Toulouse School of Economics

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Applied Econometrics Project

The Causal Effect of Plain Packaging on Cigarette Consumption and Smoking-Related Outcomes

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Introduction

0.1 Context and Motivation

In France, tobacco use still causes around 75,000 preventable deaths each year (*Santé Publique France*). To address this public health challenge, the French government introduced mandatory plain packaging for all tobacco products in 2017. The aim was to reduce the marketing appeal of cigarettes, neutralize brand identity, and put health warnings at the forefront.

This policy was part of the broader *Programme National de Réduction du Tabagisme* launched in 2014, which combined price increases, awareness campaigns with graphic warnings, and support for smokers trying to quit. The 2017 reform thus offers enough post-treatment observations to go beyond perceptions and measure actual behavioral responses.

0.2 Literature Review and the French Gap

Since the foundational work of Chaloupka and Warner (1999), health economists have widely recognized that tobacco consumption is highly responsive to policy interventions, particularly price elasticity and comprehensive advertising bans. Building on this established framework, recent literature has shifted focus to the ultimate form of marketing restriction: plain packaging. Most research on plain packaging focuses on early adopters, such as Australia, the United Kingdom, and Ireland. Studies generally show three main effects: a decrease in the visual appeal of packs, a modest decline in consumption, and short-term spikes in quit attempts immediately after the reform (Wakefield et al, 2013).

However, isolating the effect of packaging from other simultaneous measures—such as national campaigns or the tax increases highlighted by Chaloupka and Warner as the primary driver of smoking cessation—is highly challenging. In France, this issue is particularly noticeable: most available evidence relies on self-reported surveys, which capture perceptions rather than actual behavior. As a result, there is a lack of empirical studies measuring the concrete impact of the 2017 reform on cigarette sales or health outcomes.

0.3 Research Question and Contribution

This study aims to fill that gap. Our analysis of this reform is structured around a core research question : Did plain packaging reduce cigarette consumption in France, and did it modify health outcomes or cessation behavior?

Our project focuses on three main dimensions: first, its impact on cigarette consumption, second, its health outcomes, and third, its influence on the use of cessation aids.

1 Cigarette Consumption

1.1 Empirical Strategy

We estimate the causal effect of the 2017 French plain packaging reform on cigarette consumption using a Two-Way Fixed Effects (TWFE) Difference-in-Differences estimator on a panel of 13 European countries over 2010–2019:

$$\boxed{\ln(\text{Sales}_{it}) = \alpha + \beta \cdot \text{Treatment}_{it} + \gamma X_{it} + \mu_i + \lambda_t + \varepsilon_{it}}$$

where $\text{Treatment}_{it} = 1$ for France in 2017, μ_i and λ_t are country and year fixed effects, and standard errors are clustered at the country level. The 12 control countries were selected as EU members that did not implement plain packaging over 2010–2019, ensuring a clean counterfactual. Three control variables are included: the unemployment rate, the Tobacco Price Index and the average cigarette price. The parallel trends assumption is supported both graphically (Figure 1) and by a Wald test on pre-treatment France×Year interactions ($\chi^2 = 1.186$, $p = 0.325$).

1.2 Results

1.2.1 Baseline TWFE

The treatment coefficient is negative and significant at the 5% level ($\hat{\beta} \approx -0.0376$, $p = 0.045$), implying that plain packaging reduced cigarette sales per capita in France by approximately $100(e^{-0.0376} - 1) = -3.7\%$ relative to the control group, consistent with evidence from Australia where a comparable reform reduced monthly sales by around 7.5% (Bonfrer et al., 2020).

The unemployment rate is the only control significant at conventional levels ($p < 0.01$). Full regression results are reported in Table 1.

1.2.2 Dynamic Treatment Effects

To allow the treatment effect to vary by post-reform year, we estimate:

$$\ln(\text{Sales}_{it}) = \alpha + \beta_{17} \cdot \mathbf{1}[\text{France}_i \times t \geq 2017] + \beta_{18} \cdot \mathbf{1}[\text{France}_i \times t \geq 2018] + \beta_{19} \cdot \mathbf{1}[\text{France}_i \times t \geq 2019] + \gamma X_{it} + \mu_i + \lambda_t + \varepsilon_{it}$$

The effect is negligible in 2017 ($\hat{\beta}_{17} = 0.022$, $p = 0.437$), turns negative and significant in 2018 ($\hat{\beta}_{18} = -0.053$, $p = 0.002$, $\approx -5.1\%$), and intensifies in 2019 ($\hat{\beta}_{19} = -0.079$, $p < 0.001$, $\approx -7.6\%$). This gradual pattern relies mainly on two mechanisms. Plain packaging reduces the perceived appeal of cigarettes and strengthens quit intentions through repeated exposure (Wakefield et al., 2013). Moreover, because cigarettes are addictive goods, a reduction in current consumption lowers the stock of addiction capital and thus the marginal utility of future consumption, amplifying the initial demand shock over time (Becker & Murphy, 1988). Full results are reported in Table 2.

1.3 Robustness Checks

Event study. To further validate the parallel trends assumption and trace year-by-year dynamics, we estimate:

$$\ln(\text{Sales}_{it}) = \alpha + \sum_{\substack{k=-7 \\ k \neq -1}}^{2} \delta_k \cdot \mathbf{1}[t - 2017 = k] \cdot \text{France}_i + \gamma X_{it} + \mu_i + \lambda_t + \varepsilon_{it}$$

where $k = -1$ (2016) is the omitted reference year. Pre-treatment coefficients ($k \in \{-6, \ldots, -2\}$) are all small and non-significant, supporting the parallel trends assumption. Post-reform, the effect reaches -10.7% by $k = 2$ ($\hat{\delta}_2 = -0.113$, $p < 0.01$). The event-study plot is reported in Figure 2.

Leave-one-out. Re-estimating the TWFE excluding one control country at a time yields effects ranging from -2.0% to -5.1%, always negative, confirming the result is not driven by a single country.

Placebo tests. Assigning fake reform dates to France in 2014, 2015, and 2016 yields coefficients of -0.032 ($p = 0.213$), -0.023 ($p = 0.325$), and -0.027 ($p = 0.197$), all insignificant, confirming the absence of pre-reform trends.

2 Tobacco-Related Healthcare Use

2.1 Data and sample selection

Our empirical strategy relies on administrative health data from the French National Health Insurance (*Assurance Maladie – Ameli*) covering the period from 2015 to 2023. We focus on individuals aged 15 to 54 years old. This age range was chosen for two main reasons. First, it corresponds to the core working-age population, which is often the primary target of behavioral public health policies such as the plain packaging reform. Second, restricting the analysis to this group allows us to limit potential confounding effects related to aging, such as the higher prevalence of chronic conditions and the natural deterioration of health observed in older populations.

2.2 Identification strategy and econometric specification

To estimate the causal effect of the 2017 plain packaging reform on healthcare utilization rates, we implement three Difference-in-Differences (DiD) specifications using Ordinary Least Squares (OLS) with a consistent linear model across all frameworks. The choice of treated and control pathologies is guided by their clinical responsiveness to tobacco cessation and socio-economic distribution.

Our first specification evaluates the immediate health effects by comparing Acute Coronary Syndrome (ACS) with Diabetes. ACS is treated because it responds quickly to smoking cessation (Lightwood & Glantz, 1997), while Diabetes serves as a control to account for broader socio-demographic trends (Yusuf et al., 2004). Rising diabetes would normally increase ACS incidence, so any deviation in ACS relative to Diabetes suggests that reductions in smoking are counteracting demographic trends.

