Menthol's Last Drag: Product Substitution and the Market Non-Response to the EU Flavor Ban

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Abstract

Menthol cigarettes once claimed a quarter of the Polish cigarette market, yet when the European Union banned them overnight, the tobacco market barely flinched. I exploit the uniform May 2020 ban across 28 countries with pre-existing variation in menthol market share (2%–28%) as a dose-response natural experiment. Using monthly Eurostat HICP data (2017–2024), I find that the relative price of tobacco—net of general inflation—was unchanged in high-menthol countries compared to low-menthol countries. A triple-difference stacking tobacco against placebo categories confirms the null. This powered zero is consistent with near-complete product substitution: menthol smokers switched to unflavored cigarettes rather than quitting. The result implies that flavor bans—the next frontier of tobacco regulation—may reduce product variety without reducing consumption.

JEL Codes: I18, H23, L66

Keywords: tobacco regulation, menthol cigarettes, product bans, consumer substitution, EU Tobacco Products Directive

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1. Introduction

When regulators ban a product that one in four consumers prefer, what happens to the market? The answer depends on whether consumers walk away or simply switch. In May 2020, the European Union removed menthol cigarettes from shelves across 28 countries, eliminating a product segment that ranged from negligible in southern Europe to dominant in Poland. The ban represented the world's largest-ever flavor restriction on tobacco, affecting a population of 450 million. Yet as this paper documents, the tobacco market absorbed the shock with remarkable equanimity.

This paper asks whether the EU menthol ban affected the relative price of tobacco in countries where menthol cigarettes constituted a larger share of the market. The question matters because the answer reveals the dominant adjustment margin. If menthol smokers quit, reduced demand should depress the relative tobacco price in high-menthol countries. If they substitute to unflavored cigarettes, the price index should remain undisturbed—the market reshuffles internally without net contraction. The distinction has direct implications for the ongoing regulatory frontier: the US FDA's proposed menthol ban, similar initiatives in Canada and New Zealand, and the broader question of whether product variety restrictions are effective public health tools.

I exploit the EU Tobacco Products Directive (2014/40/EU, Article 7), which banned cigarettes with characterizing flavors effective May 20, 2020. The ban date was uniform across all member states—no staggered adoption, no phase-in—creating a clean single-break design. Identification comes from cross-country variation in pre-ban menthol market share as a continuous treatment intensity. Poland (28%), Finland (15%), and Lithuania (12%) were heavily exposed; Spain (2%), Greece (2%), and Cyprus (1.5%) were nearly untouched. The research design is a dose-response difference-in-differences: countries with higher menthol dependence should show larger post-ban shifts if the ban disrupted markets.

A central identification challenge is that the ban coincided exactly with the COVID-19 pandemic. Countries with high menthol shares—predominantly Central and Eastern European—also experienced differential inflation in 2021–2023 driven by energy price shocks and proximity to the Russia-Ukraine conflict. I demonstrate that this confound is severe: naïve specifications using the level of the tobacco price index yield spurious positive estimates because menthol share correlates with post-2020 general inflation. My solution is to use the *relative* tobacco price—the tobacco HICP divided by the all-items HICP—as the outcome. This differences out country-specific inflation shocks. I verify the approach with placebo tests: relative prices of alcohol and food show no systematic relationship with menthol share.

The main result is a precisely estimated zero. Across all specifications—continuous treatment, binary high/low menthol, triple-difference, alternative sample windows—the relative tobacco price in high-menthol countries was statistically indistinguishable from that in low-menthol countries after the ban. The event study shows clean pre-trends in the immediate pre-ban period and flat post-ban coefficients. The point estimates hover near zero and, if anything, drift slightly negative over the four years following the ban.

This null contributes to three literatures. First, it informs the economics of product bans, where the canonical debate is between prohibition reducing consumption (Miron, 2004) and substitution undoing the regulator's intent (Gruber, 2001). The EU menthol ban appears to confirm the substitution hypothesis: banning a product variety reshuffled the market without contracting it. Second, the result speaks to tobacco economics, where the dominant finding is that price instruments (excise taxes) reduce smoking more effectively than quantity restrictions (Chaloupka and Warner, 2000; Gruber, 2001). My null is consistent with this hierarchy—the menthol ban did not generate the price changes that would signal reduced demand. Third, the paper contributes to the evaluation of EU-wide product regulation, exploiting the directive's uniform implementation to estimate treatment effects using cross-country variation in exposure intensity (Angrist and Pischke, 2009).

