further checks. The first is a synthetic control that weights the donor groups to match treated catering's pre-2020 path, achieving a flat pre-trend by construction (pre-period slope about 0.0002 per month, $p = 0.73$). Because it does not rely on parallel trends, it is robust to the pre-trend problem that affects the cut. Its pass-through estimates are positive and well below full at every event and are consistent with the main analysis; the cut coincides with the detrended estimate at about 0.27. The bootstrap intervals are wide because the synthetic control relies on a small number of aggregate series, so it corroborates the direction and magnitude of pass-through without sharpening inference (Table C8). We also report the detrended pass-through across the full confidence interval for the drift parameter. The detrended cut ranges from about 0.12 to 0.43, and full pass-through is rejected at every value, so the qualitative conclusion does not depend on the trend adjustment. Table 9 reports this sensitivity.

We further probe the collection-cell limitation using the outlet identifier as a proxy for a continuing outlet. Restricting to outlets observed continuously for at least 48 or 60 months, the closest available approximation to a fixed physical outlet, leaves pass-through essentially unchanged (cut about 0.10 vs 0.12 in the baseline; restorations about 0.28 and 0.24). This suggests that the estimates reflect genuine repricing of continuing

outlets rather than compositional turnover, although the released data do not identify physical venues, a limitation common to all users of the source.

Finally, we provide direct evidence on the timing of repricing to address anticipation. Both restorations were announced in the March 2021 Budget, so anticipation could in principle contaminate the pre-periods. Tracking the monthly frequency of price changes for treated catering, repricing is at ordinary levels around the March 2021 announcement, at about 0.09 to 0.10, but rises sharply at the two implementation dates to 0.19 in October 2021 and 0.30 in April 2022, with moves concentrated in upward directions. Repricing therefore clusters at implementation rather than at announcement, which indicates that the rise estimates reflect implementation rather than anticipation. This pattern of anticipation is consistent with Büttner and Madzharova (2021), who find that preannounced VAT reforms in European micro-data generate temporary shifts in activity concentrated in the month before implementation, with the main price and quantity adjustments occurring at and after implementation rather than at announcement.

5. Implications for consumer prices

The estimates in the preceding sections concern the transmission of each statutory rate change to posted prices. To gauge what the policy meant for consumers in aggregate, we translate them into a contribution to headline inflation. The treated catering basket accounts for 37.1 parts per thousand of the CPI, so a full and immediate pass-through of the 2020 cut would have mechanically subtracted about half a percentage point from the headline CPI at the point of implementation. Our estimates imply a much smaller CPI-level effect.

Table 10 sets out the arithmetic under two weightings. The equal-quote estimates correspond to the paper's preferred average-quote estimand, while the expenditure-weighted estimates are conceptually more appropriate for CPI aggregation, so we report both. Applying the estimated pass-through to the statutory rate change and the treated basket weight, the cut's

Table 9. Detrended pass-through across the drift parameter λ .

λ	Cut Φ	Rise 1 Φ	Rise 2 Φ
0 (no detrend)	0.11	0.28	0.26
0.00002 (CI lower)	0.12	0.28	0.26
0.0010	0.22	0.25	0.20
0.0015 (point est.)	0.27	0.23	0.17
0.00308 (CI upper)	0.44	0.19	0.07

Notes: Detrended pass-through $\Phi = (\theta - \lambda \cdot h) / \pi$ for each event, evaluated across the bootstrap confidence interval for the drift λ . Full pass-through ($\Phi=1$) is rejected at every value. The cut point estimate ranges from about 0.12 to 0.44, which is why we report the detrended cut as a scenario rather than a correction.

Table 10. Contribution of each VAT change to headline CPI (percentage points).

Event	Basket wt (/1,000)	Statutory Δ (log)	Φ (equal-quote)	Implied CPI (eq), pp	Φ (exp-weighted)	Implied CPI (exp), pp	Full-PT counterfactual, pp
Cut (Jul 2020)	37.1	-0.1335	0.119	≈ -0.06	0.08	≈ -0.04	≈ -0.50
Rise 1 (Oct 2021)	37.1	+0.0690	0.276	$\approx +0.07$	0.31	$\approx +0.08$	$\approx +0.26$
Rise 2 (Apr 2022)	37.1	+0.0645	0.264	$\approx +0.06$	0.38	$\approx +0.09$	$\approx +0.24$

Notes: The implied contribution under estimated pass-through applies the estimated pass-through to the statutory change and the treated basket weight; the counterfactual assumes full pass-through ($\Phi=1$). Figures are approximate and illustrative of magnitude; the exercise abstracts from general-equilibrium and second-round effects and from any response of quantities.