For validity checks, we conduct a placebo test comparing Upper Aerodigestive Tract (UADT) cancers with acute appendicitis. Tobacco increases UADT cancer risk, but carcinogenesis

typically involves a 10–30 year latency (Barsouk et al., 2023), so no effect is expected by 2023. Appendicitis provides a stable, exogenous control unrelated to public health policies (Ferris et al., 2017); observing a null effect supports the credibility of our main design.

Finally, to explore behavioral mechanisms, we compare consultations for tobacco use disorders with alcohol use disorders. As Becker and Murphy (1988) show, these addictions share similar dynamics, socio-demographics, and exposure to macroeconomic conditions, making alcohol disorders a relevant counterfactual. If the reform specifically affects smoking behavior, alcohol-related consultations should remain stable, indicating that observed effects operate through a tobacco-specific channel.

The baseline econometric specification is:

$$\boxed{\mathrm{Y}_{it} = \alpha + \beta_1 \mathrm{Group}_i + \sum_{k=2016}^{2023} \gamma_k \mathrm{Year}_{k,t} + \delta(\mathrm{Group}_i \times \mathrm{Post}_t) + \epsilon_{it}}$$

where Y_{it} is the Healthcare Utilization Rate for pathology i in year t, γ_k are Year Fixed Effects, and δ is the DiD coefficient capturing the absolute change in consultation rates for the treated pathology relative to the control following the 2017 reform.

2.2.1 Parallel trends assumption

The validity of a Difference-in-Differences (DiD) estimator depends on the parallel trends assumption, which requires treated and control outcomes to follow similar trajectories absent treatment. With only two pre-treatment years (2015–2016), formal statistical tests lack power, so we rely on visual inspection.

Pre-treatment trends appear reasonably parallel for ACS vs. Diabetes (Figure 3) and for the falsification test of UADT cancers vs. Appendicitis (Figure 4), supporting the DiD approach. In contrast, trends differ for Tobacco vs. Alcohol use disorders, violating the assumption; here, alcohol serves only as a qualitative counterfactual. Despite the lack of strict causal inference, the post-2017 divergence is striking: tobacco-related consultations rise sharply, while alcohol-related consultations remain stable, providing descriptive evidence of a behavioral shift following the reform (Figure 5).

2.3 Results

2.3.1 Short term impact (ACS vs Diabetes)

Our baseline linear model estimates an reduction of 0.112 percentage points in ACS consultations relative to diabetes. However, this effect is not statistically significant (p = 0.108), (Table 3). Similarly, the log-linear specification suggests a relative decrease of 10.68%, yet again failing to reach conventional significance (p = 0.119), see (Table 4). While both models show a consistent downward trend, the lack of statistical significance indicates that these short-term changes should be interpreted with caution.

2.3.2 Falsification Test (UADT Cancer vs. Appendicitis)

The estimated impact on UADT is not statistically significant ($p = 0.3808$), as we can see in (Table 5), which aligns with what we know from the medical literature. Unlike acute conditions such as Appendicitis, UADT cancers develop over many years or even decades. Therefore, it is not surprising that we do not see a large decrease in UADT incidence just three to six years after the law was implemented. In fact, this lack of effect can be seen as a useful negative control, showing that our model is reliable and does not produce false signals where there should be none.

2.3.3 Behavioral Mechanism (Tobacco vs. Alcohol Use Disorders)

Even without strict causal inference, this drastic divergence between treatment and control groups provides compelling evidence of a massive behavioral shift in tobacco addiction consultations. The DiD estimates show a striking increase: +0.47 percentage points ($p = 0.012$) in the linear model (Table 6) and a 279.99% surge ($p = 0.003$) in the log-linear specification (Table 7). However, since the parallel trends assumption appears violated, the precise causal interpretation remains uncertain.

2.4 Conclusion

Although the effect on ACS is only marginally significant ($p = 0.108$) (Table 3), it stands in contrast to the complete absence of any signal for UADT cancers ($p = 0.3808$) (Table 5). This

pattern suggests that the reform may be beginning to influence fast-response pathologies, such as vascular diseases, while it remains too early to detect an impact on long-latency conditions, such as cancers. In addition, the data reveal a highly significant and dramatic increase in tobacco addiction consultations ($p = 0.003$) (Table 7). Even though pre-trend diagnostics indicate a violation of the parallel trends assumption, this pronounced surge likely reflects a broader shift in health-seeking behavior rather than a strictly causal effect of the reform.

3 Nicotine Substitutes Uptake

3.1 Empirical Strategy

This section evaluates the impact of the introduction of plain packaging on nicotine replacement therapy (NRT) boxes sold in pharmacies. To estimate this effect, we consider an Interrupted Time Series framework applied to monthly data on NRT boxes sold in French pharmacies, from 2014 to 2024. The baseline equation is given by:

$$\begin{aligned}\log(\text{Rate}_t) = \beta_0 + \beta_1 \text{NOV2016}_t + \beta_2 \text{DEC2016}_t + \beta_3 \text{D2017}_t + \beta_4 \text{P2017}_t + \beta_5 \text{D2018}_t + \beta_6 \text{P2018}_t \\ + \sum_{m=2}^{12} \alpha_m \text{Month}_{m,t} + \beta_7 \text{COVID2020}_t + \beta_8 \text{COVID2021}_t + \beta_9 \text{Time}_t + \beta_{10} \text{IPC_logratio}_t + \epsilon_t\end{aligned}$$

Where $\log(\text{Rate}_t)$ is the logarithm of the number of NRT boxes sold per 1,000 inhabitants. The $NOV2016$ and $DEC2016$ are dummy indicators to capture potential anticipation effects and to account for the first edition of the *Mois Sans Tabac* campaign launched in November 2016. $D2017$ and $D2018$ capture level shifts in 2017 and 2018, while $P2017$ and $P2018$ represent the associated post-policy linear trends. The specification also controls for the COVID period, and for price dynamics with the IPC_logratio variable. The variables of primary interest for the 2017 policy are therefore $D2017$ and $P2017$.

3.2 Results

3.2.1 Basic ITS regression

Results of the basic ITS regression are reported in Table 8. The coefficient on D2017 is highly significant and indicates an increase in NRT boxes sold per 1,000 inhabitants of 136%

$(100(e^{0.86} - 1) \approx 136\%)$ after the implementation of the policy. However, β_{P2017} is not significant.

Moreover, residual diagnostics reveal substantial autocorrelation. From Table 9, we can see that we strongly reject the absence of autocorrelation. We thus have to either use Newey–West heteroskedasticity and autocorrelation consistent (HAC) standard errors or explicitly model the error process.

First, we run the same ITS with HAC standard errors (see Table 10). We see that β_{D2017} is still highly significant and does not change the coefficient. Moreover, β_{P2017} becomes significant when autocorrelation is accounted for, with a negative sign, meaning that the policy's effect gradually lost momentum over time.

3.2.2 GLS with autoregressive residuals of order 1

Given the strong autocorrelation in the residuals, another approach consists of explicitly modelling the error process. Looking at Figure 6 and 7 and also at Table 11, we estimate a GLS model with AR(1) residuals. Thus we run the same regression, but we add the assumption that:

$$\epsilon_t = \rho\epsilon_{t-1} + u_t.$$

The estimates of this regression are reported in Table 12. The estimate of β_{D2017} is similar to that obtained in the baseline ITS specification, also significant. However, β_{P2017} is not statistically significant.

