

Dissertation

**Pricing, Pass-Through, and
Market Intervention: Essays on
Health, Retail, and Transport
Economics**

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1 Introduction

This dissertation, titled *Pricing, Pass-Through, and Market Intervention: Essays on Health, Retail, and Transport Economics*, compiles five essays in the field of empirical industrial organization, investigating the impact of government interventions and the influence of structural shocks on the price-setting behavior of firms and market outcomes. The central objective of this research is to evaluate how firms and consumers respond to significant policy shifts across distinct market environments.

Specifically, Section 2 examines the German pharmacy market, focusing on the causal effects of banning prescription drug rebates on consumer redemption behavior (Section 2.1) and the dynamics of cost pass-through in the brick-and-mortar (B&M) pharmacy sector (Section 2.2). Section 3 evaluates the pass-through of temporary fuel tax reductions in France, Italy, and Germany (Section 3.1), and further analyses the pricing implications of the agency model versus the wholesale model for e-books on Amazon in the United Kingdom (Section 3.2). Finally, in Section 4, the dissertation investigates the impact of the *Deutschlandticket* on motorway and extra-urban car traffic outcomes.

A unifying theme across these research articles is the application of causal inference methods and broader econometric techniques to questions in industrial organization. To identify the causal effects of interventions, the research employs methods that leverage quasi-experimental designs, exploiting exogenous variation in policy or market structure. Specifically, the analyses predominantly rely on the fundamental difference-in-differences (DiD) framework, incorporating recent advancements such as staggered difference-in-differences and synthetic difference-in-differences (SDiD) estimators to isolate the average treatment effect on the treated (see Sections 2.1, 3.1, and Section 4). Complementing this framework, the dissertation employs alternative identification strategies to address specific data structures and research questions. One article utilizes propensity score matching and a

double machine learning approach to minimize selection bias and control for high-dimensional non-linear heterogeneity (Section 3.2). Finally, another article builds upon an asymmetric panel error correction model with high-dimensional fixed effects, which allows for the distinction between short- and long-run wholesale-to-retail price pass-through rates in German brick-and-mortar pharmacies, while the granularity and details of the data simultaneously enable the direct econometric assessment of product-level markups and Lerner indices (Section 2.2).

The first manuscript, *Evaluation of a Partial Ban of Rx-Rebates in Germany*, investigates patients’ price sensitivity for prescription (Rx) drugs and the substitutability between online and B&M pharmacies. The study exploits a natural experiment created by the 2020 Local Pharmacy Support Act (“Vor-Ort-Apotheken Stärkungsgesetz”), which prohibited online pharmacies from granting rebates on Rx drugs to members of the statutory health insurance scheme, while privately insured and self-pay patients remained unaffected. To evaluate this policy, the analysis utilizes a novel dataset obtained from merchandise information systems, encompassing individual transaction data from nearly half of all German offline pharmacies between 2018 and 2022. Aggregated to an annual panel, the causal identification in the DiD approach relies on comparing prescriptions issued to statutory health insurance members (the treatment group) against prescriptions issued to privately insured and self-pay patients (the control group), as the latter were not exposed to the policy intervention. The empirical results show that the partial ban induced a shift in consumer behavior, increasing offline pharmacy Rx sales by 1.42 % to 1.67 %. A detailed distributional analysis using revenue deciles reveals that the policy’s financial benefits were asymmetric. The stated goal of the intervention was to support brick-and-mortar pharmacies, yet while those in the lowest revenue decile experienced only a minor average annual profit increase of approximately €1,587, pharmacies in the highest revenue decile captured roughly four times that amount, gaining about €6,077 annually. Given these relatively modest gains, it seems unlikely that the policy had a substantial impact on pharmacies’ decisions to remain in the market – a conclusion corroborated by a 7.7 % decline in pharmacy numbers (from 18,461 to 17,041) between 2021 and 2024 (ABDA, 2024, 2025).

Contextualizing these empirical findings within the broader literature, the manuscript fits two distinct research streams: public health service provision and the digital transformation of retail markets. While the existing health economics lit-

erature predominantly evaluates the clinical and primary care roles of pharmacies (e.g., Agomo, 2012; Costa et al., 2019; Di Novi et al., 2020; Perraudin et al., 2016) or focuses on higher-level care facilities, such as physicians or hospitals (e.g., Duminy et al., 2022; Haschka et al., 2020; Herwartz & Schley, 2018; Li & Liu, 2021; Mays et al., 2009), this study emphasizes how price competition with online pharmacies may influence the profitability, which ultimately dictates the financial sustainability of B&M pharmacies. Furthermore, the analysis adds to the debates regarding the substitutability of online and offline retail channels (e.g., Brynjolfsson & Smith, 2000; Cavallo, 2017; Couture et al., 2021), as well as to the broader literature on digital public health (e.g., Iyamu et al., 2022), which explores how digitization can enhance collective health outcomes. Ultimately, the findings demonstrate that while consumers do react to price parity by shifting prescription redemptions towards local B&M pharmacies, the mild and asymmetric financial benefits of such a policy are insufficient to halt or even reverse the ongoing decline in pharmacy numbers. Consequently, this intervention alone cannot secure a comprehensive, nationwide B&M pharmacy network. Instead, further reforms, e.g., targeting pharmacy remuneration, would be necessary to ensure the financial sustainability required to maintain the network.

In the second manuscript, *Cost Pass-Through, Price Competition, and Market Power in Brick-and-Mortar Pharmacies: Evidence from Germany*, I investigate the extent of market power, price competition, and cost pass-through dynamics within the non-prescription segments of the German pharmacy market, namely for over-the-counter (OTC) drugs and non-pharmaceuticals. To empirically assess pricing behavior and dynamics, the study utilizes a high-frequency proprietary dataset encompassing approximately a billion daily transaction records from over 6,300 B&M pharmacies and roughly 213,000 unique products between 2018 and 2022. Crucially, the granularity of this data enables the direct evaluation of product-level unit markups and Lerner indices, a key metric reflecting market power (Lerner, 1934). In addition, by employing a non-linear, asymmetric, high-dimensional fixed effects error correction model, the analysis explicitly isolates the short-run effects, the adjustment speeds, and the long-run pass-through rates of positive and negative wholesale price (i.e., cost) shocks. The empirical results reveal a pronounced *rockets and feathers* phenomenon: retail prices adjust significantly faster to wholesale price increases than to decreases. This asymmetry is predominantly driven by niche products, whereas bestsellers exhibit higher short-run pricing inertia. In the

long run, however, pass-through rates consistently exceed unity (ranging from 1.3 to 1.7) across the market, indicating systematic wholesale price overshifting. Furthermore, a spatial analysis demonstrates that rural pharmacies effectively offset structurally lower transaction volumes (-7.6%) by charging OTC unit markups approximately 8.3% higher than those of their urban competitors, neutralizing profit disparities.

By situating these findings within the broader academic discourse, the manuscript connects the empirical literature on cost pass-through with theoretical frameworks of spatial competition and multi-product retail pricing. Existing research frequently documents the rockets and feathers phenomenon (e.g., Bacon, 1991; Balaguer & Ripollés, 2016; Borenstein et al., 1997; Peltzman, 2000; Tappata, 2009) and systematic long-run cost overshifting across various industries (e.g., Besley & Rosen, 1999; Delipalla & O'Donnell, 2001; Kenkel, 2005; Miller et al., 2017; Pless & Van Benthem, 2019; Stolper, 2024; Young & Bielińska-Kwapisz, 2002). Theoretical models often attribute this overshifting to strongly log-convex demand under imperfect competition (Mrázová & Neary, 2017; Ritz, 2024; Weyl & Fabinger, 2013), which allows firms to profitably expand markups following cost shocks. Building on this foundation, this study illustrates how such pricing dynamics, specifically cost overshifting and markup expansion, emerge and persist within a partially regulated environment common to pharmacy markets.

Furthermore, the analysis discusses the spatial dimension of market power in pharmacy markets. While structural models of pharmacy entry posit that high, government-mandated markups are required to offset lower demand density and ensure rural pharmacy viability (Schaumans & Verboven, 2008), this study highlights that rural pharmacies may leverage local pricing power to achieve higher unit markups in the OTC and non-pharmaceutical segments. By contextualizing these findings within the broader literature on search frictions and multi-product pricing (e.g., Bliss, 1988; Florez-Acosta & Herrera-Araujo, 2020; Lal & Matutes, 1994; Tappata, 2009; Varian, 1980; Zhou, 2014), the study elucidates the underlying drivers of this cost overshifting and markup expansion. On the one hand, pharmacies may exploit consumer inertia, as well as spatial and informational frictions in pure standalone OTC and non-pharmaceutical transactions. Especially for OTC drugs, acute medical necessity likely limits consumers' price sensitivity and willingness to search for cheaper alternatives. On the other hand, pharmacies may strategically leverage the strictly regulated Rx segment as a traffic-building an-

chor that guarantees in-store footfall, allowing them to capture complementary demand for OTC drugs and non-pharmaceuticals, thereby exploiting one-stop shopping. Together, these findings point to a broader cross-subsidization strategy. By exploiting these market frictions, pharmacies can extract higher markups on non-prescription products from captive consumers who lack viable alternative pharmacies, allowing them to cover their overall operational fixed costs. In this regard, as recent evidence demonstrates that losing this Rx anchor results in severe profitability losses (Anderes, 2026), this structural reliance underscores the acute vulnerability of the traditional pharmacy model to emerging digital shocks, such as electronic prescriptions, that threaten to divert physical footfall.

From a policy perspective, these findings imply that the financial viability of a comprehensive, nationwide rural pharmacy network relies on this implicit cross-subsidization. While online pharmacies have aggressively captured market share in the OTC segment over the past decades – surging from 5 % in 2008 to nearly 23 % in 2024 (ABDA, 2021, 2025; Statista, 2024a) – B&M pharmacies have sustained most of their profitability by relying on the regulated Rx segment (where online pharmacies have captured only 0.9 %–1.4 % ABDA, 2024, 2025). However, the mandatory rollout of electronic prescriptions (E-Rezept) in 2024 threatens to fundamentally decouple this Rx transaction from the physical point of redemption. If this digital disruption successfully diverts essential prescription traffic toward online competitors, it risks collapsing the one-stop shopping advantage, thereby undermining the implicit cross-subsidization mechanism that currently helps sustain the financial viability of the aforementioned pharmacy network. Thus, policy makers might need to reconsider how the nationwide provision can be guaranteed, given that the number of pharmacies has reached a new low of 16,601 in 2025 and has been in a steady decline since its peak of 21,602 in 2008 (ABDA, 2024; Statista, 2024b).¹

In the third manuscript, *Pass-through of temporary fuel tax reductions: Evidence from Europe*, we evaluate the pass-through of temporary fuel tax reductions aimed at mitigating the surge in energy prices following the Russian invasion of Ukraine in 2022. Using a staggered DiD approach based on Callaway and Sant’Anna (2021) and a daily panel dataset of fuel prices at the service station chain level, the study examines the staggered implementation of temporary fuel tax cuts across France,

¹See, <https://www.abda.de/aktuelles-und-presse/pressemitteilungen/detail/2025-apothekenzahl-sinkt-auf-16601-betriebsstaetten/>.

Italy, and Germany. To isolate the causal effect of these policies, fuel stations from Austria, Estonia, Lithuania, and Latvia serve as never-taker control units. Our findings demonstrate a complete pass-through of the tax reductions to consumers for both diesel and E5 gasoline, though the speed of this pass-through varied; it was rapid in Germany and Italy but took almost two weeks in France. In line with the complete pass-through finding, retail margins exhibit no significant average increase; however, a relatively small, statistically significant initial margin increase is noted in the first few days after the introduction, which levels off within a week. The overall results are consistent with recent literature on the German market that finds incomplete to complete pass-through (e.g., Bernhardt et al., 2023; Dovern et al., 2023; Kahl, 2024; Schmerer & Hansen, 2023; Seiler & Stöckmann, 2023). In a broader perspective, the study contributes a nuanced perspective to the literature on empirical tax incidence and cost pass-through in retail fuel markets, which frequently documents incomplete or asymmetric pass-through (rockets and feathers). Ultimately, high public awareness, the threat of antitrust measures, and competitive market dynamics might be the reason that these temporary tax reductions successfully lowered consumer prices. However, the suitability of such interventions remains highly debatable due to their significant fiscal burden and inherent distributional and climate-economic shortcomings.

In the fourth manuscript, *E-book Pricing Under the Agency Model: Lessons from the UK*, we investigate differences between the agency model and the traditional wholesale model in the e-book market on Amazon in United Kingdom. Under the agency model – a vertical private contract – publishers set retail prices while retailers receive a sales royalty, a structure that effectively mirrors the publisher-controlled pricing mechanisms of Fixed Book Price (FBP) systems. In contrast, the wholesale model leaves retail pricing authority to the seller, corresponding to a regime without FBP. To quantify the impact of this vertical restraint, which is closely related to resale price maintenance, the study utilizes a cross-sectional dataset of 12,001 e-book titles collected from the Amazon.co.uk platform in February 2020, covering titles published between 2010 and 2020. This dataset ensures extensive market coverage, given that Amazon accounted for approximately 87 % of UK e-book sales at that time. To empirically identify the effect of the agency model, the study employs a Latent Dirichlet Allocation text-mining approach to derive genres and control for unobserved heterogeneity. Furthermore, propensity score matching and double machine learning are used alongside instrumental vari-

able estimators to rigorously correct for potential selection biases and endogeneity, ensuring that agency-priced books are compared against highly similar wholesale-priced titles.

The analysis reveals that e-books sold under the agency model are systematically *cheaper* than comparable titles sold under the wholesale model, challenging the traditional antitrust view that vertical restraints invariably increase consumer prices. Specifically, the study finds an average price reduction of approximately 20 % for agency-priced books. Crucially, this aggregate effect is heterogeneous and predominantly driven by niche (long tail) books with lower sales rankings. For bestsellers, the chosen pricing model yields no statistically significant price differences. This finding contrasts with previous empirical literature, such as from De los Santos and Wildenbeest (2017), who estimated an 8–18 % price increase for bestsellers; however, their study could not disentangle the agency effect from anti-competitive Most-Favored Nation (MFN) clauses present at the time. By analyzing an environment without MFN clauses and expanding the focus beyond bestsellers, our results support theoretical models suggesting that the agency model can benefit consumers (e.g., Condorelli et al., 2018; Foros et al., 2017; Gaudin & White, 2014; Johnson, 2020; L. Lu, 2017). Specifically, the literature suggests that delegating pricing to publishers lowers retail prices by eliminating double marginalization (L. Lu, 2017), mitigating strategic lock-in pricing where retailers initially lower prices only to raise them later (Johnson, 2020), and curbing a retailer’s incentive to set high prices when controlling a complementary reading device (Gaudin & White, 2014). Furthermore, agency pricing can also yield lower retail prices when consumers exhibit a higher price sensitivity between books than between platforms (Foros et al., 2017), and can enhance welfare when retailers hold privileged information about consumer valuations (Condorelli et al., 2018). From a scientific perspective, while the cross-sectional nature of the dataset prevents the observation of dynamic price adjustments, the large number of observations allows for highly robust econometric analyses. Ultimately, our findings provide important empirical evidence for the broader FBP debate, demonstrating that granting pricing authority to publishers does not inherently result in higher consumer prices. Rather, it potentially *decreases* retail prices in the lower-volume segment, thereby making niche content more affordable and actively supporting the accessibility of variety in the book market despite the suppression of intra-brand retail price competition.

In the fifth, and last, manuscript, *Derailing the Car? The Effect of Germany’s Deutschlandticket on Road Traffic*, we evaluate the causal impact of a nationwide public transport fare reduction on extra-urban passenger traffic. In May 2023, the German government introduced the *Deutschlandticket* – a permanent, flat-rate public transport subscription model – with the explicit goal of incentivizing a modal shift from motorized private transport to public transit to reduce greenhouse gas emissions. To assess whether this demand-side subsidy successfully reduced car usage, the study utilizes a comprehensive panel dataset of monthly traffic volumes from 1,372 automatic counting stations across the motorway and main road networks of Germany and four control countries (Austria, Switzerland, Finland, and Great Britain) spanning from January 2018 to December 2024.

To construct a valid counterfactual for German traffic volumes, the analysis employs a modified SDiD framework based on Arkhangelsky et al. (2021). To avoid bias stemming from bad controls from the donor pools, present in our high-dimensional, heterogeneous sample, the study enforces a regularized, synthetic control weighting algorithm. This approach restricts the algorithm to match on absolute traffic levels and enforces sparsity, ensuring that treated units are compared only to structurally similar control roads while we simultaneously control for exogenous local meteorological shocks (e.g., temperature, wind speed, and ice days).

The empirical findings demonstrate that the *Deutschlandticket* failed to induce a lasting modal shift away from private cars. At the aggregate level, the average treatment effect on extra-urban traffic volumes is economically negligible and statistically indistinguishable from zero. While a temporal decomposition reveals a transient, statistically significant traffic reduction of approximately 7.5 % during the initial summer months of the policy (peaking in July 2023), this substitution effect dissipated rapidly and did not persist into the subsequent year. These results are highly robust to the spatial composition of the donor pool – verified through a leave-one-out analysis – and a Minimum Detectable Effect analysis confidently rules out any aggregate traffic reductions larger than 3 % to 4 %.

Overall, these findings carry implications for the design of sustainable transport and climate policies. The results align with the broader transport economics literature, where the cross-price elasticity between private car use and public transit fares has been found to be highly inelastic. Furthermore, the analysis in-

icates that standalone demand-side subsidies are insufficient to overcome the non-monetary generalized costs of public transport – such as reliability issues, travel time, and the crowding externalities exacerbated by sudden demand shocks in capacity-constrained networks (e.g., Liebensteiner et al., 2024; Q.-L. Lu et al., 2024). Consequently, the study concludes that the efficacy of the Deutschlandticket as a climate policy instrument is questionable, suggesting that long-term environmental objectives and modal shifts would be more efficiently served by reallocating substantial public funds toward supply-side infrastructure investments, corroborated by the literature (Schäfer, 2026).

2 Health Market

2.1 Evaluation of a Partial Ban of Rx-Rebates in Germany

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- BECCLE Competition Policy Conference, 2023, Bergen, Norway.
- 17th RGS Doctoral Conference in Economics, 2024, Ruhr Graduate School in Economics, Essen, Germany.

Evaluation of a Partial Ban of Rx-Rebates in Germany

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Abstract

We investigate patients' price sensitivity for prescription (Rx) drugs with regards to patronizing online or brick-and-mortar pharmacies. In doing so, we exploit a policy change in Germany that prohibited online pharmacies from granting rebates to one part of the population, the members of the statutory health insurance scheme. This policy change created a natural experiment, allowing us to analyze its impact on the pharmaceutical market using Difference-in-Differences (DiD). Utilizing a novel dataset obtained from merchandise information systems, we find that the ban led to a shift in consumer behavior, increasing offline pharmacy Rx sales by 1.42 % to 1.67 %. In a second step, we assess to what extent the policy change achieved its alleged goal of supporting brick-and-mortar pharmacies. Our findings indicate that pharmacies with low revenues, which are most exposed to market exit, saw only a minor increase in annual revenues of around € 1,587. At the same time, pharmacies in the highest decile gained roughly four times that amount. This indicates that the policy change alone was insufficient to reverse the declining trend in pharmacy numbers in Germany. To strengthen the comprehensive supply of pharmaceuticals to the general population, additional reforms seem necessary.

Keywords: Pharmacies, Prescription Drugs, Regulation, Public Health

JEL Codes: L5, I18

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1 Introduction

The accessibility of pharmaceutical products stands as a cornerstone of public health, directly influencing treatment availability and adherence (Mays et al., 2009; Herwartz and Schley, 2018; Haschka et al., 2020; Li and Liu, 2021; Atella et al., 2017). This availability is shaped by price and non-price factors of medications, such as the spatial density of brick-and-mortar pharmacies. The emergence and rapid growth of online pharmacies have introduced a significant shift in this landscape, offering consumers potentially lower prices, particularly for over-the-counter (OTC), and, in some countries, also for prescription (Rx) drugs, and increased convenience (Lostakova et al., 2012; Heinsohn and Flessa, 2013; Long et al., 2022). This enhanced accessibility, however, comes with potential trade-offs for the traditional brick-and-mortar pharmacy sector.

The increasing competition from online retailers poses a challenge to the viability of offline pharmacies, potentially leading to closures as consumers seek lower online prices. This is a familiar pattern from other industries (An and Chung, 2023; Chava et al., 2024), and raises concerns about the geographic availability of physical pharmacies. A dense network of those pharmacies is shown to be vital for providing not only medications but also essential health advice and immediate pharmaceutical support (Di Novi et al., 2020; Catalano et al., 2024). The emergence of “pharmacy deserts” could disproportionately affect vulnerable populations, those with limited digital literacy, and individuals needing immediate access. Increasing the density of the pharmacy network, however, usually comes at a cost because, e.g., pharmacy reimbursement would have to increase.

In this environment, regulatory bodies face the challenge of balancing the benefits of lower prices offered by online pharmacies with the need to maintain a robust and geographically diverse network of offline pharmacies.¹ One key regulatory lever involves interventions that influence drug prices or patient’s co-payments across different sales channels. Understanding how consumers respond to payment differentials between online and offline pharmacies is therefore paramount for policymakers. By analyzing consumer behavior in the face of varying payments, this research aims to provide valuable insights into the potential consequences of different regulatory approaches and their impact on both market dynamics and public health outcomes.

¹This may include a reliable and safe supply with pharmaceuticals, especially in emergency situations or when pharmaceuticals are difficult to deliver (e.g., due to cooling requirements).

Our paper uses the German market as an example. The German Pharmacy Act requires brick-and-mortar pharmacies to provide the general population with access to medications. However, the number of pharmacies dropped by roughly 12.5% from 2010 (21,441) to 2020 (18,753) (ABDA, 2024, p. 9). This decline coincides with a rise in competition from foreign online pharmacies. Their market share for OTC drugs increased from around 5% in 2008 to 20% by 2020 (ABDA, 2021; Statista, 2024). Initially, regulation rendered prices for Rx drugs uniform, which limited price competition to OTC medications. However, a 2016 European Court of Justice ruling (Case No.: C-148/15) allowed online pharmacies to also offer discounts in the form of vouchers on Rx drugs (Albrecht et al., 2020). These vouchers indirectly reduced patients’ co-payments. In this ruling, the judges claimed that the German government had failed to show that a system of uniform Rx prices was an effective tool to achieve the alleged goal of securing a comprehensive supply of pharmaceuticals to the general population.

In the light of these developments, on December 15, 2020, the German government implemented the so-called Local Pharmacy Support Act (“Vor-Ort-Apotheken Stärkungsgesetz”, henceforth VOASG), with the goal to strengthen brick-and-mortar pharmacies. This law prohibits online pharmacies from rewarding the majority of patients (those covered by the mandatory statutory health insurance scheme, making up roughly 90% of the population) with vouchers for purchasing prescription drugs. The other part of the population, privately insured individuals and self-pay patients, are still allowed to be granted vouchers. This law, by creating a differential impact on patients with statutory versus private insurance and self-pay, provides a natural experiment to study the effect of a ban on rebates on prescription drug sales.

We employ a novel dataset for this study, constructed from high-frequency sales data provided by the major merchandise information system (MIS) suppliers in Germany. This dataset encompasses individual transaction data from approximately 9,231 offline pharmacies, representing nearly half of all German pharmacies, for the period January 1, 2018, to October 31, 2022.

We find that rebates in the form of vouchers significantly affect patient’s choice regarding online and offline pharmacies: the partial ban led to an increase in offline sales of around 1.42 to 1.67 % for an average brick-and-mortar pharmacy compared to a counterfactual scenario without the ban. This result shows that patients’ choice regarding the two retail channels is affected by price differentials. This insight is particularly relevant in light of the 2016 European Court of Justice ruling mentioned above, which legalized

vouchers granted by foreign online pharmacies. Our research provides empirical evidence regarding consumers’ price-sensitivity, and therefore, suggesting that online rebates could potentially erode offline sales and profitability.

We further investigate whether the introduction of the VOASG successfully mitigated large-scale pharmacy closures. Economic theory posits that market exit occurs when opportunity costs exceed revenues, resulting in negative economic profits. Given that pharmacies with lower revenues are generally more susceptible to market exit, we employed a quantile analysis by grouping pharmacies into revenue deciles to assess how the average policy-induced profits varied across these pharmacies with different revenues. We show that, in absolute terms, large pharmacies benefited more strongly from the rebate ban than smaller pharmacies. For instance, the estimated additional *annual* profits generated by the rebate ban for pharmacies in the first and tenth deciles were approximately €1,587 and €6,077, respectively. Notably, the *additional annual profit* for the lowest decile equates to around 40 % of the average *monthly* income of an employee in Germany in 2021 (Destatis, <https://t.ly/N2dJq>). This aspect is particularly relevant because German law requires that pharmacies are owned and run by the pharmacists themselves (owner-management). The findings also indicate that the majority of pharmacies experienced only a mild increase in profits, with a median increase in profits of €3,246. Given these relatively modest gains, it seems unlikely that the policy had a substantial impact on pharmacies’ decision to enter or exit the market. This is corroborated by the developments in 2021-2024, when the number of pharmacies dropped from 18,461 (2021) to 17,041 (2024) by another 7.7% of pharmacies closed (ABDA, 2024, 2025).

This article complements the growing body of research on public health service provision. While many studies have focused on hospitals or physicians in specific countries such as the US (Mays et al., 2009; Duminy et al., 2022), China (Li and Liu, 2021), and Germany (Herwartz and Schley, 2018; Haschka et al., 2020), our research examines the role of pharmacies. These establishments often serve as the final link in the pharmaceutical supply chain, delivering medications to the general public. Previous research on pharmacies has investigated specific services, such as their role in delivering primary care, improving adherence or providing non-prescription medications (Smith, 2009; Agomo, 2012; Perraudin et al., 2016; Costa et al., 2019; Di Novi et al., 2020). Our research, in contrast, analyzes how price competition with online pharmacies affects pharmacy profitability and, consequently, the financial sustainability of their services.

We also contribute to the literature on digitization, particularly the de-

bate surrounding the substitutability of offline and online services (Brynjolfsson and Smith, 2000; Brown and Goolsbee, 2002; Sinai and Waldfogel, 2004; Jin and Kato, 2007; Goldmanis et al., 2010; Cavallo, 2017; Couture et al., 2021) and “digital public health”, which explores how digitization can improve population health (Iyamu et al., 2022; Wong et al., 2022; Yurkovich et al., 2024). In that respect, we investigate the extent to which online pharmacies complement or substitute traditional brick-and-mortar pharmacies in supplying pharmaceuticals to the public (Coenen et al., 2011; an der Heiden and Meyrahn, 2017). Given that patients are price sensitive, policymakers should take into account that price competition between the offline and online channel can have a detrimental effect on the network of brick-and-mortar pharmacies.

The remainder of this paper is structured as follows. Section 2 provides essential background on Germany’s health insurance system, focusing on patient reimbursement, co-payments, pharmacy remuneration, and the role of online pharmacy vouchers. We then present our data and descriptive statistics in Section 3. Our estimation strategy is detailed in Section 4, with the results of our analysis following in Section 5. Finally, Section 6 discusses the limitations of our study, and Section 7 provides concluding remarks.

2 Institutional Background

2.1 Patient Reimbursement and Co-payments for Rx Drugs in Germany

In Germany, most residents are mandated to hold health insurance.² This system offers two primary options: statutory health insurance and private health insurance. As of 2021, around 73.3 Mio. and 8.7 Mio. citizens were members of the statutory and private health insurance scheme, respectively.³ The statutory health insurance is mandatory for all employees below a certain income threshold.⁴ Employees whose income exceeds this threshold may opt out of the statutory health insurance and enroll in private health insurance instead. The same applies to civil servants and self-employed individuals, who are not subject to the threshold. However, individuals who are pri-

²Federal Ministry of Health, <https://t.ly/8Ev6L>.

³Statista, <https://t.ly/pyzwt>.

⁴Further information can be found on the website of the German government: <https://www.bva.bund.de/SharedDocs/FAQs/DE/Bundesbedienstete/Arbeit-Beruf/Entgelt/090jae.html>.

vately insured can only switch back to the statutory health insurance system if their income falls below that income threshold and they are under the age of 55.⁵ Additionally, private health insurers typically conduct a comprehensive health screening of applicants before offering a contract, which allows them to adjust premiums or deny coverage based on pre-existing conditions. Consequently, privately insured individuals are, on average, wealthier and in better health (see, e.g., (Ochmann et al., 2020)).

Rx drugs are prescribed by physicians and dispensed by both traditional and online pharmacies. This holds irrespective of the patients' insurance. Reimbursement, however, differs between statutory and private health insurance. Members of the statutory health insurance are typically required to make co-payments, contributing a portion of the drug's cost. These co-payments are calculated based on the retail price, the pharmacy selling price.⁶ If the pharmacy selling price is . . .

- . . . below EUR 5 the co-payment equals the pharmacy selling price,
- . . . between EUR 5 and EUR 50 the co-payment equals EUR 5,
- . . . between EUR 50 and EUR 100 the co-payment equals 10% of the pharmacy selling price,
- . . . above EUR 100 the co-payment is capped at EUR 10.

Further payments beyond the aforementioned co-payments are possible, which depend on rebates between the patients' insurance company and the drug manufacturers.⁷ The remaining difference between the price of a prescribed drug and the co-payment and any applicable rebates is typically covered by the insurance provider.

Privately insured individuals can select from a range of insurance contracts, each offering distinct reimbursement structures. Typically, these schemes require patients to initially cover costs out-of-pocket, followed by reimbursement from the insurance provider upon submission of invoices. Contracts can be customized to accommodate individual needs, such as making exceptions for high-cost hospitalizations.

⁵For further information, see, e.g., the website of largest German health insurance provider *TK*, <https://www.tk.de/vertriebspartner/faq/neuaufnahme/wechsel-von-pkv-zu-gkv-2064274>.

⁶A more detailed description of the relevant (and regulated) prices can be found in the Appendix, Section A.2.

⁷These rebates fluctuate frequently, often on a quarterly basis, and systematic data on their exact amounts is generally unavailable.

2.2 Pharmacy Remuneration for Rx Drugs

The remuneration for the dispensation of Rx drugs in Germany is regulated. Brick-and-mortar and mail-order pharmacies (including those located abroad), receive a fixed fee of € 8.35 per package, in addition to a variable component that constitutes 3 % of the wholesale price. Over time, several components have been introduced which either increase or decrease remuneration. These components and, as a prerequisite for the analyses presented in Section 5.2, a detailed description on how to compute a pharmacy's remuneration for a given prescription, are presented in the Appendix (Section A.2).

2.3 Online Pharmacy Vouchers

Patients ordering their prescribed Rx drugs from online pharmacies were rewarded with vouchers. Prior to the introduction of the VOASG, basically all prescriptions were eligible. Since the introduction of the law, online pharmacies are no longer allowed to give vouchers for prescriptions that are covered by the statutory health insurance. These vouchers typically offer a value ranging from € 2.50 to € 10. They are redeemable against the purchase of non-prescription products or can be credited to the customer's account.⁸

In economic terms, these vouchers function similarly to a rebate on Rx drugs, with the distinction that their application is restricted to lowering the price of OTC drugs bought from the specific online pharmacy. Given the typically low price elasticity of demand for Rx drugs (see Section 4.1), such a rebate primarily represents a transfer from the pharmacy to the patient, thereby functioning as a mechanism for price competition. However, it is crucial to consider that patients' out-of-pocket payments are only a fraction of the total cost of an Rx drug – the co-payments (see Section 2.1). Moreover, a stated policy objective is to ensure a safe and high-quality supply by pharmacies of medicinal products throughout the country.⁹ That goal might be compromised due to vouchers and the resulting shift in sales away from local pharmacies. In that case, insurance premiums might increase to maintain the minimum standard of services. Consequently, interpreting rebates

⁸For a detailed description, see the homepage of the two largest online pharmacies active in Germany, *Shopapothke* and *DocMorris*, <https://www.docmorris.de/rezepte/rezept-bonus> and <https://www.shop-apotheke.com/lp/rezeptbonus/>. The voucher schemes of the two are fairly similar.

⁹See paras. 33-34 in the ruling C-148/15 of the European Court of Justice, <https://curia.europa.eu/juris/document/document.jsf?text=&docid=184671&pageIndex=0&doclang=EN&mode=lst&dir=&occ=first&part=1&cid=3568174>.

on Rx drugs in the same manner as rebates on conventional goods is inappropriate. Further discussions on the distributional dimension of the partial ban of vouchers can be found in Section 5.2.

In the context of this goal, it is important to note that price competition – including practices such as vouchers, rebates or even promotional gifts¹⁰ – is prohibited among German pharmacies. Consequently, the VOASG only impacted mail-order pharmacies located outside of Germany.¹¹

3 Data and Descriptive Statistics

3.1 Data Handling

Our analyses are based on three data sets. First, and central to the analyses, is a unique data set that comprises information on sales of German brick-and-mortar pharmacies. The data were obtained from three major suppliers of MIS: AWINTA, ADG and Pharmatechnik.¹² MIS oversee the entire system of inventory management and provide both hardware and software solutions to pharmacies, essentially handling the IT infrastructure. Our data includes all transactions conducted by a pharmacist with a customer. Only specialty drugs (e.g. cytostatics) are excluded. The data also contain information on pharmacy selling price, transaction revenue, patient co-payments, and the central pharmaceutical number (PCN) that uniquely identifies each product. To ensure customer anonymity and protect trade secrets, pharmacy location information is limited to the first two digits of the zip code. In Germany, there are 95 two-digit zip code regions.