implied contribution to headline CPI is about -0.06% points under equal-quote weighting and about -0.04% points under expenditure weighting, compared with a full-pass-through counterfactual of about -0.50% points. Either way, of the half-point of disinflation that complete transmission would have delivered, the great majority did not materialize in posted prices; even under the detrended cut scenario, which raises the estimate to roughly a quarter, the implied contribution would still be only about -0.13% points. The two restorations add measurably more, with implied contributions of about $+0.07$ and $+0.06$ under equal-quote weighting and about $+0.08$ and $+0.09$ under expenditure weighting, since their partial pass-through applied to increases. These are implied CPI-level effects under the estimated pass-through, not observed contributions at implementation; for the cut in particular, the coefficient measures persistent relative-price movement from quotes beginning in

September 2020 rather than an immediate July effect. Because the cut is not cleanly identified, its entry in this arithmetic is illustrative only and carries no evidentiary weight; it is retained solely to convey orders of magnitude. The restorations' entries, which do not share that identification problem, can be read as informative.

Figure 9 compares the implied and full-pass-through contributions.

Two qualifications apply. As an accounting translation, not a welfare calculation, it shows what entered the headline index, not how the unpassed portion was divided between margins, wages, and employment, which our data cannot observe. It also abstracts from any demand response, the channel through which a temporary cut is intended to stimulate demand (Bachmann et al. 2021); if prices barely moved, that channel was muted. The available CPI quotes therefore indicate little persistent consumer-price effect from September 2020 onward.

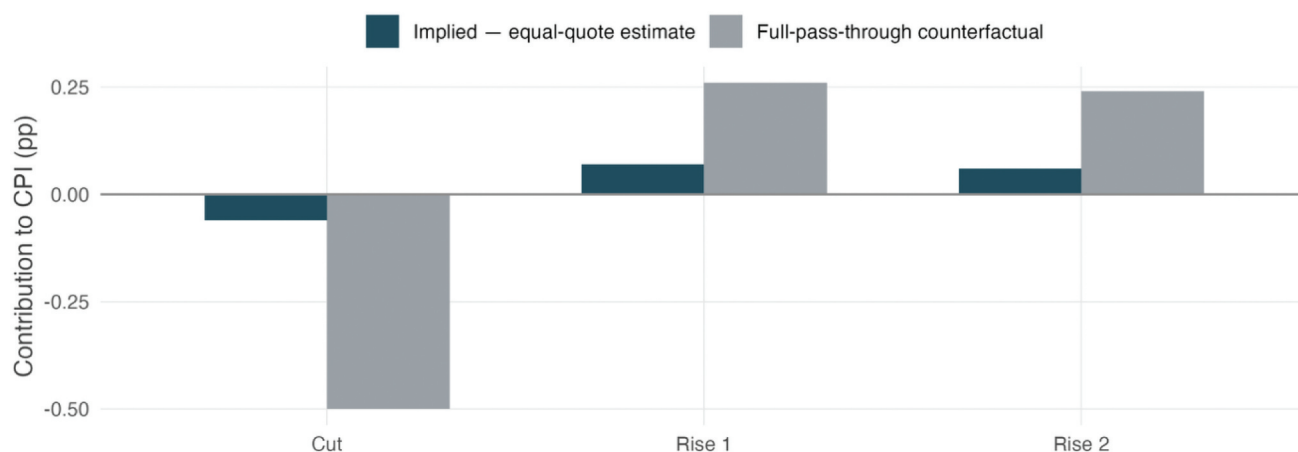


Figure 9. What reached consumers: contribution to headline CPI. Note: Implied CPI contribution using the preferred equal-quote pass-through estimates versus the full-pass-through counterfactual, by event. Expenditure-weighted CPI implications are reported in Table 10. The treated catering basket accounts for 37.1 parts per thousand of the CPI.

6. Discussion and conclusion

This study examines how much of the UK's temporary hospitality VAT changes reached consumers through posted prices, using CPI price-quote microdata, with on-trade alcohol as a within-sector control that was left untouched. Three findings emerge. The 2020 cut passed through only weakly and imprecisely, though full pass-through can be firmly rejected; the two restorations passed through partially but positively; and the apparent asymmetry between them, visible in the raw numbers, does not survive a reasonable adjustment for pre-existing trends. Under these averages lies a story about repricing incidence: when prices did move, they moved close to the mechanical benchmark, but at the cut, the upward and downward revisions cancelled out, so little net change ever reached consumers. Beneath the averages there is also systematic variation across outlets: chain outlets passed through more than independents, significantly so at the October 2021 restoration, consistent with chains repricing more often and facing lower repricing costs. The cut's weak transmission is also economically understandable. In mid-2020 the sector faced an existential demand shock, and retaining the temporary relief to rebuild liquidity is what one would expect if the cut functioned as sectoral support rather than as price relief; on this reading the near-absence of consumer-price pass-through is consistent with, rather than a puzzle for, the policy's design.