To assess the adequacy of the AR(1) specification, we decided to look again at the Durbin-Watson statistic, reported in Table 13 indicating that some residual autocorrelation remains and is not captured by the AR(1) specification. Moreover, as shown in Table 14, the estimated parameter $\hat{\rho}$ is very close to unity. We know that in this type of modelisation, a ρ really close to 1 suggests that the error process may be close to a unit root, which raises concerns about model stability. Even though the ADF (Augmented Dickey-Fuller) test rejects the unit root hypothesis (see Table 14), the ACF and PACF of the residuals still display persistent autocorrelation, as the Durbin-Watson test suggested.

We therefore decided not to continue with this specification for placebo tests, and instead rely on the ITS with HAC standard errors.

3.2.3 Placebo Tests

For the placebo test of our model, we decided to implement new variables in the regression, replacing $D2017_t$ and $P2017_t$. These variables will be $DPlacebo2016_t$ and $PPlacebo2016_t$, constructed like $D2017_t$ and $P2017_t$.

Since we already added anticipation effects dummies ($NOV2016_t$ and $DEC2016_t$), we decided to run 2 placebo regressions, one without the anticipation effects dummies, and one with. All regressions use Newey-West standard errors. Results of the two regressions are reported in Table 15 and 16.

The first regression (without anticipation) shows a significant $\beta_{PPlacebo2016}$ ($p < 0.01$), suggesting potential model misspecification or the presence of an additional structural change around 2016. To be sure that 2016 is really a year that we have to account for, we need to be sure that this significance is not due to anticipation effects. We therefore run another regression, integrating anticipation dummies. However, $\beta_{PPlacebo2016}$ is still statistically significant. This result suggests that factors other than anticipation effects may explain the unusual dynamics observed in 2016. To account for this unexplained structural change, we augment the baseline specification with a level dummy $D2016$ (equals 1 from January to October 2016 and from January 2017 onward, thereby isolating the November–December 2016 anticipation period.) and an associated post-2016 trend $P2016$ (long-term linear trend after 2016) into the core model.

3.2.4 Final model, Results and Limitations

Our final model is thus:

$$\begin{aligned}\log(\text{Rate}_t) = \beta_0 &+ \beta_1 \text{D2016}_t + \beta_2 \text{NOV2016}_t + \beta_3 \text{DEC2016}_t + \beta_4 \text{P2016}_t + \beta_5 \text{D2017}_t + \beta_6 \text{P2017}_t \\ &+ \beta_7 \text{D2018}_t + \beta_8 \text{P2018}_t + \beta_9 \text{COVID2020}_t + \beta_{10} \text{COVID2021}_t + \sum_{m=2}^{12} \alpha_m \text{Month}_{m,t} \\ &+ \beta_{11} \text{Time}_t + \beta_{12} \text{IPC_logratio}_t + \epsilon_t\end{aligned}$$

The estimation results are reported in Table 17.

After accounting for the 2018 policy and controlling for potential 2016 anticipation effects, the plain packaging policy implemented in 2017 is associated with an estimated increase of approximately 171.8% in NRT boxes sold per 1,000 inhabitants. While the policy generates a

strong short-term increase in NRT uptake, the estimated post-policy trend suggests that this effect gradually loses momentum over time. This pattern is consistent with findings in the literature (Wakefield et al., 2013).

Both specifications present important limitations. In particular, residual diagnostics reveal substantial autocorrelation that the AR(1) specification fails to fully capture. This suggests that the underlying error process may be more complex than assumed in the GLS model. In addition, the failed placebo tests suggest that the estimated policy effect may be overstated. This overestimation may arise from pre-existing trends affecting the 2017 period, but also from the difficulty of isolating the effect of plain packaging from other tobacco control policies (Chaloupka and Warner, 1999).

General Conclusion & Limitations

France's introduction of plain packaging is associated with a decline in cigarette consumption, with effects that appear to strengthen over time. We also observe a sharp short-term increase in the use of nicotine replacement therapies, which may reflect a rise in quit attempts, although this effect seems to fade over time. While some early changes may appear for short-term health dynamics and smoking-related consultations, no impact is yet detectable for long-latency diseases.

However, these findings should be interpreted with caution. Several limitations affect the reliability of our estimates across the three parts of this analysis. The analysis relies exclusively on aggregated data, which prevents controlling for individual-level heterogeneity and makes it difficult to account for all relevant confounders. The health outcomes model additionally suffers from a limited number of pre-reform years, reducing our ability to credibly establish the parallel trends assumption. Furthermore, the lack of statistical significance in several specifications in the health-related outcomes part may reflect insufficient statistical power rather than an absence of effects. Finally, the time series models used in the third part of our analysis raise concerns about autocorrelation in the residuals, which may lead to underestimated standard errors and should undermine the interpretation of our results.

Tables and Figures

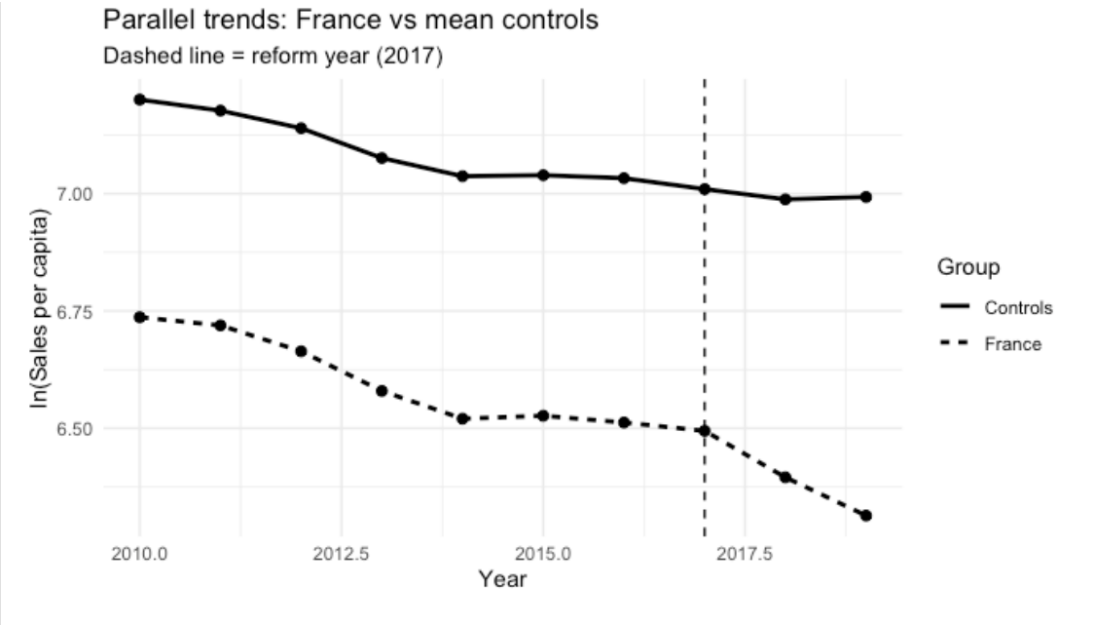

Figure 1: Parallel Trends: France vs. Control Group (2010–2019). Dashed line indicates the 2017 reform.