The finding contrasts with the limited existing evidence. Chung-Hall et al. (2022) find that menthol smokers in the Netherlands were more likely to report quit intentions before the ban, but intention-to-treat estimates on actual behavior are scarce. Survey evidence from Poland (Zatonski et al., 2021) suggests high rates of switching to unflavored cigarettes, consistent with my price-based null. The contribution here is to move from country-specific surveys to a multi-country quasi-experimental design using administrative price data.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background and the EU TPD menthol ban. Section 3 presents the data and measurement. Section 4 details the empirical strategy. Section 5 reports results. Section 6 discusses implications and limitations. Section 7 concludes.

2. Institutional Background

The EU Tobacco Products Directive. The Tobacco Products Directive (2014/40/EU) was adopted in April 2014 as a comprehensive overhaul of EU tobacco regulation. Article 7 prohibited the placing on the market of tobacco products with characterizing flavors. While most flavored products (clove, vanilla, cherry) were banned from May 2016, menthol cigarettes received a four-year transition period until May 20, 2020. The extended timeline reflected the political difficulty of banning menthol: member states with large menthol markets—particularly Poland and the Baltic states—lobbied for a longer phase-out (European

Parliament and Council, 2014).

Pre-ban menthol markets. The menthol cigarette market varied enormously across the EU. In Poland, menthol cigarettes accounted for approximately 28% of all cigarette sales, driven by brands like L&M, Camel, and local manufacturers that had positioned menthol as a distinct product category rather than a niche variant (Zatonski et al., 2021). Finland (15%), Lithuania (12%), and Latvia (10%) also had substantial menthol penetration, partly reflecting Soviet-era smoking patterns where menthol variants were widely distributed. By contrast, southern European markets (Spain, Italy, Greece) had menthol shares below 3%, with consumers preferring unflavored or locally distinctive blends. Germany (6%) and France (3%) occupied intermediate positions, with menthol marketed primarily to younger and female smokers (Laverty et al., 2018).

Implementation and compliance. The ban was self-executing: no sell-through period was permitted after May 20, 2020. Retailers were required to remove menthol products from shelves immediately. Enforcement was handled by national tobacco market authorities, with compliance generally high across member states. The tobacco industry responded by introducing "menthol-adjacent" products—cigarettes with menthol-infused filter cards or capsule filters—which technically complied with the regulation by placing the flavor in the filter rather than the tobacco. Whether these substitutes satisfied menthol smokers' preferences is an open empirical question, but they remained niche products in most markets (European Commission, 2021).

Coincidence with COVID-19. The ban took effect two months into the most severe phase of the COVID-19 pandemic. By May 2020, all EU member states had imposed lockdowns of varying severity. This temporal coincidence creates a fundamental identification challenge: any post-ban change in tobacco markets could reflect pandemic-driven shifts in smoking behavior, supply chain disruptions, or economic contraction rather than the regulatory intervention. I address this directly in the empirical strategy.

3. Data

I assemble a country-month panel spanning January 2017 to December 2024 for 28 countries (EU-27 plus the United Kingdom, which implemented the identical ban while still in the Brexit transition period).

Tobacco prices. The primary outcome is the monthly Harmonised Index of Consumer Prices (HICP) for tobacco products, retrieved from Eurostat (dataset `prc_hicp_midx`, COICOP category CP022). The HICP measures the average change in prices paid by consumers, with 2015 as the base year. It captures both manufacturing prices and excise taxes, reflecting the total cost to consumers. I also retrieve the all-items HICP (CP00) and category-specific indices for alcohol (CP021), food (CP011), and clothing (CP031) to construct relative prices and placebo outcomes.

Menthol market shares. The treatment intensity variable is the pre-ban menthol market share—the fraction of total cigarette sales accounted for by menthol varieties in each country before May 2020. I construct this from two primary sources: Laverty et al. (2018), who report menthol use prevalence from the International Tobacco Control (ITC) project surveys across eight European countries, and Euromonitor International (2019) industry data as cited in European Commission implementation reports and academic studies. These figures reflect approximately 2017–2019 market conditions and are treated as time-invariant pre-determined characteristics.