These results speak to a literature divided in its findings. The asymmetric, downward-sticky pass-through of Benzarti et al. (2020) and the full, symmetric pass-through that Bernardino et al. (2025) find for Portuguese food describe almost opposite worlds; our evidence falls between them and hints at why both can be true. Bernardino et al. (2025) study frequently reports retail food prices, where price changes are salient and widely noticed during a high-inflation period. By contrast, we study catering, a labour-intensive service in which VAT is folded into menu prices rather than shown separately, and prices change infrequently. In that environment, the pressure to pass a cut through is weaker, and the room to leave it unreflected in prices is larger accordingly, which is consistent

with both our low estimates and the similarly limited transmission found by Benzarti and Carloni (2019) in French restaurants. More broadly, consumption-tax incidence is not a structural constant; it depends on the repricing environment of that sector, and evidence from retail goods does not necessarily extend to services.

Our finding also aligns naturally with the only earlier study of this episode. Onnis et al. (2025), examining hotel accommodation with cross-country controls, likewise found positive but incomplete and discretionary pass-through of the same cut. Working from national microdata and a within-sector control, we reach the same conclusion for a larger share of the relief and extend it to the two restorations and to the mechanism underlying the averages. That two independent designs, in different sub-sectors and using different data and controls, agree that transmission was limited lends added weight to the conclusion.

The policy reading is straightforward. Set aside the cut's emergency purpose, which was to support a sector during lockdown, and judge it purely by its effect on the prices people paid. Little of the 2020 reduction is visible in lower posted prices in the quotes available from September 2020 onwards, and its implied impact on headline inflation was substantially smaller than full transmission would have implied. For the ongoing debate over a permanent reduced rate, whose case rests squarely on consumer prices, that is a cautionary finding. It does not close the question, since a reduced rate might still be defended on other grounds, such as employment, firm survival, or bringing spending forward, but it provides little support for assuming that a reduced rate will translate proportionately into lower consumer prices. These conclusions pertain to a single sector during an exceptional period, so we frame their broader implications as conjectures for future work rather than general claims about reduced VAT rates.

Several limitations bound these conclusions, and each suggests further studies. Identification relies on a modest number of items, which leaves the bootstrap intervals, especially for the cut, relatively wide; the small cut estimate is better read as evidence against full pass-through than as a sharp point estimate. The cut also fell during the most

disrupted stretch the sector has known, and while the within-sector control and the coverage rule are designed to absorb pandemic shocks shared by catering and alcohol, we cannot be certain 2020 affected the two groups identically. Harmonized microdata spanning the several European hospitality and food VAT changes of 2020–23 would ease that worry and, at the same time, allow the central claim advanced here, that the role of the repricing environment, particularly the frequency and direction of price adjustment, can be tested across sectors rather than inferred from one, to be tested across sectors rather than inferred from one. The pre-trends between the treated and control groups are also not flat, which we test formally and report openly. Identification of the cut therefore rests not on parallel trends but on the robustness of the conclusion across control groups and trend assumptions. We regard the cut as consistently small and incomplete rather than as cleanly identified, and we have shown that full pass-through is rejected under every control and every value of the trend adjustment we consider.

Two further limitations concern the data. The released microdata identify only an anonymized collection cell rather than a physical outlet, so the mechanism evidence describes repricing at the cell level; we have shown the pattern survives on single-quote endpoint pairs, but the identifier cannot establish continuity of the underlying outlet. And we observe only posted prices, so we can measure how much of each change reached consumers, but not how the remainder was divided among profits, wages, and employment. Linking price pass-through to firm outcomes, using employer-employee or firm-accounts data, would trace where the unpassed portion of a cut accrues and so convert an incidence estimate into a welfare account, while matching prices to quantities would identify the demand response that is the intended channel of a temporary cut, but that price data alone cannot assess.

The 2020 hospitality VAT cut showed little persistent transmission in the available CPI quotes from September 2020 onward; its later withdrawal was passed on only in part; and the difference between the two, suggestive though it is, fails to be supported. Because a temporary cut can depress pass-through when firms face repricing costs twice, this episode provides only limited

support for claims that a reduced hospitality VAT rate would translate into proportionately lower consumer prices. What actually reaches consumers from a change in consumption tax, this episode suggests, depends less on the statutory rate than on whether and how prices in the affected sector adjust.

Author contributions

CRedit: **Siamand Hesami**: Conceptualization, Data curation, Formal analysis, Investigation, Methodology, Software, Validation, Visualization, Writing – original draft, Writing – review & editing.

Disclosure statement

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Data availability statement

The price-quote microdata are Crown Copyright, obtained from the UK Office for National Statistics and available from it under licence; the authors cannot redistribute them. The analysis code is available from the authors on reasonable request.

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