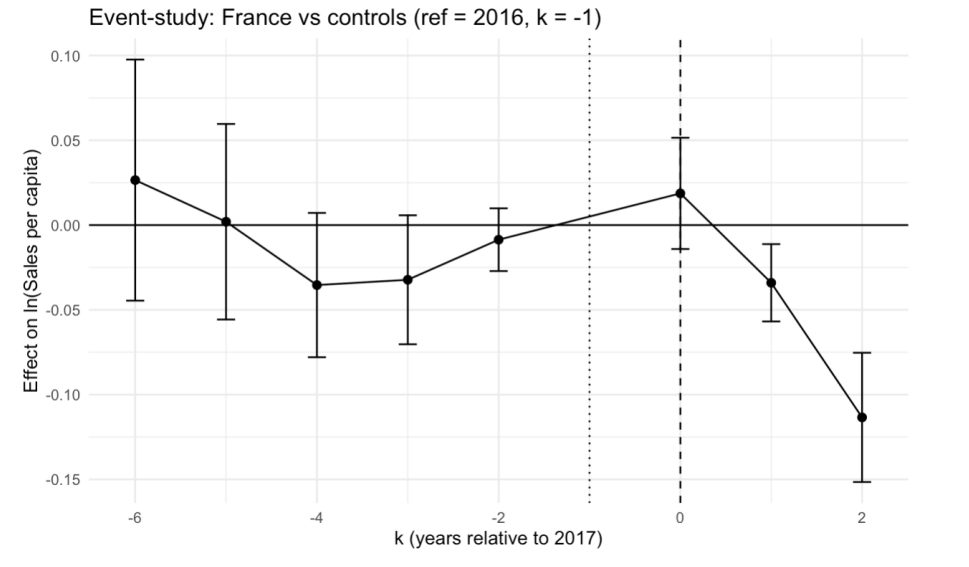

Figure 2: Event Study: Dynamic Treatment Effects on Cigarette Sales per Capita (ref. year = 2016).

Table 1: Baseline TWFE Estimates

	Estimate	Std. Error	t-value	p-value
Treatment (France × Post-2017)	-0.0376^{**}	0.0168	-2.238	0.045
Unemployment rate	-0.0245^{***}	0.0078	-3.153	0.008
Tobacco Price Index	-0.0004	0.0012	-0.373	0.716
Average price	-0.0125	0.0212	-0.586	0.568
Country FE		Yes		
Year FE		Yes		
Clustered SE		Country level		
Observations		130		

Notes: $^{**}p < 0.05$, $^{***}p < 0.01$. Dependent variable: ln(Sales per capita).

Table 2: Dynamic Treatment Effects (2017–2019)

	Estimate	Std. Error	t-value	p-value
Post-2017	0.0217	0.0270	0.803	0.437
Post-2018	-0.0528^{***}	0.0136	-3.869	0.002
Post-2019	-0.0794^{***}	0.0180	-4.410	< 0.001
Unemployment rate	-0.0240^{**}	0.0078	-3.076	0.010
Tobacco Price Index	-0.0003	0.0011	-0.262	0.797
Average price	-0.0129	0.0218	-0.592	0.565
Country FE & Year FE		Yes		
Clustered SE		Country level		
Observations		130		

Notes: $^{**}p < 0.05$, $^{***}p < 0.01$. Dependent variable: ln(Sales per capita).

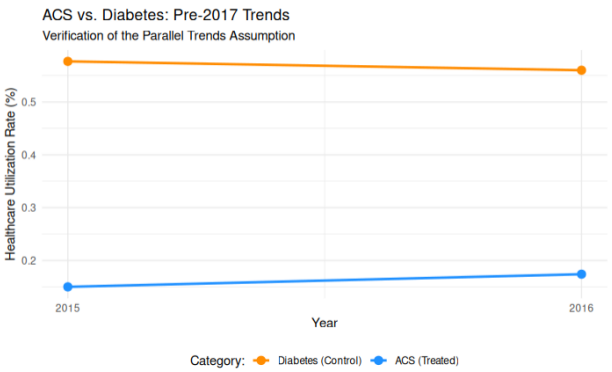

Figure 3: Pre-treatment trends: ACS vs. Diabetes (2015-2016)

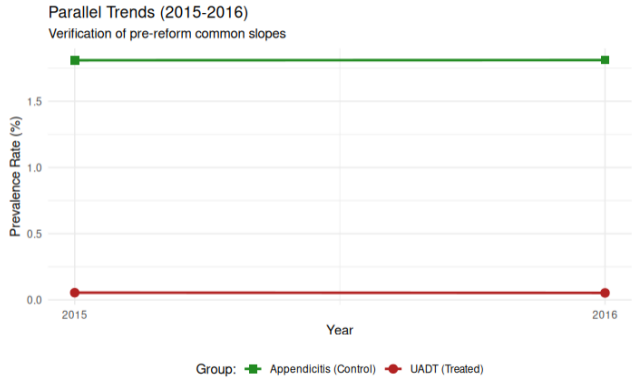

Figure 4: Pre-treatment trends: UADT vs. Appendicitis (2015-2016)

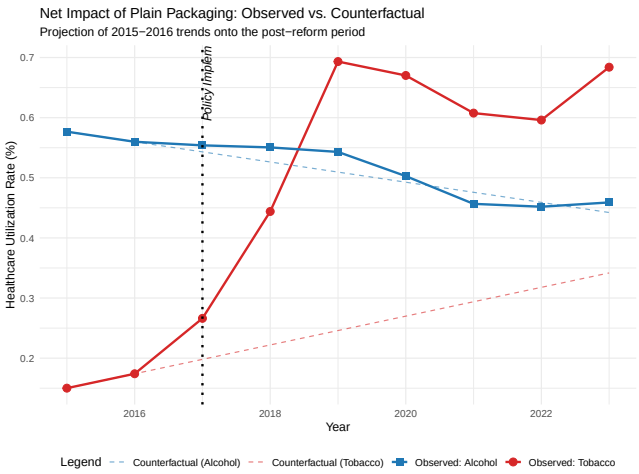

Figure 5: Observed vs. Counterfactual

Table 3: ACS vs. Diabetes (Linear Specification)

Variables	**Estimate**	**Std. Error**	**p-value**
Intercept (Baseline 2015, Diabetes)	1.810^{***}	0.046	0.001
Group (ACS)	-1.756^{***}	0.054	0.001
DiD Estimator (Group × Post)	−0.112	0.061	0.108
Year Fixed Effects	Included (2016–2023)		

Notes: Standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Dependent Variable: Healthcare Utilization Rate (%)

Table 4: ACS vs. Diabetes (Log-linear Specification)

Variables	**Estimate**	**Std. Error**	**p-value**
Intercept (Baseline 2015, Diabetes)	0.602^{***}	0.049	<001
Group (ACS)	-3.515^{***}	0.064	<001
DiD Estimator (Group × Post)	−0.113	0.064	0.119
Percentage Change (DiD)	**-10.68%**		
Year Fixed Effects	Included (2016–2023)		

Notes: Standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Dependent Variable: Healthcare Utilization Rate (%)

Table 5: UADT vs. Appendicitis (Linear Specification)

Variables	**Estimate**	**Std. Error**	**p-value**
Intercept	0.133^{***}	0.001	0.001
Group (Treated)	-0.076^{***}	0.001	0.001
DiD Estimator (Interaction)	−0.001	0.001	0.381
Year Fixed Effects	Included (2016–2023)		

Notes: Standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Dependent Variable: Healthcare Utilization Rate (%)

Table 6: Tobacco vs. Alcohol (Linear Specification)

Variables	**Estimate**	**Std. Error**	**p-value**
Intercept	0.567^{**}	0.106	0.001
Group (Treated)	-0.406^{*}	0.123	0.013
DiD Estimator (Interaction)	0.470^{*}	0.139	0.012
Year Fixed Effects	Included (2016–2023)		

Notes: Standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Dependent Variable: Healthcare Utilization Rate (%)

Table 7: Tobacco vs. Alcohol (Log-linear Specification)