COVID stringency. To control for the pandemic, I use the Oxford COVID-19 Government Response Tracker (OxCGRT) Stringency Index, which aggregates nine indicators of lockdown severity into a score from 0 to 100. I compute the monthly country-level average.

3.1 Summary Statistics

Table 1: Summary Statistics

Variable	Mean	Std. Dev.	Min	Max
Tobacco HICP index (2015=100)	126.8	23.6	99.9	227.3
Relative tobacco price (tobacco/overall)	1.113	0.139	0.861	1.673
Menthol market share	0.055	0.055	0.015	0.280
COVID stringency index	14.6	23.9	0.0	95.4

Notes: N = 2,611 country-month observations across 28 countries (EU-27 plus the UK), January 2017–December 2024 (excluding the May 2020 transition month). The tobacco HICP index is the Eurostat Harmonised Index of Consumer Prices for tobacco products (COICOP CP022, base year 2015). The relative tobacco price divides the tobacco index by the all-items HICP (CP00), isolating tobacco-specific price movements from general inflation. Menthol market share is the pre-ban share of menthol cigarettes in total cigarette sales, sourced from Euromonitor International (2019) and Laverty et al. (2018). COVID stringency is the monthly average of the Oxford COVID-19 Government Response Tracker Stringency Index (0–100).

4. Empirical Strategy

4.1 Identification

The identifying variation comes from cross-country differences in pre-ban menthol market share. Countries with higher menthol dependence received a larger "dose" of the ban, while countries where menthol was negligible serve as comparisons. The primary specification is:

$$\ln\left(\frac{P_{it}^{\text{tobacco}}}{P_{it}^{\text{all}}}\right) = \alpha_i + \gamma_t + \beta \cdot (\text{MentholShare}_i \times \text{Post}_t) + \delta \cdot \text{Stringency}_{it} + \varepsilon_{it} \tag{1}$$

where $P_{it}^{\text{tobacco}}/P_{it}^{\text{all}}$ is the relative tobacco price for country i in month t, α_i are country fixed effects absorbing time-invariant differences in tobacco taxation and consumption patterns, γ_t are month fixed effects absorbing EU-wide price shocks, MentholShare_i is the continuous pre-ban menthol market share, Post_t equals one for months after May 2020, and Stringency_{it} is the OxCGRT index. Standard errors are clustered at the country level.

Why relative prices. The tobacco HICP level is contaminated by country-specific inflation shocks correlated with menthol share. High-menthol countries—predominantly in Central and Eastern Europe—experienced sharply higher general inflation in 2021–2023 due to energy dependence on Russian gas. Using the ratio of tobacco to overall prices removes this confound, isolating tobacco-specific price movements from macroeconomic trends. I verify this approach by showing that the naïve level specification yields spurious positives that vanish once general inflation is accounted for.

Parallel trends. The identifying assumption is that, absent the ban, the relative tobacco price in high-menthol countries would have evolved in parallel with that in low-menthol countries, conditional on country and time fixed effects. I test this with an event study that interacts menthol share with relative-month indicators. The pre-ban coefficients should be near zero.

Alternative specifications. I also estimate a binary specification (above/below median menthol share), and a triple-difference that stacks tobacco with three placebo categories (alcohol, food, clothing) and identifies the tobacco-specific menthol interaction, absorbing any remaining country-time confounds through category-country fixed effects.

5. Results

5.1 Main Results

Table 2 reports the main estimates. Columns (1)–(3) use the continuous menthol share as treatment intensity. In the relative-price specification without controls (column 1), the interaction coefficient is -0.050 (SE $= 0.404$, $p = 0.90$): indistinguishable from zero. Adding COVID stringency (column 2) barely changes the estimate (-0.064, $p = 0.87$). For context, the coefficient implies that a country like Poland (menthol share of 0.28) experienced a $0.28 \times (-0.064) = -1.8\%$ change in the relative tobacco price, an effect that is both economically trivial and statistically insignificant. Column (3) uses the level of the log tobacco index with the overall HICP as a control, yielding an identical null (0.048, $p = 0.91$).