The MIS dataset encompasses transactions of 9,231 brick-and-mortar pharmacies, covering nearly half of all pharmacies in Germany. The data capture individual sales transactions from January 1, 2018, to October 31, 2022. As a result of data preparation (in particular, balancing the MIS data and merging them with the following two data sets), the number of observations in the actual analyses is lower, as will be explained below.

The second data set contains information on package sizes or doses, which are classified using the N-classification system established by the Fed-

¹⁰See, for example, ruling 076/2019 from June 6, 2019 of the German Federal Court of Justice.

¹¹A small number of German pharmacies also offer mail-order services. As they were already prohibited from offering rebates, the VOASG did not directly affect their pricing policies, though it did alter the overall competitive landscape.

¹²See <https://www.awinta.de>, <https://www.adg.de> and <https://www.pharmatechnik.de>.

eral Institute for Drugs and Medical Devices (BfArM) in Germany.¹³ The N-classification system categorizes pharmaceutical packages into three size classes based on estimated daily doses: N1 (approximately 10 days), N2 (approximately 30 days), and N3 (approximately 100 days).¹⁴ Data on N-classification for each PCN were obtained from the healthcare data and analytics provider IQVIA and from the largest German health insurance fund *Techniker Krankenkasse*.¹⁵

Data on package sizes is used for three reasons. First, package sizes are used as controls in our estimations (see Section 4.2). Second, to ensure our analyses are restricted to pharmaceuticals, we excluded all items from the MIS dataset lacking package size information, i.e., medical devices such as prescribed face-masks, anti-thrombosis stockings and ventilators, or other special items such as Covid-19 vaccines. Third, a robustness check is performed where sales are measured in terms of doses instead of packages (see Appendix, Section A.6).

The third data set comprises information on the evolution of health insurance memberships. The data are obtained from the Federal Ministry of Health and the Association of Private Health Insurance.¹⁶

As a first step of this process of preparing the data, we balanced the MIS data set. To ensure a consistent sample, we excluded pharmacies that were not continuously active throughout the entire observation period. Pharmacies could have exited the sample due to closure or inactivity, a change in legal name, or a switch in their MIS provider. We also excluded pharmacies with minimal sales and those specializing in the supply of expensive medications. To mitigate volatility in prescription data between groups, which can, for instance, be attributed to seasonal patterns (e.g., influenza waves) and the effects of the COVID-19 pandemic (e.g., lockdowns, infection waves), we aggregated the data to an annual level. We excluded 2022 from the analysis, as the data set covers only the first three quarters of that year. As a second step, and as explained above, we used data on N-classifications to ensure

¹³See https://www.bfarm.de/DE/Arzneimittel/Arzneimittelinformationen/Packungsgroessen/_node.html.

¹⁴An alternative would be to use the concept of daily defined doses (DDD) (see, e.g., WHO Collaborating Centre for Drug Statistics Methodology (2024) for more information). However, the availability of information on DDD is limited in our data set.

¹⁵See <https://www.tk.de/resource/blob/2058850/3f65533a18b118a9ebcf585ef2830c40/rabattvertraege-pzn-liste-gesamt-data.pdf>.

¹⁶See <https://www.bundesgesundheitsministerium.de/themen/krankenversicherung/zahlen-und-fakten-zur-krankenversicherung/mitglieder-und-versicherte> and <https://www.pkv-zahlenportal.de/werte/2012/2022/12/pers-kkv/basket/result>

that the data set only contains sales of actual pharmaceuticals.¹⁷ Our data set ranges from 2018–2021 and contains information on 6,396 pharmacies. This represents around 35 % of the total number of pharmacies in Germany over the course of our sample. The estimations presented below are either conducted on the two-digit zip code-level or on the pharmacy-level, which affects the number of observations.

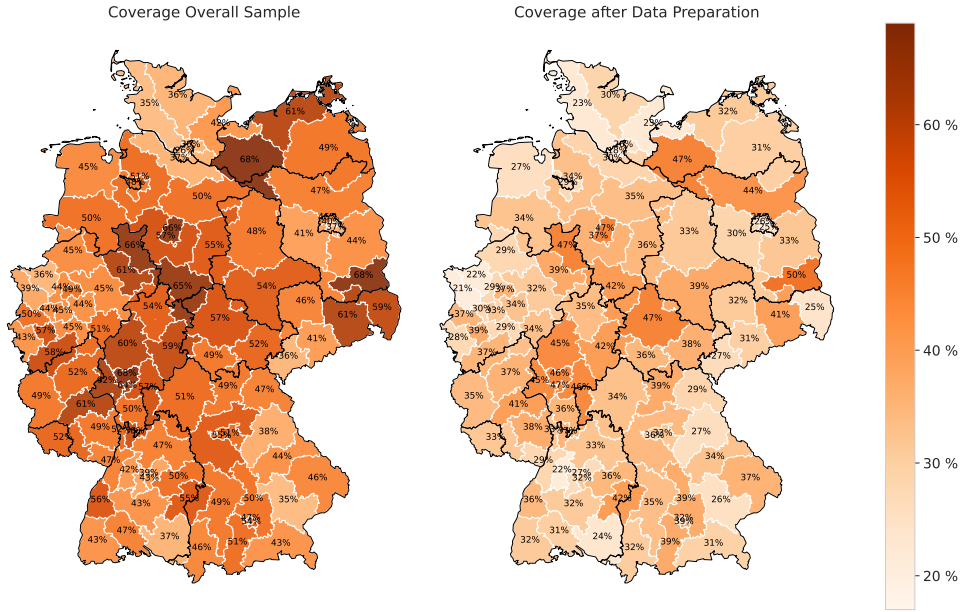


Figure 1: Coverage of Pharmacy Sales Data Across German Regions. Source: MIS suppliers and web page of *Apothekenumschau* (<https://www.apotheken-umschau.de/apothekenfinder/>), scraped on September 29, 2020.

Figure 1 visualizes the geographic scope of pharmacy sales data across Germany. The left panel illustrates the overall sample coverage, encompassing 9,231 pharmacies (roughly 50 % of all German pharmacies during the observation period). The right panel zooms in on the coverage of the final sample, which comprises 6,396 pharmacies (roughly 35 % of all German pharmacies over the observation period). The color intensity within each region corresponds to the percentage of coverage, with darker shades signifying higher data representation. Each two-digit zip code area is outlined in white, and the borders of Germany’s federal states are shown in black. A comparison of both panels reveals that, despite data balancing and processing, our

¹⁷To further validate that, in particular, medical devices are excluded from the data set, we used the German pharmaceutical directory *Gelbe Liste Pharmaindex* (<https://www.gelbe-liste.de/>).

sample retains coverage across all two-digit zip codes and federal states in Germany, with a median coverage of approximately 34 % per two-digit zip code.

3.2 Descriptive Statistics

Table 1 reports summary statistics at the pharmacy level, differentiated between private (Table 1a) and statutory health prescriptions (Table 1b).¹⁸

	N	Mean	SD	Min	Median	Max
Sales in Packages	25,584	4,813	3,163	10	4,070	44,925
Sales in Doses	25,584	298,614	197,482	460	252,505	2,974,320
Gross Revenue in Euro	25,584	270,516	306,129	162	210,954	25,378,764
Net Revenue in Euro	25,584	228,072	258,416	136	177,681	21,326,693
Net Revenue (w.o. lump sum fees) in Euro	25,584	227,170	258,020	134	176,931	21,320,546
Total Remuneration in Euro	25,584	45,634	31,097	85	38,357	851,716
Average Remuneration in Euro	25,584	9.44	0.681	8.5	9.32	32.6
Gross Pharmacy Selling Price in Euro/Package	25,584	54.5	27.7	16.2	49.7	999
Net Pharmacy Selling Price in Euro/Package	25,584	45.9	23.4	13.6	41.9	840
Quantity weighted average doses	25,584	62	5.49	14.3	62.7	82.4

(a) Summary statistics for private prescriptions.

	N	Mean	SD	Min	Median	Max
Sales in Packages	25,584	29,565	15,673	46	26,350	161,496
Sales in Doses	25,584	2,104,195	1,153,345	3,200	1,875,830	13,016,770
Gross Revenue in Euro	25,584	1,727,631	1,229,671	1,094	1,459,565	43,379,709
Net Revenue in Euro	25,584	1,456,591	1,037,334	935	1,230,356	36,453,413
Net Revenue (w.o. lump sum fees) in Euro	25,584	1,407,000	1,018,456	853	1,183,201	36,375,761
Total Remuneration in Euro	25,584	237,891	127,178	343	211,198	1,362,374
Average Remuneration in Euro	25,584	8.07	0.779	7.18	7.92	36.1
Gross Pharmacy Selling Price in Euro/Package	25,584	59.2	31.7	22.8	53.4	1,206
Net Pharmacy Selling Price in Euro/Package	25,584	49.9	26.7	19.5	45	1,013
Quantity weighted average doses	25,584	70.7	6.05	26.8	71.5	88.7

(b) Summary statistics for statutory health prescriptions.

Table 1: Summary statistics on the pharmacy level sample. 51,168 observations in total.

The descriptive statistics include the number of observations (N), mean, standard deviation (SD), median, minimum, and maximum for annual sales in packages or doses, gross revenue, net revenue, net revenue excluding lump sum fees, total remuneration, quantity weighted average doses, average remuneration, and both gross and net pharmacy selling price.

Figure 2 shows the evolution of memberships in the statutory and private health insurance. It can be observed that membership shares remain stable

¹⁸Summary statistics at the two-digit zip code level are reported in Table A.1 in the Appendix.

over the observed period.

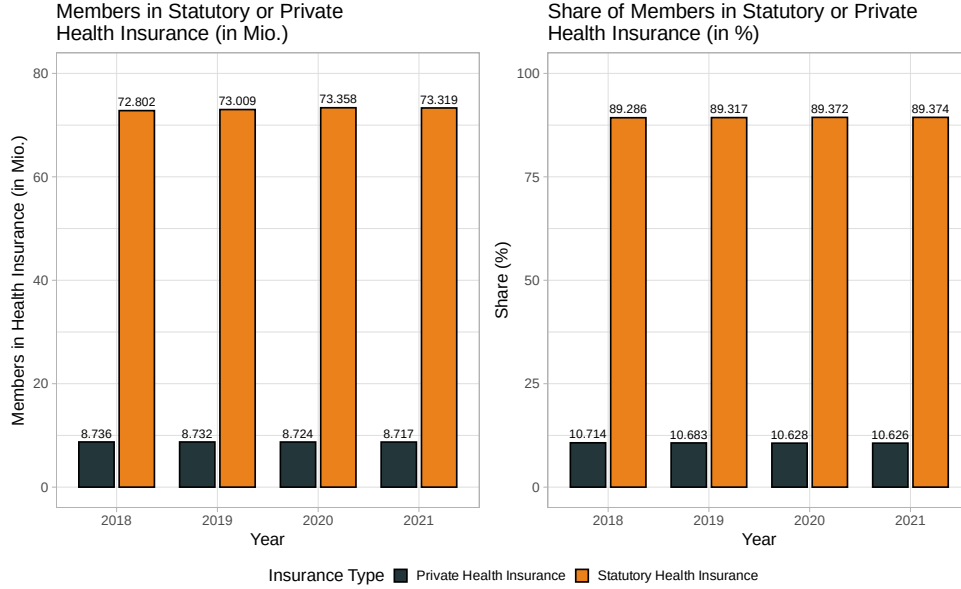


Figure 2: Evolution of private and statutory health insured members.

4 Estimation Strategy

4.1 Identification

In this section, we outline our identification strategy, which leverages the differential impact of the VOASG – introduced in December 2020 – on members compared to non-members of the statutory health insurance system. This natural experiment allows us to apply a difference-in-differences (DiD) approach and to conduct an event study to assess causal effects. Prescriptions to members of the statutory health insurance, directly affected by the VOASG, form the treatment group, while prescriptions to privately insured individuals and self-pay patients, unaffected by the reform, serve as the control group.

Using privately insured and self-pay patients as a control group for the policy analysis at hand is justified because the treatment was explicitly aimed at the statutory health insurance population. The policy change, the introduction of the VOASG, directly impacted the financial incentives for that

population by banning rebates on prescription drugs from mail-order pharmacies. In contrast, the financial incentives for privately insured and self-pay patients in terms of buying drugs remained unchanged; they could still receive rebates from foreign mail-order pharmacies. This makes them a valid control group for a DiD analysis, as they were not exposed to the policy intervention.

The subsequent discussion will elaborate on the characteristics of the market and the two insurance groups vis-à-vis the stable unit treatment value assumption (SUTVA), anticipation and selection effects as well as the common trends assumption.

As described above, a central feature of Rx drugs is that they are prescribed by physicians and dispensed by both traditional and online pharmacies. This general rule applies to all physicians in Germany and holds irrespective of insurance type. Both the private and the statutory health insurance systems generally adhere to standardized reimbursement rates and co-payments for prescribed medications. Members of the statutory health insurance cannot easily switch to private health insurance (see below), and opting to pay out-of-pocket requires them to cover the full price of the medication, which is much higher than the co-payment (see Section 2). Irrespective of the specificities of the insurance schemes, drug prices are the same in offline and online channels. The only difference in the price dimension is that, after the VOASG came into effect on December 15, 2020, members of the statutory health insurance were no longer entitled to rebates in the form of vouchers for online purchases. However, these pharmacies are still permitted to provide rebates to privately insured consumers.¹⁹ We use exactly this policy change in the price dimension as a natural experiment for our analyses. It is also important to note that our analyses are conducted on an annual basis and prescriptions are valid for only 28 days for members of the statutory health insurance and at most three months for private prescriptions.²⁰ Prescriptions are issued to individual patients and filled independently; the sale of one prescription does not affect the likelihood or volume of other prescriptions being written or dispensed, either within or across pharmacies. These characteristics make interferences or spillovers across units, as well as anticipation and selection effects implausible (for instance, “Ashenfelter dip”; see (Ashenfelter, 1978; Heckman and Smith, 1999; Roth et al., 2023; Abbring

¹⁹More information are provided by the German Federal Ministry of Health, see <https://www.bundesgesundheitsministerium.de/apotheken.html>.

²⁰See <https://t1p.de/iu100>. Yearly aggregation also avoids the risk of confounding treatment effects with timing distortions which could arise from the privately insured delaying filling or bundling prescriptions over weeks.

and Van den Berg, 2003; Malani and Reif, 2015)).

There is also no indication that systematic *level* differences between members of the statutory health insurance and privately insured or self-pay patients would lead to a violation of the common trend assumption or a lack of overlap in characteristics (common support assumption). Those differences may arise from the specificities of each insurance scheme outlined in Section 2 (type of employment, income thresholds, insurance contracts, and health screening). Rather than systematic level differences, the common trend assumption rests on a parallel evolution over time, which is discussed in this section. Moreover, given the institutional rules and barriers to switching, members of the statutory health insurance could not simply switch to private health insurance (and vice versa, see above), *noncompliance* is not a concern in our analyses. This is corroborated by the observation that the group composition did not change over the observation period (see, Figure 2). Overall, the policy can thus be considered exogenous to the individuals' choice regarding insurance schemes.²¹

To wrap up the discussion on the characteristics of the market and the health insurance schemes in Germany, we briefly discuss the role of co-payments and rebates from the viewpoint of the patients. The results presented below should be interpreted with the understanding that patients are unlikely to forgo necessary medication solely because of the absence of rebates. The demand for prescription drugs is generally inelastic due to their nature (Gatwood et al., 2014; Yeung et al., 2018): a patient's need for medication is often diagnosed by a physician and is not easily deferred.

As health insurance typically covers the majority of the costs, co-payments are of minor importance. Furthermore, low-income patients and those with chronic diseases can be exempt from co-payments.²² Thus, while rebates potentially influence a patient's choice of pharmacy (online or offline), they should not impact overall drug consumption. Given the inelastic nature of demand, any shift in offline sales is expected to be accompanied by a similar shift in online sales. We thus interpret an increase in offline sales as a shift from the online to the offline channel.

Lastly, we quantify the market impact of the VOASG by examining the number of packages dispensed. This metric is of primary importance to our

²¹We also conducted several sensitivity analyses indicating that the findings are maintained even when allowing for some degree of unobserved post-treatment violation of parallel trends (see Appendix, Section A.4, esp. Figure A.3).

²²For further information, see the website of the Federal Ministry of Health, <https://www.bundesgesundheitsministerium.de/zuzahlung-und-erstattung-arzneimittel.html>.

analyses because pharmacy compensation is directly linked to it, and we will use the results of the DiD estimation to assess the additional profits of pharmacies with varying revenue levels.²³

4.2 Static and Dynamic TWFE

Following the distinction in Roth et al. (2023, Chapter 3.2), this section presents the estimation equations of our static and dynamic two-way fixed effects (TWFE) analyses. The dynamic TWFE, also referred to as event study design, contains leads and lags of the treatment, which allows us to capture dynamic effects and assess pre-trends.²⁴ The estimation equation for the dynamic TWFE model (event study) reads as follows:

$$\ln(y_{pgt}) = \alpha_{pg} + \gamma_t + \sum_{\tau \neq 2020} \beta_{\tau} D_{g\tau} + W'_{pgt} \mu + \epsilon_{pgt}. \quad (1)$$

Here, y denotes sales for pharmacies p and statutory health or private prescriptions g at time t (in years). The outcome variable y is measured in logs. Fixed effects α_{pg} and γ_t are included to capture cross-sectional heterogeneity for each combination of p and g , as well as time-varying effects that contemporaneously affected all pharmacies p and prescription types g . The time effects account for common macro-level shocks, such as national lockdowns during the pandemic. The term $\sum_{\tau \neq 2020} D_{g\tau}$ represents indicator variables that take the value of one if prescription g is a statutory health prescription in the period 2018 to 2021. The model includes 2018-2019 as leads and 2021 as a lag. As is standard in the literature (see Cunningham (2021, Chapter 9.4) or Freyaldenhoven et al. (2019)), the event study is normalized to the last pre-intervention period ($\tau = 2020$), which is therefore excluded from the summation. The coefficient β_{2021} in Equation (1) refers to the average treatment effect on the treated (ATT).²⁵ The coefficients $\beta_{\tau} \in \{2018, 2019\}$ capture the pre-treatment effects, which allow us to examine dynamics prior to the policy intervention.

Covariates are included in W_{pgt} to control for observable differences between pharmacies (or prescription types) whose outcome trends might otherwise diverge. By conditioning on these factors, we strengthen the plausibil-

²³Robustness checks using doses instead of packages are reported in the Appendix, Section A.6.

²⁴The estimations are conducted using R's *fixest* package (Bergé, 2018).

²⁵The ATT of the dynamic TWFE can be interpreted in a similar way as the ATT of the static TWFE estimation presented below because there is only one post-treatment period.

ity that the (conditional) parallel trends assumption holds. In the dynamic TWFE, W_{pgt} contains the quantity-weighted average of doses per pharmacy p and group g at time t .²⁶ (In the static TWFE, W_{pgt} contains further covariates, see below.) This covariate is calculated based on Germany’s N-classification system. As explained in Section 3.1, this system categorizes packages into three sizes: N1 (10 doses), N2 (30 doses), and N3 (100 doses). While not exact, this system provides a reasonable approximation of doses per package. To account for potential variations in package sizes that could influence our results, we calculate a weighted average of package sizes at the two-digit zip code or pharmacy level for each group. For instance, a combination of 15 N1 and 5 N2 packages (a total of 20 packages) would equate to approximately 300 doses. This corresponds to an quantity weighted average of 15 doses per package (300 doses/ 20 packages). (The appendix (Section A.6) contains a robustness check, where package size instead of number of packages sold is used as the dependent variable.)

Since the outcome variable is measured in logs, β_τ is interpreted as a percentage change. The exact ATT reads $e^{\beta_\tau} - 1$, although this transformation is negligible when the effect is close to zero, as it is in our case.

In the remainder of this section, we will present the static TWFE specification. This is a restricted version of the dynamic TWFE/event study specification in which the treatment is summarized by a single post-treatment indicator. The static TWFE estimation equation reads

$$\ln(y_{pgt}) = \alpha_{pg} + \gamma_t + \beta D_{gt} + W'_{pgt}\mu + \epsilon_{pgt}, \quad (2)$$

where the notation is similar as in Equation (1). Similar to the event study, D_{gt} is an indicator variable that equals one if g represents a statutory health prescription at time $t = 2021$. The coefficient β represents the ATT.

Equation (2) is estimated in two variants. First, we include the share of members of the statutory health insurance relative to the total of insured individuals at the national level as a control in W_{pgt} (see Column (A) in Section 5.1). This covariate is measured at the national level and varies annually. Second, a group-specific (treatment or control) time trend is included (see Column (B) in Section 5.1). Both variants control for potential confounding effects of shifts in insurance enrollment, even though such effects are unlikely, as indicated by Figure 2.

²⁶Our results are robust to the inclusion of additional socio-demographic information (household composition, size and income) as covariates, see the Appendix, Section A.7.

4.3 Heterogeneity Analysis

As explained in Section 1, the VOASG was introduced to strengthen the comprehensive supply of pharmaceuticals to the general population during a period of a shrinking number of brick-and-mortar pharmacies. To evaluate the effects of the policy change, we extend the analysis by computing the additional remuneration generated by the introduction of the VOASG on pharmacies with heterogeneous revenues.

Economic theory predicts that, all else equal, pharmacies with lower revenues are more exposed to market exit. We therefore categorize pharmacies into deciles based on their revenue. The first decile includes the 10% of pharmacies with the lowest revenue, the second decile includes the next 10% with the second-lowest revenue, and so on.

For this analysis, we removed outliers from the dataset to mitigate the influence of potential data errors on the revenue deciles. Given that distributional stratifications are sensitive to extreme observations at the tails, this step ensures more stable subgroup assignments and coefficient estimates. Appendix A.9 presents a comparison of the results.²⁷

We tested whether the ATTs differ between pharmacies with different revenues. In doing so, we incorporated an interaction term between the treatment indicator and dummies for the respective revenue deciles into Equation (2). A subsequent Wald test was conducted to evaluate the null hypothesis that the resulting decile-specific ATT estimates were jointly equal to a single, average ATT (Cameron and Trivedi, 2005, Chapter 7.2). This hypothesis could not be rejected. (A detailed discussion is provided in the Appendix, Section A.9.) Based on this finding, we calculate the average additional profits for each pharmacy using the ATT estimate from the TWFE model (2), and present the distribution by decile. The results of this analysis are presented in Section 5.2.

²⁷Appendix A.8 demonstrates that the results of the static and dynamic TWFE estimations are robust to outliers.

5 Results

5.1 Static and Dynamic TWFE

Table 2 provides an overview of the estimated coefficients measuring the effect of ban on rebates due to the VOASG (see Equation (1)).²⁸ The table contains four different specifications. Column (1) presents results aggregated at the *two-digit zip code level*. Columns (2) to (4) reports the results for an aggregation at *pharmacy-level*. The first and second specifications (columns (1) and (2)) respectively contain a fixed effect on the zip code and pharmacy level, each one with robust standard errors.²⁹ The third and fourth specifications (columns (3) and (4)) contain a fixed effect at the pharmacy level, with standard errors clustered at the zip code level and at a combination of the zip code and prescription type level, respectively.

	(1)	(2)	(3)	(4)
Lead 2018	-0.0036 (0.0034)	-0.0047* (0.0028)	-0.0047** (0.0021)	-0.0047 (0.0034)
Lead 2019	0.0003 (0.0025)	0.0017 (0.0020)	0.0017 (0.0012)	0.0017 (0.0022)
Lag 2021	0.0161*** (0.0038)	0.0178*** (0.0027)	0.0178*** (0.0015)	0.0178*** (0.0033)
Weighted Average of Doses	0.0033 (0.0026)	-0.0023* (0.0014)	-0.0023 (0.0019)	-0.0023 (0.0022)
Observations	760	51,168	51,168	51,168
Adj. R2	0.9997	0.9906	0.9906	0.9906
FE: Year	X	X	X	X
FE: 2 digit zip code & prescription type	X			
FE: Pharmacy & prescription type		X	X	X
Std. Errors	Robust	Robust	by: 2-digit-zip code	by: 2-digit-zip code & prescription type
<i>Note:</i> Standard Errors in parenthesis. * p < 0.1, ** p < 0.05, *** p < 0.01				

Table 2: Estimation results for Equation (1).

In what follows, we use Column (4) as our preferred specification, i.e., standard errors clustered at the two-digit zip code and prescription-type level.

²⁸To ensure the robustness of our findings, we performed a simplified DiD analysis by directly comparing the pre- and post-VOASG mean sales between the treatment and control groups. The results of this analysis, which align with those presented in the main text, can be found in the Appendix (Section A.3).

²⁹Note that aggregating the data at the zip code level (column (1)) results in a lower number of observations (95 two-digit zip code regions observed over four years, differentiated by statutory versus private prescriptions) than aggregation at the pharmacy level (column (2)), as several pharmacies are located within the same zip code.

This accounts for spatial correlation in unobserved shocks among pharmacies in the same local market and correlation within prescription-type groups. The latter is especially relevant because statutory health prescriptions and private prescriptions operate under different institutional regimes (see Section 2). These institutional differences potentially generate correlated shocks, i.e. demand shocks within each prescription group. The ATT resulting from our preferred specification in the dynamic TWFE estimation reads 0.0178. Figure 3 presents a graphical representation of the event study estimates.

Table 2 shows that the pre-treatment coefficients δ_{2018} and δ_{2019} are statistically insignificant and close to zero in our preferred specification (4). In the alternative specifications (2) and (3), which are not used in the following analyses, the lead for 2018 is significant at the 10 %-level and 5 %-level, respectively. The table also shows that the ATTs are similar across the two aggregation schemes.³⁰

Overall, the statistical insignificance of the pre-treatment coefficients provides initial evidence that parallel trends are plausible (Roth et al., 2023, Sections 4.3–4.4). However, to validate this, we conduct sensitivity analyses using the power diagnostic framework of Roth (2022) and the robust inference methods of Rambachan and Roth (2023). The power diagnostics indicate that our results are unlikely to be driven by pre-testing bias or low statistical power (see Appendix, Figure A.2). Furthermore, the robust inference analysis – which allows for valid causal inference even if parallel trends do not hold exactly – demonstrates that the estimated treatment effects remain statistically significant even when allowing for substantial post-treatment deviations (nearly 1.75 times the magnitude of the maximum observed pre-treatment difference; see Appendix, Figure A.3). These detailed robustness checks are presented in the Appendix, Section A.4.

³⁰Additionally, the results remain robust when outliers are removed (see Appendix, Tables A.7 and A.8).

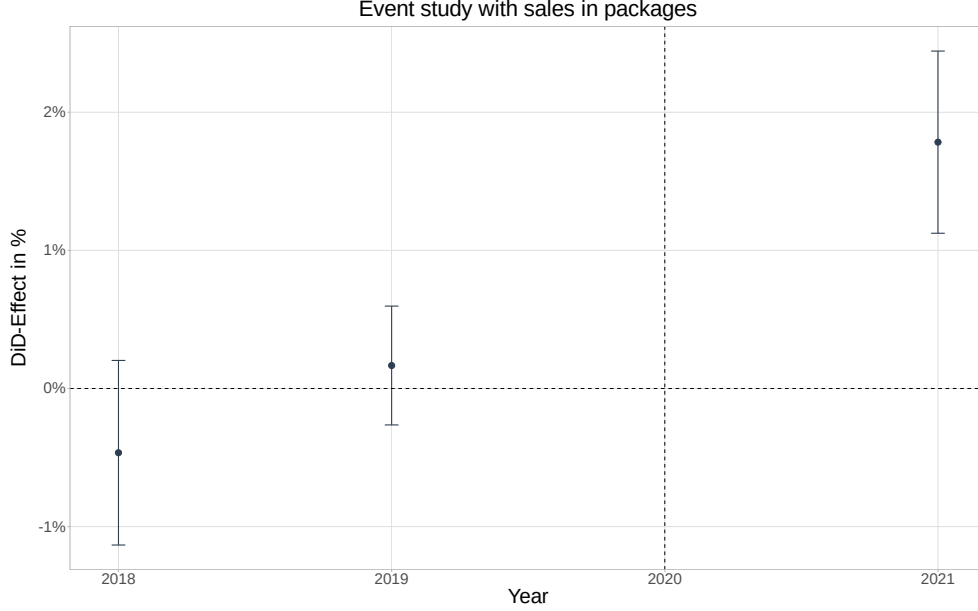


Figure 3: Event study results with 95 % confidence intervals based on Table 2 column (4).

Table 3 presents the results of the static TWFE model (see Equation (2)).³¹ We estimate two model specifications: a baseline model (A) and a model with a time-trend (B).

Table 3 shows an ATT of 0.0167 without a time trend (Column A) and 0.0142 with a time trend (Column). All results are statistically significant at the 1 % level. Thus, sales in brick-and-mortar pharmacies increased by 1.42 % to 1.67 % due the ban on Rx rebates introduced by the VOASG compared to a counterfactual scenario without the policy. This suggests that, even though the *overall demand* for Rx drugs is inelastic (Gatwood et al., 2014; Yeung et al., 2018), a notable portion of patients is price sensitive when it comes to the *choice of the retail channel*. Thus Rx rebates lead to patients switching to that sales channel where Rx drugs are offered more cheaply.³²

³¹The Appendix, Section A.5, contains the output table including all four specifications, see above.

³²Industry data indicate a 21.5% reduction in reimbursements from statutory health insurance providers to foreign mail-order pharmacies (ABDA, 2023, p. 19). Considering the estimated small market share of these pharmacies in the prescription drug market (approximately 1% as reported by Albrecht et al. (2020), depending on the basket of pharmaceuticals considered), the estimated ATT appears plausible.

	Column A: Without Trend	Column B: With Trend
ATT	0.0167*** (0.0034)	0.0142*** (0.0037)
Share of Statutory Health Insured Members	2.2213 (1.9185)	
Weighted Average of Doses	-0.0022 (0.0021)	-0.0022 (0.0022)
Time Trend of Treatment-Group		0.0023 (0.0017)
Observations	51,168	51,168
Adj. R2	0.9906	0.9906
FE: Year	X	X
FE: Pharmacy & prescription type	X	X
Std. Errors	by: 2-digit-zip code & prescription type	by: 2-digit-zip code & prescription type

Note:

Standard Errors in parenthesis.

* p < 0.1, ** p < 0.05, *** p < 0.01

Table 3: Results of the DiD-estimation.

5.2 Heterogeneity Analysis

In this section, we compute the average additional profits that pharmacies in each revenue decile realized per year due to the introduction of the VOASG. As explained in Section 4.3, we use the ATT that results from Estimation 2 to quantify the impact of the policy, in particular, the ATT obtained from the static TWFE estimation with a time trend.³³ The results are presented in Figure 4 and reveal a notable heterogeneity in the magnitude of the effects across different revenue deciles.

Figure 4 illustrates the distribution of the absolute impact of the partial ban on rebates across pharmacies with varying revenue levels. This figure was constructed by multiplying each pharmacy's remuneration for dispensing Rx drugs by the ATT 0.0149.

The dotted lines in Figure 4 represent the average increase in profits for each decile. Our findings suggest that, on average, the rebate ban generated additional annual profits for pharmacies in the first decile of around €1,587. In contrast, the corresponding gain for pharmacies in the tenth decile is roughly four times greater, reaching €6,077. Figure 4 also indicates that 50 % of the pharmacies experienced an increase in profits of less than €3,246

³³Note that for the analyses in this section, outliers were removed from the data set (see Section 4.3). The respective ATT for the analysis in this section is 0.0149, which differs slightly from the ATT of 0.0142 reported in Table 3, as shown in Table A.7, Panel B, Column (4). Further analyses, including a computation of additional profits using decile specific ATTs and retaining outliers, can be found in the Appendix, Section A.9.

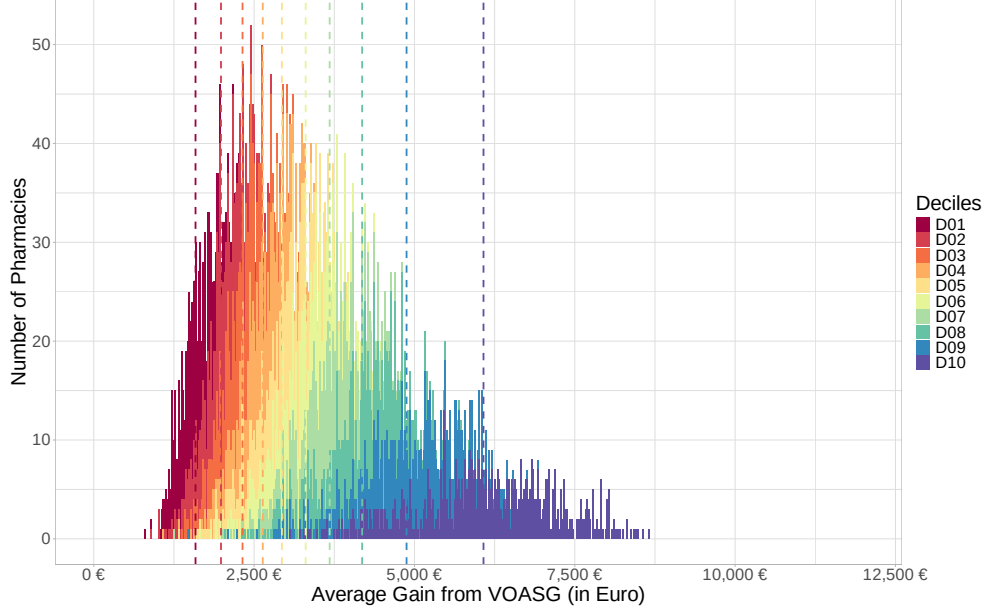


Figure 4: DiD coefficient $\beta = 0.0149$ from Table 3, Panel B. *Average Gain from VOASG* = $Sales \times \frac{\beta}{1+\beta} \times \text{Average Remuneration Per Sale}$.