Variables	**Estimate**	**Std. Error**	**p-value**
Intercept (Baseline 2015, Alcohol)	-0.595^{**}	0.233	0.038
Group (Tobacco)	-1.257^{***}	0.269	0.003
DiD Estimator (Group × Post)	1.335^{***}	0.305	0.003
Percentage Change (DiD)	**+279.99%**		
Year Fixed Effects	Included (2016–2023)		

Notes: Standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Dependent Variable: Healthcare Utilization Rate (%)

Table 8: ITS without HAC

	Value	Std.Error	t-value	p-value
(Intercept)	-1.107032***	0.132850	-8.333	4.92e−13
NOV2016	0.401121	0.307180	1.306	0.1947
DEC2016	0.804059*	0.310165	2.592	0.0110
D2017	0.858424***	0.207507	4.137	7.45e−05
D2018	0.915408***	0.210271	4.353	3.29e−05
P2017	-0.036950	0.025228	-1.465	0.1462
P2018	0.032229	0.023731	1.358	0.1775
COVID2020	-0.109519	0.140911	-0.777	0.4389
COVID2021	-0.226184	0.132730	-1.704	0.0915
Month2	-0.027419	0.120005	-0.228	0.8197
Month3	-0.036248	0.122290	-0.296	0.7675
Month4	-0.173922	0.121818	-1.428	0.1566
Month5	-0.182140	0.121359	-1.501	0.1366
Month6	-0.085792	0.121286	-0.707	0.4810
Month7	-0.113010	0.121625	-0.929	0.3551
Month8	-0.184633	0.122009	-1.513	0.1334
Month9	-0.075627	0.122642	-0.617	0.5389
Month10	0.073361	0.123493	0.594	0.5538
Month11	0.052300	0.126635	0.413	0.6805
Month12	0.064509	0.127283	0.507	0.6134
Time	0.005942	0.009195	0.646	0.5197
IPC_logratio	4.309858***	1.055913	4.082	9.13e−05

Notes: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 9: Diagnostics of residual autocorrelation

Test	**Statistic**	**df**	**p-value**
Box–Ljung (lag 12)	$\chi^2 = 233.04$	12	$< 2.2 \times 10^{-16}$
Durbin–Watson	$DW = 0.29776$	—	$< 2.2 \times 10^{-16}$

Table 10: ITS with HAC

	Value	Std.Error	t-value	p-value
(Intercept)	-1.1070319***	0.0654548	-16.9129	$< 2.2e{-}16$
NOV2016	0.4011211***	0.0664126	6.0398	$2.776e{-}08$
DEC2016	0.8040591***	0.0603292	13.3279	$< 2.2e{-}16$
D2017	0.8584242***	0.0480382	17.8696	$< 2.2e{-}16$
D2018	0.9154082**	0.3390475	2.6999	0.0081704
P2017	-0.0369503**	0.0116018	-3.1849	0.0019422
P2018	0.0322292***	0.0093339	3.4529	0.0008203
COVID2020	-0.1095194	0.2219503	-0.5167	0.6065143
COVID2021	-0.2261842	0.2354526	-0.9606	0.3390995
Month2	-0.0274189	0.0230218	-1.1910	0.2365324
Month3	-0.0362480	0.1518144	-0.2388	0.8117864
Month4	-0.1739224	0.1294286	-1.3438	0.1821257
Month5	-0.1821405	0.1250205	-1.4569	0.1483449
Month6	-0.0857924	0.1000036	-0.8579	0.3930451
Month7	-0.1130099	0.0755523	-1.4958	0.1379232
Month8	-0.1846329**	0.0644447	-2.8650	0.0051029
Month9	-0.0756267	0.0497499	-1.5201	0.1316962
Month10	0.0733615	0.0406119	1.8064	0.0739247
Month11	0.0523003	0.0683575	0.7651	0.4460504
Month12	0.0645093	0.0588645	1.0959	0.2758104
Time	0.0059418**	0.0018498	3.2120	0.0017836
IPC_logratio	4.3098582	2.4178892	1.7825	0.0777664

Notes: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

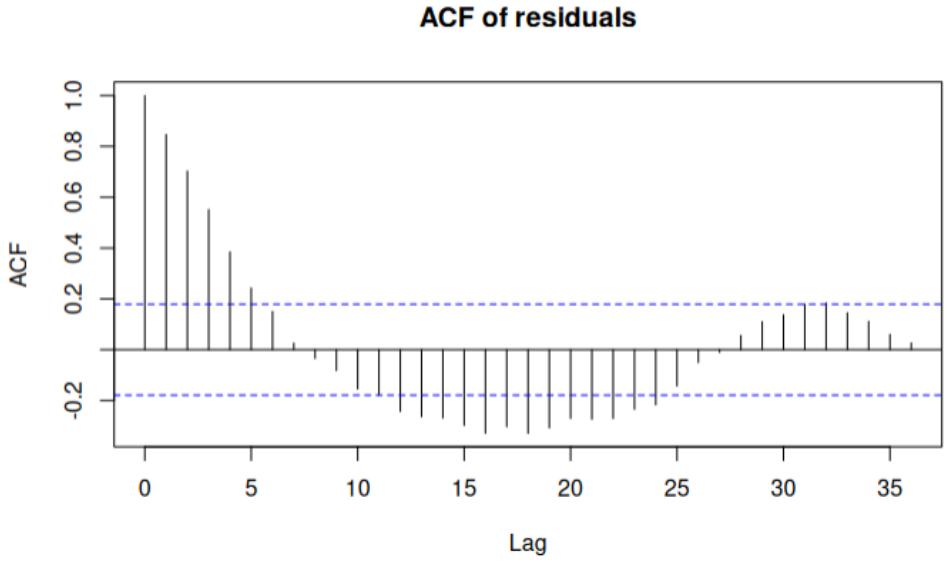

Figure 6: ACF of the residuals of the ITS regression.

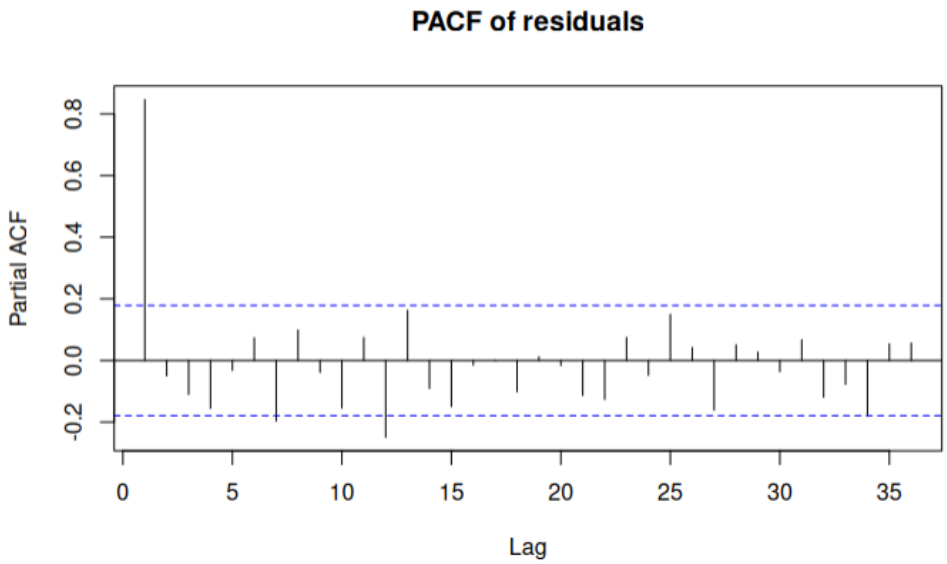

Figure 7: PACF of the residuals of the ITS regression.