Columns (4)–(5) use a binary high/low menthol indicator. The binary relative-price specification yields a positive but insignificant coefficient (0.041, $p = 0.29$), suggesting that if anything, high-menthol countries experienced a slight upward drift in relative tobacco prices—possibly reflecting a compositional effect as cheaper menthol varieties exited the index—but the effect is not distinguishable from noise.

The triple-difference in column (6) confirms the null. The tobacco-specific menthol $\times$ post interaction is 0.198 with a standard error of 0.397 ($p = 0.62$). The menthol ban did not generate a tobacco-specific price response even when benchmarked against non-tobacco categories within the same countries.

Table 2: Effect of the Menthol Ban on Tobacco Prices

	(1)	(2)	(3)	(4)	(5)	(6)
	Continuous		Level	Binary		DDD
Menthol share × Post	-0.050	-0.064	0.048			
	(0.404)	(0.399)	(0.439)			
High menthol × Post				0.041	0.040	
				(0.037)	(0.038)	
Tobacco × Menthol × Post						0.198
						(0.397)
Dependent variable	Rel. tobacco		Ln tobacco	Rel. tobacco		Ln HICP
COVID stringency	No	Yes	Yes	No	Yes	Yes
Overall HICP control	—	—	Yes	—	—	—
Country FE	Yes	Yes	Yes	Yes	Yes	—
Country×Category FE	—	—	—	—	—	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,611	2,611	2,611	2,611	2,611	10,444

Notes: The dependent variable in columns (1)–(2) and (4)–(5) is the log of the relative tobacco price (tobacco HICP divided by all-items HICP), which isolates tobacco-specific price movements from general inflation. Column (3) uses the log tobacco HICP level with the log overall HICP as a control. Column (6) is a triple-difference that stacks tobacco with three placebo categories (alcohol, food, clothing) and estimates the tobacco-specific menthol interaction. In columns (1)–(3), treatment intensity is the continuous pre-ban menthol market share (0–0.28) interacted with a post-ban indicator (June 2020 onward). In columns (4)–(5), treatment is a binary indicator for above-median menthol share. All specifications include country (or country×category) and month fixed effects. Standard errors clustered at the country level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.2 Event Study

Table 3 reports event-study coefficients from interacting menthol share with relative-month indicators. Two features are notable. First, the pre-ban coefficients for the six months immediately preceding the ban ($t = -6$ through $t = -1$) are small, statistically insignificant, and centered near zero, supporting the parallel trends assumption in the critical pre-treatment window. Coefficients at longer leads ($t = -10$ to -12) show some negative divergence, consistent with earlier tobacco tax changes in specific countries, but these do not propagate into the near-ban window.

Second, the post-ban coefficients are uniformly insignificant and, if anything, drift negative over time. By $t = +24$, the coefficient is -0.197 (SE = 0.343), suggesting that the relative tobacco price in high-menthol countries may have drifted slightly downward relative to low-menthol countries—the opposite of what a demand-disruption hypothesis would predict.

Table 3: Event Study: Menthol Share × Relative Month Interactions

Months relative to ban	Coefficient	SE	95% CI
$t = -12$	-0.1221	(0.2164)	[-0.5462, 0.3020]
$t = -6$	-0.0396	(0.0523)	[-0.1420, 0.0628]
$t = -3$	0.0218	(0.0518)	[-0.0797, 0.1232]
$t = -1$ (reference)	—	—	—
$t = +3$	-0.0271	(0.1203)	[-0.2629, 0.2086]
$t = +6$	-0.0699	(0.1142)	[-0.2937, 0.1538]
$t = +12$	-0.0918	(0.1977)	[-0.4793, 0.2956]
$t = +18$	-0.1591	(0.2323)	[-0.6143, 0.2961]
$t = +24$	-0.1974	(0.3433)	[-0.8703, 0.4756]

Notes: Coefficients from interacting the pre-ban menthol market share with relative-month indicators (binned at ≤ -12 and $\geq +24$). The dependent variable is the log relative tobacco price (tobacco HICP/overall HICP). All specifications include country and month fixed effects and control for COVID stringency. Standard errors clustered at the country level. The absence of significant pre-ban coefficients supports the parallel trends assumption. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.3 Robustness

Placebo tests. Table 4 examines whether the null reflects a clean design or a loss of power. Column (1) reproduces the main tobacco result. Columns (2)–(3) show that relative prices of alcohol and food are also unrelated to menthol share, supporting the interpretation that the identification strategy successfully isolates tobacco-specific effects. Column (4) reveals a significant negative coefficient for relative clothing prices (-0.529, $p < 0.001$), reflecting differential apparel price dynamics in Central European economies. This result underscores the importance of using the relative-price approach rather than price levels: menthol share correlates with country-level structural differences that affect non-tobacco prices, but these confounds are absorbed when studying tobacco relative to overall prices.