(median). In contrast, the average gain across all pharmacies is at €3,500. The distribution of the average gains from the policy between deciles is left-skewed, which indicates that these gains were unevenly distributed across pharmacies, with *larger* pharmacies actually benefiting more strongly.

These results can be further contextualized. As discussed previously, pharmacies exit the market when opportunity costs exceed revenues. The question, therefore, is whether the additional profits are meaningful enough to sustain pharmacies in the market. To evaluate this, consider that the average *monthly* income of a German employee in 2021 was €4,100 (Destatis, <https://t.ly/N2dJq>). In contrast, the *annual* gain for pharmacies in the first decile approximates 40 % of this amount. The median pharmacy experienced an increase equivalent to 80 % of this figure. In other words, pharmacy owners' yearly gains were substantially less than the average monthly wage of an employee. Given that these owners are highly skilled professionals, the impact of the VOASG on their revenues can be considered relatively low.

Against the backdrop of these findings, the continued decline in the number of brick-and-mortar pharmacies after 2020 is unsurprising. By the end of 2024, the total number had decreased to 17,041, reflecting a 9.1 % reduction compared to 2020, where that number was 18,753 (ABDA, 2025). While external factors, such as the war in Ukraine and the pandemic, influenced the

market, the policy change appears to have failed to address the underlying causes of pharmacy closures.

Using the estimated ATT (see Section 5.1), we also computed the foregone savings of consumers due to the ban on rebates. In doing so, we assume that the partial ban of vouchers leads to an increase in expenditure of those patients who, without the policy change, would have purchased online. Given that vouchers historically ranged between €2.5 and €10, the estimated aggregate foregone savings of those prescriptions that are no longer filled online amount to €9.1 million in our sample.

This value should not be directly interpreted as a net loss in consumer surplus for the following reason. The German regulatory framework designates pharmacies as fulfilling a central public health mandate, specifically ensuring a “safe and high-quality supply of medicinal products to the German population” (ECJ ruling C-148/15, par. 31). Competition from mail-order pharmacies potentially erodes the revenue base (rents) necessary for maintaining the required density of the brick-and-mortar network. Should the viability of this network be compromised, supplementary funding would be necessary to maintain the minimum standard of service. These additional costs would most likely be drawn from the health insurance system. Accordingly, aggregate patient savings from Rx rebates could potentially be offset by higher insurance premiums necessary to subsidize and sustain the stationary pharmacy infrastructure. The political and distributional dimensions of Rx rebates are beyond the scope of this article.

6 Limitations

Our study is subject to certain limitations. While it relies on the most comprehensive and detailed dataset available on the German pharmacy market (to our knowledge), the data is limited to the two-digit zip code level. Consequently, we are unable to further specify the precise location of a given pharmacy. If the data were more granular, we could have conditioned the effects on specific socio-geographic factors, such as rural versus urban areas and the specific competitive landscape of offline pharmacies.

A comparable dataset encompassing online sales is currently unavailable. Our interpretation, which posits that e-commerce experiences losses equivalent to the gains of the offline channel, is thus contingent on the assumption of inelastic demand.

It is essential to consider these limitations when interpreting our findings. However, these caveats do not affect the validity of our identification strategy.

7 Conclusion

We find that the partial ban on rebates led to an increase in offline sales of around 1.42 to 1.67 % for an average brick-and-mortar pharmacy compared to a counterfactual scenario in which rebates would not have been banned. Even though the aggregate demand for Rx drugs is inelastic (see, e.g., Gatwood et al. (2014); Yeung et al. (2018)), this finding shows that patients are price-sensitive when selecting their retail channel for purchasing them. Moreover, with an inelastic total demand for Rx drugs, the increase in offline sales corresponds to a decrease in online sales by the same amount. Our findings reveal that a substantial portion of consumers respond to price differences and rebates for Rx drugs by selecting the more affordable retail channel. This insight is particularly relevant in light of the 2016 European Court of Justice ruling (Case No.: C-148/15) mentioned above, which legalized rebates offered by foreign online pharmacies. In this ruling, the judges claimed that the German government had failed to show that a system of uniform Rx prices was an effective tool to achieve the alleged goal of securing the comprehensive supply of pharmaceuticals to the general population. Our research provides empirical evidence that consumers are price-sensitive, suggesting that online rebates could potentially erode offline sales and profitability.

The policy change appears to have had an asymmetric effect on pharmacies. We estimated that annual additional profits ranged from €1,587 - €6,077. Notably, the *additional annual profit* for the lowest decile equates to around 40 % of the average *monthly* income of an employee in Germany in 2021. The findings also indicate that the majority of pharmacies experienced only a mild increase in profits, with a median increase in profits of €3,246. Pharmacies in the top revenue decile experienced an increase in remuneration that was almost four times stronger than the increase in lowest decile. This suggests that larger pharmacies benefited disproportionately from the rebate ban compared to smaller pharmacies.

These findings suggest that while implemented with the goal of supporting at-risk pharmacies, the law alone is only one part of a broader policy to mitigate market exit. This is corroborated by the developments in 2021-2023, when another 7.7 % of pharmacies closed. This suggests that further reforms are necessary to reverse the trend of declining pharmacy numbers. Such reforms can, for instance, specifically target pharmacy remuneration for dispensing Rx drugs.

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A Appendix

A.1 Additional Descriptive Statistics

	N	Mean	SD	Min	Median	Max
Sales in Packages	380	324,031	142,870	49,170	318,565	811,735
Sales in Doses	380	20,104,566	8,895,330	2,943,970	19,632,930	52,735,480
Gross Revenue in Euro	380	18,212,872	8,596,615	2,352,004	17,272,254	47,405,812
Net Revenue in Euro	380	15,355,223	7,252,049	1,976,448	14,514,720	39,837,398
Net Revenue (w.o. lump sum fees) in Euro	380	15,294,487	7,225,670	1,965,727	14,452,409	39,659,807
Total Remuneration in Euro	380	3,072,350	1,361,241	455,879	2,990,969	7,735,853
Average Remuneration in Euro	380	9.47	0.201	9.1	9.44	11.6
Gross Pharmacy Selling Price in Euro/Package	380	55.8	8.15	40.8	54.4	142
Net Pharmacy Selling Price in Euro/Package	380	47	6.91	34.3	45.9	120
Number of Pharmacies per 2 digit zip-code	380	67.3	24.9	15	63	140
Quantity weighted average doses	380	61.9	2.88	49.8	62.2	67.9

(a) Summary statistics for **private prescriptions**.

	N	Mean	SD	Min	Median	Max
Sales in Packages	380	1,990,476	768,530	268,410	1,889,334	4,567,226
Sales in Doses	380	141,667,669	55,842,314	16,980,600	137,918,450	352,740,100
Gross Revenue in Euro	380	116,315,039	45,882,895	32,142,838	112,818,147	302,983,105
Net Revenue in Euro	380	98,066,905	38,704,371	27,010,156	95,233,452	254,601,598
Net Revenue (w.o. lump sum fees) in Euro	380	94,728,159	37,466,388	26,546,225	92,013,684	246,803,740
Total Remuneration in Euro	380	16,016,291	6,168,609	2,582,257	15,460,563	37,608,010
Average Remuneration in Euro	380	8.06	0.237	7.76	8.02	10.1
Gross Pharmacy Selling Price in Euro/Package	380	59.2	9.63	46.9	57.4	142
Net Pharmacy Selling Price in Euro/Package	380	49.9	8.13	39.4	48.4	119
Number of Pharmacies per 2 digit zip-code	380	67.3	24.9	15	63	140
Quantity weighted average doses	380	71.1	3.15	62.6	70.8	79.3

(b) Summary statistics for **statutory health prescriptions**.

Table A.1: Summary statistics on the two-digit-zip code sample. 760 observations in total.

A.2 Remuneration in Detail

When dispensing an Rx drug, pharmacists receive a fixed fee of € 8.35 per package and a variable fee of 3 % of the wholesale price. The latter is referred to as AEP (§3 AMPreisV). For prescriptions covered by statutory health insurance, this remuneration is reduced by a gross lump-sum fee of € 1.77 (net € 1.49; see §130 (1) SGB V) paid to the insurance company, provided that remuneration is paid within 10 days.

The remuneration of pharmacies for dispensing Rx drugs is regulated and can be computed based on the pharmacy selling price AVP:

$$(Gross) AVP = (1 + VAT) \cdot (Net) AVP \quad (A.1)$$

$$(Net) AVP = r_f + r_v + pDL + nDZ + AEP. \quad (A.2)$$

As shown in Equation (A.1), gross AVP is calculated by multiplying net AVP by the value-added tax rate, which is 19% in Germany (16% for the first two quarters of 2020). Net AVP, Equation (A.2), consists of a fixed rate, $r_f = \text{€ } 8.35$, in addition to a variable component, $r_v = 0.03 \cdot AEP$. Recall that AEP refers to the wholesale price. Moreover, AVP includes two lump-sum fees per prescription drug in net terms: a fee for so-called pharmaceutical services (*pharmazeutische Dienstleistung* (pDL)) of $pDL = \text{€ } 0.20$, and a fee for emergency pharmacy services (*Notdienstzuschlag* (nDZ)) of $nDZ = \text{€ } 0.20$.^{A1} Rearranging Equation (A.2) gives the wholesale list price AEP as a function of the AVP and the other components of a pharmacy's remuneration:

$$AEP = \frac{(Net) AVP - (r_f + pDL + nDZ)}{1.03}. \quad (A.3)$$

Based on AEP, we can compute the compensation per Rx-drug dispensed:

$$\begin{aligned} R(AEP) &= \underbrace{(Net) AVP - pDL - nDZ - STHF}_{(Net) AVP \text{ w.o. lump sum fees}} - AEP \\ &= r_f + r_v - STHF \\ &= \text{€ } 8.35 + 0.03 \cdot AEP - \text{€ } 1.49. \end{aligned} \quad (A.4)$$

Here, $STHF = \text{€ } 1.49$ denotes the additional net lump-sum fee per statutory health insurance prescription drug (with $STHF = 0$ for private prescriptions). For example, an Rx-drug prescribed under statutory health insurance with $AEP = \text{€ } 50$ results in a remuneration of $R(50) = \text{€ } 8.36$.

Regarding the technical implementation, our analysis requires aggregating the previously introduced metrics. We first construct these metrics at the granular transaction level (PCN) and then sum them to the two-digit zip code or pharmacy level p , differentiated by group g , and year t .

We define *Gross Revenue* as *Gross AVP* multiplied by sales and *Net Revenue* as *Net AVP* multiplied by sales. We also calculate a *Net Revenue without lump sum fees* by taking the *Net AVP* excluding the pDL , nDZ , and

^{A1}A detailed description can be found on the homepage of the Federal Union of German Associations of Pharmacists (ABDA): <https://www.abda.de/apotheke-in-deutschland/preise-und-honorare/beispielrechnung/>; only available in German language.

STHF elements, multiplied by sales. On the cost side, *Total Variable Costs* are defined as the *AEP* multiplied by sales. Based on the *AEP* and sales, we derive the *Total Remuneration*. We also calculate the *Average Remuneration per Sale* by dividing the *Total Remuneration* by the corresponding sales volume.

In Tables A.1 and 1, we provide descriptive statistics for these values, differentiated by private and statutory health prescriptions. Finally, Table A.2 summarizes the results by decile. The deciles themselves are constructed based on the *Total Net Revenue* (the sum of *Net Revenue* from private and statutory health prescriptions) for each pharmacy p and year t .

	Deciles	Mean	SD	Min	P25	Median	P75	Max
Gross Revenue (in Euro) from Statutory Health Prescriptions by Pharmacy in 2021	D01	580,080	156,013	15,254	491,653	603,985	698,144	854,951
	D02	845,066	104,747	375,124	786,548	850,854	917,596	1,075,456
	D03	1,030,760	105,772	649,517	969,128	1,043,482	1,104,922	1,257,101
	D04	1,194,577	112,944	519,110	1,131,587	1,204,685	1,273,092	1,425,791
	D05	1,375,613	129,665	790,864	1,305,537	1,394,115	1,459,279	1,642,521
	D06	1,567,512	149,516	600,022	1,498,459	1,587,123	1,672,184	1,858,956
	D07	1,797,739	162,622	375,326	1,714,875	1,808,712	1,905,814	2,151,529
	D08	2,093,037	191,242	1,330,587	1,991,520	2,107,104	2,213,523	2,512,798
	D09	2,584,094	277,183	1,006,973	2,400,928	2,570,882	2,777,514	3,255,386
	D10	4,300,888	2,459,499	1,356,382	3,215,381	3,669,349	4,499,821	43,379,709
Net Revenue (in Euro) from Statutory Health Prescriptions by Pharmacy in 2021	D01	487,448	131,100	12,818	413,153	507,532	586,653	718,418
	D02	710,125	88,023	315,225	660,962	714,970	771,032	903,745
	D03	866,161	88,882	545,816	814,387	876,749	928,504	1,056,386
	D04	1,003,823	94,906	436,225	950,918	1,012,339	1,069,819	1,198,048
	D05	1,155,948	108,966	664,572	1,097,087	1,171,471	1,226,262	1,380,185
	D06	1,317,203	125,639	504,220	1,259,211	1,333,716	1,405,173	1,562,156
	D07	1,510,672	136,647	315,400	1,441,071	1,519,767	1,601,474	1,808,006
	D08	1,758,817	160,707	1,118,132	1,673,543	1,770,661	1,860,093	2,111,606
	D09	2,171,473	232,929	846,196	2,017,586	2,160,422	2,334,044	2,735,619
	D10	3,615,027	2,068,273	1,139,815	2,701,990	3,083,480	3,781,260	36,453,413
Net Revenue w.o. lump sum fees (in Euro) from Statutory Health Prescriptions by Pharmacy in 2021	D01	467,598	126,278	12,339	395,049	488,781	562,332	689,964
	D02	682,142	84,526	301,853	634,221	685,886	738,775	871,718
	D03	832,989	85,490	523,595	785,060	843,612	892,430	1,020,360
	D04	965,571	91,483	420,673	913,836	974,303	1,030,302	1,166,132
	D05	1,112,873	104,927	646,402	1,055,069	1,128,102	1,180,965	1,347,118
	D06	1,268,841	120,979	492,045	1,213,262	1,281,520	1,352,468	1,507,382
	D07	1,456,848	132,929	310,243	1,389,072	1,463,156	1,544,132	1,736,182
	D08	1,696,339	155,664	1,077,493	1,610,803	1,709,882	1,794,451	2,049,326
	D09	2,098,092	226,555	826,700	1,949,226	2,089,525	2,255,999	2,650,902
	D10	3,518,845	2,063,884	1,132,121	2,605,533	2,986,705	3,686,305	36,375,761
Total Remuneration (in Euro) from Statutory Health Prescriptions by Pharmacy in 2021	D01	91,027	26,791	811	75,108	92,412	109,882	161,093
	D02	128,985	24,592	34,619	112,844	129,671	146,051	199,690
	D03	153,614	26,194	54,756	136,446	154,370	169,928	232,846
	D04	177,283	28,328	72,882	159,305	177,330	195,997	279,156
	D05	200,387	35,326	72,548	178,379	205,176	222,864	336,177
	D06	225,541	39,135	61,799	201,654	231,378	252,488	351,801
	D07	252,320	43,782	29,144	230,213	256,633	282,355	395,502
	D08	293,036	53,458	111,920	262,928	298,828	326,168	438,986
	D09	347,264	70,089	100,193	304,126	354,230	395,881	548,914
	D10	477,572	159,606	62,925	383,579	464,683	545,987	1,362,374
Total Net Revenue (in Euro) per Pharmacy from Rx in 2021	D01	578,794	137,690	13,900	506,428	610,769	686,318	742,372
	D02	837,992	54,275	743,267	790,458	840,029	887,387	926,583
	D03	1,012,171	47,992	927,058	971,426	1,012,312	1,053,558	1,093,675
	D04	1,170,606	45,439	1,093,754	1,131,899	1,170,290	1,208,243	1,253,679
	D05	1,342,582	49,182	1,254,728	1,301,047	1,342,594	1,386,771	1,425,902
	D06	1,520,760	54,344	1,426,486	1,471,885	1,523,097	1,567,248	1,616,638
	D07	1,736,137	69,247	1,617,787	1,677,458	1,733,688	1,796,548	1,859,943
	D08	2,020,544	101,603	1,860,461	1,931,778	2,014,162	2,102,377	2,210,105
	D09	2,487,262	178,349	2,211,326	2,326,876	2,472,564	2,634,715	2,833,108
	D10	4,146,048	2,454,613	2,834,122	3,083,185	3,512,988	4,332,263	38,040,902
Sales (in Packages) of Statutory Health Prescriptions per Pharmacy in 2021	D01	11,622	3,577	65	9,533	11,674	14,079	21,474
	D02	16,383	3,470	3,754	14,064	16,472	18,747	26,626
	D03	19,421	3,730	4,693	17,066	19,527	21,780	30,928
	D04	22,395	4,052	9,103	19,850	22,368	25,128	37,431
	D05	25,220	5,089	6,234	22,002	25,870	28,368	44,777
	D06	28,314	5,616	7,127	24,886	29,058	32,118	47,427
	D07	31,513	6,372	3,019	28,280	32,110	35,812	52,184
	D08	36,579	7,767	8,910	32,511	37,480	41,223	57,426
	D09	42,964	10,150	11,115	37,122	44,318	49,844	71,714
	D10	56,315	21,206	4,497	43,045	55,847	66,855	153,627
Average Remuneration per Sale (in Euro) from Statutory Health Prescriptions by Pharmacy in 2021	D01	7.88	0.345	7.22	7.7	7.83	7.99	12.5
	D02	7.92	0.251	7.47	7.75	7.87	8.02	9.22
	D03	7.95	0.291	7.53	7.79	7.9	8.05	11.7
	D04	7.95	0.232	7.46	7.81	7.92	8.05	9.58
	D05	8	0.34	7.44	7.82	7.92	8.1	11.6
	D06	8.02	0.318	7.42	7.83	7.95	8.11	10.1
	D07	8.08	0.414	7.58	7.86	7.97	8.14	11
	D08	8.09	0.492	7.53	7.86	7.98	8.14	12.6
	D09	8.19	0.537	7.58	7.9	8.01	8.26	12.3
	D10	8.84	2.17	7.62	7.98	8.19	8.77	36.1

Table A.2: Summary statistics for the presented calculations by deciles 1 to 10.

A.3 Simplified DiD Analysis

A simplified DiD analysis can be conducted by comparing mean sales of the treatment and control groups before and after the introduction of the VOASG are compared. Figure A.1 depicts annual sales categorized by prescriptions to members of the statutory health insurance system and private prescriptions. The top subfigure depicts absolute values and the bottom one displays sales normalized to 2020.

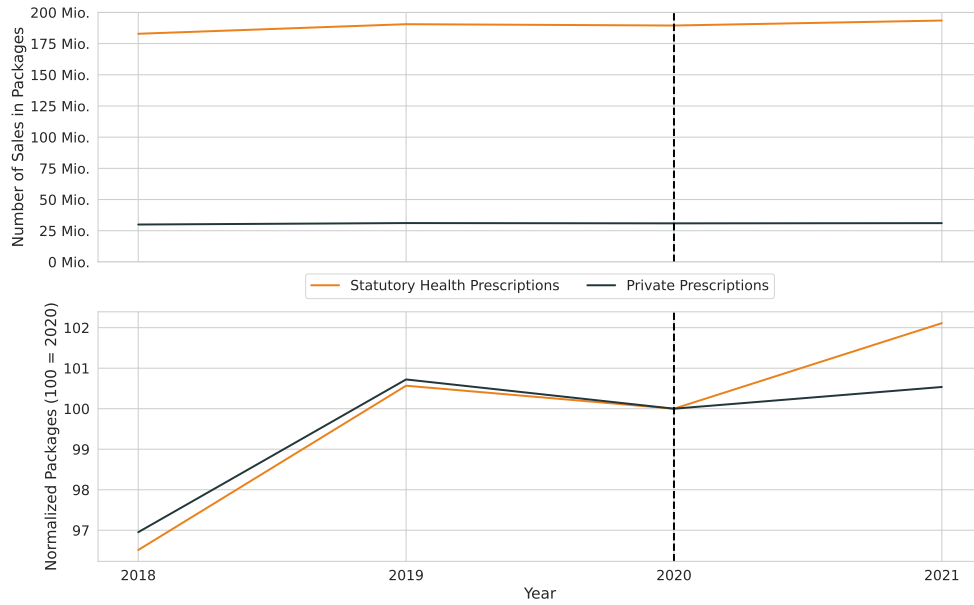


Figure A.1: Evolution of sales in packages by statutory health and private prescriptions. The top figure denote sales in absolute values, while the bottom figure depict sales normalized to the year 2020.

Figure A.1 shows that Post-VOASG, prescription drug sales in the treatment group increased relative to the control group.^{A2} In particular, calculating the ATT by simply comparing the mean values before and after the treatment (without considering any covariates), we find an ATT of roughly 0.0176, i.e., sales of brick-and-mortar pharmacies increased by roughly 1.76 % compared to a counterfactual of a state without VOASG. This result should be viewed as a first indication.

^{A2}The figure also shows that prescriptions issued to members of the statutory health insurance are six times higher than those issued to privately insured and self-pay patients. This discrepancy is explained by the fact that most Germans are covered by statutory health insurance, as already explained in Section 1.

A.4 Pre-Trend Sensitivity Analyses

In Table 3, we presented the results of the dynamic TWFE model derived from Equation (1). The initial assessment of the pre-treatment period (Section 5.1) showed that the individual coefficients for the pre-treatment leads in 2018 and 2019 for the preferred specification were not statistically distinguishable from zero. This provides evidence that parallel trends are plausible.

Another common approach that goes beyond individual t-tests involves a joint significance test (e.g., a Wald Test/Chi-Square Test) to assess inference of the pre-treatment coefficients simultaneously (see, e.g., Cameron and Trivedi, 2005, Chapter 7.2; Roth, 2022, Section B). In accordance with this approach, we conduct a standard test for joint significance. Recognizing the potential known limitations of individual t-tests and joint significance tests (Roth, 2022), we then proceed to a comprehensive sensitivity analysis, applying the pre-test power diagnostic framework of Roth (2022) and the robust inference framework of Rambachan and Roth (2023) to assess the validity of our analyses of causality.

A Wald test for joint insignificance of the pre-trends indicates a mixed pattern across specifications. While column (1) does not reject the null hypothesis of joint insignificance ($p = 0.476$), columns (2), (3), and (4) reject the null hypothesis at the 10 %, 1 %, and 5 % significance levels, respectively ($p = 0.062, p = 0, p = 0.0124$). Specifically, the rejection in the more stringently clustered specifications (Columns 3 and 4) indicates that a violation of the parallel trends assumption, particularly at the pharmacy aggregation level, cannot be ruled out statistically, by just relying on a joint significance test.

	Event Studies			
	(1)	(2)	(3)	(4)
p-value (Wald Test/Chi-Square Test) (H_0 : Leads = 0)	0.4756	0.062	0	0.0124

Table A.3: Pre-trend test on joint significance based on results from Table (2). Note: * indicates the p -value is less than 0.0001 and rounds to zero at four decimal places.

When interpreting these findings, it is important to note that these tests (individual t-tests and joint significance tests) may suffer from low power in the context of pre-testing in DiD analyses (Roth, 2022). To overcome this issue, we conduct further analyses proposed by Roth (2022) and Rambachan and Roth (2023).

The first approach is a power diagnostic framework proposed by Roth

(2022). This approach diagnoses potential biases arising from pre-testing by conducting a power calculation to determine the magnitude of a plausible underlying violation (specifically, assuming a linear trend violation) representing the potential unconditional bias. Afterwards, the expected value of the coefficients is calculated conditional on the sample successfully passing the pre-trend test. This reveals the selection bias in the post-treatment effect estimates, which is introduced because the analysis is restricted only to samples that “pass” the pre-trend test (i.e., those that exhibit an empirically flat pre-trend). By comparing the estimated coefficients with the potential unconditional bias and the induced selection bias, we can assess whether the observed treatment effect is likely driven by these confounding factors rather than a true causal impact.

Second, we employ the relative magnitudes bounds (Δ^{RM}) approach proposed by Rambachan and Roth (2023). In doing so, we determine the so-called *breakdown value* – the maximum plausible violation of parallel trends our result can withstand before losing statistical significance. This method defines a sensitivity parameter $\overline{M}$ which assumes the potential unobserved post-treatment violation (in trend changes) is no more than $\overline{M}$ times the maximum corresponding difference observed in the pre-treatment period. We then compute robust confidence intervals for the treatment effect under increasing values of $\overline{M}$. If the confidence interval remains statistically significant (i.e., excludes zero) even for relatively large values of $\overline{M}$, it provides strong evidence that our main conclusion is robust to considerable potential violations of the parallel trends assumption.

The results of both analyses are presented for our preferred specification (Table 2, Column 4) in Figures A.2 and A.3, respectively.^{A3} We first address the results of the power diagnostics tests (Roth, 2022) and then the results of the relative magnitudes bounds approach Rambachan and Roth (2023).

Figure A.2 displays the estimated event-study coefficients (black dots) alongside two counterfactual scenarios designed to diagnose pre-test bias.

The power diagnostics tests (derived from the *pretrends* package) are conducted with a statistical power of roughly 80%. This value is a widely accepted convention in power analysis for determining the minimum detectable effect size (Cohen, 1988), and is therefore proposed for pre-trend analyses to ensure a high probability of detecting a meaningful violation of the common trend assumption (Roth, 2022, Section C). According to a power computa-

^{A3}The analysis uses the R packages *pretrends* (Roth, 2022) and *HonestDiD* (Rambachan and Roth, 2023). See <https://github.com/jonathandroth/pretrends> and <https://github.com/asheshrambachan/HonestDiD>, respectively.

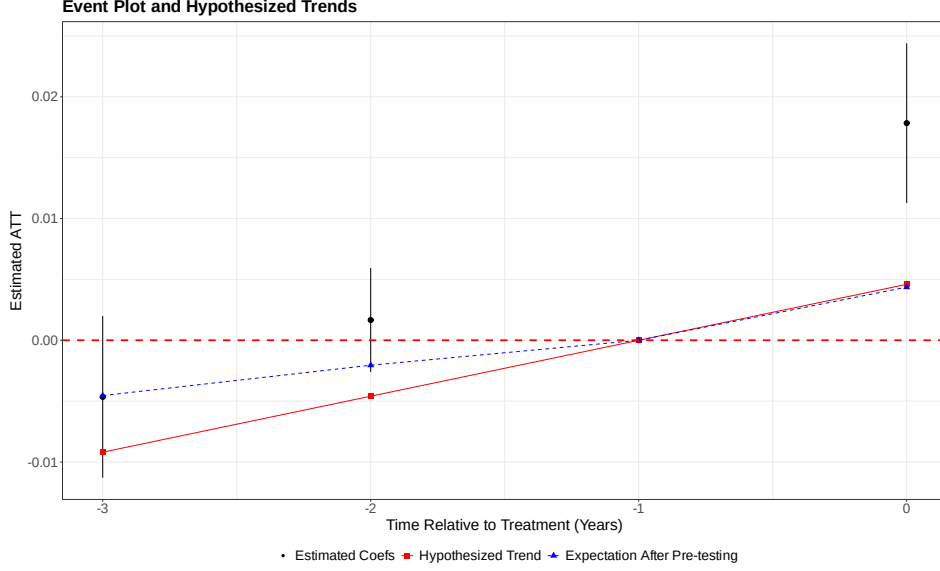


Figure A.2: Dynamic DiD Coefficients and Pre-Test Bias Diagnostics. The plot compares the estimated treatment path (black dots) against the assumed unconditional bias (“Hypothesized Trend”) and the induced selection bias (“Expectation After Pre-testing”). The large vertical distance between the estimated post-treatment effect and the bias lines suggests the treatment effect is not driven by confounding trends.

tion conducted with the *pretrends* R package, targeting 80% power allows our pre-test to detect a hypothetical linear trend violation with a slope of 0.0046.

This hypothetical trend (“Hypothesized Trend”, solid red line in Figure A.2) assumes an underlying linear non-parallel difference in trends (slope of 0.0046). This trend represents an unconditional bias that would contaminate the post-treatment estimate if the common trends assumption was violated. Furthermore, a coefficient is estimated conditional on a successful passing of the pre-trend tests (“Expectation After Pre-testing”, blue dashed line). This line represents the (hypothetical) conditional bias that would be estimated if one only proceeded with the analysis after successfully passing a pre-trend test (i.e., finding an insignificant pre-trend).

Figure A.2 shows a substantial distance between the post-treatment coefficient and both the “Hypothesized Trend” (red solid line) and the “Expectation After Pre-testing” (blue dashed line). This shows that the magnitude of the observed ATT is significantly larger than either the hypothetical unconditional bias or the selection bias induced by pre-testing. This shows that our main results are unlikely to be driven by pre-test biases.

To conclude our power diagnostics test, we compute a Bayes Factor (BF). The BF is the ratio of the probability of passing the pre-test under a hypothesized alternative trend (H_A) relative to passing under the parallel trends assumption (H_0), where $\text{BF} = \mathbb{P}(\text{Pass} \mid H_A) / \mathbb{P}(\text{Pass} \mid H_0)$. In our case, the computed Bayes Factor is 0.2125. Since $0.2125 < 1$, observing a non-significant pre-trend provides evidence in favor of the parallel trends assumption (H_0) over the alternative assumption (H_A). This value not only favors the parallel trends assumption (H_0) but is small enough to provide “moderate” evidence for H_0 over the alternative H_A (Jeffreys, 1961, Appendix B; Kass and Raftery, 1995; Lee and Wagenmakers, 2014, Chapter 7.2).^{A4}

This sensitivity power analysis provides further evidence that the parallel trends assumption holds and indicates that the estimated effect is unlikely to be driven by a potential pre-existing trend.

In the remainder of this section, the results of the second sensitivity analysis, the relative magnitudes bounds (Δ^{RM}) approach, is presented. Figure A.3 displays the robust 95 % confidence interval derived from the relative magnitudes bounds Δ^{RM} . The figure plots the robust confidence intervals (red bars) for the ATT as a function of the scaling factor $\bar{M}$. The reference point at $\bar{M} = 0$ (blue bar) represents the conventional confidence interval, which is calculated under the strict assumption that the parallel trends condition holds exactly.

The robust confidence interval remains above zero for all tested values of the scaling factor $\bar{M}$ up to a breakdown value of 1.75. This indicates the estimated treatment effect is robust: the significant positive finding is maintained even when allowing the unobserved post-treatment violation of parallel trends to be nearly twice as large ($\bar{M} = 1.75$) as the maximum pre-treatment difference observed in the data. In comparison, the analysis of Silverio-Murillo et al. (2024, Appendix, Figure 2) finds that their results are no longer significant at $\bar{M} = 0.2$. Our breakdown point is nearly 10 times larger. In another comparison, Robbiano et al. (2025, Figure 4) identify a breakdown point of $\bar{M} = 1.5$ (86% of our breakdown value), which they characterize as robust to “medium” violations of parallel trends. Finally, our findings appear even more robust than one of the key examples provided in the original methodology paper. In their re-analysis of Benzarti and Carloni (2019), Rambachan and Roth (2023, Figure 5) show that the robust

^{A4}We interpret the strength of the evidence using standard classifications (Jeffreys, 1961; Kass and Raftery, 1995; Lee and Wagenmakers, 2014). These scales classify a Bayes Factor between 1/3 (0.33) and 1/10 (0.1) as providing “moderate evidence” in favor of the null hypothesis (H_0). Our computed BF of 0.2125 falls directly within this range, providing evidence that the parallel trends assumption holds (relative to the specific alternative H_A).

confidence interval for the average post-treatment effect ($\bar{\tau}_{post}$ in the original paper) includes zero when $\bar{M} = 1.0$, while the first-period effect (τ_{2009} in the original paper) breaks down around $\bar{M} = 2.0$. As our results are robust up to $\bar{M} = 1.75$, this sensitivity analysis provides strong support for the validity of our findings, demonstrating a level of robustness that meets that of other available benchmark examples in the literature.

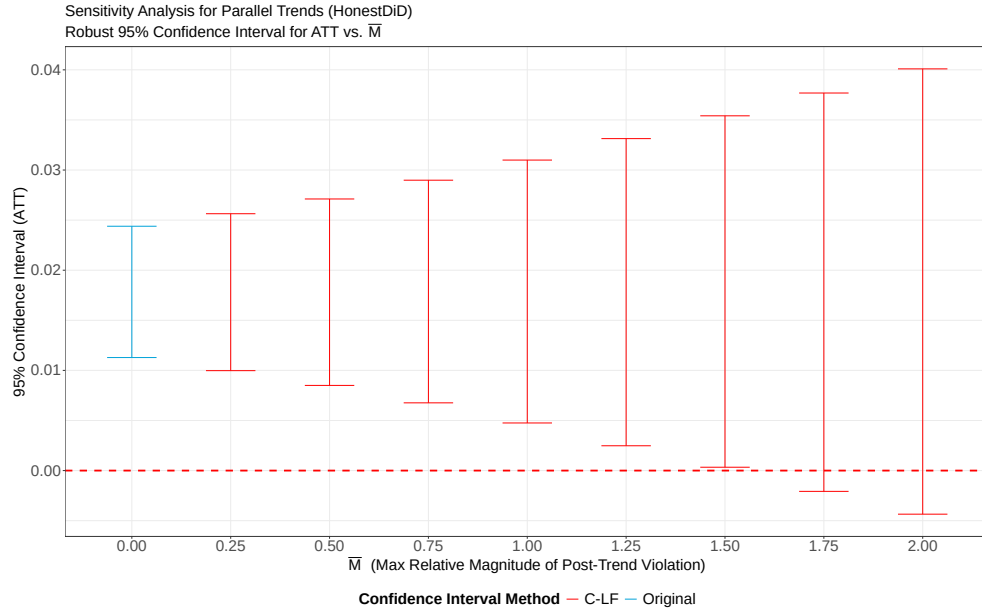


Figure A.3: Sensitivity Analysis: Robustness of ATT to Parallel Trends Violation ($\bar{M}$). The figure displays the robust 95 % confidence interval as a function of the maximum allowed relative bias ($\bar{M}$). The ATT is confirmed robust, as the interval remains entirely above zero even when the post-treatment violation is scaled up to twice the maximum observed pre-treatment difference.