Table 11: AIC and BIC of different GLS ARMA(p,q) regression

Model	AIC	BIC	dAIC	dBIC
ARMA(1,0)	-76.32	-14.277	0.000	0.000
ARMA(2,0)	-74.95	-10.327	1.365	3.950
ARMA(1,1)	-74.92	-10.291	1.401	3.986
ARMA(1,2)	-73.41	-6.206	2.901	8.071
ARMA(3,0)	-72.95	-5.742	3.365	8.535
ARMA(0,3)	-16.16	51.054	60.161	65.331
ARMA(0,2)	12.68	77.303	88.994	91.579
ARMA(0,1)	45.41	107.448	121.724	121.724

Table 12: GLS estimates with autoregressive residuals

	Value	Std.Error	t-value	p-value
(Intercept)	-1.1558095	68.36866	-0.016906	0.9865
NOV2016	0.4554935***	0.11589	3.93095	0.0002
DEC2016	0.8668422***	0.16698	5.191267	0.0000
D2017	0.9202685***	0.20196	4.999616	0.0000
D2018	0.1587431	0.12039	1.318607	0.1904
P2017	-0.0111956	0.04027	-0.278034	0.7816
P2018	0.0281395	0.03465	0.812896	0.4169
COVID2020	0.0743985	0.08784	0.846975	0.3991
COVID2021	0.0332501	0.08753	0.367169	0.7139
Month2	-0.0519260	0.03282	-1.581954	0.1169
Month3	0.0366065	0.04613	0.794384	0.4274
Month4	-0.1238211*	0.05252	-2.357607	0.0202
Month5	-0.1611233***	0.05667	-2.843038	0.0054
Month6	-0.0914737	0.05932	-1.541909	0.1265
Month7	-0.1345364*	0.06059	-2.220266	0.0287
Month8	-0.2448474***	0.06060	-4.038746	0.0001
Month9	-0.1387618*	0.05889	-2.353619	0.0204
Month10	-0.0183406	0.05062	-0.327398	0.7440
Month11	-0.0144403	0.04889	-0.295357	0.7683
Month12	-0.0250619	0.03906	-0.641609	0.5226
Time	0.0083766	0.02362	0.354671	0.7236
IPC_logratio	0.2944732	0.66171	0.445021	0.6573

Notes: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 13: Diagnostic of residual autocorrelation of GLS

Test	**Statistic**	**df**	**p-value**
Box–Ljung (lag 12)	$\chi^2 = 24.338$	12	0.01829

Table 14: Estimate of ρ and ADF.

Coeficient	Value	Augmented Dickey-Fuller Test	p-value
$\hat{\rho}$	0.9999988	-3.4394	0.051

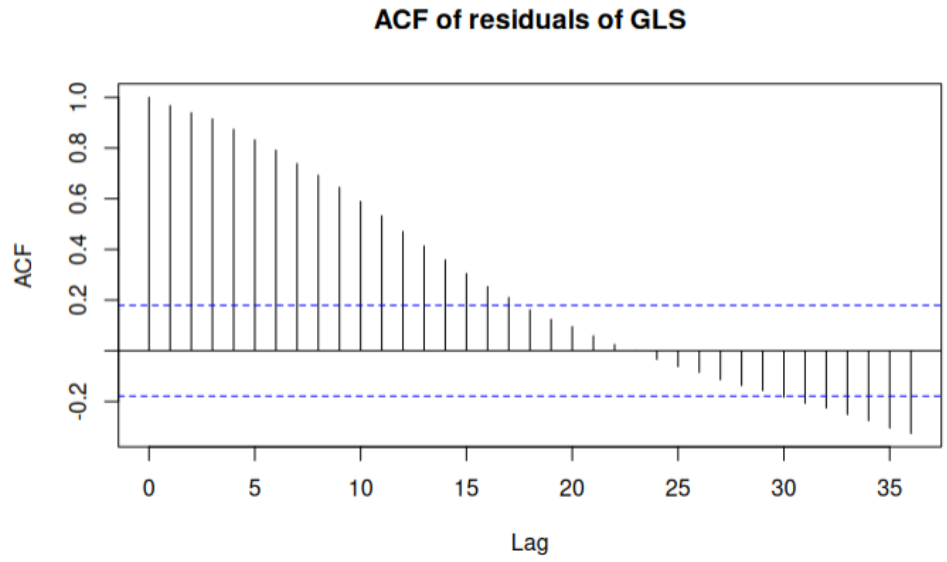

Figure 8: ACF of the residuals of the GLS AR(1) regression.

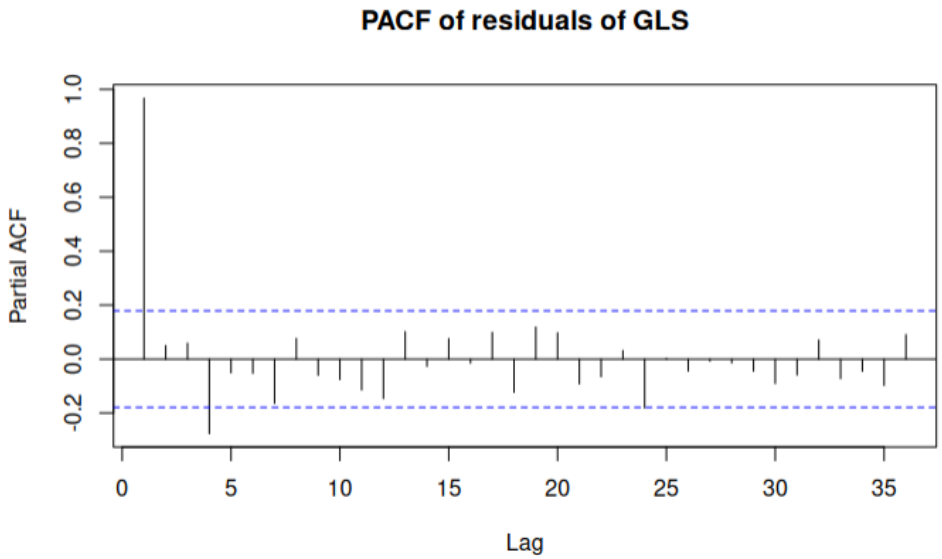

Figure 9: PACF of the residuals of the GLS AR(1) regression.

Table 15: ITS Placebo test without anticipation estimates

	Value	Std.Error	t-value	p-value
(Intercept)	-1.0254059***	0.0906261	-11.3147	$< 2.2e{-}16$
DPlacebo2016	0.1227672	0.1385795	0.8859	0.37780
PPlacebo2016	0.0469230***	0.0057259	8.1948	$8.644e{-}13$
D2018	0.5406708	0.3517164	1.5372	0.12739
P2018	-0.0373878***	0.0081370	-4.5948	$1.266e{-}05$
COVID2020	-0.0830519	0.2284939	-0.3635	0.71702
COVID2021	-0.2029585	0.2461338	-0.8246	0.41157
Month2	-0.0375145*	0.0183128	-2.0485	0.04313
Month3	-0.0492243	0.1455983	-0.3381	0.73601
Month4	-0.1968906	0.1295752	-1.5195	0.13179
Month5	-0.2154753	0.1264878	-1.7035	0.09158
Month6	-0.1293526	0.1037337	-1.2470	0.21532
Month7	-0.1661530	0.0904672	-1.8366	0.06924
Month8	-0.2487112*	0.0948726	-2.6215	0.01012
Month9	-0.1485228	0.0877866	-1.6919	0.09379
Month10	-0.0098706	0.0836748	-0.1180	0.90633
Month11	0.0023353	0.0642874	0.0363	0.97110
Month12	0.0448915	0.0322577	1.3917	0.16712
Time	-0.0072780	0.0068447	-1.0633	0.29021
IPC_logratio	4.0721356	2.4326049	1.6740	0.09726