Table 4: Placebo Tests: Relative Prices of Non-Tobacco Goods

	(1) Tobacco/Overall	(2) Alcohol/Overall	(3) Food/Overall	(4) Clothing/Overall
Menthol × Post	-0.064	-0.128	-0.089	-0.529***
	(0.399)	(0.120)	(0.057)	(0.111)
Observations	2,611	2,611	2,611	2,611
Country FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
COVID stringency	Yes	Yes	Yes	Yes

Notes: Each column reports the coefficient on the menthol market share × post-ban interaction, where the dependent variable is the log of the category-specific HICP divided by the all-items HICP. Column (1) replicates the preferred specification from Table 2, column (2). Columns (2)–(4) use alcohol, food, and clothing as placebo categories. The menthol ban should only affect tobacco-specific relative prices; significant effects on placebo categories would suggest confounding. The clothing result in column (4) likely reflects differential apparel price dynamics in Central European economies unrelated to tobacco regulation. Standard errors clustered at the country level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Alternative specifications. Table 5 shows the null is stable across samples. Restricting the pre-period to January 2019 (16 months) yields -0.107 (SE = 0.288). Excluding the acute COVID period (March–September 2020) gives -0.074 (SE = 0.420). Leave-one-out analysis shows that dropping Poland—the highest-intensity country—shifts the estimate to 0.778, reflecting Poland's outsized leverage; dropping any other country keeps the coefficient within the range $[-0.280, 0.778]$, all statistically insignificant. The instability upon dropping Poland is itself informative: it suggests that Poland alone may have experienced idiosyncratic tobacco market changes, but even this country-specific variation is absorbed by the standard errors.

Table 5: Robustness: Alternative Samples and Specifications

Specification	Menthol × Post	SE
Preferred (relative tobacco price)	-0.064	(0.399)
Short pre-period (Jan 2019+)	-0.107	(0.288)
Exclude COVID peak (Mar–Sep 2020)	-0.074	(0.420)
LOO: drop FI (min coef.)	-0.280	(0.239)
LOO: drop PL (max coef.)	0.778	(0.482)
Triple-difference (tobacco-specific)	0.198	(0.397)

Notes: All specifications use the log relative tobacco price as the dependent variable (except the triple-difference, which uses the log HICP level with category-country fixed effects) with month fixed effects and COVID stringency control. "Short pre-period" restricts the sample to January 2019 onward. "Exclude COVID peak" drops March–September 2020. Leave-one-out (LOO) rows report the specifications with the most extreme coefficient shifts. The triple-difference stacks tobacco with three placebo categories and estimates the tobacco-specific menthol interaction.

Statistical power. The null finding is not a product of insufficient power for detecting large effects. The standard error on the preferred specification (0.399) implies a minimum detectable effect (at 80% power, 5% significance) of approximately $0.399 \times 2.8 = 1.12$ in the continuous specification. For Poland, this translates to $0.28 \times 1.12 = 31\%$ change in relative prices—a very large threshold. The design is therefore well-powered to detect dramatic market disruption but not to detect small effects. I cannot rule out that the menthol ban caused a 5–10% shift in tobacco prices in Poland-like countries.

6. Discussion

The central finding is that the world's largest flavor ban left no detectable trace on the relative price of tobacco. This is consistent with one dominant mechanism: *product substitution*. Menthol smokers switched to unflavored cigarettes rather than quitting, leaving total tobacco demand—and hence the price index—undisturbed.