A.5 All Specifications for the Static TWFE

	(1)	(2)	(3)	(4)
Panel A: Sales in Packages				
ATT	0.0155*** (0.0038)	0.0167*** (0.0028)	0.0167*** (0.0015)	0.0167*** (0.0034)
Share of Statutory Health Insured Members	1.7573 (1.8341)	2.2213 (1.5346)	2.2213* (1.1892)	2.2213 (1.9185)
Weighted Average of Doses	0.0036 (0.0026)	-0.0022 (0.0014)	-0.0022 (0.0019)	-0.0022 (0.0021)
Observations	760	51,168	51,168	51,168
Adj. R2	0.9997	0.9906	0.9906	0.9906
Std. Errors	Robust	Robust	by: 2-digit-zip code	by: 2-digit-zip code & prescription type
Panel B: Sales in Packages with Trends				
ATT	0.0137*** (0.0046)	0.0142*** (0.0035)	0.0142*** (0.0019)	0.0142*** (0.0037)
Weighted Average of Doses	0.0034 (0.0026)	-0.0022 (0.0014)	-0.0022 (0.0019)	-0.0022 (0.0022)
Time Trend of Treatment-Group	0.0018 (0.0017)	0.0023* (0.0014)	0.0023** (0.0011)	0.0023 (0.0017)
Observations	760	51,168	51,168	51,168
Adj. R2	0.9997	0.9906	0.9906	0.9906
FE: Year	X	X	X	X
FE: 2 digit zip code & prescription type	X			
FE: Pharmacy & prescription type		X	X	X
Std. Errors	Robust	Robust	by: 2-digit-zip code	by: 2-digit-zip code & prescription type
<i>Note:</i> Standard Errors in parenthesis. * p < 0.1, ** p < 0.05, *** p < 0.01				

Table A.4: Results of all specifications of the DiD-estimation (see Section 5.1).

A.6 Package Size as Dependent Variable

As a robustness check, we use doses as the dependent variable in equation (2) instead of incorporating them as a covariate only. Recall that information on doses were obtained from the German N-classification system, see Section 3.1. Table A.5 displays the results of this alternative estimation.

The table's interpretation and structure mirror those of Table A.4. The ATT reported in Table A.5 are comparable to those in the main text, ranging from 0.0140 to 0.0153.

	(1)	(2)	(3)	(4)
Panel A: Sales in NDD				
ATT	0.0140*** (0.0037)	0.0153*** (0.0027)	0.0153*** (0.0015)	0.0153*** (0.0033)
Share of Statutory Health Insured Members	-1.6937 (1.8100)	-1.0949 (1.5322)	-1.0949 (1.1510)	-1.0949 (1.9052)
Weighted Average of Doses	0.0196*** (0.0025)	0.0145*** (0.0013)	0.0145*** (0.0018)	0.0145*** (0.0020)
Observations	760	51,168	51,168	51,168
Adj. R2	0.9998	0.9917	0.9917	0.9917
Std. Errors	Robust	Robust	by: 2-digit-zip code	by: 2-digit-zip code & prescription type
Panel B: Sales in NDD with Trends				
ATT	0.0146*** (0.0045)	0.0152*** (0.0034)	0.0152*** (0.0019)	0.0152*** (0.0037)
Weighted Average of Doses	0.0196*** (0.0025)	0.0144*** (0.0013)	0.0144*** (0.0018)	0.0144*** (0.0020)
Time Trend of Treatment-Group	-0.0011 (0.0017)	-0.0005 (0.0014)	-0.0005 (0.0010)	-0.0005 (0.0017)
Observations	760	51,168	51,168	51,168
Adj. R2	0.9998	0.9917	0.9917	0.9917
FE: Year	X	X	X	X
FE: 2 digit zip code & prescription type	X			
FE: Pharmacy & prescription type		X	X	X
Std. Errors	Robust	Robust	by: 2-digit-zip code	by: 2-digit-zip code & prescription type

Note:

Standard Errors in parenthesis.

* p < 0.1, ** p < 0.05, *** p < 0.01

Table A.5: TWFE DiD estimation with doses as dependent variable.

A.7 Socio-Demographic Covariates

In this robustness check, we control for changes in household composition and income across two-digit zip code regions, accounting for variation in the shares of younger and older residents, average household size, and income levels. We use socio-demographic data provided by the marketing firm Acxiom.^{A5} Following Equation (2), W_{pgt} is augmented with the following covariates: income per person, the share of individuals below age 18, the share above age 65 (with all others as the reference group), and the shares of one- to four-person households (households with five or more members serve as the reference group). Results for Equation (2) are presented in Table A.6.

The ATTs reported in Table A.6 are close to those in the baseline specifications (Table A.4), ranging from 0.0142 to 0.0167.

	(1)	(2)	(3)	(4)
Panel A: Sales in Packages				
ATT	0.0155*** (0.0036)	0.0167*** (0.0028)	0.0167*** (0.0015)	0.0167*** (0.0033)
Share of Statutory Health Insured Members	1.7806 (1.8036)	2.2219 (1.5348)	2.2219* (1.1952)	2.2219 (1.8688)
Weighted Average of Doses	0.0035 (0.0024)	-0.0022 (0.0014)	-0.0022 (0.0019)	-0.0022 (0.0022)
Observations	760	51,168	51,168	51,168
Adj. R2	0.9997	0.9906	0.9906	0.9906
Std. Errors	Robust	Robust	by: 2-digit-zip code	by: 2-digit-zip code & Treated
Panel B: Sales in Packages with Trends				
ATT	0.0136*** (0.0045)	0.0142*** (0.0035)	0.0142*** (0.0019)	0.0142*** (0.0038)
Weighted Average of Doses	0.0034 (0.0025)	-0.0022 (0.0014)	-0.0022 (0.0019)	-0.0022 (0.0022)
Time Trend of Treatment-Group	0.0018 (0.0016)	0.0023* (0.0014)	0.0023** (0.0011)	0.0023 (0.0016)
Observations	760	51,168	51,168	51,168
Adj. R2	0.9997	0.9906	0.9906	0.9906
FE: Year	X	X	X	X
FE: 2 digit zip code & prescription type	X			
FE: Pharmacy & prescription type		X	X	X
Std. Errors	Robust	Robust	by: 2-digit-zip code	by: 2-digit-zip code & Treated

Note:

* p < 0.1, ** p < 0.05, *** p < 0.01

Table A.6: Estimation results from the static TWFE specification based on Equation (2), including socio-demographic covariates.

^{A5}See, Acxiom Deutschland GmbH, Frankfurt (2024).

A.8 Outlier Influence

As a robustness check, and in order to conduct the heterogeneity analysis (see Section 4.3), we removed outliers from the dataset. This was done to mitigate the influence of potential data errors on the results. This section demonstrates that the results are robust to outliers.

Outliers were removed using a two-sided trimming procedure. Pharmacies were removed if, in any given year of our observation period (2018-2021), its total annual prescription sales volume and/or its average annual remuneration per sale fell outside the central 95 % range (i.e., below the 2.5th percentile or above the 97.5th percentile) of the distribution for that respective year. For example, Fagereng et al. (2020, p. 122) apply a similar methodology to their data by trimming the “distribution of returns in each year at the top and bottom 0.5 %” to drop outlier observations. In general, our results are robust to trimming these outliers; see Appendix, Section A.8. We also excluded sales via courier services and to nursing homes. This trimming reduces the number of pharmacies in our data set from 6,396 to 5,487.

The results for the static TWFE specification with the trimmed sample (Table A.7) confirm the baseline findings (full sample) reported in Table A.4.

	(1)	(2)	(3)	(4)
Lead 2018	-0.0035 (0.0035)	-0.0026 (0.0022)	-0.0026 (0.0021)	-0.0026 (0.0030)
Lead 2019	0.0011 (0.0026)	0.0023 (0.0016)	0.0023* (0.0012)	0.0023 (0.0021)
Lag 2021	0.0162*** (0.0039)	0.0174*** (0.0023)	0.0174*** (0.0014)	0.0174*** (0.0031)
Weighted Average of Doses	0.0009 (0.0031)	-0.0006 (0.0006)	-0.0006 (0.0009)	-0.0006 (0.0008)
Observations	760	43,896	43,896	43,896
Adj. R2	0.9997	0.9938	0.9938	0.9938
FE: Year	X	X	X	X
FE: 2 digit zip code & prescription type	X			
FE: Pharmacy & prescription type		X	X	X
Std. Errors	Robust	Robust	by: 2-digit-zip code	by: 2-digit-zip code & prescription type

Note:

Standard Errors in parenthesis.

* p < 0.1, ** p < 0.05, *** p < 0.01

Table A.8: Estimation results for Equation (1), where outliers have been trimmed.

	(1)	(2)	(3)	(4)
Panel A: Sales in Packages				
ATT	0.0155*** (0.0039)	0.0165*** (0.0023)	0.0165*** (0.0015)	0.0165*** (0.0032)
Share of Statutory Health Insured Members	1.5718 (1.8638)	1.1123 (1.2091)	1.1123 (1.1569)	1.1123 (1.7127)
Weighted Average of Doses	0.0013 (0.0030)	-0.0006 (0.0006)	-0.0006 (0.0009)	-0.0006 (0.0008)
Observations	760	43,896	43,896	43,896
Adj. R2	0.9997	0.9938	0.9938	0.9938
Std. Errors	Robust	Robust	by: 2-digit-zip code	by: 2-digit-zip code & prescription type
Panel B: Sales in Packages with Trends				
ATT	0.0136*** (0.0048)	0.0149*** (0.0029)	0.0149*** (0.0018)	0.0149*** (0.0036)
Weighted Average of Doses	0.0012 (0.0030)	-0.0006 (0.0006)	-0.0006 (0.0009)	-0.0006 (0.0008)
Time Trend of Treatment-Group	0.0017 (0.0017)	0.0013 (0.0011)	0.0013 (0.0010)	0.0013 (0.0015)
Observations	760	43,896	43,896	43,896
Adj. R2	0.9997	0.9938	0.9938	0.9938
FE: Year	X	X	X	X
FE: 2 digit zip code & prescription type	X			
FE: Pharmacy & prescription type		X	X	X
Std. Errors	Robust	Robust	by: 2-digit-zip code	by: 2-digit-zip code & prescription type

Note:

Standard Errors in parenthesis.

* p < 0.1, ** p < 0.05, *** p < 0.01

Table A.7: Estimation results from the static TWFE specification based on Equation (2), where outliers have been trimmed.

A.9 Heterogeneity Analysis

This section is organized into three parts. First, we present the descriptive statistics underlying the figure in Section 5.2. Second, results of the decile-specific ATT analysis are detailed. Third, to highlight the specific influence of outliers, we present the corresponding results using the full sample at the end of this section.

Since the distribution of pharmacy revenues in the full sample exhibits significant skewness, where outliers potentially influence the definition of the deciles disproportionately, we use the trimmed sample as described in Section A.8. By trimming the sample, we mitigate the noise introduced by these extreme values and prevent outliers (and potential data errors) from driving the results of this distributional analysis. Trimming is particularly important at this point, because the decile-specific analysis is more susceptible to those outliers, so that estimations based on the trimmed data set are more stable.

Section 5.2, particularly Figure 4, provides a histogram visualizing the *Average Gain from VOASG* for each pharmacy. This measure is derived from the ATT $\beta = 0.0149$ (see Table A.7, Panel B, Column (4)), the volume of sales from statutory health prescriptions per pharmacy, and the *Average Remuneration per Sale* per pharmacy for the year 2021 (see Table A.2). The *Average Remuneration per Sale* is calculated for each pharmacy by dividing its total remuneration by its total sales (see Section A.2).

To highlight the derivation, e.g., consider a pharmacy that processed 24,000 statutory health prescriptions in 2021, and earned an average of 8 Euro per prescription. The VOASG would have resulted in earnings of:

$$24,000 \times \frac{0.0149}{1 + 0.0149} \times 8 \text{ Euro} = 2,819 \text{ Euro}$$

$$(\text{Average Gain from VOASG} = \text{Sales} \times \frac{\beta}{1 + \beta} \times \text{Average Remuneration Per Sale}).$$

Table A.9 displays summary statistics for the *Average gain from VOASG* by decile.

In the remainder of this section, we present the results of the estimations of decile-specific ATTs and results using the full sample. In doing so, a modified version of Equation (2) is estimated:

$$\ln(y_{pgt}) = \alpha_{pg} + \gamma_t + \gamma_d + \sum_{d=1}^{10} \beta_d \mathbf{1}\{\text{Decile}_{pt} = d\} \times D_{gt} + W'_{pgt} \mu + \epsilon_{pgt}. \quad (\text{A.5})$$

	Deciles	Mean	SD	Min	P25	Median	P75	Max
Average Gain from VOASG (in Euro) by β	D01	1,587	258	795	1,399	1,582	1,775	2,253
	D02	1,984	329	897	1,771	2,002	2,215	2,761
	D03	2,320	346	1,374	2,085	2,368	2,573	3,197
	D04	2,635	387	1,073	2,388	2,664	2,931	3,430
	D05	2,933	466	1,319	2,626	3,013	3,262	3,979
	D06	3,304	524	1,651	2,963	3,381	3,690	4,466
	D07	3,678	530	1,259	3,377	3,694	4,045	4,844
	D08	4,185	635	2,047	3,810	4,246	4,633	5,852
	D09	4,877	817	1,475	4,396	4,938	5,481	6,922
	D10	6,077	1,111	3,061	5,410	6,076	6,854	8,665

Table A.9: Summary statistics for the *Average gain from VOASG* by decile using $\beta = 0.0149$ from Table A.7, Panel B, Column (4).

In Equation (A.5), the index d denotes the revenue decile to which pharmacy p belongs at time t . The term γ_d represents fixed effects accounting for decile-specific heterogeneity. The indicator variable $\mathbf{1}\{Decile_{pt} = d\}$ equals one if pharmacy p falls into decile d in period t .^{A6} The interaction between the DiD indicator variable D_{gt} and the decile indicators identifies decile-specific treatment effects, β_d . These coefficients therefore capture decile-specific ATTs which basically capture whether the treatment had a qualitatively different effect on lower-revenue vs higher-revenue pharmacies. The estimation results are presented in Table A.10.

The coefficients for “ATT for Decile x ” capture the interaction between the DiD indicator variable and the respective decile, i.e., the decile-specific ATT.

We conducted a Wald Test to test whether all coefficients are significantly different from the ATT obtained in the main text ($\beta = 0.0149$, see Section 5). The H_0 (ATT for Decile 1 = ATT for Decile 2 = ... = 0.0149) could not be rejected at the usual p-values. This means that, in statistical terms, the decile-specific ATTs are not so pronounced that we can assume qualitative differences in the treatment effects of the VOASG for each decile. For completeness, Figure A.4 illustrates the distribution of the absolute impact of the partial ban on rebates across pharmacies with varying revenue levels.

Assuming decile-specific ATTs despite the aforementioned results of the Wald-test reveals a more pronounced difference in the effects across different revenue deciles. Specifically, the increase in Rx sales for deciles one to seven ranges from approximately 0.92 % to 1.46 % across all panels in Table A.10. In contrast, the effect for the highest three deciles is one point five to two

^{A6}Note that $Decile_{gt}$ is not collinear with the fixed effects α_{pg} as pharmacies can shift between deciles over time.

	(1)	(2)	(3)
Weighted Average of Doses	-0.0017*** (0.0005)	-0.0017** (0.0007)	-0.0017** (0.0007)
DiD for Decile 1	0.0128*** (0.0047)	0.0128** (0.0053)	0.0128** (0.0056)
DiD for Decile 2	0.0111*** (0.0043)	0.0111*** (0.0038)	0.0111** (0.0046)
DiD for Decile 3	0.0107*** (0.0040)	0.0107** (0.0041)	0.0107** (0.0043)
DiD for Decile 4	0.0146*** (0.0042)	0.0146*** (0.0041)	0.0146*** (0.0044)
DiD for Decile 5	0.0127*** (0.0040)	0.0127*** (0.0034)	0.0127*** (0.0044)
DiD for Decile 6	0.0092** (0.0037)	0.0092** (0.0036)	0.0092** (0.0040)
DiD for Decile 7	0.0131*** (0.0037)	0.0131*** (0.0030)	0.0131*** (0.0036)
DiD for Decile 8	0.0182*** (0.0038)	0.0182*** (0.0030)	0.0182*** (0.0039)
DiD for Decile 9	0.0208*** (0.0037)	0.0208*** (0.0031)	0.0208*** (0.0037)
DiD for Decile 10	0.0190*** (0.0040)	0.0190*** (0.0039)	0.0190*** (0.0047)
Observations	43,896	43,896	43,896
Adj. R2	0.9959	0.9959	0.9959
FE: Year	X	X	X
FE: Pharmacy & prescription type	X	X	X
FE: Deciles	X	X	X
Std. Errors	Robust	by: 2-digit-zip code	by: 2-digit-zip code & prescription type
Chi-Square Test (H_0 : DiD Coefficient = 0.0149)	0.204	0.107	0.228

Note:

Standard Errors in parenthesis.

* p < 0.1, ** p < 0.05, *** p < 0.01

Table A.10: Results of the DiD-estimation for revenue deciles.

times larger, ranging from 1.82 % to 2.08 %.^{A7}

The figure was constructed by multiplying each pharmacy's remuneration for dispensing Rx drugs by its corresponding ATT (see Table A.10). One can

^{A7}A more granular analysis of the effects is constrained by a lack of pharmacy-specific information. Specifically, data on the exact locations of pharmacies would allow for an assessment of the spatial distribution of revenues, and for testing whether pharmacies in urban or rural areas benefited more or less strongly from the policy. In this context, it is worth noting that the effect is most likely not driven by exit of pharmacies. As we use a balanced sample of pharmacies, we would expect that, in our sample, sales to *both* insurance types increases over time. Moreover, such a trend would be absorbed by time fixed effects.

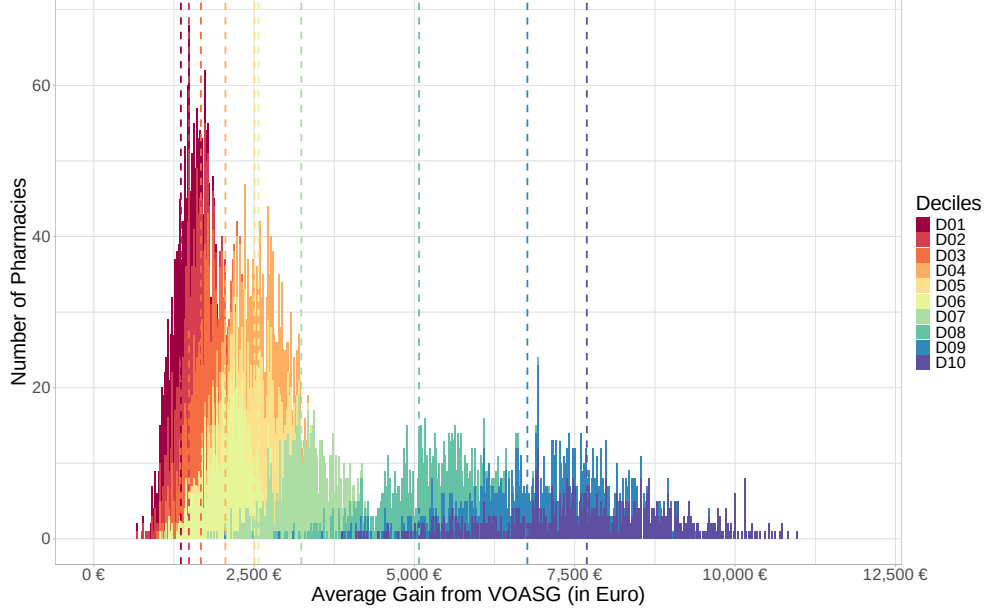


Figure A.4: DiD coefficient for each decile (see Table A.10). $Average\ Gain\ from\ VOASG = Sales \times \frac{\beta_{Decile}}{1+\beta_{Decile}} \times Average\ Remuneration\ Per\ Sale$.

see that the results resemble a bimodal distribution. Low-revenue pharmacies' additional annual gains (in Deciles 1-7) ranged from roughly €700 to €4,300. In contrast, the higher deciles (Deciles 8-10) experienced a more pronounced increase. This can also be seen in Table A.11, which reports the descriptive statistics of the average annual gains for each decile.

	Deciles	Mean	SD	Min	P25	Median	P75	Max
Average Gain from VOASG (in Euro) by $\beta_{Deciles}$	D01	1,359	221	681	1,198	1,355	1,520	1,929
	D02	1,484	246	671	1,325	1,498	1,657	2,066
	D03	1,671	249	990	1,502	1,706	1,854	2,303
	D04	2,569	377	1,046	2,328	2,598	2,858	3,345
	D05	2,508	398	1,128	2,245	2,576	2,789	3,401
	D06	2,053	326	1,026	1,841	2,101	2,293	2,775
	D07	3,236	467	1,108	2,971	3,249	3,558	4,261
	D08	5,072	770	2,481	4,618	5,147	5,615	7,094
	D09	6,761	1,132	2,045	6,093	6,845	7,597	9,595
	D10	7,688	1,405	3,872	6,845	7,687	8,671	10,962

Table A.11: Summary statistics for the *Average gain from VOASG* by decile using β_d from Table A.10.

Even though, the heterogeneity between pharmacies in this analysis is more pronounced than that reported in Section 5.2, the finding is the same: the gains of smaller pharmacies are noticeably lower than that of higher pharmacies. Thus, both analyses imply that the gains from the introduction of

VOASG were unevenly distributed across pharmacies, with *larger* pharmacies actually benefiting more strongly.

To conclude this section, we present the results using the full sample, which retains all outliers (Tables A.12 – A.14). Table A.12 presents the decile-specific ATT estimates for the full sample. A comparison with the trimmed sample (Table A.10) reveals that including outliers introduces substantial noise into the estimation, particularly at the lower end of the revenue distribution. For instance, the coefficient for Decile 1 becomes negative and statistically insignificant (-0.0018), while Decile 3 drops to near zero (0.0045 , insignificant). Conversely, the estimates for the upper deciles remain large and highly significant.

Crucially, the Wald test for the full sample rejects the null hypothesis that the decile-specific coefficients are jointly equal to the average ATT ($\beta = 0.0142$) in Columns (2) and (3) ($p = 0.004$ and $p = 0.011$, respectively). By comparison, with the trimmed the Wald test could not reject the null. This shows how outliers exacerbate the heterogeneity of the policy effects.

The economic implications of these outliers are visualized in Figures A.5 and A.6, and summarized in Tables A.13 and A.14. When allowing for decile-specific effects (Table A.14), the influence of the top-performing pharmacies becomes apparent. The maximum gain in Decile 10 reaches over €25,000, which is more than three times the maxima observed in the trimmed analysis.

Importantly, however, the results as well as their interpretations presented in the main text become even more apparent. The gains of smaller pharmacies become even lower while those of large pharmacies, in particular those in the tenth decile, become even larger. Thus, the results of the heterogeneity analysis remain qualitatively robust to outliers.

	(1)	(2)	(3)
Weighted Average of Doses	-0.0033*** (0.0013)	-0.0033* (0.0017)	-0.0033 (0.0020)
DiD for Decile 1	-0.0018 (0.0114)	-0.0018 (0.0106)	-0.0018 (0.0116)
DiD for Decile 2	0.0121** (0.0051)	0.0121*** (0.0043)	0.0121** (0.0053)
DiD for Decile 3	0.0045 (0.0046)	0.0045 (0.0040)	0.0045 (0.0044)
DiD for Decile 4	0.0178*** (0.0043)	0.0178*** (0.0038)	0.0178*** (0.0043)
DiD for Decile 5	0.0132*** (0.0042)	0.0132*** (0.0034)	0.0132*** (0.0046)
DiD for Decile 6	0.0120*** (0.0040)	0.0120*** (0.0036)	0.0120*** (0.0040)
DiD for Decile 7	0.0148*** (0.0040)	0.0148*** (0.0031)	0.0148*** (0.0039)
DiD for Decile 8	0.0235*** (0.0043)	0.0235*** (0.0031)	0.0235*** (0.0042)
DiD for Decile 9	0.0185*** (0.0043)	0.0185*** (0.0033)	0.0185*** (0.0042)
DiD for Decile 10	0.0175*** (0.0051)	0.0175*** (0.0045)	0.0175*** (0.0054)
Observations	51,168	51,168	51,168
Adj. R2	0.9930	0.9930	0.9930
FE: Year	X	X	X
FE: Pharmacy & prescription type	X	X	X
FE: Deciles	X	X	X
Std. Errors	Robust	by: 2-digit-zip code	by: 2-digit-zip code & prescription type
Chi-Square Test (H_0 : DiD Coefficient = 0.0142)	0.032	0.004	0.011

Note:
Standard Errors in parenthesis.
* p < 0.1, ** p < 0.05, *** p < 0.01

Table A.12: Results of the DiD-estimation for revenue deciles using the full sample.

	Deciles	Mean	SD	Min	P25	Median	P75	Max
Average Gain from VOASG (in Euro) by β	D01	1,274	375	11.3	1,051	1,293	1,538	2,254
	D02	1,805	344	484	1,579	1,814	2,044	2,794
	D03	2,149	367	766	1,909	2,160	2,378	3,258
	D04	2,481	396	1,020	2,229	2,481	2,742	3,906
	D05	2,804	494	1,015	2,496	2,871	3,118	4,704
	D06	3,156	548	865	2,822	3,238	3,533	4,923
	D07	3,531	613	408	3,221	3,591	3,951	5,534
	D08	4,100	748	1,566	3,679	4,181	4,564	6,142
	D09	4,859	981	1,402	4,255	4,957	5,539	7,681
	D10	6,682	2,233	880	5,367	6,502	7,640	19,063

Table A.13: Summary statistics for the *Average gain from VOASG* by decile using $\beta = 0.0142$ from Table 3, Panel B, the full sample.

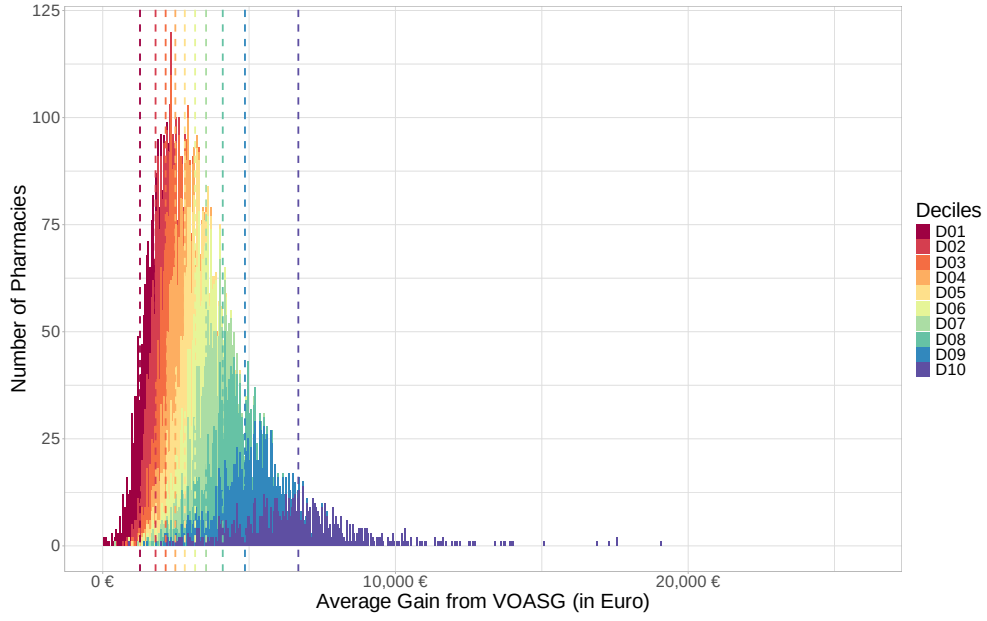


Figure A.5: DiD coefficient $\beta = 0.0142$ from Table 3, Panel B, using the full sample.
 $Average\ Gain\ from\ VOASG = Sales \times \frac{\beta}{1+\beta} \times Average\ Remuneration\ Per\ Sale.$

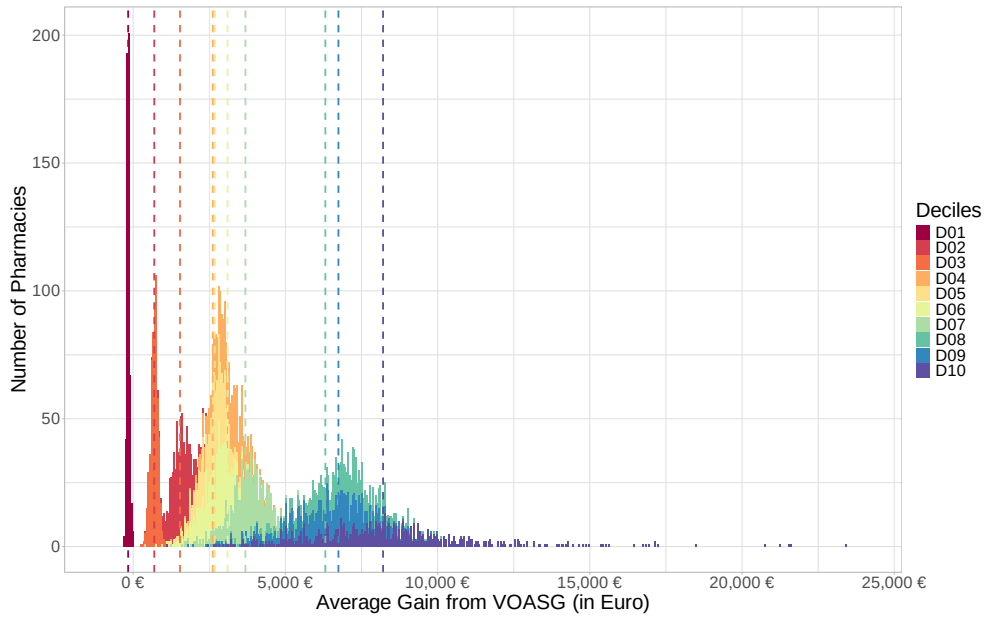


Figure A.6: DiD coefficient for each decile (see Table A.12) using the full sample.
 $Average\ Gain\ from\ VOASG = Sales \times \frac{\beta_{Decile}}{1+\beta_{Decile}} \times Average\ Remuneration\ Per\ Sale.$

	Deciles	Mean	SD	Min	P25	Median	P75	Max
Average Gain from VOASG (in Euro) by $\beta_{Deciles}$	D01	-165	48.5	-292	-199	-167	-136	-1.47
	D02	1,538	293	413	1,346	1,547	1,742	2,382
	D03	693	118	247	615	696	766	1,050
	D04	3,102	496	1,275	2,787	3,102	3,429	4,884
	D05	2,614	461	946	2,327	2,676	2,907	4,385
	D06	2,677	465	734	2,394	2,747	2,997	4,176
	D07	3,687	640	426	3,364	3,750	4,126	5,779
	D08	6,742	1,230	2,575	6,049	6,875	7,504	10,100
	D09	6,312	1,274	1,821	5,528	6,439	7,196	9,978
	D10	8,208	2,743	1,082	6,593	7,987	9,384	23,416

Table A.14: Summary statistics for the *Average gain from VOASG* by decile using β_d from Table A.12 and the full sample.

2.2 Cost Pass-Through, Price Competition, and Market Power in Brick-and-Mortar Pharmacies: Evidence from Germany

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- 52nd Annual Conference of the European Association for Research in Industrial Economics (EARIE), 2025, Valencia, Spain.
- 19th Annual CRESSE Conference on Competition and Regulation, 2025, Chania Crete, Greece.

Cost Pass-Through, Price Competition, and Market Power in Brick-and-Mortar Pharmacies: Evidence from Germany

Maximilian Maurice Gail*

March 17, 2026

Abstract

In the German pharmacy market, strictly regulated prescription (Rx) drug prices coexist with free pricing for over-the-counter (OTC) drugs. This paper investigates the extent of market power in the non-prescription segment (OTC drugs and non-pharmaceuticals) and explores the role of Rx drugs in facilitating this. Leveraging nearly a billion daily observations from over 6,300 pharmacies and 213,000 products (2018–2022), this study estimates wholesale-to-retail pass-through rates via an error correction model (ECM) and analyzes product-level markups (Lerner Indices). Results reveal asymmetric pass-through (*rockets and feathers*): retail prices adjust to cost increases faster than decreases, a dynamic driven by niche products while bestsellers exhibit pricing inertia. Long-run pass-through rates (1.3–1.7) indicate systematic cost overshifting. However, while OTC drugs sustain high long-run markups – corroborated by Lerner Indices that are 35 % higher than those of non-pharmaceuticals – non-pharmaceuticals face more competitive pricing dynamics, passing through cost decreases more aggressively than increases. Furthermore, a spatial analysis reveals that rural pharmacies exhibit higher long-run pass-through rates and appear to compensate for lower transaction volumes (–7.6 %) with markups 8.3 % higher than urban competitors, effectively nullifying aggregate gross profit differences. These spatial dynamics are characterized by rural pharmacies exhibiting greater short-run pricing inertia, followed by stronger long-run pass-through. The findings suggest that pharmacies leverage the regulated Rx segment to facilitate higher markups and systematic overshifting. Consequently, the financial viability of rural networks relies partially on this cross-subsidization from Rx drugs, a model facing risks from increasing digitization.