Notes: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 16: ITS Placebo test with anticipation estimates

	Value	Std.Error	t-value	p-value
(Intercept)	-1.0319115***	0.0879868	-11.7280	< 2.2e−16
DPlacebo2016	0.0706766	0.1451361	1.6038	0.11197
PPlacebo2016	0.0452196***	0.0049864	9.0686	1.270e−14
NOV2016	0.1014003	0.0632233	1.6038	0.11197
DEC2016	0.4766593***	0.0388872	12.2575	< 2.2e−16
D2018	0.5435451	0.3747676	1.4504	0.15015
P2018	-0.0384853***	0.0083676	-4.5939	1.268e−05
COVID2020	-0.0885994	0.2386590	-0.3712	0.71126
COVID2021	-0.2086633	0.2551047	-0.8180	0.41537
Month2	-0.0377674*	0.0182635	-2.0679	0.04128
Month3	-0.0515631	0.1460810	-0.3530	0.72486
Month4	-0.1995086	0.1306642	-1.5269	0.13001
Month5	-0.2182774	0.1272493	-1.7154	0.08944
Month6	-0.1323746	0.1070836	-1.2362	0.21925
Month7	-0.1695581	0.0943224	-1.7976	0.07531
Month8	-0.2521558*	0.1009444	-2.4980	0.01416
Month9	-0.1525449	0.0961049	-1.5873	0.11567
Month10	-0.0140845	0.0950505	-0.1482	0.88251
Month11	-0.0130974	0.0870351	-0.1505	0.88069
Month12	-0.0083579	0.0755871	-0.1106	0.91218
Time	-0.0046587	0.0086948	-0.5358	0.59331
IPC_logratio	4.1325343	2.4656324	1.6761	0.09692

Notes: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 17: Final ITS model estimates, HAC standard errors

	Value	Std.Error	t-value	p-value
(Intercept)	-1.0385791***	0.0963974	-10.7739	$< 2.2e{-}16$
D2016	0.3119823*	0.1281822	2.4339	0.016786
NOV2016	0.7736894***	0.1019113	7.5918	$2.056e{-}11$
DEC2016	1.1948724***	0.1120072	10.6678	$< 2.2e{-}16$
P2016	0.0012059	0.0065236	0.1848	0.853739
D2017	1.0029874***	0.1020840	9.8251	$3.532e{-}16$
D2018	0.9452510**	0.3245134	2.9128	0.004455
P2017	-0.0201153*	0.0091714	-2.1933	0.030702
P2018	0.0365087**	0.0116493	3.1340	0.002288
COVID2020	-0.1140758	0.1964608	-0.5807	0.562834
COVID2021	-0.2293627	0.2235595	-1.0260	0.307490
Month2	-0.0224011	0.0266806	-0.8396	0.403218
Month3	-0.0271401	0.1539943	-0.1762	0.860476
Month4	-0.1598100	0.1311496	-2.2185	0.226008
Month5	-0.1629755	0.1282666	-1.2706	0.206941
Month6	-0.0615930	0.1081839	-0.5693	0.570458
Month7	-0.0838587	0.0809473	-1.0360	0.302820
Month8	-0.1503559*	0.0655544	-2.2936	0.023994
Month9	-0.0364963	0.0628557	-0.5806	0.562846
Month10	0.1175406	0.0667469	1.7610	0.081424
Month11	0.0950207	0.0841347	1.1294	0.261549
Month12	0.1104038	0.0815633	1.3536	0.179043
Time	-0.0165920	0.0116851	-1.4199	0.158867
IPC_logratio	4.3404233	2.3534191	1.8443	0.068223

Notes: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

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Data Appendices

Cigarette Consumption

A. Construction of the Panel Dataset

Data Sources

The panel dataset combines annual country-level information from two main sources.

Tobacco-related variables. All tobacco market variables were obtained from the PMI Tobacco Economics Database provided by Philip Morris International:

`https://www.pmi.com/tobacco-economics/tobacco-database`

This database provides harmonized annual data for European countries, including:

- Retail cigarette sales volumes,
- Average retail prices,
- Tobacco price indices.

These variables are consistently defined across countries, which ensures comparability for panel analysis.

Macroeconomic variables. Macroeconomic controls were obtained from the World Bank's *World Development Indicators (WDI)* database. The following variables were downloaded at annual frequency:

- GDP (current US$),
- GDP growth (annual %),
- Inflation (consumer prices, annual %),
- Population (total),
- Unemployment rate (% of total labor force).

All macroeconomic variables cover the period 2010–2019.

Manual Panel Construction

The dataset was manually constructed in Excel.

Each data source was first downloaded separately at annual frequency. Country names were harmonized across datasets to ensure consistency. The different datasets were then manually aligned by matching country names and calendar years.

Each row of the final dataset corresponds to a unique *country-year* observation.

The final dataset contains annual observations over the period 2010–2019.

Country Coverage

The final panel retains the following 13 European countries:

Austria, Belgium, Croatia, Estonia, France, Germany, Italy, Netherlands, Poland, Portugal, Slovakia, Slovenia, and Spain.

Table 18: Panel Coverage

Dimension	Description
Countries	13 European countries
Time period	2010–2019
Frequency	Annual
Maximum observations	$13 \times 10 = 130$

B. Variable Construction

Dependent Variable

Retail cigarette sales volumes were obtained from the PMI database. Sales were divided by total population to compute sales per capita:

$$(\text{Sales per capita})_{it} = \frac{\text{Total cigarette sales}_{it}}{\text{Population}_{it}}$$

The dependent variable is defined as:

$$\text{lnSales_pc}_{it} = \log((\text{Sales per capita})_{it})$$

The logarithmic transformation reduces skewness and allows coefficients to be interpreted approximately as percentage changes.

Treatment Variable

The treatment indicator captures the implementation of the plain packaging reform in France in 2017:

$$\text{Treatment}_{it} = \begin{cases} 1 & \text{if Country} = \text{France and Year} \geq 2017, \\ 0 & \text{otherwise.} \end{cases}$$

Event-Time Variable

For the event-study specification:

$$\text{event_time}_t = \text{Year}_t - 2017$$

Event time is bounded between -6 and 2. The omitted reference category is $k = -1$ (year 2016).

Dynamic Treatment Indicators

For dynamic specifications, the following indicators are constructed:

- `Post2017`: equals 1 for France from 2017 onward,
- `Post2018`: equals 1 for France from 2018 onward,
- `Post2019`: equals 1 for France from 2019 onward.

Control Variables

The baseline specification includes:

- `unemployment_rate` (World Bank),
- `GDP growth` (World Bank),

- `inflation` (World Bank),
- `TobaccoPriceIndex` (PMI),
- `AVG_price` (PMI).

Macroeconomic controls capture aggregate demand conditions, while tobacco-specific price variables account for fiscal and pricing dynamics affecting cigarette consumption.

Table 19: Variable Definitions and Sources

Variable	Definition	Type	Source
lnSales_pc	Log of sales per capita	Continuous	PMI
Treatment	France × Post-2017	Dummy	Constructed
event_time	Year - 2017	Integer	Constructed
TobaccoPriceIndex	Tobacco price index	Continuous	PMI
AVG_price	Average cigarette price	Continuous	PMI
unemployment_rate	Unemployment rate	Continuous	World Bank
GDP growth	Annual GDP growth (%)	Continuous	World Bank
inflation	CPI inflation (%)	Continuous	World Bank
Population	Total population	Continuous	World Bank

C. Final Estimation Sample

The final regression sample consists of country-year observations from 13 countries observed over the period 2010–2019.