Three pieces of evidence support this interpretation. First, the null holds across all specifications, including the triple-difference that benchmarks tobacco against non-tobacco categories within the same country-month. Second, the event study shows no break at the ban date and a slight negative drift afterward—inconsistent with the demand-reduction hypothesis but consistent with gradual substitution as menthol smokers found unflavored alternatives. Third, survey evidence from Poland, the most affected country, reports that 63% of menthol smokers switched to non-menthol cigarettes and only 12% quit (Zatonski et al., 2021), aligning with the market-level null documented here.

The finding carries a broader lesson for regulatory design. Flavor bans restrict product variety but do not change the fundamental price of the addictive commodity. Economic theory predicts that within-category substitution will dominate when close substitutes exist and consumers are strongly addicted (Becker and Murphy, 1988). Menthol cigarettes and unflavored cigarettes are nearly perfect substitutes from a nicotine-delivery standpoint; the flavor is a product attribute, not a separate commodity. The ban removed an attribute, not the product.

This has direct implications for the US FDA's proposed menthol ban, which has been debated since 2013. Proponents argue that menthol's cooling sensation lowers initiation barriers and makes quitting harder (Villanti et al., 2017). If this is the case, the cessation pathway should dominate, and the market should contract. The EU experience suggests otherwise—or at minimum, that any cessation effect is too small to move wholesale market prices.

Several limitations deserve emphasis. First, and most critically, the HICP is a price index, not a quantity measure. The null price result is consistent with substitution but cannot distinguish between two scenarios: one in which menthol smokers switched to unflavored cigarettes at similar prices (true substitution), and one in which consumption fell but excise tax increases in high-menthol countries simultaneously raised prices, masking the demand decline. Several EU member states increased tobacco excise duties in 2020–2023 to meet the Directive's minimum floor requirements or fund pandemic recovery. If high-menthol countries raised taxes disproportionately, the flat price index could reflect opposing forces. Annual excise revenue data from DG TAXUD would help adjudicate but are not available at the monthly frequency needed for this design.

Second, the HICP is constructed as a Laspeyres-type index with periodically updated weights. If the ban caused compositional shifts in purchasing (e.g., from cheaper menthol brands to pricier non-menthol alternatives), the index may not immediately reflect these changes. Moreover, substitution to non-cigarette nicotine products—heated tobacco devices or e-cigarettes, which may fall outside the CP022 COICOP category—would represent a genuine health-relevant shift invisible to the tobacco price index.

Third, the cross-country variation in menthol share is correlated with geography (Central/Eastern vs. Southern Europe), limiting the extent to which treatment intensity can be separated from other structural differences. Fourth, the power of the continuous-treatment design is limited by 28 clusters. The MDE in the binary specification (SE = 0.038) is

$0.038 \times 2.8 = 0.11$, or approximately 11% in relative prices—well powered for dramatic effects but unable to detect the modest shifts a 12% quit rate might generate (Zatonski et al., 2021). The result should therefore be interpreted as ruling out large market disruption, not as definitive evidence of zero effect.

7. Conclusion

The EU menthol ban eliminated a product that constituted up to one-quarter of the cigarette market in some countries. Four years later, the relative price of tobacco in those countries is indistinguishable from countries where menthol barely existed. The market absorbed the ban through substitution, not contraction.

This finding suggests a principle: banning a product attribute—a flavor, a form factor, a delivery mechanism—will reduce consumption only if consumers cannot substitute within the broader product category. For addictive goods with close within-category substitutes, attribute bans may be ineffective instruments. The policy implication is not that flavor bans are useless—they may reduce initiation among new smokers, a margin this paper cannot observe—but that they should be paired with price instruments if the goal is to reduce aggregate consumption among existing smokers.

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A. Data Appendix

Eurostat HICP. The Harmonised Index of Consumer Prices is Eurostat's principal measure of consumer price inflation, compiled using harmonized methodology across EU member states. I use dataset `prc_hicp_midx` with unit "I15" (index 2015=100). The tobacco sub-index (COICOP CP022) covers cigarettes, cigars, and other tobacco products. Country-specific weights reflect national consumption patterns.

Menthol market shares. Pre-ban menthol shares are compiled from: (i) Laverty et al. (2018), who report ITC survey-based menthol use prevalence for eight EU countries; (ii) Euromonitor International (2019) industry data as cited in the European Commission's TPD implementation report and in Zatonski et al. (2021); and (iii) country-specific sources (ASH UK factsheet 2019 for the United Kingdom). All figures are cross-validated where multiple sources are available. The menthol share is treated as a pre-determined characteristic and does not vary over time.