JEL codes: L11, L13, L42, L50, L51, L81, I11.

Keywords: Over-The-Counter Drugs, Cost Pass-Through Estimation, Pharmacy Market, Markups, Error Correction Model, Price Competition, Rockets and Feathers.

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1 Introduction

In recent decades, numerous European countries have liberalized their pharmacy markets to foster competition (see, e.g., López Vila et al., 2023; Martins et al., 2015). Economically, these reforms aimed to lower drug prices and improve access via several deregulatory measures: granting pricing autonomy for over-the-counter (OTC) medications, relaxing ownership and dispensing constraints (Lluch & Kanavos, 2010; Vogler et al., 2014), abolishing regional entry quotas (Heinsohn & Flessa, 2013), and reclassifying certain prescription drugs as OTC (Andersson & Hatziaandreu, 1992). Germany aligned with this trend by implementing major reforms in 2004 (Heinsohn & Flessa, 2013; Stargardt et al., 2007).

Despite these regulatory shifts, rigorous empirical evidence on the evolution of competition in the pharmacy sector remains limited. International evidence utilizing micro-level data yields mixed conclusions. For instance, Moura and Barros (2020) report only modest price reductions in Lisbon, whereas reforms in Nordic markets fostered competition while precipitating consolidation (Anell, 2005; Anell & Hjelmgren, 2002). Broader European evidence similarly indicates that while liberalization improves access, it does not consistently lower consumer prices (Lluch & Kanavos, 2010; Vogler et al., 2014). Beyond Europe, Jo et al. (2022) reveal a critical trade-off in South Korea: while deregulation successfully reduced prices and improved access to the liberalized drugs, it accelerated the exit of incumbent pharmacies, thereby reducing overall consumer access to comprehensive pharmaceutical services. Most recently, Anderes (2026) highlights the structural vulnerability of traditional brick-and-mortar (B&M) pharmacies: after losing their local prescription (Rx) monopoly to self-dispensing physicians in specific Swiss regions, pharmacies suffered a permanent 9% profit decline alongside a 10% drop in Rx revenue. Driven by a 50% collapse in acute care sales, this shock forced a compensatory pivot toward non-drug health services.

In Germany, previous research relied primarily on pharmacist surveys, which suggest that price competition is muted (Heinsohn & Flessa, 2013; Stargardt et al., 2007). However, such *stated preference* data often fails to capture the strategic realities and details revealed by actual transaction behavior captured by *revealed preference* data (see, e.g., Louviere et al., 2000, Chapter 8.2). Furthermore, reliance on aggregated data (e.g., price indices) risks severe bias. As demonstrated by Imbs et al. (2005) and Nakamura and Steinsson (2008), aggregation obscures micro-level heterogeneity, leading to biased estimates of price rigidity (Bogoev & Sergi, 2012; Pesaran & Smith, 1995). Consequently, this aggregation bias risks masking the true extent of local market power. For instance, Jo et al. (2022, Table 1) document that prior policy evaluations in the pharmacy market have predominantly relied on country-level data, which obscures micro-level heterogeneity. While Jo et al. (2022) advance this literature by utilizing pharmacy-panel data, they are

limited to monthly aggregate revenues and annual city-level average prices of a representative sample of 50 OTC drugs.

In contrast, this paper bridges these gaps by analyzing a novel comprehensive proprietary dataset of transaction-level records from 9,231 B&M pharmacies between 2018 and 2022 (yielding a balanced panel of 6,338 pharmacies and covering approximately 213,000 unique products, comprising nearly a billion transactions). Unlike previous studies, these data allow for the precise observation of wholesale and retail prices, markups, and quantities of (almost) any product sold. A paramount strength of this dataset is that observing the exact wholesale price directly yields the economic marginal cost of dispensing a drug, eliminating the need for proxy variables or structural modeling, and allows for the measurement of the Lerner Index.¹ Furthermore, the high-frequency nature of the data permits the estimation of an asymmetric high-dimensional fixed effects error correction model (ECM) in the form of a non-linear autoregressive lag (NARDL) model (Balaguer & Ripollés, 2016; Salisu & Isah, 2017; Shin et al., 2014). Following the standard approach in the cost pass-through literature, this framework enables to explicitly measure both the speed of short-run adjustments and the magnitude of long-run pass-through. Specifically, by distinguishing between positive and negative cost shocks (e.g., Borenstein et al., 1997), dynamic pricing asymmetries (*rockets and feathers*) can be evaluated. Identification relies on the structural feature that individual pharmacies are atomistic and take product-specific wholesale prices as given. Since procurement terms with wholesalers are typically negotiated on an annual basis, high-frequency cost shocks are exogenous to idiosyncratic local demand. Crucially, the model includes pharmacy-by-product and time fixed effects, which absorb all time-invariant heterogeneity (such as location, quality, and brand loyalty) and common aggregate shocks. Consequently, the pass-through coefficients are identified solely from within-unit variation, orthogonal to static market power or spatial differentiation.

The analyses establish four primary stylized facts. First, the analyses reveal substantial pricing inertia and asymmetric pass-through, consistent with the *rockets and feathers* phenomenon (e.g., Bacon, 1991; Borenstein et al., 1997; Ritz, 2015; Tappata, 2009). Pharmacies transmit cost increases significantly faster than decreases, though the magnitude varies structurally by segment (OTC drugs and non-pharmaceuticals). For OTC drugs and non-

¹The Lerner Index is calculated as $(P - W)/P$ (based on Lerner, 1934), where P denotes the retail price and W represents the wholesale price, which serves as a proxy for marginal cost. This relies on the assumption that other operating expenses (primarily personnel salaries, rent, and laboratory infrastructure) represent fixed common costs in both the short and long run that do not vary with the sale of an additional unit. Furthermore, marginal inventory holding costs are negligible; the German wholesalers typically guarantee deliveries up to three times daily, allowing pharmacies to operate on a just-in-time basis and minimizing the capital and space required for stock-keeping. See https://www.phagro.de/app/uploads/2021/06/Phagro-Broschüre-2021_web-1.pdf.

pharmaceuticals, adjustment dynamics align with the 2-to-1 ratio frequently documented in the literature for both wholesale cost and value-added tax shocks (e.g., Benzarti et al., 2020; Peltzman, 2000). Pharmacies pass through initial cost increases at rates of approximately 26 % for OTC drugs and 41 % for non-pharmaceuticals, compared to only 12 % and 23 %, respectively, for cost decreases. This disparity highlights that OTC pricing is notably stickier, suggesting that while cross-channel competition (from drugstores and general retailers) disciplines non-pharmaceutical pricing, the regulatory exclusivity of OTC drugs – which can only be purchased in pharmacies (see Section 2.2) – and their acute medical necessity allow pharmacies to protect margins from cost reductions. In the long run, however, this asymmetry disappears and pass-through rates exceed unity (overshifting), reaching approximately 1.5 for OTC drugs and 1.4 for non-pharmaceuticals.

These short-run dynamics are consistent with theoretical models in which frictions, such as menu or search costs, delay short-run price cuts (e.g., Hellerstein, 2008; Loy et al., 2016; Nakamura & Zerom, 2010; Varian, 1980). By contrast, for systematic long-run overshifting, standard oligopoly models show that systematic long-run pass-through relies on four components: the elasticities of demand and marginal cost (supply), the curvature of demand, and the intensity of competition (Ritz, 2024; Weyl & Fabinger, 2013). Under monopoly or monopolistic competition with constant marginal costs, however, the role of demand elasticity vanishes, and systematic overshifting relies entirely on a log-convex demand curvature (Bulow & Pfleiderer, 1983; Mrázová & Neary, 2017). Because pharmacies purchase drugs directly from wholesalers and face negligible stocking or inventory costs, their marginal costs can be assumed to be constant (see Footnote 1). Consequently, log-convexity may play an important role in explaining the observed overshifting. Economically, log-convexity can reflect *fat tails* in the distribution of consumer valuations (Gabaix et al., 2016; Weyl & Fabinger, 2013), meaning that as prices rise, the remaining consumer base becomes increasingly price-insensitive, making it optimal for firms to overshift costs. This theoretical mechanism perfectly aligns with the finding that more rural pharmacies can sustain higher unit markups and exhibit higher long-run pass-through rates (see following stylized facts).

Second, contrary to the premise that rural pharmacies are financially disadvantaged by lower consumer density (which results in lower turnover and reduced economies of scale), they offset structurally lower transaction volumes (-7.6 %) by setting OTC unit markups approximately 8.3 % higher than their urban competitors. Consequently, total gross profits in more rural pharmacies are statistically indistinguishable from those in urban locations. This aligns with the structural analysis of Schaumans and Verboven (2008, p. 969), who demonstrate that while general entry liberalization is feasible, maintaining geographic coverage in smaller markets requires shielding profitability, potentially through nonuniform (higher) markups. The empirical

evidence presented here highlights that a functionally equivalent mechanism operates in the segment of OTC-drugs and non-pharmaceuticals: rural incumbents, which are shielded from the profit-eroding effects of entry typically found in denser markets, leverage their local market power to extract rents through higher OTC and non-pharmaceutical prices, possibly to satisfy their solvency constraint. This power may be grounded in the regulated Rx segment – which generates approximately 75 % of a pharmacy’s gross profits (see Section 3.3) – acting as a strategic *traffic builder* (Dhar & Hoch, 1997). By serving as a regulated anchor that draws consumers to the pharmacies, it may induce them to bundle Rx prescriptions with OTC and non-pharmaceutical products to minimize search and travel costs. This conjecture is corroborated by descriptive evidence indicating that approximately one-fifth of OTC and non-pharmaceutical sales stem from such joint purchases (see Section 3.3). Theoretically, such a finding is consistent with models of *one-stop shopping* and multi-product search frictions (e.g., Florez-Acosta & Herrera-Araujo, 2020; Klemperer, 1995; Lal & Matutes, 1994; Messinger & Narasimhan, 1997; Zhou, 2014). This mechanism may allow incumbents to extract higher margins from OTC drugs and non-pharmaceuticals compared to the absence of such a regulated anchor, which may be consistent with the theoretical models featuring, search-constrained, captive and uninformed consumers (e.g., Bliss, 1988; Ronayne & Taylor, 2022; D. O. Stahl, 1989; K. Stahl, 1982; Varian, 1980). The critical reliance on this regulated anchor is empirically validated by Anderes (2026), who demonstrates that exogenous shocks to Rx footfall cause severe profit erosion for pharmacies. Applied to this study, this highlights the threat to the viability of the cross-subsidization model.

Third, regarding cost pass-through, market power seems to drive a distinct temporal pattern: rural pharmacies display stronger short-run inertia followed by greater long-run markup expansion. This divergence is statistically significant for non-pharmaceuticals (approximately 1.35 vs. 1.38 for urban and rural pharmacies, respectively), while for OTC drugs, the effect is only weakly significant despite a comparable economic magnitude (approximately 1.44 vs. 1.46). This finding highlights that pharmacies protected by spatial barriers are potentially not compelled to absorb cost shocks but instead leverage local power to pass on cost volatility. This behavior mirrors findings in other industries characterized by high fixed costs and capacity constraints, such as cement (Miller et al., 2017), gas stations (Stolper, 2024) and solar leasing (Pless & Van Benthem, 2019). Overshifting is also found in many other industries such as a variety of retail goods (Besley & Rosen, 1999), alcoholic beverages (Kenkel, 2005; Young & Bielińska-Kwapisz, 2002) and cigarettes (Delipalla & O’Donnell, 2001), where imperfect competition, log-convex demand, a convex-cost structure or spatial frictions facilitate systematic cost overshifting into higher equilibrium prices to insulate margins (see, e.g., Miravete et al., 2023; Ritz, 2024; Stolper, 2024; Weyl & Fabinger, 2013).

In the pharmacy context, this pricing power may be functionally necessary: facing high fixed operating costs – including mandatory staffing and laboratory infrastructure – that must be recovered across a constrained transaction volume, incumbents are compelled to exploit the unregulated OTC segment (even extending to non-pharmaceuticals). This aligns with the *waterbed effect* observed in many industries such as telecommunications (Genakos & Valletti, 2011), and mirrors the regulatory spillovers documented in pharmaceutical manufacturing (Dubois & Lasio, 2018), where price constraints in one segment alter equilibrium pricing in others (e.g., to cover common fixed costs). Theoretically, this dynamic arises naturally in multi-product firms (such as pharmacies) facing common fixed costs (Schiff, 2008) and follows retail pricing models where incumbents cross-subsidize infrastructure by concentrating markups on their least price-sensitive products (Bliss, 1988).

Fourth, market power is highly heterogeneous across the product portfolio, revealing a strategic segmentation between bestseller and niche items. While the overall asymmetry is driven predominantly by niche products, bestsellers exhibit distinct pricing inertia and higher long-run pass-through rates. This inertia is consistent with *add-on pricing* strategies (Ellison, 2005) or the fairness-based theory of price rigidity (Rotemberg, 2005), where firms maintain stable nominal prices to avoid triggering customer anger. However, the substantial long-run overshifting suggests that once prices do adjust, pharmacies exploit brand loyalty associated with bestseller drugs to pass on cost shocks. This mirrors the behavior documented by Granlund and Bergman (2018), where high-visibility brand-name drugs adjust significantly slower to shocks than generics. Conversely, non-pharmaceuticals exhibit a long-run reverse asymmetry (cost decreases are passed on more strongly than increases), consistent with their role as a secondary *traffic builder* fighting against competition from drugstores. Thus, the *rockets and feathers* effect is not a uniform friction (such as expected from menu costs for printing labels), but seems to be a targeted strategy maximizing rent extraction where consumer attentiveness is lowest.

The remainder of this paper is organized as follows. Section 2 details the institutional setting of the German pharmacy market. Section 3 describes the data, data handling and presents the descriptive statistics. Section 4 outlines the identification strategy used to isolate short- and long-run cost pass-through (CPT) asymmetries, as well as structural heterogeneity across pharmacies and product ranks. Section 5 reports the estimation results, where Section 5.1 provides the main CPT results, Section 5.2 provides robustness, Section 5.3 explores structural heterogeneity, and Section 5.4 examines the spatial dimension of market power, linking urbanization levels to markup strategies, profitability, and CPT dynamics. Section 6 discusses the broader economic implications and acknowledges methodological limitations. Finally, Section 7 summarizes the findings and concludes.

2 Institutional Background

Although this analysis draws on the German pharmacy market, its findings can be generalized to other countries as this framework mirrors the structural environment found in 24 out of 30 European nations (López Vila et al., 2023). While the OTC segment is liberalized in the majority of member states (e.g., Italy, Portugal, and Greece), the market for Rx drugs remains heavily regulated across most European healthcare systems to ensure uniform access and affordability. In Germany, as in many peer countries (e.g., Austria, Belgium, France, Spain, Denmark), pharmacies are bound by fixed (statutory) prices or strict reimbursement limits for Rx drugs (see, Vogler et al., 2011, 2014), effectively eliminating price competition in this segment. Consequently, this common regulatory asymmetry likely allows for the transfer of findings and implications to other regulated pharmacy markets.

Specifically, the German pharmacy market is characterized by a strict regulatory dichotomy that shapes local competition. Uniform pricing laws for Rx drugs eliminate price competition, forcing pharmacies to rely on the liberalized OTC market to exercise local pricing strategies. Furthermore, pharmacies compete on several non-price dimensions. As detailed by Albrecht et al. (2020, pp. 59–64), these include service quality (e.g., counseling intensity, courier services), assortment depth, and strategic location choice. Crucially, while the transaction-level data cannot capture unobservable service dimensions like counseling, it perfectly isolates the intersection of two primary strategic levers: pricing behavior for OTC and non-pharmaceutical products, and spatial differentiation. The following sections outline these regulatory constraints, ownership structures, and competitive dynamics shaping the German pharmacy market that underpin the empirical framework employed in this analysis.

2.1 Regulatory Framework and Ownership Structure

The German pharmacy market operates under a strict regulatory regime that was fundamentally reshaped by the Statutory Health Insurance Modernization Act (*Gesundheitsmodernisierungsgesetz*, GMG) in 2004. Despite liberalization measures that legalized mail-order dispensing (see Section 2.3), the market remains highly fragmented due to the persistence of prohibition of third-party ownership (*Fremdbesitzverbot*). This regulation bans corporate chains, restricting ownership exclusively to licensed pharmacists (*approved pharmacists*). While the 2004 reform relaxed the *one-pharmacist, one-pharmacy rule* (*Mehrbesitzverbot*) to allow ownership of up to four locations (one main pharmacy and three branches), these branches must be located in close geographic proximity.² Consequently, the local markets consist en-

²Branch pharmacies must be located in the same county (*Landkreis*) or independent city, or in a directly adjacent county (§ 2(4) ApoG (*Apothekengesetz*)).

tirely of independent small-to-medium B&M pharmacies, distinguishing it from the corporate chain models prevalent elsewhere in Europe.³

Following the 2004 reform, the number of pharmacies peaked at 21,602 in 2008 as owners utilized the new branch regulations. However, a steady consolidation has since reduced this number to 16,601 by 2025 (a decline of $\approx 23\%$), raising concerns regarding healthcare access in rural areas.⁴

2.2 Price Regulation

A defining feature of the market is the dual regulatory regime governing Rx- and OTC drugs, which imposes distinct constraints on competition. First, the Rx segment is subject to a fixed prices regime. Under the Medicinal Products Price Ordinance (*Arzneimittelpreisverordnung*), pharmacies must charge a uniform, nationwide fixed price for prescription medications, effectively eliminating price competition.⁵ Second, the OTC segment is liberalized, allowing pharmacies to set consumer prices autonomously while benefiting from a “pharmacy-only” distribution restriction.⁶ This distinguishes OTC drugs from non-pharmaceutical products; while the latter complement a pharmacy’s assortment, they face direct cross-channel competition from drugstores and general retailers, whereas OTC drugs remain within a protected regulatory segment.

Crucially, this deregulation extends to the upstream wholesale market. While wholesale prices for Rx drugs are strictly regulated – imposing a fixed surcharge and capping discounts – the procurement of OTC products is free from such restrictions.⁷ This asymmetry typically allows high-volume pharmacies (often located in urban areas or as mail-order pharmacies) to negotiate lower unit costs for OTC inventory, whereas the fixed-price Rx margin serves as the foundation covering fixed operational costs (labor, rent,

³See, López Vila et al. (2023) for a European comparison, noting that 17 of 30 countries permit pharmacy chains.

⁴See Appendix Figure D.1. For discussions on supply security, see (Albrecht et al., 2020; Knobloch & Schröder, 2023, 2024).

⁵See § 78 AMG (*Arzneimittelgesetz*). Furthermore, for statutory health insurance patients (90 % of German patients), out-of-pocket costs are capped at a standardized co-payment (typically € 5–10), rendering demand effectively price-inelastic.

⁶See § 43 AMG.

⁷For Rx drugs, wholesalers are capped at a 3.15 % surcharge plus a fixed fee of € 0.70. While volume-based discounts (Skonti) were common during the sample period, a 2024 Federal Court of Justice (BGH) ruling has since declared them largely impermissible, retroactively highlighting the regulatory tightness of the segment. (BGH, judgment of 08.02.2024 - I ZR 137/23). See, <https://juris.bundesgerichtshof.de/cgi-bin/rechtsprechung/document.py?Gericht=bgh&Art=en&Datum=Aktuell&Sort=12288&nr=137213&pos=9&anz=1370&Blank=1.pdf> and <https://www.pharmazeutische-zeitung.de/bgh-sieht-skonto-als-rabatt-145378/>. In contrast, OTC procurement allows for any negotiated volume-based rebates.

mandated emergency services, and compliance).⁸

One argument from the literature suggests that pharmacies could utilize the liberalized OTC segment to compete on price and attract customers (Albrecht et al., 2020, p. 37-38). As Albrecht et al. (2020, p. 58-60 and 65) highlight, aggressive OTC pricing may serve as a lever to capture the demand for *bundled* (joint) purchases, thereby securing the associated Rx prescriptions, which cannot be discounted directly, or additional OTC drugs. However, this mechanism may also operate in reverse. Instead of using OTC drugs to attract Rx volume, pharmacies may leverage the Rx prescription to increase margins on OTC drugs and non-pharmaceuticals. Thus, the necessity of the Rx visit and fixed prices may create a captive consumer base, allowing the pharmacy to extract rents through higher margins on the secondary OTC purchase (due to one-stop shopping). Consequently, where local competition is limited – typically in rural areas – margins are expected to be systematically higher. The empirical occurrence of such bundled purchases will be analyzed in Section 3.3, while the analysis of local competition is conducted in Section 5.4.

2.3 Online Competition and the Limits of Local Market Power

The liberalization of mail-order dispensing in 2004 introduced a structural break in the pharmacy market, establishing an *outside option* for consumers that constrains the pricing power of local pharmacies. While B&M pharmacies remain legally tethered to physical locations (§ 11a ApoG), large foreign incumbents (e.g., DocMorris, Shop-Apotheke) have captured significant market share by leveraging economies of scale to undercut local prices. This outside option is critical for the subsequent analysis because even the most spatially rural local pharmacy can be expected to face competition from these ubiquitous mail-order pharmacies.

Data confirms that mail-order pharmacies aggressively target the more price-elastic OTC segment. On average, online competitors offer discounts of approximately 28 % below the recommended retail price whereas B&M pharmacies only discount by 9 % (Albrecht et al., 2020, p. 38). Leveraging economies of scale, superior management and marketing resources (Albrecht et al., 2020, p. 16-17), these entrants have expanded rapidly in the OTC

⁸The wholesale market is a highly concentrated oligopoly (market shares in 2024: Phoenix (30%), Alliance Healthcare (24%), NOWEDA (24%), Sanacorp (17%) and Pharma Privat (5%); see <https://www.rebmann-research.de/teil-6-der-pharmazeutische-grosshandel-ist-mehr-als-nur-zulieferer-von-apotheken>). Evidence on details of negotiations between wholesalers and B&M pharmacies is scarce, but information suggests that wholesalers exhibit substantial negotiation power and negotiations usually occur once for the succeeding year (establishing the overall purchasing conditions). See, e.g., <https://www.apotheke-adhoc.de/nachrichten/detail/markt/treuhand-konditionen-besser-verhandeln/> and <https://www.apotheke-wirtschaft.de/heftarchiv/2018/03/wie-sie-sich-am-besten-vorbereiten.html>.

segment. Over the last decades, their market share in the OTC segment has surged from 5 % in 2008 to 22.7 % by 2024 (ABDA, 2021, 2025; Statista, 2024a). In contrast, their share of the Rx market remains marginal (0.9 %–1.4 %) due to the urgency of acute care and regulatory barriers (ABDA, 2024, 2025). This dichotomy reinforces the mechanism described above: while B&M pharmacies retain the Rx volume due to immediate availability, their ability to cross-subsidize this service via high OTC margins is constantly threatened by the lower-priced online alternatives (see discussion in Section 6).

The competitive balance between B&M pharmacies and foreign mail-order rivals was significantly disrupted by a 2016 ruling of the European Court of Justice (ECJ). The Court held that imposing German fixed-price regulations on foreign mail-order pharmacies constituted an unjustified restriction on the free movement of goods.⁹ This decision introduced a period of regulatory arbitrage, allowing foreign entrants to offer (small) bonuses on Rx drugs while domestic incumbents remained bound by fixed prices.¹⁰

To restore regulatory parity, the German government enacted the *Vor-Ort-Apotheken-Stärkungsgesetz* (Local Pharmacy Support Act) in December 2020.¹¹ This legislation mandated that all pharmacies – including foreign mail-order – adhere to fixed co-payment prices for prescriptions covered by statutory health insurance, i.e., forbidding rebates that were previously exploited by foreign competitors. While rebates remain legal for private prescriptions and privately insured patients (≈ 10 % of all insured persons), the Act effectively neutralized the price advantage of online competitors.¹²

This institutional shift, combined with the persistently low Rx market share of mail-order pharmacies (≈ 1 %) throughout the sample period, underscores that B&M pharmacies successfully capture and retain consumers through the Rx segment. This structural captivity provides the foundation for the cross-subsidization discussion in the following sections. While the recent mandatory rollout of electronic prescriptions (E-Rezept)¹³ in 2024 is expected to fundamentally alter these dynamics, the market structure during the sample period was defined by the high transaction costs of the analogue

⁹ECJ Case C-148/15 (Judgment of 19 October 2016), <https://curia.europa.eu/juris/document/document.jsf?text=&docid=184671&pageIndex=0&doclang=EN&mode=lst&dir=&occ=first&part=1&cid=3568174>.

¹⁰Foreign pharmacies typically structured these discounts as bonuses redeemable on future purchases of OTC or non-pharmaceutical goods.

¹¹For details on the market impact, see (Gail et al., 2025).

¹²See German Federal Ministry of Health (<https://www.bundesgesundheitsministerium.de/themen/krankenversicherung/zahlen-und-fakten-zur-krankenversicherung/mitglieder-und-versicherte>). 73.3 million statutory health insured vs. 8.7 million privately insured patients in 2020.

¹³See the German Federal Ministry of Health’s information on the e-prescription rollout: <https://www.bundesgesundheitsministerium.de/e-rezept.html>.

paper regime.¹⁴ The electronic prescription removes this barrier, potentially reducing the switching cost to a digital transaction (Albrecht et al., 2020, p. 76). Consequently, the captive nature of the Rx consumer, proposed in this analysis to contextualize the findings, represents a structural feature of the paper-based system, which may face erosion as digitization decouples the Rx transaction from the physical point of sale.

3 Data, Descriptive Statistics and Stylized Facts

This section details the high-frequency transaction dataset. Section 3.1 outlines the data source and variable construction, followed by Section 3.2 detailing data handling and balancing procedures. Section 3.3 presents descriptive statistics characterizing market structure, price dynamics, and profitability, concluding with an analysis of joint purchasing patterns (bundle purchases).

3.1 Data Sources

The empirical analysis relies on a proprietary high-frequency dataset comprising transaction records for 9,231 B&M pharmacies from January 2018 to June 2022, including approximately 4 billion observations. The panel covers roughly 50 % of the German pharmacies, ensuring broad representativeness.¹⁵ Data access was provided by the three leading suppliers of pharmacy merchandise information systems (MIS).¹⁶

Raw transaction records consist of individual line items captured at the point of sale. Table 1 provides an overview of the data structure and the relevant variables. Products are uniquely identified via the pharmaceutical central number (PCN), a standardized 8-digit code capturing the brand, manufacturer, package size, and dosage form.¹⁷ This granularity allows for the precise segmentation of sales into non-pharmaceuticals (e.g., cosmetics, medical devices), OTC drugs, and price-regulated Rx pharmaceuticals. For each transaction, the dataset records the quantity sold, the wholesale price, and the retail price. The analysis exclusively utilizes net-of-VAT retail

¹⁴During this period, patients wishing to use online pharmacies were required to physically mail the original paper prescription, creating a significant friction that effectively shielded local pharmacies from digital competition.

¹⁵Germany had 18,461 pharmacies at the end of 2021; see <https://www.abda.de/aktuelles-und-presse/zdf/>.

¹⁶Providers include Awinta (<https://www.awinta.de/>), ADG (<https://www.adg.de/>), and Pharmatechnik (<https://www.pharmatechnik.de/>). An MIS manages a pharmacy's technological and informational infrastructure, including inventory management, automatically logging item-level data.

¹⁷The PCN (in German: *Pharmazentralnummer*) is the official German identification key for all pharmaceutical products and medical devices. See, https://www.bfarm.de/EN/Medical-devices/Tasks/DiGA-and-DiPA/Digital-Health-Applications/Interesting-facts/_node.html.

Variable/Indicator	Level	Description
<i>Transaction Data Variables</i>		
Sales Volume	Product/Pharmacy	Number of packages sold per transaction.
Net Retail Price	Product/Pharmacy	Daily Net Retail Price to the consumer (in €).
Wholesale Price	Product/Pharmacy	Daily Wholesale Price (Cost) paid by the pharmacy (in €).
Absolute Markup	Product/Pharmacy	Net retail price minus wholesale price.
Relative Markup (Lerner-Index)	Product/Pharmacy	Absolute Markup divided by Net retail price.
<i>Product Characteristics</i>		
PCN	Product	Unique 8-digit product identifier capturing brand, manufacturer, package size, and dosage form.
ATC1-5	Product	Hierarchical Anatomical Therapeutic Chemical classification; ATC4/5 denote the chemical substance level. Unique to each PCN.
<i>Aggregated Pharmacy Statistics</i>		
Total Sales Volume	Pharmacy	Total number of packages sold.
Revenue	Pharmacy	Total turnover generated (in €).
Total costs	Pharmacy	Total variable costs from sales (calculated as Wholesale Prices × Sales Volume).
Gross Profits	Pharmacy	Revenue minus total costs.
<i>Spatial and Socio-Demographic Characteristics</i>		
Location of Pharmacy	Two-digit zip code	Pharmacies are pseudonymized at the two-digit zip code level.
Degree of Urbanization	Two-digit zip code	Classification (Urban/Intermediate/Rural) based on the Eurostat definition; sourced from the Federal Statistical Office of Germany.
Socio-Demographics	Two-digit zip code	Includes population structure, household counts, and average purchasing power (sourced from Acxiom Deutschland GmbH, Frankfurt (2024)).

Table 1: Overview of Variables and Data Structure

prices.¹⁸ This approach further allows for the calculation of the absolute markup and the relative markup (Lerner Index) at the transaction level (see Footnote 1). Aggregating these unit margins by sales volume yields the total gross profit per pharmacy, defined here as total net revenue minus the cost of goods sold, excluding fixed operating expenses such as labor or rent.¹⁹

To ensure strict data privacy, the MIS pseudonymized individual pharmacies using unique identifiers and restricted spatial resolution by truncating the standard five-digit postal code to the leading two digits. Consequently, identity and location of each pharmacy remain unobservable, with the two-digit region serving as the spatial unit for geographical market definition.

To control for product heterogeneity, transaction records are enriched with detailed attribute data via the PCN identifier. Key characteristics – including brand, manufacturer, dosage form, and active ingredient – are primarily sourced from the healthcare analytics provider IQVIA. This meta-data facilitates the classification of substances according to the Anatomical Therapeutic Chemical (ATC) system (see next paragraph). To ensure comprehensive coverage across the entire portfolio, the dataset is supplemented with web-scraped data from major online pharmacies (DocMorris, Shop-Apotheke) and the *Gelbe Liste Pharmaindex*.²⁰

In the empirical analyses, an average competitor price variable will be used, ensuring that the analyses capture price changes and responses among similar chemical substances from other pharmacies. To determine this, PCN identifiers are mapped to the ATC classification, a hierarchical framework based on standards maintained by World Health Organization (WHO) and the European Pharmaceutical Market Research Association (EPHMA) that categorizes pharmaceuticals from broad anatomical groups (Level 1) to specific chemical substances (Level 4/5) (see, e.g., Hollingworth & Kairuz, 2021).²¹ The analysis relies on the ATC4 or ATC5 level as the primary definition of the relevant product market. From a demand perspective, products sharing the same ATC4 or ATC5 code are functionally equivalent, sharing both the active ingredient and the route of administration, and are thus

¹⁸This is done to prevent the mechanical inflation of pass-through estimates by the value-added tax (VAT) rate.

¹⁹In the case of Rx drugs, the remuneration per drug is fixed for every German B&M pharmacy and follows a specific statutory scheme (see, e.g., Gail et al., 2025, Appendix A).

²⁰See <https://www.docmorris.de/>, <https://www.shop-apotheke.com/>, and <https://www.gelbe-liste.de/>. The *Gelbe Liste* serves as a standard pharmaceutical reference for medical professionals.

²¹The dataset integrates the standard WHO version (https://www.whocc.no/atc/structure_and_principles/) with the EPHMA (<https://www.ephma.org/anatomical-classification>) adaptation to ensure comprehensive coverage. See <https://www.who.int/tools/atc-ddd-toolkit/atc-classification>.

treated as close substitutes.²²

In addition, to facilitate a descriptive analysis of sales volume and profitability across different medical domains, the granular ATC codes are aggregated into broader therapeutic categories. This aggregation is utilized to highlight the distribution of pharmacy sales, revenues and profits across major health indications, and will be a part of the robustness analysis. As outlined in Appendix Table D.1, specific ATC2 codes are mapped to distinct therapeutic systems. This classification scheme groups related treatment areas – such as *Respiratory System* (ATC1 code: R) or *Musculoskeletal System and Pain Management* (comprising analgesics like N02 and anti-inflammatory drugs like M01) – into consolidated categories.

To analyze socio-demographic heterogeneity, the transaction records are augmented with external geographic and socio-economic data. Data on the degree of urbanisation are sourced from the Federal Statistical Office of Germany, utilizing Eurostat definitions to classify five-digit postal code areas into urban, intermediate, and rural zones based on population density and settlement patterns.²³ Additionally, socio-economic covariates – including population structure, household counts, and average purchasing power – are obtained from Acxiom at the same five-digit level.²⁴ A central constraint is the spatial resolution of the pharmacy data, which is restricted to the two-digit postal code level (comprising 95 distinct regions, versus 8,170 five-digit codes). To align the datasets, the high-resolution socio-demographic and urbanisation data are aggregated from the five-digit to the two-digit level. Consequently, all regional heterogeneity variables utilized in this part of the analysis are constructed at the two-digit postal-code level (refer to Section 5.4 for detailed aggregation procedures).

3.2 Data Handling

Prior to the estimations, raw transaction records are aggregated to the daily level for each pharmacy-product pair. While the majority of data points represent single transactions (as will be detailed in Table 2), multiple sales of the same product (PCN) within a single day are consolidated into one daily observation by summing quantities and computing the volume-weighted average price.