All regressions include:

- Country fixed effects,
- Year fixed effects,
- Standard errors clustered at the country level.

Identification relies on a Difference-in-Differences framework, comparing the post-2017 evolution of cigarette sales in France to contemporaneous changes in control countries.

Tobacco-Related Healthcare Use

A. Data Source

The data used in this study were extracted from the open data repository of the French National Health Insurance (*Assurance Maladie - Open Data Ameli*). Specifically, we compiled our raw figures from 9 datasets providing patient headcounts broken down by pathology, age group, and gender, corresponding to one dataset per year from 2015 to 2023. (*Effectif des patients par pathologie et par classe d'âge selon le sexe*). This datasets provide the annual headcount of patients covered by all health insurance schemes, presented by pathology, chronic treatment or episode of care, and by age group, according to gender.

Variable Construction

We restricted our sample to individuals aged 15 to 54 years old (both sexes combined).

The primary dependent variable, the **Healthcare Utilization Rate** (*Outcome*), was constructed as a percentage. It was calculated by taking the sum of patients treated for a specific pathology within the 15-54 age group and dividing it by the total population of that same age group for the corresponding year:

$$\text{Outcome}_{it} = \left(\frac{\text{Treated Patients}_{it}}{\text{Total Population}_{t}} \right) \times 100$$

For our Difference-in-Differences (DiD) framework, we defined three distinct models pairing a treated group (affected by tobacco regulations) with a control group (unaffected):

- **Model 1:** Acute Coronary Syndrome (ACS) [Treated] vs. Diabetes [Control].
- **Model 2:** Upper Aerodigestive Tract (UADT) Cancers [Treated] vs. Appendicitis [Control].
- **Model 3:** Tobacco Use Disorders [Treated] vs. Alcohol Use Disorders [Control].

B. Econometric Specification

To estimate the causal impact of the 2017 plain packaging reform, we employ a Difference-in-Differences (DiD) approach with time fixed effects. The year of the dataset was converted into a categorical factor to control for unobserved, time-varying shocks that affect all groups equally. 2015 is used as the baseline reference year.

The standard specification applied to our models is defined as follows:

$$\text{Outcome}_{it} = \alpha + \beta_1 \text{Group}_i + \sum_{k=2016}^{2023} \gamma_k \text{Year}_{k,t} + \delta(\text{Group}_i \times \text{Post}_t) + \epsilon_{it}$$

Where the coefficient of interest is δ, which captures the causal effect of the policy on the treated group. The variables are detailed in Table 20 below.

Table 20: Description of Model Variables

Variable	**Type**	**Description**
Outcome_{it}	Continuous	The healthcare utilization rate (in percentage) for a given disease group i in year t.
Group_i	Binary	A dummy variable indicating treatment status. • **Model 1:** 1 if ACS (Treated), 0 if Diabetes (Control). • **Model 2:** 1 if UADT (Treated), 0 if Appendicitis (Control). • **Model 3:** 1 if Tobacco Use (Treated), 0 if Alcohol Use (Control).
$\text{Year}_{k,t}$	Binary	Year fixed effects (dummy variables for each year from 2016 to 2023). It accounts for general time trends. The year 2015 is omitted to serve as the reference period.
Post_t	Binary	A dummy variable equal to 1 for the post-reform period (2017 and onwards), and 0 otherwise.
$\text{Group}_i \times \text{Post}_t$	Binary	The interaction term (*Inter_Group_Post* in our code). It equals 1 only for the treated group in the post-treatment period. Its coefficient (δ) is the DiD estimator.
ϵ_{it}	Continuous	The error term.

Log-Linear Specification

In addition to the standard linear model, we estimate a log-linear specification for each of the

three models. This approach allows us to evaluate the relative (percentage) change associated with the 2017 reform, which is particularly relevant when comparing diseases with different baseline prevalence rates.

For this specification, the dependent variable is log-transformed:

$$\log(\text{Outcome}_{it}) = \alpha + \beta_1 \text{Group}_i + \sum_{k=2016}^{2023} \gamma_k \text{Year}_{k,t} + \delta(\text{Group}_i \times \text{Post}_t) + \epsilon_{it}$$

Nicotine Substitutes Uptake

A. Data Sources and Scope

Our primary dataset is built from the Medic'AM databases provided by the French National Health Insurance (CNAM). These records provide monthly consumption data for the "Régime Général". We consolidated semi-annual publications to build a continuous time series of the volume of boxes sold. To account for external factors, we supplemented this with INSEE data regarding population demographics and Consumer Price Indices (CPI).

B. Product Identification and Aggregation

We isolated Nicotine Replacement Therapy (NRT) products using the ATC code `N07BA01`. This classification covers all medical smoking cessation aids, such as patches, gums, and lozenges.

To construct the final monthly series $Total_t$, we extracted all line items for code `N07BA01` and summed the sales volumes across all product presentations for each month t:

$$Total_t = \sum_{i \in \text{ATC}} \text{Sales}_{i,t}$$

Where i represents each unique product reference (CIP) within the targeted ATC class.

C. Relative Tobacco Price Index

To isolate the specific evolution of tobacco prices from general inflation, we constructed a relative price variable ($IPC_logratio_t$). This is defined as the ratio between the CPI including tobacco and the CPI excluding tobacco:

$$IPC_logratio_t = \ln\left(\frac{CPI_{\text{with tobacco},t}}{CPI_{\text{without tobacco},t}}\right)$$

This ratio filters out economy-wide price volatility, serving as a proxy for the real-term price increases driven by tobacco-specific tax policies.

D. Contextual Considerations and Data Integrity

To ensure transparency and interpretability of the results, one major factor must be taken into consideration, the regulatory shift of 2018. In May 2018, France transitioned from a fixed annual €150 allowance for NRT to a standard 65% reimbursement scheme. This policy change likely induced a structural break in the series.

E. Variable Construction

Based on the aggregated data, several variables were constructed to prepare the dataset for econometric analysis:

- **Consumption Rate:** To account for demographic changes, the total volume V_t was normalized by the monthly population estimate (Pop_t):

$$Rate_t = \frac{Total_t}{Pop_t}$$

- **Log-Transformation:** We calculated the natural logarithm of the consumption rate, $\ln(Rate_t)$, to allow for the estimation of percentage changes (elasticities).
- **Temporal Variables and Policy Shocks:**
  - **Time Trend ($Time$):** A linear sequence starting at 0 for the first observation to capture the underlying evolution of NRT sales.
  - **Seasonality ($Month$):** Dummy variables (1–12) to control for recurring monthly fluctuations.
  - **Intervention Dummies (Level Shifts):** Binary variables were created to isolate specific events:

* Dummies for November and December 2016 (`NOV2016`, `DEC2016`) to capture potential anticipation effect, as manufacturers of cigarette packages halted their production in November 2016.
* *Annual Shifts:* Indicators for the years 2017 and 2018 (`D2017`, `D2018`) to account for structural changes in the reimbursement landscape.
* *COVID-19 Impact:* Binary indicators for 2020 and 2021 (`COVID2020`, `COVID2021`) to control for disruptions in pharmacy visits and healthcare behavior during the pandemic.

– **Trend Breaks (Splines):** To allow for changes in the *slope* of the trend after major policy shifts, two interaction terms were calculated:

$$P2017 = \max(Time - 24, 0) \quad ; \quad P2018 = \max(Time - 36, 0)$$

These variables capture the acceleration (or deceleration) in sales growth following the 2017 and 2018 reforms.