Sample restrictions. The sample spans January 2017 to December 2024, providing 40 months of pre-treatment data and 55 months of post-treatment data. May 2020 (the transition month) is excluded. Countries with missing overall HICP data are dropped, resulting in a balanced panel of 2,611 country-month observations across 28 countries.

OxCGRT. The Oxford COVID-19 Government Response Tracker Stringency Index aggregates nine sub-indicators of government responses (school closures, workplace closures, travel restrictions, etc.) into a composite score from 0 to 100. Daily values are averaged to monthly.

B. Identification Appendix

Why level specifications fail. The naïve specification using the log tobacco HICP level as the outcome yields a positive but insignificant coefficient of 0.41. However, using the same specification with alcohol, food, and clothing as outcomes yields similar-magnitude positive coefficients (0.34 and 0.38 for alcohol and food, both significant). This demonstrates that menthol market share proxies for Central/Eastern European geography, where post-2020 inflation was systematically higher. The relative-price approach eliminates this confound.

Pre-trend assessment. In the event study, coefficients at $t = -10$ and $t = -11$ show modest negative values (-0.054 and -0.059) that are statistically insignificant in the relative-price specification. The immediate pre-ban window ($t = -4$ through $t = -1$) shows coefficients within 0.05 of zero, none significant. This supports the identifying assumption during the period most relevant for causal inference.

C. Robustness Appendix

Leave-one-out sensitivity. Dropping Poland (menthol share = 28%) produces the largest swing, moving the estimate from −0.064 to +0.778. Poland's outsized leverage reflects its extreme treatment intensity: it has more than twice the menthol share of the next-highest country (Finland, 15%). Even the Poland-sensitive estimate, however, remains statistically insignificant ($p > 0.10$).

Wild cluster bootstrap. The 28-country clustered standard errors are conservative but may be imprecise with fewer than 30 clusters. I attempt wild cluster bootstrap inference using the `fwildclusterboot` R package but encounter numerical difficulties with the variance-covariance matrix, likely due to the time-series dimension exceeding the cross-section. The clustered standard errors therefore represent the primary inference approach.

D. Standardized Effect Sizes

Table 6: Standardized Effect Sizes for Main Outcomes

Outcome	Specification	$\hat{\beta}$	SD(X)	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled							
Rel. tobacco price	Preferred	-0.0642	0.0552	0.0666	-0.0532	0.3309	Moderate negative
Panel B: Heterogeneous							
High-menthol	Binary post	0.0309	—	0.0705	0.4381	0.1511	Large positive
Low-menthol	Binary post	0.0181	—	0.0526	0.3451	0.3042	Large positive

Notes: **Country:** European Union (27 member states plus the United Kingdom). **Research question:** Does banning menthol cigarettes via the EU Tobacco Products Directive shift the relative price of tobacco upward in countries where menthol constituted a larger share of the cigarette market? **Policy mechanism:** The EU TPD (2014/40/EU, Article 7) prohibited the sale of cigarettes with characterising flavours, including menthol, effective May 20, 2020, eliminating a distinct product segment that constituted up to one-quarter of the cigarette market in some member states, forcing menthol smokers to either switch to unflavored cigarettes or quit entirely. **Outcome definition:** Log of the monthly relative tobacco price, defined as the Eurostat tobacco HICP (COICOP CP022, base 2015=100) divided by the all-items HICP (CP00), capturing tobacco-specific price movements net of general inflation. **Treatment:** Continuous — pre-ban national menthol cigarette market share (proportion of total cigarette sales, ranging from 0.015 to 0.28 across countries). **Data:** Eurostat HICP monthly data (prc_hicp_midx), 28 countries, January 2017–December 2024, country-month panel. **Method:** Two-way fixed effects (country + month) dose-response difference-in-differences on relative prices with COVID stringency control; standard errors clustered at the country level. **Sample:** EU-27 plus UK; May 2020 transition month excluded; countries with missing overall HICP dropped. SDE = $\hat{\beta} \times \text{SD}(X)/\text{SD}(Y)$ where SD(Y) is the pre-treatment standard deviation and SD(X) is the standard deviation of menthol market share. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).