To ensure a stable market structure, the sample is restricted to a balanced panel of pharmacies that remain continuously active (each quarter)

²²A single chemical substance may be assigned to multiple ATC4/5 codes if the route of administration differs (e.g., Ibuprofen is classified under M01AE01 for oral use but M02AA13 for topical application). Defining the market at the ATC5 level prevents the erroneous treatment of disparate forms (e.g., ointment vs. tablets) as substitutes.

²³See https://www.destatis.de/DE/Themen/Laender-Regionen/Regionales/Gemeindeverzeichnis/_inhalt.html and <https://ec.europa.eu/eurostat/web/degree-of-urbanisation/information-data>.

²⁴See Acxiom Deutschland GmbH, Frankfurt (2024).

throughout this window.²⁵ It is important to note that while the set of pharmacies is fixed, the panel remains unbalanced at the product level, as the specific set of PCNs transacted varies daily for each pharmacy based on local demand and inventory.

To mitigate distortions from data recording errors (e.g., implausible booking entries), the dataset undergoes an outlier cleaning procedure. Specifically, the distributions of prices, price changes, and sales are trimmed using a percentile-based cutoff corresponding to a five-sigma threshold.²⁶

Following these procedures, the final balanced panel covers the period from the first quarter of 2018 through the second quarter of 2022 and comprises 6,338 unique pharmacies, representing approximately one-third of all German B&M pharmacies. This sample size ensures substantial depth and broad geographical representation. As illustrated in Figure 1, the dataset maintains wide spatial coverage even after balancing, demonstrating that the included pharmacies are geographically dispersed across all major two-digit postal-code regions.²⁷

²⁵Data were supplied in distinct batches, leading to potential attrition unrelated to market dynamics. As it is not feasible to definitively distinguish between genuine market exit and the discontinuation of data provider contracts, this balancing criterion ensures that all included entities are consistently observed over the entire sample duration.

²⁶This is implemented by excluding observations that fall outside the theoretical quantiles associated with a standard deviation of five. For sales, the lower percentile cutoff implies a value below the minimum feasible transaction size (one unit). Consequently, the procedure effectively operates as a one-sided trim, removing only the extreme upper tail. While the broader literature often employs more restrictive techniques – such as trimming the top and bottom 0.5 % of the distribution (e.g., Fagereng et al., 2020, p. 122) – the conservative criterion adopted here prioritizes the retention of genuine market volatility. This approach effectively removes technical errors while preserving the integrity of the underlying data structure.

²⁷Since official administrative data on the number of pharmacies by two-digit postal-code region are not publicly available, market coverage was approximated by scraping location data from <https://www.apotheken-umschau.de/apothekenfinder/> on February 24, 2021.

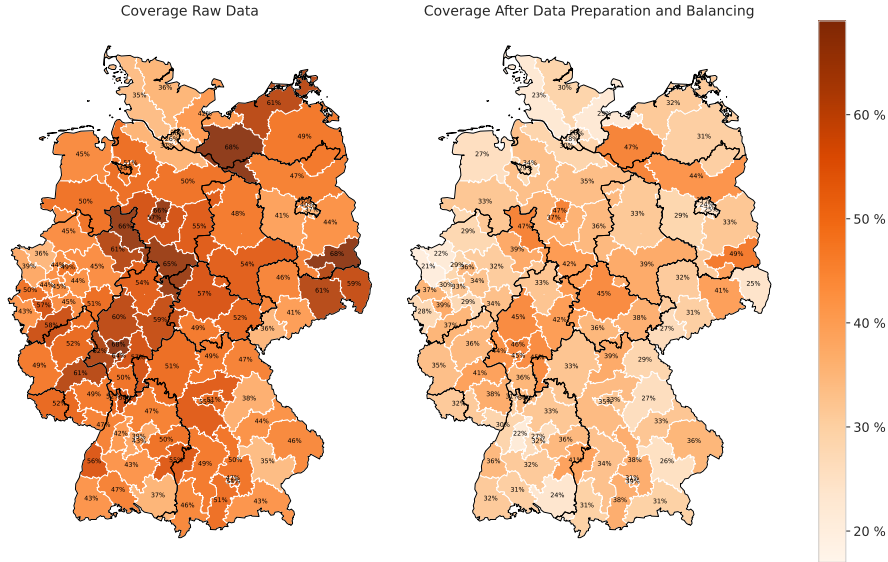


Figure 1: Coverage of Pharmacy Sales Data Across German two-digit zip-code regions. White (black) lines indicate the two-digit zip-code region (federal state) contours. Source: MIS suppliers and web page of *Apothekenumschau* (<https://www.apotheken-umschau.de/apothekenfinder/>), scraped on February 24, 2021.

The initial, unbalanced dataset comprises approximately 1.46 billion non-prescription packages, encompassing both OTC drugs and non-pharmaceuticals such as medical devices and cosmetics. Following the pharmacy balancing and outlier cleaning procedures, the final analytical sample retains 1.21 billion packages, representing 83.4 % of the initial non-prescription volume.²⁸ This volume corresponds to 913,035,552 distinct transaction line items. Notably, the outlier trimming procedure itself resulted in negligible attrition, with 99.87 % of observations in the balanced panel remaining intact.

Although the balanced panel is constructed primarily for the econometric analysis of OTC drugs and non-pharmaceutical competition, all available Rx drug transaction records are retained for the descriptive analysis in Section 3.3.²⁹ This retention allows for a comprehensive assessment of the pharmacies' overall economic performance – including total revenue, aggregate sales volume, and gross profit structure – before the analysis focuses exclusively on the non-prescription segment.

²⁸In monetary terms, retention is similarly high: of the initial €11.98 billion in non-prescription revenue, €10.02 billion (83.65 %) is preserved in the balanced sample.

²⁹For the balanced set of pharmacies, this segment accounts for an additional 1.36 billion Rx packages.

3.3 Descriptive Statistics and Stylized Facts

This section characterizes the dataset structure and market dynamics. First, summary statistics at the PCN level are presented to establish the central tendencies and distributional properties of the empirical sample. Second, the distribution of price adjustments is examined to characterize the frequency of price changes. Third, pharmacy sales, revenues and gross profits are decomposed into OTC, non-pharmaceutical, and Rx segments to quantify the relative economic contribution of each channel. Finally, the prevalence of joint purchases – the concurrent purchase of prescription and non-prescription items – is analyzed to provide descriptive evidence on potential cross-segment complementarities.

	Mean	SD	P001	P01	P05	P25	Median	P75	P95	P99	P999
Sales in Packages	1.38	1.75	1	1	1	1	1	1	3	6	15
(Net) Retail Price in € per Package	8.64	7.32	0.92	1.51	2.12	4.87	7.47	10.1	20.2	34	73.5
Wholesale Price in € per Package	4.95	5.29	0.38	0.58	0.91	2.48	3.99	5.62	12.8	23.8	53.2
Absolute Markup in € per Package	3.69	2.5	0.05	0.47	1	2.13	3.26	4.59	7.87	12.5	22.6
Ratio Retail Price to Wholesale Price	1.95	4.14	1.01	1.14	1.34	1.63	1.87	2.07	2.85	3.83	5.66
Relative Markup (Lerner-Index)	0.455	0.116	0.0119	0.121	0.256	0.385	0.466	0.516	0.649	0.739	0.823
Δ (Net) Retail Price in € per Package	0.0156	0.705	-5.9	-2.08	-0.26	0	0	0	0.42	2.22	6.17
Δ Wholesale Price in € per Package	0.00866	0.346	-2.89	-0.76	-0.04	0	0	0	0.12	0.91	3.2
Δ Between Sales in Days	22.8	69.8	1	1	1	2	5	16	90	314	951
Count											
Number of Observations	504,909,293										
Unique Pharmacy IDs	6,338										
Unique 2 digit zip codes	95										
Unique Year-Days	1,642										
Unique PCN	44,623										
Unique PCN-Pharmacy Combinations	17,699,806										

(a) Summary statistics for OTC drugs.

	Mean	SD	P001	P01	P05	P25	Median	P75	P95	P99	P999
Sales in Packages	1.27	4.45	1	1	1	1	1	1	2	5	16
(Net) Retail Price in € per Package	9.39	10	0.46	0.67	1.25	3.32	6.72	12.5	25.2	47	84
Wholesale Price in € per Package	6.35	7.28	0.18	0.44	0.79	2.07	4.26	8.36	17.7	35.1	64.5
Absolute Markup in € per Package	3.04	3.39	0.01	0.12	0.31	1.05	2.23	4.03	8.29	14.6	27.9
Ratio Retail Price to Wholesale Price	1.67	14.7	1	1.04	1.16	1.35	1.5	1.7	2.15	2.85	5.7
Relative Markup (Lerner-Index)	0.338	0.121	0.00228	0.0429	0.14	0.261	0.335	0.413	0.534	0.649	0.825
Δ (Net) Retail Price in € per Package	0.0181	0.919	-7.58	-2.77	-0.46	0	0	0	0.62	2.91	7.94
Δ Wholesale Price in € per Package	0.00863	0.527	-4.66	-1.31	-0.14	0	0	0	0.24	1.42	4.74
Δ Between Sales in Days	40.4	94.7	1	1	1	4	12	35	173	476	1,084
Count											
Number of Observations	408,126,138										
Unique Pharmacy IDs	6,338										
Unique 2 digit zip codes	95										
Unique Year-Days	1,642										
Unique PCN	168,493										
Unique PCN-Pharmacy Combinations	31,199,825										

(b) Summary statistics for Non-pharmaceuticals.

Table 2: Summary statistics. Note: P001 = 0.1th percentile, P01 = 1th percentile, P05 = 5th percentile, P25 = 25th percentile, P75 = 75th percentile, P99 = 99th percentile, and P999 = 99.9th percentile.

Table 2 presents detailed summary statistics for the (pharmacy) balanced panel. Panel (a) reports descriptive metrics for OTC drugs, while Panel (b) covers non-pharmaceutical products. The table summarizes the distributions of daily sales, wholesale and net retail prices. To capture the structure of profitability, it further reports the absolute markup, the relative markup (Lerner Index), and the retail-to-wholesale price ratio. Additionally, the

magnitude of price adjustments is documented via the differenced variables (Δ Wholesale Price and Δ Retail Price). To characterize product turnover rates, the variable Δ *Between Sales in Days* records the time elapsed between consecutive transactions of the same product.³⁰ In addition to means and standard deviations, a broad range of percentiles (P001 to P999) is provided to document the substantial heterogeneity and extreme values in both segments.

The dataset’s granularity is reflected in the substantial observation counts. The OTC segment comprises approximately 505 million product-pharmacy-day observations derived from 17.7 million unique product-pharmacy combinations. This segment covers 44,623 unique PCNs. As detailed in Appendix Tables D.2 and D.3, these products are not uniformly distributed; rather, the market is structurally dominated by acute care treatments. Specifically, products for the *Respiratory System* (e.g., cold remedies) and the *Musculoskeletal System* (e.g., pain management) act as the primary volume drivers, accounting for 32.3 % and 23.3 % of total OTC sales volume, respectively. The non-pharmaceutical segment of Table 2 is similarly extensive, containing 408 million observations across a more diverse range of 168,493 unique products and 31.2 million product-pharmacy pairs. However, this segment is heavily concentrated in general goods, with the *No ATC Code* category – representing diverse personal care and hygiene items – comprising 59.7 % of the segment’s volume (see Tables D.2 and D.3). Pooling these three leading categories reveals a high degree of concentration as they collectively account for approximately 61 % of the entire sales volume in the sample.³¹ Across both segments (OTC drugs and non-pharmaceuticals), the descriptive statistics track the same set of 6,338 balanced pharmacies distributed across 95 two-digit postal code regions over a 1,642-day time horizon.

Both segments exhibit distinct structural characteristics. Sales are highly right-skewed across both categories, anchored by a median of one package per transaction-day, indicating that bulk purchases are rare. While average net retail prices are broadly comparable (€8.64 for OTC versus €9.39 for non-pharmaceuticals), the underlying cost structures diverge significantly. Specifically, OTC drugs incur relative lower average wholesale costs (€4.95) than non-pharmaceuticals (€6.35). Consequently, the OTC segment exhibits superior profitability, commanding an average Lerner Index of 0.455 com-

³⁰Example: If a specific product (identified by its PCN) is sold on March 1st and the next transaction for the same product occurs on March 5th, the variable takes a value of 4. Consequently, lower values indicate high-turnover, whereas high values characterize slow-moving niche items.

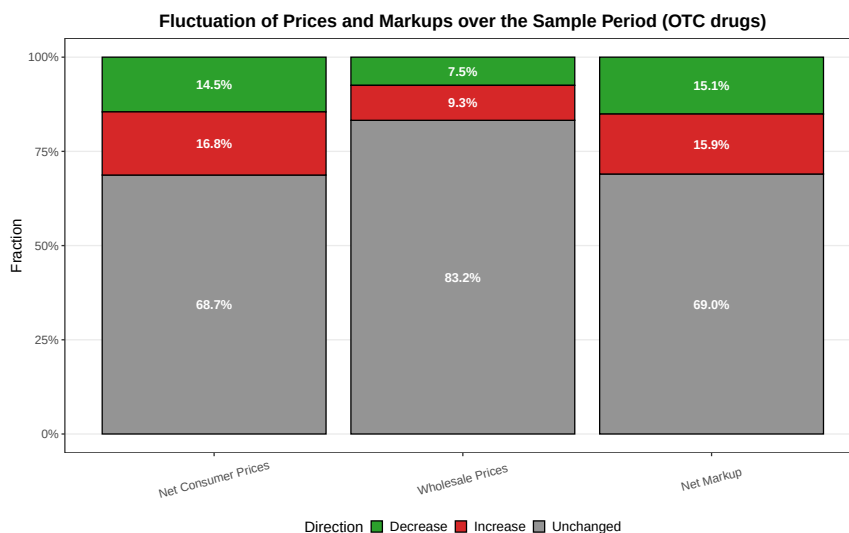
³¹In terms of revenue, the market structure is similarly concentrated, though slightly less pronounced due to price heterogeneity. Within the OTC segment, *Respiratory* and *Musculoskeletal* products generate 26.8 % and 19.2 % of revenue, respectively. In the non-pharmaceutical segment, *No ATC Code* category accounts for 53.3 % of revenue. Combined, these three categories represent 52.3 % of the total revenue in the balanced panel.

pared to 0.338 for non-pharmaceutical goods. Thus OTC drugs exhibit 35 % higher Lerner Indices than non-pharmaceuticals.

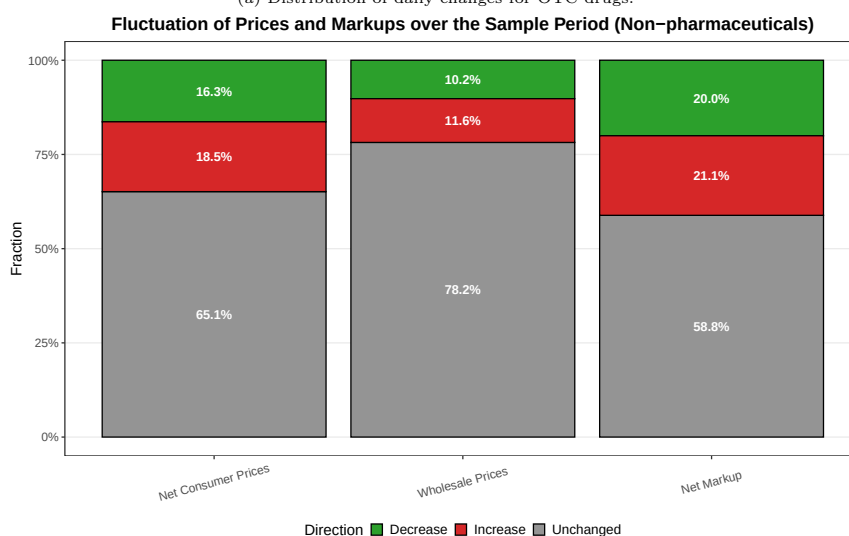
Beyond static levels, the table captures the dynamics of price adjustment through the differenced variables Δ Net Retail Price and Δ Wholesale Price. These metrics are fundamental for the analysis of asymmetric pass-through in Section 5.1. Crucially, these differentials are computed relative to the *last observed transaction* for each pharmacy-product pair, rather than the previous calendar day (e.g., the median interval between consecutive OTC drug transactions is five days). This approach ensures that price changes are accurately identified even for products with irregular sales frequencies, bridging periods of inactivity without generating spurious zero-price changes, which would occur if days without sales were artificially populated with the last known price. As indicated by the mean values (e.g., €0.0156 for OTC retail price changes), the average transaction-to-transaction adjustment is small, yet the wide range highlights the presence of substantial discrete price jumps.

To visualize the frequency and direction of these adjustments, Figure 2 decomposes the daily fluctuations of net retail and wholesale prices, and net markups. Because raw transactions are aggregated to the daily level for each pharmacy-product pair, a single observation may encompass multiple individual sales. Therefore, the distribution separates *observations* into three distinct states: decreases (green), increases (red), and periods of rigidity where values remain unchanged (grey). Because the absolute net markup is defined as the spread between retail and wholesale prices, its volatility (Δ markup) is mechanically determined by the interplay between retail price adjustments and wholesale price shocks. Consequently, a change in the absolute markup occurs whenever the pass-through of a wholesale cost shock deviates from unity (e.g., is incomplete, delayed, or overshifted), or when retail prices are adjusted independently of cost shifts.

Panel (a) illustrates the dynamics for OTC drugs. Wholesale prices remain unchanged in 83.2% of observations, though exhibiting decreases in 7.5% and increases in 9.3% of cases. Retail prices, however, are adjusted more frequently, changing in 31.3% of observations (14.5% decreases, 16.8% increases). Consequently, net markups are not constant; they fluctuate in 31.0% of cases. This discrepancy in the frequency of adjustments – where retail prices change nearly twice as often as wholesale prices – indicates that price updates do not perfectly synchronize with cost shocks in the cross-section. Panel (b) depicts the corresponding patterns for non-pharmaceutical products, which exhibit higher volatility across all dimensions. Wholesale prices change in 21.8 % of observations, while retail prices adjust in 34.9 % of cases. Consequently, net markups fluctuate more often, changing in 41.2 % of observations. Collectively, these descriptive statistics establish that markup volatility is a pervasive feature of the pharmacy market. The extent to which these retail adjustments are driven by wholesale price shocks is the central



(a) Distribution of daily changes for OTC drugs.



(b) Distribution of daily changes for non-pharmaceuticals.

Figure 2: Overall daily changes. (a) Prices and markups for OTC drugs. (b) Prices and markups for non-pharmaceuticals. Note: Fractions represent the share of total daily observations (pharmacy-product pairs) exhibiting a change, independent of sales volume.

focus of the ECM-based pass-through estimation presented in Section 5.

To provide a broader economic context, the following analysis focuses on stylized aggregates of the transaction data at the year level, distinguishing between the regulated Rx-drug sector and the unregulated segment (OTC drugs and non-pharmaceuticals). Figure 3 provides aggregate evidence of the evolution of sales, revenue, and gross profits from 2018 through 2021.³²

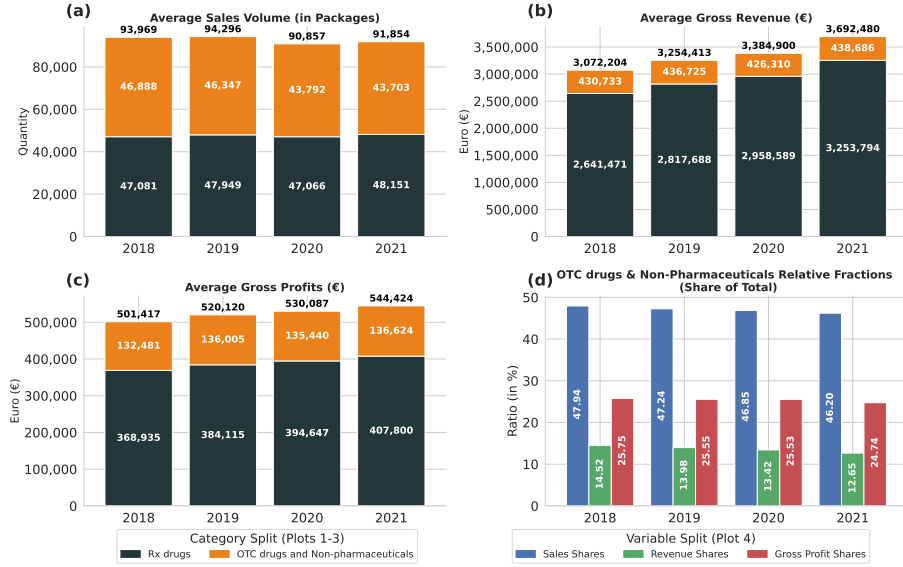


Figure 3: Evolution of average sales, revenue and gross profits per year. For further descriptive statistics highlighting the distribution across pharmacies, see Table D.4.

Panel (a) and Panel (b) of Figure 3 reveal a distinct contrast between the physical volume of trade and the financial revenue it generates. In terms of packages sold (Panel (a)), the pharmacy business is split almost evenly: on average, a pharmacy dispenses approximately 47,000 to 48,000 Rx packages annually, which is very similar to the volume of unregulated products sold (ranging from roughly 44,000 to 47,000 packages). However, when looking at revenue (Panel (b)), Rx drugs dominate. For instance, in 2021, the average pharmacy generated €3.25 million from Rx drugs compared to just €0.44 million from the unregulated segment. This huge gap exists because prescription drugs are, on average, much more expensive per package than OTC or non-pharmaceutical items.

Crucially, Panel (c) and Panel (d) highlight a shift in the composition of pharmacy profitability. Rx drugs drove the overall growth in gross profits, increasing from an average of €368,935 in 2018 to €407,800 in 2021.

³²Detailed evidence is provided in Appendix D Table D.4. Figures illustrating the distribution of gross profits, revenue, and sales are provided in Figures D.2 through D.6. Note: The year 2022 is not exhibited as only the first half of 2022 is available.

In contrast, gross profits from the unregulated segment remained virtually stagnant in nominal terms, rising only marginally from €132,481 to €136,624 over the four-year period. This nominal increase of approximately 3.1% lags behind the inflation rate of roughly 5.1% recorded in Germany between 2018 and 2021.³³ Consequently, the unregulated segment has experienced a decline in real profitability. This trend is mirrored in Panel (d), where the segment’s share of total gross profits falls from 25.75% to 24.74%. Concurrently, the average revenue share of the unregulated segment consistently remains below the gross profit share, falling from 14.52 % to 12.65 % over the sample period. Thus, OTC drugs and non-pharmaceutical products act as disproportionately strong profit drivers despite their smaller revenue share. This relative decline in profits likely reflects the growing competitive pressure from online pharmacies, which have increasingly captured market share in the non-prescription sector, leaving B&M pharmacies more dependent on the regulated prescription business (see Section 2).

Another crucial determinant of pharmacy profitability is the composition of the consumer’s shopping basket. Specifically, consumers frequently purchase OTC drugs and non-pharmaceuticals in conjunction with prescription medications – a transaction referred to in the sector as a *bundle purchase* (see Section 2.2 and Albrecht et al., 2020, p. 24).³⁴ Analyzing these joint purchasing patterns provides insights into cross-selling potential and consumer convenience. The dataset allows to use the unique *order number* to identify the combination of purchases made by a single customer. It is important to note that this method establishes a conservative *lower bound* for the true frequency of joint purchasing.³⁵

Figure 4 illustrates this relationship on an aggregate scale for the years 2018 through 2021. The figure decomposes the total volume of non-prescription sales into three mutually exclusive categories: (1) sales occurring in single-product orders (orange bars), (2) sales occurring in multi-product orders containing only non-prescription products (black bars), and (3) sales occurring in joint-purchases containing at least one Rx-drug (teal bars).

³³Based on the Consumer Price Index (CPI) data from the Federal Statistical Office of Germany, the inflation for this period is approximately 5.1%. See, <https://www-genesis.destatis.de/datenbank/online/url/1481501c>.

³⁴In this context, the term “bundle” is used descriptively to denote joint purchasing behavior rather than a strategic supply-side constraint (such as tying, pure bundling, or mixed price bundling) as rigorously defined in the industrial organization and marketing literature (Adams & Yellen, 1976; Stremersch & Tellis, 2002).

³⁵Operational realities in pharmacy practice may cause a single patient visit to be split into multiple transactions. First, if a prescribed medication is out of stock, the patient must undertake a further physical visit, separating purchases temporally. Second, patients or staff may separate insurance-reimbursed items from private purchases due to distinct workflow requirements at the point of sale. Consequently, the observed share of sales in bundles represents a conservative lower bound of this joint purchasing behavior.

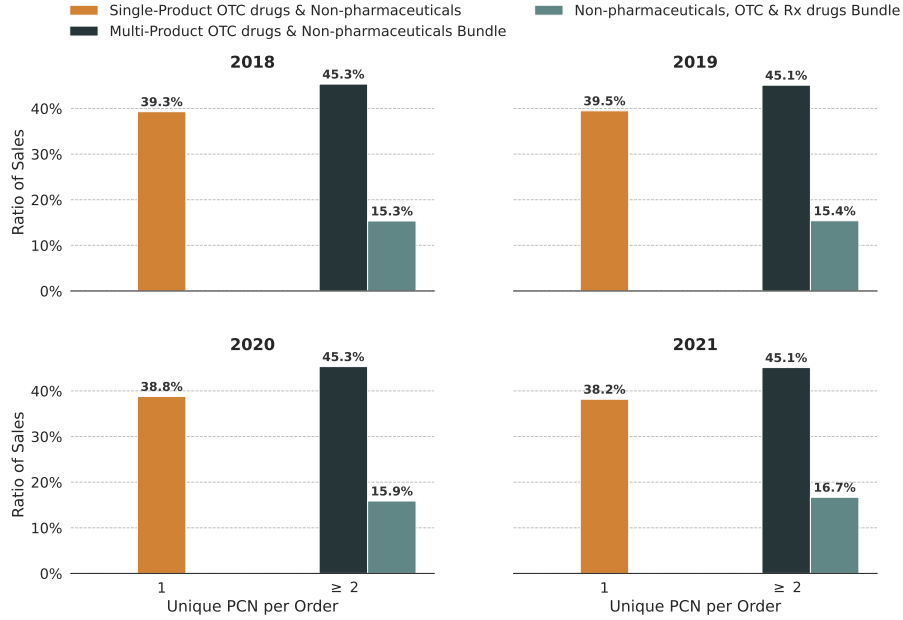


Figure 4: Sales ratio according to order size and bundle type, focusing on sales containing at least one unique PCN of OTC drugs or non-pharmaceuticals. By definition, multi-product and Rx-bundles require an order size of ≥ 2 ; therefore, single-item orders consist exclusively of standalone OTC or non-pharmaceutical purchases.

The data reveal an evolving pattern in consumer behavior. The majority of non-prescription volume is generated through pure OTC or non-pharmaceutical transactions. Specifically, single-product purchases account for approximately 38.2% to 39.5% of the volume per year, while multi-product OTC and non-pharmaceutical orders constitute the largest share, fluctuating narrowly around 45.1% to 45.3%. The Rx-bundle purchases represent a smaller portion of the market. In 2018, these mixed bundles accounted for 15.3 % of non-prescription sales. Notably, this share exhibits a gradual upward trend, reaching 16.7 % by 2021.

Viewed through the lens of the institutional background from Section 2, this descriptive finding provides an interesting context for the subsequent analysis. It suggests a potential dynamic where, as online competitors exert pressure on the market for OTC and non-pharmaceuticals, the physical Rx visit might serve as an increasingly important anchor for secondary sales in these unregulated segment. The plausibility of this relationship will be discussed extensively in Section 6 in light of the empirical estimations presented in Section 5.

4 Empirical Strategy

This section outlines the identification strategy used to estimate cost pass-through (CPT) rates.³⁶ The identification strategy relies on an error correction model (ECM), which is chosen to structurally match the economic dynamics of cost transmission. While fluctuations in wholesale prices act as exogenous cost shocks, pharmacies do not respond to them instantaneously (see Section 3.3); rather, they seem to adjust their retail prices over a dynamic path. The ECM framework is explicitly designed to capture this temporal and dynamic structure, distinguishing between the immediate short-run response to such shocks and the gradual convergence to a long-run equilibrium (see, e.g., Borenstein et al., 1997; Peltzman, 2000). Furthermore, from an econometric perspective, this framework addresses the non-stationarity and cointegrating relationship between retail and wholesale prices, thereby mitigating the risk of spurious regression bias inherent in static level specifications (Engle & Granger, 1987; Granger & Newbold, 1974).³⁷

Section 4.1 details the ECM approach. By distinguishing between short-run dynamics and the long-run equilibrium, this model allows to formally test for pricing asymmetries – referred to as the *rockets and feathers* phenomenon. The cost changes (changes in wholesale prices) are decomposed into positive and negative shocks to identify whether pharmacies adjust prices symmetrically or exhibit downward rigidity. Specifically, the bounds testing approach (Pesaran et al., 2001) is employed to test for the existence of a long-run cointegrating relationship.

Section 4.2 describes the heterogeneity analysis, which permits pass-through rates to vary across pharmacies, revenue deciles, and product segments. This step is crucial not only for identifying idiosyncratic pricing behaviors but also for addressing the heterogeneity bias described by Pesaran and Smith (1995), who demonstrate that pooling dynamic panels with heterogeneous slope coefficients can yield inconsistent estimates.

Finally, Section 4.3 outlines the strategy for the spatial and socio-demographic analysis. This framework exploits the recovered heterogeneity in pharmacy-level parameters by regressing economic outcomes – including prices, markups, and the estimated pass-through coefficients – on the degree of urbanization and local socio-demographic controls. By isolating the variation attributable to spatial structure, this specification allows for a formal test of whether rural markets exhibit distinct pricing differences, such as a structural differences between lower transaction volumes and higher unit margins, driven by local market power. Testing this mechanism is empirically straightforward because the transaction records contain exact wholesale and retail prices, allowing absolute and relative markups (the Lerner Index)

³⁶All CPT estimations and subsequent analyses are estimated separately for non-pharmaceuticals and OTC drugs.

³⁷For comparison, results from static level models are reported in Appendix A.

to be computed directly (detailed in Section 3.1).

4.1 Dynamic & Asymmetric Strategy

To investigate potential asymmetries in pricing behavior (see, e.g., Borenstein et al., 1997; Heim, 2021), the analysis employs an asymmetric ECM (Engle & Granger, 1987; Granger & Lee, 1989; Stock, 1987). Departing from early two-step estimation procedures (e.g., Balaguer & Ripollés, 2012; Galeotti et al., 2003; Peltzman, 2000), the study leans on the strategy of Balaguer and Ripollés (2016), by specifying a single-equation conditional unrestricted ECM in the style of a non-linear autoregressive distributed lag (NARDL) model (see, e.g., Greenwood-Nimmo & Shin, 2013; Pesaran & Shin, 1999; Pesaran et al., 2001; Salisu & Isah, 2017; Shin et al., 2014). This unified framework provides superior power properties and estimation efficiency (Banerjee et al., 1998), while allowing for the identification of long-run level relationships irrespective of whether the underlying variables are $I(0)$, $I(1)$, or mutually cointegrated (Pesaran et al., 2001).³⁸ To account for unobserved heterogeneity, the model is estimated using a high-dimensional fixed effects estimator.³⁹ Furthermore, while the specification is estimated in first differences to ensure stationarity, it explicitly retains lagged levels to capture both the immediate price adjustments to cost shocks and the gradual correction toward the long-run equilibrium – a dynamic distinction shown to be critical in pharmaceutical markets, where Granlund and Bergman (2018) demonstrate that prices exhibit substantial inertia, requiring multi-period adjustments to fully reflect structural market changes.⁴⁰ This enables the simultaneous recovery of short-run impact parameters and long-run equilibrium targets in a unified framework.

The estimation specification is defined such that the subscripts denote product j (identified by PCN), pharmacy f (uniquely located in two-digit zip code g), calendar day t and calendar month m . Therefore, the unit of observation is defined at the granular product-pharmacy-day level (jf, t) . The following specification is estimated:

³⁸Given the high dimensionality of the data, standard panel cointegration tests (e.g., Westerlund, 2007) are computationally infeasible. Instead, the long-run relationship is confirmed via the significant negative coefficient of the error correction term (t-test) and the joint significance of all lagged level variables (Wald test). Furthermore, panel unit root tests confirm a mixture of $I(0)$ and $I(1)$ series, see Appendix Table E.1.

³⁹Given the high-frequency nature of our data ($T = 1,642$ and the inclusion of numerous fixed effects), the dynamic panel bias (Nickell, 1981) is of order $O(1/T)$ and therefore negligible.

⁴⁰The finding of non-stationarity and co-integration between retail and wholesale prices is common in the literature (Loy et al., 2016).

$$\begin{aligned}
\Delta p_{jft} = & \mathcal{I}_{jft}^+ \left\{ \underbrace{\rho^+ \Delta w_{jft} + \sum_{k=1}^K \beta_k^+ \Delta w_{jft-k} + \sum_{k=1}^K \phi_k^+ \Delta p_{jft-k}}_{\text{Short-Run Dynamics (+)}} \right. \\
& \left. + \underbrace{\gamma^+ p_{jft-\tau} + \lambda^+ w_{jft-\tau}}_{\text{Long-Run Adjustment (+)}} \right\} \\
& + \mathcal{I}_{jft}^- \left\{ \underbrace{\rho^- \Delta w_{jft} + \sum_{k=1}^K \beta_k^- \Delta w_{jft-k} + \sum_{k=1}^K \phi_k^- \Delta p_{jft-k}}_{\text{Short-Run Dynamics (-)}} \right. \\
& \left. + \underbrace{\gamma^- p_{jft-\tau} + \lambda^- w_{jft-\tau}}_{\text{Long-Run Adjustment (-)}} \right\} \\
& + \omega \Delta \text{Days}_{jft} + \sigma^\Delta \Delta \bar{w}_{-f,ag,t} + \sigma^\tau \bar{w}_{-f,ag,t-\tau} \\
& + \alpha_{jf} + \eta_t + \delta_{fm} + \mu_{jft},
\end{aligned} \tag{1}$$

where $\Delta p_{jft} = p_{jft} - p_{jft-\tau}$ and $\Delta w_{jft} = w_{jft} - w_{jft-\tau}$ represent the change in retail and wholesale prices, respectively, between the current transaction t and the previous transaction τ .⁴¹ To capture the temporary dynamics of the adjustment path, the model can include up to K lags of the cost changes (Δw_{jft-k}) and the autoregressive price changes (Δp_{jft-k}). The asymmetry is captured by the indicator variables $\mathcal{I}_{jft}^+$ and $\mathcal{I}_{jft}^-$, which partition the cost shocks based on their direction. Specifically, $\mathcal{I}_{jft}^+$ takes the value of 1 if $\Delta w_{jft} > 0$ (Cost Increase) and 0 otherwise, while $\mathcal{I}_{jft}^-$ takes the value of 1 if $\Delta w_{jft} < 0$ (Cost Decrease) and 0 otherwise.

As specified, the model explicitly controls for the lagged retail price level, $p_{jft-\tau}$, to capture mean-reversion dynamics (Borenstein et al., 1997). Retaining the lagged wholesale cost level ($w_{jft-\tau}$) allows to recover the long-run equilibrium target (see next paragraph). As stated, the single-equation conditional ECM is adapted for the high-dimensional fixed effects structure. The term α_{jf} denotes combined pharmacy-product fixed effects, which absorb all time-invariant heterogeneity specific to each drug within each pharmacy (e.g., baseline price levels and time-invariant local markups). Additionally, η_t controls for common daily shocks, δ_{fm} absorbs pharmacy-specific monthly trends (e.g., drifts) and μ_{jft} denotes the idiosyncratic error term. Finally, to account for the irregular time structure of the panel, the variable ΔDays_{jft} (calculated as $t - \tau$) controls for the time elapsed between observations. This

⁴¹Due to irregular transaction records the difference operator Δ must be defined relative to the last observed transaction τ .

ensures that price changes occurring over longer intervals (e.g., over weekends or due to infrequent transactions) are measured accurately and remain directly comparable to overnight adjustments.

Since individual pharmacies act as short-run price-takers in the wholesale market, their local retail pricing decisions cannot influence the upstream wholesale price. This structurally rules out reverse causality. Furthermore, the inclusion of high-dimensional fixed effects mitigates omitted variable bias: day fixed effects η_t absorb aggregate demand or supply shocks; pharmacy-month fixed effects δ_{fm} capture time-varying pharmacy heterogeneity (such as intra-year bargaining dynamics); and pharmacy-product fixed effects α_{jf} control for time-invariant heterogeneity. Collectively, these features isolate the exogenous variation in costs, permitting the interpretation of the estimated coefficients as the pass-through of wholesale cost shocks on retail prices. While pharmacies may negotiate volume-based discounts, these contracts typically are fixed over longer horizons (e.g., annually, see Footnote 8). Consequently, the high-frequency daily variation in wholesale costs ($\Delta w_{jf,t}$) exploited here reflects exogenous updates to the wholesale price rather than endogenous daily or monthly renegotiations.

Most importantly, this flexible specification permits to decompose the adjustment dynamics into three distinct mechanisms: (i) *Short-Run Asymmetry*: The coefficients ρ^+ and ρ^- estimate the immediate pass-through rate on the day of the shock, while the lag coefficients $\beta_k^\pm$ and $\phi_k^\pm$ capture the persistence and momentum of the short-run adjustment path. A finding of $\rho^+ > \rho^-$ (or a strictly larger cumulative short-run effect) provides evidence of *rockets and feathers*, indicating that prices adjust upward more sharply in response to cost increases than they adjust downward for cost declines. (ii) *Asymmetric Speed of Adjustment*: The coefficients γ^+ and γ^- measure the rate at which the pharmacy corrects towards the long-run equilibrium. If $|\gamma^+| > |\gamma^-|$, it implies that pharmacies close the gap to the new equilibrium faster when prices are below the target (after a cost increase) than when prices are above the target (after a cost drop). (iii) *Long-Run Equilibrium Asymmetry*: The coefficients λ^+ and λ^- capture the direct response to the lagged cost level, which, combined with the adjustment speeds γ^+ and γ^- , define the long-run attractor of the system. The long-run pass-through target θ is recovered non-linearly as the ratio of the lagged cost and price coefficients (see, e.g., Greenwood-Nimmo & Shin, 2013):

$$\theta^k = -\frac{\lambda^k}{\gamma^k}, \quad \text{for } k \in \{+, -\}. \quad (2)$$

This relationship is derived by imposing the steady-state condition where all

short-run fluctuations have ceased ($\Delta p_{jf,t} = \Delta w_{jf,t} = 0$).⁴² Consequently, θ^k represents the permanent transmission rate of a cost shock to retail prices, i.e., the long-run equilibrium pass-through rate, once the adjustment process is complete. This enables to test for long-term asymmetry where the final equilibrium price depends on the direction of the shock (Galeotti et al., 2003; Meyer & von Cramon-Taubadel, 2004).

Model validity is confirmed via Bounds Tests for cointegration (Pesaran et al., 2001), while coefficient equality and long-run symmetries are evaluated using standard Wald tests and the Delta method.⁴³

Drawing on the dynamic panel framework of Pesaran et al. (1999), the long-run speed of adjustment is restricted to be homogeneous across pharmacies to exploit the efficiency gains of a pooled estimation. This assumption is relaxed in the next Section 4.2, where cross-sectional variation in pricing dynamics of pharmacies and their products is investigated.

Last, competitors' wholesale prices are included (the short-run effect $\Delta \bar{w}_{-f,agt}$ and the lagged component $\bar{w}_{-f,agt}$), motivated by standard oligopoly theory. In a simple Nash-Bertrand framework, a firm's (equilibrium) pricing depends on the costs of its rivals. As noted by MacKay et al. (2014), omitting competitors' costs can severely bias pass-through estimates if cost shocks are correlated across firms. Due to the dimensionality of the product space, including the individual costs of all rival products is infeasible. Following the literature (e.g., Miller et al., 2017; Muehlegger & Sweeney, 2022), controlling for competitors' average wholesale prices serves as a tractable alternative. The average competitor wholesale price is calculated at the broader chemical substance level (ATC4 or ATC5) within the same geographical market g (two-digit postal code). This aggregation acknowledges that consumer substitution primarily occurs between products sharing the same active ingredient, such as Ibuprofen (M01AE01). Defining the market at this level ensures that disparate forms of the same chemical (e.g., tablets vs. topical creams) are correctly treated as distinct product markets. The coefficient σ^Δ captures the immediate strategic response to concurrent changes in

⁴²Under this condition, the error correction mechanism implies that $0 = \gamma^k p_{jf,\tau} + \lambda^k w_{jf,\tau}$. Rearranging this equation yields the steady-state relationship $p_{jf,\tau} = (-\lambda^k/\gamma^k) w_{jf,\tau}$, where $\theta^k = (-\lambda^k/\gamma^k)$ represents the long-run coefficient.

⁴³In detail, a series of Wald tests are performed to formally evaluate the model validity and the presence of asymmetry (see, e.g., Cameron & Trivedi, 2005, Chapter 7.2). Furthermore, following Pesaran et al. (2001), a Bounds Test is performed by testing the joint significance of the lagged level coefficients ($\gamma^\pm, \lambda^\pm$) to confirm the existence of a valid cointegrating relationship. Conditional on this relationship, the specifics tests for equality and nullity are conducted by the following program: (1) tests on equality of short-run coefficients ($\rho^+ = \rho^-$) using a standard F-test; (2) tests on equality of the adjustment speeds ($\gamma^+ = \gamma^-$); (3) tests on the equality of the lagged wholesale prices ($\lambda^+ = \lambda^-$); (4) tests on the joint nullity of all four parameters ($\gamma^+ = \gamma^- = \lambda^+ = \lambda^- = 0$); and (5) the Delta Method is employed to approximate the standard errors of the non-linear long-run targets (Oehlert, 1992; Cameron and Trivedi, 2005, Chapter 7.2.8) and test the null hypothesis of symmetry in the long-run equilibrium ($\theta^+ = \theta^-$).

rivals' costs, while σ^τ governs the long-run equilibrium relationship. If prices exhibit strategic complementarity (Bulow et al., 1985; Vives, 1990), both coefficients are expected to be positive, indicating that pharmacies raise prices when their competitors face higher costs, reflecting a strategic response to the resulting price increases initiated by those competitors.

4.2 Heterogeneity Strategy

The dynamic specification in Section 4.1 yields estimates of aggregate market-wide pricing dynamics. To capture structural differences across pharmacies and time, this framework is generalized by relaxing the assumption of constant parameters. Specifically, the ECM of Equation (1) is extended by allowing the pass-through and speed-of-adjustment coefficients to vary at the pharmacy-year level. This disaggregated specification serves as the foundation for the socio-demographic analysis (see Section 5.4), which exploits the resulting heterogeneity to examine pricing disparities between urban and rural markets. The heterogeneous specification is given by:

$$\begin{aligned} \Delta p_{jf,t} = & \mathcal{I}_{jf,t}^+ \left\{ \rho_{fy}^+ \Delta w_{jf,t} + \gamma_{fy}^+ p_{jf,t-\tau} + \lambda_{fy}^+ w_{jf,t-\tau} \right\} \\ & + \mathcal{I}_{jf,t}^- \left\{ \rho_{fy}^- \Delta w_{jf,t} + \gamma_{fy}^- p_{jf,t-\tau} + \lambda_{fy}^- w_{jf,t-\tau} \right\} \\ & + \omega \Delta Days_{jf,t} + \eta_t + \delta_{fm} + \mu_{jf,t} \end{aligned} \quad (3)$$

where the subscript fy denotes that the parameters ρ_{fy} , γ_{fy} , and θ_{fy} are estimated specifically for each pharmacy-year pair. Estimating this equation is computationally intensive, as it requires interacting every price and cost variable with thousands of pharmacy-year indicators. This results in a high-dimensional problem, necessitating the use of the specialized variable-slope algorithms provided by the *fixest* package (Bergé, 2018).⁴⁴

In contrast to the pooled baseline, this specification omits autoregressive lags and product fixed effects (α_{jf}) for two critical reasons. First, the inclusion of lags would drastically inflate the parameter space, reducing the degrees of freedom available for identifying the coefficients in years with lower transaction volumes. Second, and economically more significant, the omission of product fixed effects is necessitated by the limited within-product variance available in the data (see Figure 2). Since wholesale costs for many products remain stable within a single year, a fixed-effects specification would fail to identify the cost parameters for a large subset of the portfolio. By relaxing the fixed effects, the equilibrium parameters are identified using both

⁴⁴ All computations were conducted on the following hardware: AMD Ryzen Threadripper PRO 7965WX 24 Cores (48 Threads) with 256 GB of system memory running Ubuntu 24.04.

temporal and cross-sectional variation which effectively assumes that the pharmacy applies a consistent pricing strategy across its assortment, rather than relying solely on time-series volatility.

This flexible specification allows to recover the full distribution of short-run asymmetries and adjustment speeds across the market, as well as derive heterogeneous pharmacy-specific long-run targets. The resulting distributions of the estimated short-run coefficients $(\hat{\rho}_{fy}^+, \hat{\rho}_{fy}^-)$ and the derived long-run targets $(\hat{\theta}_{fy}^+, \hat{\theta}_{fy}^-)$ are visualized in Section 5.3.

Finally, to assess whether pricing dynamics for the most economically significant items differ from the full sample, Equation (1) is re-estimated using restricted samples of top-ranked products.⁴⁵ Specifically, the analysis is stratified by the top 10, 100, and 1,000 products according to three distinct metrics: sales, total revenue, and gross profit. This approach yields nine distinct estimation specifications, allowing for a granular examination of whether pass-through rates and asymmetric adjustments in the most important segments diverge from the dynamics observed in the aggregate market.

4.3 Socio-Demographic Analysis

To analyze whether the aggregate pricing dynamics mask heterogeneity across market types, pharmacy-level outcomes – specifically prices, markups, sales, and the CPT rates estimated in Section 5 – are related to the degree of urbanization and local population characteristics (see Section 3.1). This estimation determines if market structure, particularly the distinction between urban and rural environments, drives systematic differences in these economic parameters. The following specification is estimated:

$$\begin{aligned}
O_{fgy} = & \alpha + \nu \text{OTC}_d + \sum_{k \in \{I, R\}} D_g^k (\beta_k + \gamma_k \text{OTC}_d) \\
& + \sum_{\tau=2019}^{2022} \phi_\tau \mathbf{1}\{y = \tau\} \\
& + X'_{gy} \delta + \varepsilon_{fgy},
\end{aligned} \tag{4}$$

where O_{fgy} represents the outcome of interest for pharmacy f , in two-digit zip code region g , for drug type d , in year y . The intercept α captures the estimated outcome for the reference category: non-pharmaceutical products sold in *Urban* pharmacies during the base year 2018. Accordingly, the coefficients ϕ_τ measure the aggregate temporal deviations for each subsequent year τ relative to this baseline. The dummy variables D_g^k indicate whether a region is *Intermediate* or *Rural*. The coefficients within the first summation distinguish between general and product-specific regional effects: β_k captures the baseline level difference for region k , while γ_k measures the

⁴⁵For simplicity, autoregressive lags are omitted from the regression.

additional difference for OTC drugs ($OTC_d = 1$) in that region. The vector X'_{gy} controls for regional income and demographics to ensure the results capture underlying differences in spatial market concentration, rather than merely reflecting variations in local purchasing power.

As explained at the end of Section 3.1, all socio-demographic variables and the degree of urbanization are originally defined at the five-digit postal-code level and are aggregated to the two-digit level to match the geographical definition of markets (see Figure D.8). Because the degree of urbanization is a categorical measure describing spatial land structure and population density,⁴⁶ it is aggregated using two distinct methods to ensure robustness: (i) the mode (assigning the most frequently occurring category within each spatial unit) and (ii) area-weighting (see Figure D.9). These two schemes account for different spatial aggregation dynamics: the mode reflects the most frequent category among sub-units, which heavily captures the high count of small rural municipalities, whereas area-weighting naturally emphasizes physically more expansive regions, revealing that intermediate regions (towns and suburbs) actually constitute the largest land area within many two-digit zones. In contrast, because socio-demographic variables represent continuous aggregate population metrics (such as total income), they are aggregated using population-weighted averages to ensure representativeness on a per capita basis.

The dependent variable O_{fgyd} comprises a set of pharmacy-level performance metrics aggregated by year and drug category. Consequently, variables such as net retail or wholesale prices do not refer to individual products but represent volume-weighted averages reflecting the specific composition of goods sold by pharmacy f within category d . For the empirical estimation, the natural logarithm is applied to wholesale and retail prices, absolute markups, sales volumes, and gross profits.⁴⁷ In contrast, the Lerner Index and the estimated pass-through coefficients are modeled in levels, as the Lerner Index represents a dimensionless ratio and pass-through rates measure the absolute Euro-for-Euro transmission of cost shocks.

5 Cost Pass-Through Results

The analysis proceeds in four stages. First, Section 5.1 applies the ECM to investigate pooled pass-through asymmetries by distinguishing between short-run shock effects and the speed of adjustment toward the long-run equilibrium. Then, Section 5.2 validates these baseline findings by extending the lag structure to account for delayed cost transmission. Section 5.3 analyzes structural heterogeneity by relaxing parameter pooling assumptions,

⁴⁶See, <https://ec.europa.eu/eurostat/web/degree-of-urbanisation/methodology>.

⁴⁷This transformation handles the right-skewed nature of the data and allows the coefficients to be interpreted as semi-elasticities (approximate percentage changes).

examining pricing dynamics across pharmacy and year, by revenue deciles and top-ranked products. Finally, Section 5.4 details the results for the spatial and socio-demographic analysis. Throughout this section, the focus remains on reporting the empirical estimates, while the theoretical mechanisms and broader economic implications of these findings are discussed comprehensively in Section 6.

5.1 Dynamic & Asymmetric Results

Table 3 reports the estimation results of Equation (1) with $K = 0$ (without additional lags),⁴⁸ while Figure 5 visualizes the adjustment paths through cumulative impulse-response functions (CIRFs).⁴⁹

The analysis distinguishes between the transmission of positive wholesale cost shocks (Δw^+) and negative shocks (Δw^-) across OTC drugs (Columns (1)–(4)) and non-pharmaceuticals (Columns (5)–(8)). To highlight the robustness of the findings across different specifications, the results are presented sequentially. Columns (1) and (5) establish the baseline asymmetric response without fixed effects on the product level, capturing immediate pass-through ($\rho^\pm$) and direction-specific mean-reversion adjustment speeds ($\gamma^\pm$ and $\lambda^\pm$) and long-run coefficients ($\theta^\pm$); Columns (2) and (6) include competitor effects; and Columns (3) and (7) include PCN fixed effects. Finally, the fully flexible specifications in Columns (4) and (8) includes fixed effects at the pharmacy-PCN level, accounting for detailed cross-sectional unobservable heterogeneity.

The results provide compelling empirical evidence of *rockets and feathers* across both segments. In all specifications, the immediate pass-through of cost increases (ρ^+) is significantly larger than that of cost decreases (ρ^-). For OTC-drugs (Column (4)), the immediate response to a cost increase is 0.263, while the response to a cost decrease is only 0.123. This implies that pharmacies pass on approximately 26 % of a cost increase of €1 on the

⁴⁸*Significance Codes and Interpretation:* Throughout this paper, reported statistical significance follows the scheme: $^+p < 0.1$, $^*p < 0.05$, $^{**}p < 0.01$, and $^{***}p < 0.001$ (see, e.g., Engelmann et al., 2024; Hoagland & Wang, 2025; Kim & Riegel, 2025). Consistent with the granularity of the dataset, which comprises nearly a billion of observations, emphasis will be placed on the *economic significance* – the magnitude and direction of the estimates – over the pure reliance on statistical thresholds. As noted by Lin et al. (2013) and Lucas, Shmueli, et al. (2013), in large samples, point estimates can be statistically distinguishable from zero even when economically negligible. Therefore, following recommendations (Imbens, 2021; McCloskey & Ziliak, 1996; Wasserstein & Lazar, 2016), the analysis focuses on the substantive implications of the coefficient sizes (e.g., the extent of overshifting) to determine relevance. Furthermore, in this high-power setting, non-significant results are interpreted as highly informative: given the massive sample size, a failure to reject the null hypothesis indicates the precise absence of an effect rather than a lack of statistical power (Abadie, 2020).

⁴⁹The cumulative impulse-response functions are derived in line with (Borenstein et al., 1997, p. 337), but adjusted to fit Equations (1) and (3), respectively.

Dependent Variable:	Δp_{jft}							
	OTC-drugs				Non-Pharmaceuticals			
Coefficients & Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\rho^- \Delta w_{jft}$	0.1062*** (0.0097)	0.1077*** (0.0105)	0.1170*** (0.0102)	0.1227*** (0.0113)	0.2332*** (0.0151)	0.2312*** (0.0155)	0.2329*** (0.0128)	0.2270*** (0.0144)
$\rho^+ \Delta w_{jft}$	0.3172*** (0.0163)	0.2896*** (0.0101)	0.2806*** (0.0093)	0.2629*** (0.0092)	0.3951*** (0.0326)	0.3987*** (0.0356)	0.4025*** (0.0335)	0.4075*** (0.0398)
$\gamma^- p_{jft,t-\tau}$	-0.0869*** (0.0031)	-0.0876*** (0.0031)	-0.0966*** (0.0032)	-0.1155*** (0.0038)	-0.1000*** (0.0091)	-0.1030*** (0.0096)	-0.1186*** (0.0091)	-0.1557*** (0.0132)
$\gamma^+ p_{jft,t-\tau}$	-0.1278*** (0.0061)	-0.1331*** (0.0041)	-0.1388*** (0.0042)	-0.1498*** (0.0049)	-0.1518*** (0.0142)	-0.1516*** (0.0148)	-0.1678*** (0.0147)	-0.1953*** (0.0201)
$\lambda^- w_{jft,t-\tau}$	0.1299*** (0.0049)	0.1323*** (0.0049)	0.1472*** (0.0051)	0.1775*** (0.0061)	0.1403*** (0.0129)	0.1442*** (0.0136)	0.1659*** (0.0131)	0.2176*** (0.0188)
$\lambda^+ w_{jft,t-\tau}$	0.1882*** (0.0098)	0.2011*** (0.0063)	0.2101*** (0.0064)	0.2286*** (0.0075)	0.2051*** (0.0191)	0.2043*** (0.0199)	0.2259*** (0.0197)	0.2648*** (0.0273)
$\omega \Delta Days_{jft}$	0.0007*** (1.68×10^{-5})	0.0007*** (1.6×10^{-5})	0.0007*** (1.43×10^{-5})	0.0006*** (1.5×10^{-5})	0.0004*** (1.49×10^{-5})	0.0004*** (1.55×10^{-5})	0.0004*** (1.24×10^{-5})	0.0004*** (1.39×10^{-5})
Δ Avg. Price of Competitors (σ^Δ)		8.3×10^{-5} (0.0002)	0.0010*** (0.0003)	0.0011*** (0.0002)		0.0003 (0.0003)	0.0007** (0.0002)	0.0005* (0.0002)
Lag Avg. Price of Competitors (σ^τ)		-0.0016*** (0.0002)	0.0005* (0.0002)	0.0004 (0.0002)		-0.0017** (0.0006)	-0.0008** (0.0003)	-0.0010** (0.0003)
Fit statistics								
# Year-Days	1,641	1,641	1,641	1,641	1,641	1,641	1,641	1,641
# Pharmacy ID $\times$ Year-Month	341,968	341,966	341,966	341,958	341,960	341,950	341,950	341,935
# PCN	—	—	28,724	—	—	—	109,087	—
# Pharmacy ID $\times$ PCN	—	—	—	8,772,672	—	—	—	15,174,353
Observations	487,209,481	471,465,749	471,462,173	469,293,937	376,934,836	366,362,066	366,347,873	361,946,441
Adjusted R ²	0.03556	0.03221	0.03633	0.04499	0.06426	0.06375	0.08434	0.11475
Within Adjusted R ²	0.02829	0.02502	0.02343	0.02134	0.04042	0.03925	0.04006	0.04132
Long-run Pass-Through θ^+	1.473	1.510	1.514	1.526	1.351	1.348	1.347	1.355
Long-run Pass-Through θ^-	1.495	1.510	1.523	1.537	1.403	1.400	1.398	1.398
Z-test $\theta^+ = \theta^-$	-1.964*	-0.007	-1.928+	-2.887**	-4.788***	-4.429***	-5.591***	-5.038***
Wald test $\rho^+ = \rho^-$	158.302***	236.677***	279.839***	239.834***	19.607***	17.947***	21.419***	16.966***
Wald test $\gamma^+ = \gamma^-$	71.381***	344.504***	329.714***	212.992***	17.842***	14.562***	17.555***	10.327**
Wald test $\lambda^+ = \lambda^-$	53.224***	355.548***	346.697***	218.311***	14.858***	11.886***	14.184***	7.813**
Wald test $\gamma^+ = \gamma^- = \lambda^+ = \lambda^- = 0$	204.810***	331.575***	335.293***	292.597***	39.408***	36.504***	48.115***	36.564***
Fixed Effects (FEs)								
Year-Days FEs	✓	✓	✓	✓	✓	✓	✓	✓
Pharmacy ID $\times$ Year-Month FEs	✓	✓	✓	✓	✓	✓	✓	✓
PCN FEs			✓				✓	
Pharmacy ID $\times$ PCN FEs				✓				✓

Standard errors (in parentheses) are clustered on the pharmacy ID and PCN. Wald tests show F-statistics and z-test the standard z-score. Differences in the number of observations across models stem from perfect fits and singletons.
Signif. Codes: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 3: Baseline asymmetric estimation results (Equation (1) with $K = 0$): Pass-through of positive and negative wholesale cost shocks to retail prices for the aggregate OTC drugs and non-pharmaceuticals segments.

day of the shock, but only about 12 % of a corresponding cost reduction. For non-pharmaceuticals (Column (8)), the asymmetry follows a similar pattern, with coefficients of 0.408 and 0.227, respectively. Formal Wald tests reported in Table 3 provide substantial support for this asymmetry. The null hypothesis of short-run symmetry ($\rho^+ = \rho^-$) is rejected at the 0.1 % level across all specifications, with F-statistics of 239.8 for OTC-drugs and 17.0 for non-pharmaceuticals (Columns (4) and (8)). Both OTC-drug and non-pharmaceutical estimates closely align with the 2-to-1 ratio observed in the literature (Benzarti et al., 2020; Peltzman, 2000).

The dynamic parameters of the ECM provide further insight into the adjustment process. As shown in Columns (4) and (8), the joint significance of the lagged level coefficients is evaluated, and the resulting F-statistics ($F = 292.6$ and $F = 36.6$, respectively) reject the null hypothesis of no level relationship, confirming a stable long-run cointegration.⁵⁰ Crucially, the speed of adjustment itself is asymmetric. For OTC drugs (Column (4)), the adjustment speed following a cost increase ($|\gamma^+| = 0.150$) is significantly faster than that following a cost decline ($|\gamma^-| = 0.116$). This implies that pharmacies converge toward the long-run equilibrium target more rapidly when prices are below the target (following a cost increase) than when they are above it. However, the fact that these adjustments are strictly partial suggests the presence of retail frictions; rather than (nearly) instantaneously jumping to the new long-run level, pharmacies seem to smooth price increases and decreases over time, potentially due to the presence of frictions, e.g., menu costs and/or the strategic smoothing of price changes to avoid alienating price-sensitive consumers (which will be discussed further in Section 6).

The analysis of the long-run equilibrium targets (θ) reveals a persistent overshifting behavior. In the OTC segment (Column (4)), the long-run pass-through rates for both cost increases ($\theta^+ = 1.526$) and decreases ($\theta^- = 1.537$) significantly exceed unity, indicating that pharmacies eventually recover more than 150 % of the original cost shock of one Euro. Although the z-test indicates a statistical difference ($p < 0.01$), the economic magnitudes are remarkably similar. In the non-pharmaceutical segment (Column (8)), a similar pattern emerges, though with lower absolute magnitudes ($\theta^+ = 1.355$, $\theta^- = 1.398$). This indicates that while short-run adjustments are characterized by significant rigidity, the long-run outcome in both segments involves a systematic expansion or contraction of absolute markups. For non-pharmaceuticals, the rejection of equilibrium symmetry ($\theta^+ = \theta^-$) provides evidence of a more pronounced long-term asymmetry (confirmed by

⁵⁰The test statistics are computed as a standard Wald F-statistic testing the joint significance of the lagged level terms. However, usually inference relies on the non-standard critical value bounds provided by Pesaran et al. (2001) to account for the uncertain order of integration of the regressors (upper-bound critical values approximately 5.0 at the 1 % level) which are decisively smaller than the provided results.

the Wald test: $F = -5.04$).

Rather than merely reflecting transitory pricing frictions, this persistent overshifting may be an outcome of pricing under imperfect competition, including local monopolies. Specifically, if pharmacies face a log-convex demand curve, such as an iso-elastic or log-linear demand structure (Miller et al., 2013), the profit-maximizing strategy inherently yields a pass-through rate strictly greater than one (see, e.g., Mrázová & Neary, 2017; Ritz, 2024; Weyl & Fabinger, 2013). From the current analysis, comparing the two segments already reveals a clear distinction in market power. The consistently higher long-run pass-through target for OTC drugs compared to non-pharmaceuticals (≈ 1.53 vs. 1.38) implies that pharmacies possess significantly greater pricing power for OTC drugs. This structural difference is likely driven by the captive nature of the OTC consumer, resulting in a more inelastic demand, as high search costs and the necessity of acute care allow pharmacies to extract larger rents. In contrast, the lower coefficient for non-pharmaceuticals reflects a more competitive environment, where substitution to standard drugstores or supermarkets is easier, thereby constraining the pharmacy’s ability to extend margins (for the extended discussion including the spatial dimension see Section 6).

To quantify the temporal dynamics of these adjustments, Figure 5 presents the CIRFs.⁵¹ The visual contrast in the top panel underscores the severity of the asymmetry for OTC-drugs. Following a €1 cost increase (solid red line), retail prices jump by €0.26 immediately. The more steep curvature of the subsequent adjustment reflects the high speed of correction ($|\gamma^+| \approx 0.15$), allowing prices to converge faster toward the long-run pass-through target of €1.53. Conversely, a cost decrease (solid blue line) triggers an initial drop of only €0.12. The flatter trajectory of the blue line – driven by the significantly lower adjustment speed ($|\gamma^-| \approx 0.12$) – indicates a slightly more sluggish descent that requires longer (≈ 2 days) to fully absorb the shock and finally reach the long-run target of €1.54.

For non-pharmaceuticals (bottom panel), the initial gap is smaller, with short-run effects of €0.41 and €0.23 for increases and decreases, respectively. However, the simulation explicitly visualizes the (small) structural asymmetry discussed above: the adjustment path for cost decreases (solid blue line) eventually crosses and settles at a higher long-run level (€1.40) than the path for cost increases (€1.36). This indicates that while downward adjustments suffer from short-run inertia, the long-run transmission for cost

⁵¹These trajectories are derived by iterating the estimated results of the ECM (Equation 1) to track the propagation of an initial positive or negative, one-time wholesale cost shock of €1. At the moment of the shock ($t = 0$), the price adjusts by the estimated short-run coefficient (ρ^k). In subsequent periods, the price evolves to close the gap between the current level and the long-run equilibrium (θ^k), with the convergence rate governed by the estimated speed of adjustment parameters (γ^k) from Columns (5) and (10) of Table 3.

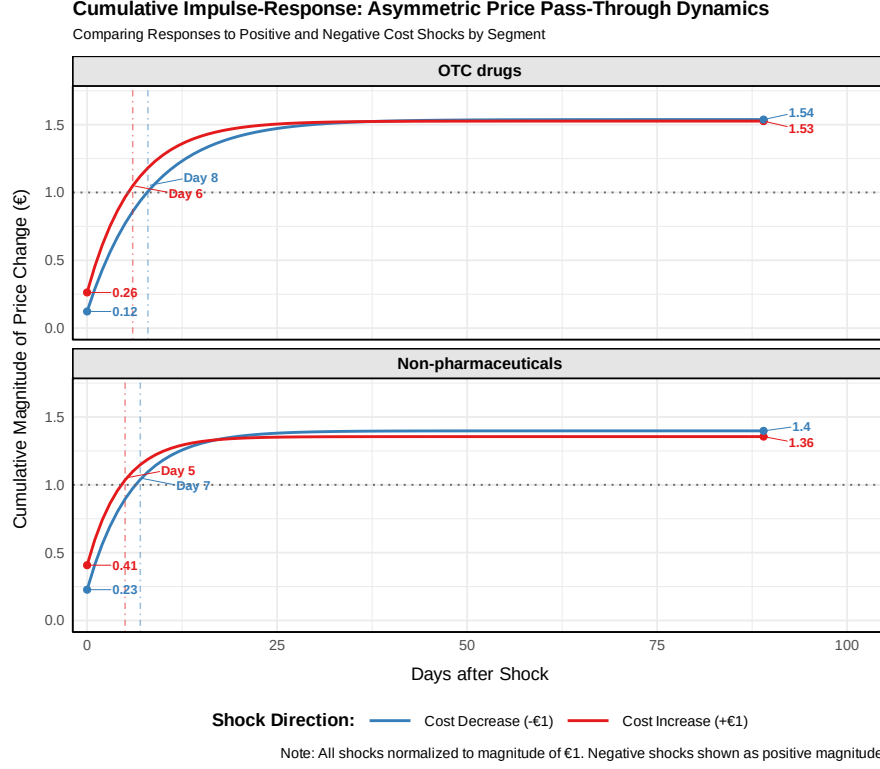


Figure 5: Asymmetric Cumulative Impulse-Response Functions ($K = 0$): Cumulative Price Adjustment to €1 Cost Shocks. The figure illustrates the dynamic path of retail price adjustments for OTC-drugs (top panel) and non-pharmaceuticals (bottom panel) following a normalized €1 wholesale cost increase (solid red curves) and decrease (solid blue curves). Negative shocks are shown in absolute magnitude for comparability. The estimated immediate short-run effects and long-run equilibrium targets are explicitly annotated at the beginning and end of each path. The horizontal dotted line represents the complete pass-through threshold (€1), while the vertical lines denote the exact number of days required for the cumulative adjustment to reach this level. Effects are derived from Columns (4) and (8) from Table 3.

savings is statistically stronger. In both regimes, the adjustment process exhibits rapid decay, with the price fully converging to its new equilibrium level within approximately one to two months. Taken in its entirety, these findings represent the standard rockets and feathers phenomenon. By passing on wholesale price increases more strongly than decreases, pharmacies effectively protect their margins in the short-run. As touched upon before, this asymmetry may be driven by pharmacies exploiting local market power, but it is also consistent with consumer search frictions.

Finally, the analysis incorporates competitor pricing behavior to test for strategic responses, i.e., strategic complementarity. The results, reported in Table 4, point to a dichotomy between statistical significance and economic magnitude. For OTC drugs (Column (4)), a statistically significant positive

short-run response is observed ($\sigma^\Delta \approx 0.001$), suggesting a weak tendency of a pharmacy to respond immediately to market-wide price changes. However, the coefficient on the lagged competitor price level (σ^τ) is statistically indistinguishable from zero, implying no stable long-run relationship. Thus, pharmacies do not align their long-run equilibrium prices with those of their competitors. This finding indicates an absence of strategic complementarity; from a broader perspective, B&M pharmacies do not appear to actively compete against each other on price.

In the non-pharmaceutical segment (Column (8)), the competitor sensitivity is even more attenuated. In the short run, the estimated response to competitor price changes (σ^Δ) is positive but economically negligible (0.0005). Similarly, while the lagged level coefficient (σ^τ) is statistically significant, its magnitude (-0.0010) implies a vanishingly small long-run pass-through of approximately 0.006 (calculated as $-\sigma^\tau/\gamma$). These findings imply that while competitor prices are statistically detectable, they are economically irrelevant compared to the magnitude of the own-cost pass-throughs.

This lack of competitor response can be interpreted in two ways: either that pharmacies operate in an environment characterized by strong spatial differentiation and monopolistic competition, where local market power renders them largely indifferent to broader market pricing, or that the utilized geographic market definition is too broad. Because the product market is already defined at the narrowest possible level (the chemical substance), it is more probable that relying on two-digit postal codes dilutes the measurement of true localized, city-level competition, capturing a broader non-competitive aggregate instead. Yet, most importantly, the inclusion of the competitor variables does not alter the other coefficient magnitudes.

5.2 Robustness Analysis

To validate the baseline findings and account for potential delayed transmission of cost shocks, Table 4 presents the results of the full ECM (Equation (1)) including up to $K = 4$ lags. For the complete set of estimates, including results for all intermediate lag specifications and full coefficient reports, see Appendix E Table E.2. Due to the computational intensity of estimating this high-dimensional fixed-effects model on the full dataset, this robustness check restricts the sample to the highest-turnover product categories:⁵² *Respiratory System* and *Musculoskeletal System/Pain Management* for OTC drugs (32 % and 23 % of sales within OTC drugs), alongside *No ATC Code*

⁵²See, Table D.2 and D.3.

for non-pharmaceuticals (60 % of sales within non-pharmaceuticals).⁵³

A key distinction from the baseline results from Section 5.1 is the magnitude of the immediate impact coefficient ($\rho^\pm$). In the most dynamic specification (Table 4 Column (3)), the day-zero pass-through for OTC drugs cost increases is $\rho^+ \approx 0.133$, roughly half the magnitude of the baseline estimate (Table 3). However, this does not imply lower responsiveness. Rather, the impact is distributed across the subsequent days, as evidenced by the statistically significant positive coefficients on the lagged cost terms ($\beta_{t-1}^+, \beta_{t-2}^+, \dots$, see Table E.2). The cumulative short-run response, visualized in Figure 6, mirrors the trajectory observed in the baseline specification (Figure 5): a sharp instantaneous impulse followed by a monotonic adjustment. However, the temporal structure differs in duration. The extended lag specification reveals substantial persistence, lengthening the convergence horizon to over a quarter even up to more than half a year before the new steady-state equilibrium is fully established.

Crucially, the inclusion of lags refines the understanding of the long-run equilibrium. Unlike the baseline model, which often detects statistical differences in long-run targets for OTC-drugs, the dynamic specification (Column (4)) shows that pass-through rates for cost increases ($\theta^+ = 1.572$) and decreases ($\theta^- = 1.550$) converge to statistically indistinguishable levels ($p = 0.177$). This reinforces the finding that while the *speed* of adjustment is asymmetric (confirming rockets and feathers), the long-run equilibrium is symmetric. Conversely, in the non-pharmaceutical segment (Column (6)), the dynamic model confirms the presence of a long-term asymmetry: the long-run pass-through of cost decreases ($\theta^- \approx 1.395$) remains higher than that of increases ($\theta^+ \approx 1.254$), a difference that remains statistically significant ($p < 0.01$).

Figure 6 visually corroborates these dynamics. The solid red lines (cost increases) exhibit a steep initial slope, capturing the rapid accumulation of the immediate and lagged coefficients. The solid blue lines (cost decreases) follow a distinctively concave, sluggish path. Yet, for OTC-drugs (top panel), the lines clearly converge to a similar asymptote, demonstrating that the observed asymmetry is purely a phenomenon of delay rather than permanent retention. In contrast, the bottom panel illustrates the long-run asymmetry in the non-pharmaceutical segment, where the slow-moving downward adjustment eventually surpasses the upward response, settling at a higher equilibrium level of 1.4. Regarding duration asymmetries, convergence toward the long-run equilibrium takes distinctly longer for decreases than for increases. For example, it takes 27 and 24 days for non-pharmaceuticals and

⁵³The selection of $K = 4$ is empirically supported by the model fit statistics reported in Tables 4. The Akaike Information Criterion (AIC) and Bayesian Information Criterion (BIC) exhibit a monotonic decrease as lags are added (comparing Columns (1) through (3) and (4) through (6)), indicating that the fully specified model with four lags provides the superior fit by capturing the distributed nature of the price adjustment process.

Dependent Variable:	Δp_{jft}					
	OTC-drugs			Non-Pharmaceuticals		
Coefficients & Variables	(1)	(2)	(3)	(4)	(5)	(6)
$\rho^- \Delta w_{jft}$	0.0772*** (0.0092)	0.0656*** (0.0067)	0.0645*** (0.0062)	0.2226*** (0.0300)	0.1948*** (0.0203)	0.1976*** (0.0167)
$\rho^+ \Delta w_{jft}$	0.1882*** (0.0088)	0.1405*** (0.0081)	0.1333*** (0.0076)	0.4481*** (0.0749)	0.4176*** (0.0748)	0.4044*** (0.0689)
$\gamma^- p_{jft,t-\tau}$	-0.0930*** (0.0062)	-0.0529*** (0.0039)	-0.0440*** (0.0033)	-0.1298*** (0.0218)	-0.0703*** (0.0140)	-0.0476*** (0.0109)
$\gamma^+ p_{jft,t-\tau}$	-0.1151*** (0.0057)	-0.0728*** (0.0036)	-0.0628*** (0.0030)	-0.1622*** (0.0363)	-0.1057*** (0.0290)	-0.0810** (0.0262)
$\lambda^- w_{jft,t-\tau}$	0.1470*** (0.0110)	0.0825*** (0.0065)	0.0682*** (0.0055)	0.1830*** (0.0315)	0.0984*** (0.0201)	0.0664*** (0.0156)
$\lambda^+ w_{jft,t-\tau}$	0.1819*** (0.0101)	0.1142*** (0.0061)	0.0988*** (0.0052)	0.2158*** (0.0494)	0.1361*** (0.0391)	0.1015** (0.0355)
Fit statistics						
# PCN-Pharmacy ID	2,491,778	2,491,778	2,491,778	6,078,232	6,078,232	6,078,232
# Year-Days	1,637	1,637	1,637	1,637	1,637	1,637
# Pharmacy-ID $\times$ Year-Month	341,772	341,772	341,772	341,307	341,307	341,307
Observations	231,400,516	231,400,516	231,400,516	172,288,976	172,288,976	172,288,976
Adjusted R ²	0.03062	0.23224	0.25603	0.13450	0.31951	0.36392
Within Adjusted R ²	0.01031	0.21615	0.24044	0.03726	0.24306	0.29245
Long-run Pass-Through θ^+	1.580	1.569	1.572	1.330	1.287	1.254
Long-run Pass-Through θ^-	1.581	1.560	1.550	1.410	1.401	1.395
Z-test $\theta^+ = \theta^-$	-0.119	0.630	1.350	-3.386***	-3.096**	-2.729**
K Lags	0	1	4	0	1	4
AIC	377,566,880.9	314,690,484.8	306,142,571.7	429,071,416.9	387,977,778.5	374,094,333.3
BIC	426,501,332.6	363,625,005.6	355,077,299.6	538,004,721.7	496,911,151.1	483,027,909.5
LogLik	-185,948,248.5	-154,510,046.4	-150,236,077.9	-208,114,527.4	-187,567,704.2	-180,625,969.7
Fixed Effects (FEs)						
PCN-Pharmacy ID FEs	✓	✓	✓	✓	✓	✓
Year-Days FEs	✓	✓	✓	✓	✓	✓
Pharmacy-ID $\times$ Year-Month FEs	✓	✓	✓	✓	✓	✓

Standard errors (in parentheses) are clustered on the pharmacy ID and PCN. Wald tests show F-statistics.

Signif. Codes: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 4: Dynamic asymmetric estimation results (Equation (1) with up to $K = 4$ lags): Short-run and long-run pass-through estimates for high-turnover OTC drugs (*Respiratory System, Musculoskeletal System and Pain Management*) and non-pharmaceuticals (*No ATC Code*). For brevity, only specifications with $K \in \{0, 1, 4\}$ are displayed. The complete set of results, including all intermediate lag specifications, is provided in Appendix E Table E.2.

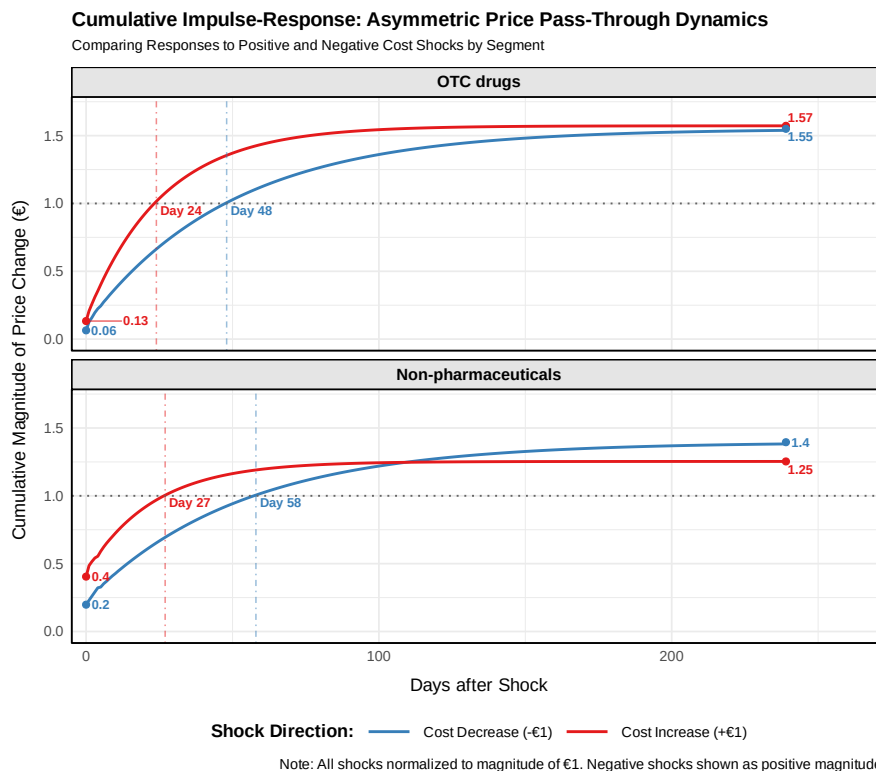


Figure 6: Asymmetric Cumulative Impulse-Response Functions utilizing Table 4, Column (4) and (8): Cumulative Price Adjustment to €1 Cost Shocks. The figure illustrates the dynamic path of retail price adjustments for OTC-drugs (top panel) and non-pharmaceuticals (bottom panel) following a normalized €1 wholesale cost increase (solid red curves) and decrease (solid blue curves). Negative shocks are shown in absolute magnitude for comparability. The estimated immediate short-run effects and long-run equilibrium targets are explicitly annotated at the beginning and end of each path. The horizontal dotted line represents the complete pass-through threshold (€1), while the vertical lines denote the exact number of days required for the cumulative adjustment to reach this level.

OTC drugs, respectively, to reach complete pass-through following a cost increase (vertical colored lines). In contrast, cost decreases require 58 and 48 days, respectively. Thus, pharmacies are approximately twice as fast to pass through cost increases as they are to pass through decreases. Furthermore, the duration until the long-run equilibrium is reached is approximately one quarter for cost increases, but extends to over more than half a year for decreases.

This structural asymmetry in the non-pharmaceutical segment – where long-run price cuts exceed price hikes ($\theta^- > \theta^+$) – potentially reflects the strategic role of these products as another traffic builder (compared to Rx drugs). Unlike medicinal OTC drugs, which are sold exclusively in pharmacies, non-pharmaceutical products (e.g., cosmetics, supplements) face direct

external competition from mass-market drugstores and supermarkets. Consequently, pricing dynamics could be driven by the incentive to defend and expand market share against rivals. When wholesale prices rise, pharmacies are constrained by the high demand elasticity of discretionary shoppers; to avoid customer switch to cheaper competitors, pharmacies are forced to absorb a larger part of the cost increase (limiting θ^+). Conversely, periods of falling wholesale prices offer a strategic opportunity to aggressively improve price competitiveness. Thus, pharmacies might utilize these episodes to overshift the cost reduction stronger than an increase ($\theta^- > \theta^+$), effectively subsidizing lower prices to stimulate footfall that can subsequently be monetized through cross-sales in the higher-margin OTC-drug segment (or even to attract customers with prescriptions).

In summary, the analysis confirms that pricing dynamics in the pharmacy market are universally characterized by short-run downward rigidity (rockets and feathers). However, the long-run equilibrium outcomes reveal a fundamental structural divergence between segments. For OTC drugs, the asymmetry is temporary: pharmacies delay price reductions but eventually converge to a symmetric, high-markup equilibrium ($\theta^+ \approx \theta^-$). Conversely, the non-pharmaceutical segment exhibits a more competitive reverse asymmetry where long-run price cuts exceed price increases ($\theta^- > \theta^+$). This behavior would be consistent with the strategic use of unregulated products as traffic builders, where pharmacies aggressively overshift cost savings to defend market share against external competition. The full discussion will be adjourned to Section 6.

5.3 Heterogeneity Analysis

The pooled estimates presented in Section 5.1 provide a robust baseline for market-wide pricing dynamics. However, these average effects may mask significant structural heterogeneity across pharmacies. To investigate this, Equation (3) is estimated, yielding a distribution of approximately 60,000 unique short-run and adjustment parameters, respectively.⁵⁴

⁵⁴For a detailed structural analysis regressing these estimated pharmacy-level pass-through coefficients on revenue deciles to test for size-dependent effects, see Appendix B.1.

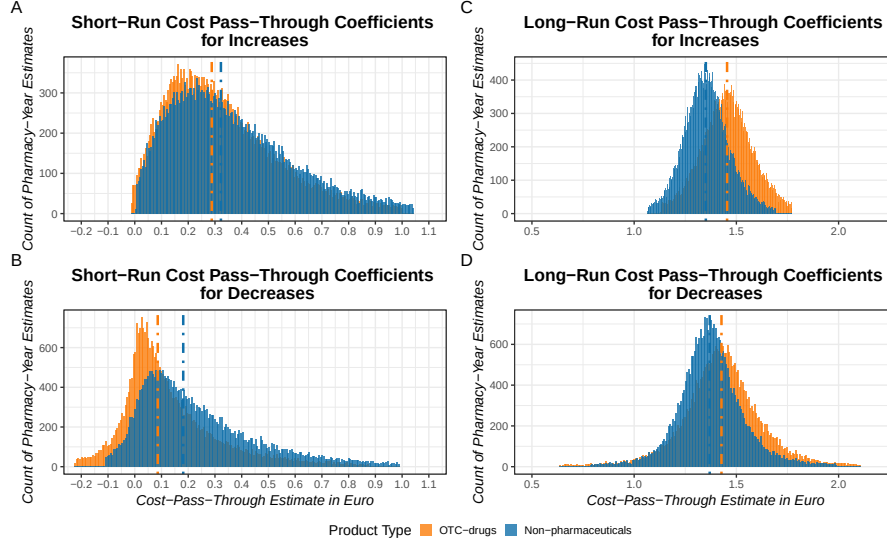


Figure 7: Distribution of Pharmacy-Level CPT Coefficients. The figure displays the histograms of estimated short-run (ρ_{fy}) and long-run (θ_{fy}) coefficients for OTC drugs (orange) and non-pharmaceuticals (blue). Panels A and B show the immediate response to cost increases and decreases, respectively. Panels C and D show the derived long-run equilibrium targets. Note: The distributions have been truncated by 2.5 % at both the lower and upper ends. Dashed lines represent the median of each distribution.

Figure 7 plots the histograms of the estimated short-run ($\hat{\rho}_{fy}$) and long-run ($\hat{\theta}_{fy}$) coefficients, distinguishing between OTC drugs (orange) and non-pharmaceuticals (blue).

Three key patterns emerge from these distributions. First, there is substantial variation in pricing behavior. CPT rates exhibit substantial dispersion, revealing heterogeneous pricing responses to identical cost shocks. Second, *rockets and feathers* are clearly visible in the distributions. While both short-run distributions (Panels A and B) exhibit right-skewness, there is a sharp contrast between their respective medians. For cost increases (Panel A), the mass is centered around 0.30–0.40; in contrast, for cost decreases (Panel B), the distribution shifts toward zero, particularly for OTC drugs, confirming that a large subset of pharmacies exhibits stark rigidity to cost savings in the short run. Third, while long-run overshifting is pervasive (distributions in Panels C and D are above unity), the structural distinction between segments remains. In both long-run panels, the OTC distributions are positioned to the right of the non-pharmaceutical distributions. This visual gap corroborates the econometric evidence that pharmacies consistently enforce higher long-run pass-through targets in the OTC market compared to the competitive non-pharmaceutical sector.⁵⁵

⁵⁵For a detailed analysis of heterogeneous pass-through dynamics across pharmacies, see Appendix B.2.

Dependent Variable:	Δp_{jft}					
	(1)	(2)	(3)	(4)	(5)	(6)
$\rho^- \Delta w_{jft}$	0.0659** (0.0138)	0.1054*** (0.0108)	0.1219*** (0.0186)	0.1403*** (0.0279)	0.0915*** (0.0138)	0.1208*** (0.0142)
$\rho^+ \Delta w_{jft}$	0.0689* (0.0226)	0.1156*** (0.0088)	0.2135*** (0.0146)	0.0963** (0.0206)	0.1423*** (0.0116)	0.2205*** (0.0121)
Fit statistics						
Observations	37,531,035	180,112,138	422,089,152	30,265,844	167,176,855	409,592,325
Adjusted R ²	0.03715	0.03413	0.02923	0.04239	0.02919	0.03591
Within Adjusted R ²	0.00405	0.00672	0.01524	0.00753	0.00959	0.01793
# Top PCN	10	100	1000	10	100	1000
Top PZN by	Sales	Sales	Sales	Revenue	Revenue	Revenue
Fraction of Total in %	13.1520	44.2220	87.3466	9.2187	37.9892	83.1285
Wald test $\rho^+ = \rho^-$	0.025	0.621	107.109***	3.521+	9.813**	140.860***
Long-run Pass-Through θ^+	1.672	1.633	1.580	1.558	1.556	1.535
Long-run Pass-Through θ^-	1.715	1.636	1.581	1.557	1.534	1.536
Z-test $\theta^+ = \theta^-$	-1.794+	-0.435	-0.100	0.131	2.613**	-0.233

Standard errors (in parentheses) are clustered on the pharmacy ID and PCN. Wald tests show F-statistics.
Signif. Codes: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

(a) OTC drugs: Top 10, 100, and 1000.

Dependent Variable:	Δp_{jft}					
	(1)	(2)	(3)	(4)	(5)	(6)
$\rho^- \Delta w_{jft}$	0.1235*** (0.0109)	0.1275*** (0.0127)	0.1824*** (0.0124)	0.1593** (0.0352)	0.1792*** (0.0184)	0.2098*** (0.0110)
$\rho^+ \Delta w_{jft}$	0.1317*** (0.0226)	0.2356*** (0.0236)	0.2704*** (0.0132)	0.2348*** (0.0267)	0.2629*** (0.0118)	0.2789*** (0.0077)
Fit statistics						
Observations	14,012,783	65,992,694	207,336,289	10,709,504	52,381,667	177,019,976
Adjusted R ²	0.04635	0.05226	0.07846	0.05001	0.07388	0.07572
Within Adjusted R ²	0.01014	0.01699	0.02665	0.02277	0.02815	0.03322
# Top PCN	10	100	1000	10	100	1000
Top PZN by	Sales	Sales	Sales	Revenue	Revenue	Revenue
Fraction of Total in %	3.8819	16.8351	52.5124	4.2602	18.1514	50.9730
Wald test $\rho^+ = \rho^-$	0.184	16.972***	28.569***	3.121+	33.704***	36.934***
Long-run Pass-Through θ^+	1.482	1.348	1.385	1.290	1.342	1.373
Long-run Pass-Through θ^-	1.470	1.347	1.397	1.287	1.334	1.375
Z-test $\theta^+ = \theta^-$	2.755**	0.155	-2.505*	0.298	1.443	-0.471

Standard errors (in parentheses) are clustered on the pharmacy ID and PCN. Wald tests show F-statistics.
Signif. Codes: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

(b) Non-pharmaceuticals: Top 10, 100 and 1000.

Table 5: Estimation results for top-ranked products by sales and revenue. Note: The complete tables can be found in the Appendix E, Tables E.3 and E.4.

To examine how the aggregate pricing dynamics reported in Section 5.1 are affected by the long-tail of many low-volume products, Equation (1) is estimated with subsets of top-ranked PCNs (with $K = 0$). Table 5 presents the results for the top 10, top 100, and top 1,000 products, ranked by sales volume and revenue (total turnover in Euro).⁵⁶ These subsets capture the majority of market activity. For instance, the top 1,000 OTC products (non-pharmaceuticals), account for approximately 83 % (52 %) of total segment revenue.⁵⁷

⁵⁶Top-ranked products are identified by aggregate sales volume or revenue over the full sample period of all pharmacies.

⁵⁷See Appendix Tables E.3 and E.4 for a full table including the lagged coefficients, detailed statistics on fixed effects, and the ranking by gross profits exhibiting similar results.

The estimates in Table 5 reveal distinct heterogeneity in pricing strategies across the assortment depth. Concerning Table 5a, for the high-frequency bestsellers (Top 10 and Top 100 by sales, Columns (1)–(2)), pricing is symmetric in both the short and long run. In Column (2), the short-run response to cost decreases ($\rho^- = 0.105$) and increases ($\rho^+ = 0.116$) is statistically indistinguishable, leading to symmetric long-run pass-through ($\theta^+ \approx \theta^- \approx 1.63$; $p = 0.664$). Interestingly, while the Top 1,000 sales products (Column (3)) exhibit significant short-run asymmetry ($\rho^+ > \rho^-$), this difference dissipates completely in the long run. In contrast, the revenue-generating assortment exhibits sustained asymmetry only in Column (5), which focuses on the Top 100 revenue products. In the short run, the short-run response to cost increases ($\rho^+ = 0.142$) significantly exceeds that for decreases ($\rho^- = 0.092$). This divergence persists into the long run, where pass-through for increases ($\theta^+ = 1.556$) remains significantly higher than for decreases ($\theta^- = 1.534$), yielding a rejection of symmetry. However, this long-run asymmetry vanishes for the broader Top 1,000 revenue products.

Table 5b presents the results for non-pharmaceuticals. For the Top 100 revenue products (Column (5)), a strong short-run asymmetry is observed ($\rho^+ = 0.263$ vs. $\rho^- = 0.179$), but this does not translate into statistically significant long-run asymmetry ($\theta^+ = 1.342$ vs. $\theta^- = 1.334$). This convergence is equally evident in the broader Top 1,000 sample (Column (6)): despite highly significant short-run asymmetry, the long-run pass-through coefficients are statistically indistinguishable ($\theta^+ \approx 1.37$ vs. $\theta^- \approx 1.38$). Thus, while non-pharmaceuticals exhibit transient asymmetric dynamics, they do not sustain the permanent asymmetric deviations observed in the high-revenue OTC drug assortment.

In summary, the heterogeneity analysis reveals that the aggregate rockets and feathers phenomenon does not appear uniformly across a pharmacy’s inventory. For bestsellers, pricing remains symmetric, potentially because consumers are highly attentive to the prices of these everyday items. However, these top-selling products simultaneously exhibit higher absolute long-run pass-through magnitudes (e.g., up to 1.715) compared to the market average of approximately 1.54. This indicates that pharmacies extract greater permanent margins on high-demand goods, even while abstaining from asymmetric pricing. Furthermore, persistent long-run asymmetry is highly concentrated, emerging exclusively within the narrowest segment of high-revenue OTC products. In contrast, for non-pharmaceuticals, asymmetry remains strictly transient across all top-ranked specifications, completely dissipating in the long run. Given that the aggregate results from Tables 3 and 4 showed long-run asymmetry for the full non-pharmaceutical sample, this effect must be driven by the long tail of low-volume products. Together, these findings suggest that pharmacies seem to employ targeted pricing strategies, maximizing absolute margins symmetrically on visible bestsellers, while isolating permanent asymmetric deviations to specific high-revenue OTC items and

the less-visible long tail of the non-pharmaceutical assortment.

5.4 Socio-Demographic Analysis

Table 6 presents results of the estimation of Equation (4) for several outcome variables and the mode aggregation function (see Section 4.3). The constant captures the baseline conditional mean for the reference group (urban pharmacies, non-pharmaceuticals, 2018). The remaining coefficients quantify the marginal effects of socio-demographics, urbanization, product type, interaction terms, and year fixed effects. The estimates indicate that rural pharmacies exhibit significantly lower sales for OTC drugs. As shown in Column (5), the interaction term for sales volume ($Rural \times OTC\ drugs$) is -0.076 , implying approximately 7.6 % lower sales. However, pharmacies compensate via raising absolute markups by 8.3 % (Column (3)).⁵⁸ Consequently, the net effect on gross profits (Column (6)) is statistically insignificant, highlighting that higher unit margins fully offset the volume deficit. In this context, rural pharmacies seem to leverage their spatial differentiation to sustain higher markups, effectively compensating for the lower demand density inherent to rural locations.⁵⁹

Furthermore, rural markets exhibit lower retail prices of 4.2 % for non-pharmaceuticals. However, this effect reverses for OTC drugs: the positive interaction term (0.083) offsets the negative value, resulting in a net rural price premium of approximately 4.1 % ($-0.042 + 0.083$). These structural differences persist even after controlling for a comprehensive set of socio-demographic covariates, such as income per capita, where a €10,000 increase in local purchasing power is associated with an 18.4 % price premium (0.184).

Crucially, the higher retail prices in rural markets are accompanied by higher wholesale prices. Column (1) indicates that while rural pharmacies purchase non-pharmaceuticals at a significantly lower wholesale price than urban pharmacies (-0.054), they face significantly higher wholesale prices for OTC drugs. The interaction term for wholesale prices is positive and statistically significant (0.082). Consequently, higher retail prices observed for OTC drugs in rural areas parallel the higher procurement costs for these products.

The analysis of the Lerner Index (Column (4)) confirms that the markup increase is proportional to the price level. Although absolute markups (in Euros) are significantly higher for rural OTC products, the interaction term for the Lerner Index, which measures the markup relative to the price, is statistically insignificant. This implies that while rural pharmacies charge

⁵⁸The baseline estimates for rural pharmacies concerning sales and markups are statistically insignificant. Thus, the total spatial effect for OTC drugs is driven entirely by the interaction term.

⁵⁹Similar results arise when the area-weighted aggregation is utilized (see Table C.3).

Dependent Variable:	log(Wholesale Price)	log(Retail Price)	log(Markup)	Lerner Index	log(Sales)	log(Gross Profits)
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	2.955*** (0.2015)	3.303*** (0.2321)	2.141*** (0.3723)	0.3037*** (0.0666)	6.034*** (0.7148)	8.176*** (0.8065)
Intermediate	-0.0095 (0.0107)	-0.0023 (0.0108)	0.0105 (0.0173)	0.0044 (0.0034)	-0.0762 (0.0505)	-0.0657 (0.0538)
Rural	-0.0544*** (0.0092)	-0.0422*** (0.0097)	-0.0184 (0.0174)	0.0078* (0.0035)	0.0272 (0.0448)	0.0088 (0.0478)
Intermediate $\times$ OTC drugs	0.0266* (0.0113)	0.0265* (0.0116)	0.0286* (0.0142)	0.0003 (0.0024)	0.0318 (0.0324)	0.0605+ (0.0324)
Rural $\times$ OTC drugs	0.0819*** (0.0122)	0.0833*** (0.0129)	0.0830*** (0.0152)	0.0007 (0.0021)	-0.0759* (0.0325)	0.0071 (0.0305)
OTC drugs	-0.2752*** (0.0081)	-0.1014*** (0.0090)	0.1878*** (0.0115)	0.1072*** (0.0017)	0.3416*** (0.0276)	0.5294*** (0.0277)
year = 2019	0.0214*** (0.0032)	0.0279*** (0.0030)	0.0407*** (0.0045)	0.0042*** (0.0010)	-0.0418*** (0.0115)	-0.0010 (0.0123)
year = 2020	0.0420*** (0.0028)	0.0562*** (0.0024)	0.0838*** (0.0030)	0.0092*** (0.0007)	-0.1347*** (0.0076)	-0.0510*** (0.0081)
year = 2021	0.0542*** (0.0042)	0.0636*** (0.0037)	0.0808*** (0.0055)	0.0060*** (0.0014)	-0.0806*** (0.0155)	0.0002 (0.0170)
year = 2022(H1)	0.0173** (0.0051)	0.0312*** (0.0043)	0.0555*** (0.0065)	0.0087*** (0.0017)	-0.7632*** (0.0192)	-0.7077*** (0.0212)
Income per Person (in 10T)	0.1748*** (0.0134)	0.1837*** (0.0126)	0.2021*** (0.0213)	0.0061 (0.0050)	0.1695* (0.0653)	0.3716*** (0.0725)
Fraction of Population $\geq$ 65	0.0061** (0.0022)	0.0042+ (0.0023)	0.0012 (0.0036)	-0.0011 (0.0008)	-0.0170+ (0.0095)	-0.0158 (0.0104)
Fraction of Population < 18	-0.0164*** (0.0041)	-0.0179*** (0.0047)	-0.0214** (0.0073)	-0.0011 (0.0013)	0.0783*** (0.0160)	0.0569*** (0.0163)
Fraction of 1-Person-Households	-0.0146*** (0.0016)	-0.0135*** (0.0019)	-0.0121*** (0.0030)	0.0006 (0.0005)	0.0273*** (0.0055)	0.0152* (0.0060)
Fraction of 2-Person-Households	-0.0266*** (0.0030)	-0.0258*** (0.0034)	-0.0256*** (0.0054)	0.0003 (0.0010)	0.0347** (0.0120)	0.0091 (0.0132)
Fit statistics						
Observations	63,380	63,380	63,380	63,380	63,380	63,380
Adjusted R ²	0.51786	0.28857	0.35409	0.60792	0.22006	0.26870

Standard errors (in parenthesis) are clustered on the two-digit zip code.

Signif. Codes: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 6: Estimation results from Equation (4) for the outcome variables: wholesale and retail prices, markups, Lerner Indices, sales and gross profits. *Note:* The degree of urbanization is aggregated using the mode method (assigning the most frequently occurring category). For results using area-weighted aggregation, see Table C.2.

higher absolute markups, their relative profit margin per unit remains statistically indistinguishable from that of urban pharmacies.

Finally, the year fixed effects reveal significant temporal dynamics during the observation period.⁶⁰ Relative to the 2018 baseline, retail prices and absolute markups exhibited a steady upward trend from 2019 through 2021. This price growth coincided with a persistent contraction in sales volumes, which reached its trough in 2020 (-0.135), likely reflecting pandemic-related demand and supply shocks. Despite this volatility in sales activity, nominal gross profits remained statistically stable in 2019 and 2021, with a significant but moderate decline observed only in 2020 (-0.051).

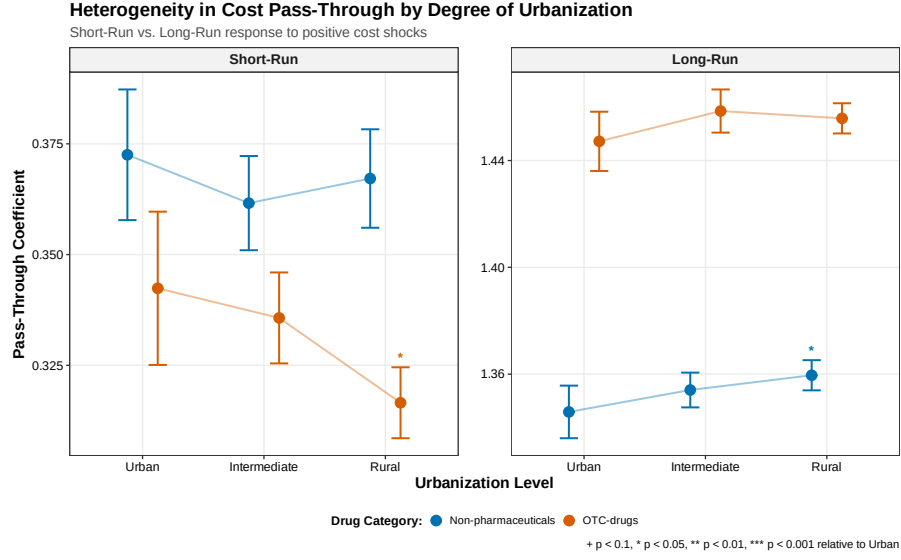
Next the short- and long-run CPT are regressed via Equation (4) on the set of regional and socio-demographic variables (see Appendix C and Tables (C.1) and (C.3) for details). To translate the regression estimates into more intuitive magnitudes, Figure 8 illustrates comparative statics for positive cost shocks utilizing *predicted marginal means* for both aggregation functions (Panel (a) and (b) correspond to the aggregation by mode and area, respectively).⁶¹

As explained in Section 4.3, presenting both spatial aggregation schemes is important because they account for different geographical biases. Aggregating by the mode treats all five-digit zip codes equally, which systematically overrepresents the high count of small rural municipalities, whereas area-weighting emphasizes more expansive intermediate regions. This distinction is relevant here: the underlying spatial classifications differ so strongly that the statistical significance of the estimates hinges on the chosen aggregation scheme, even though the directional trends remain consistent.

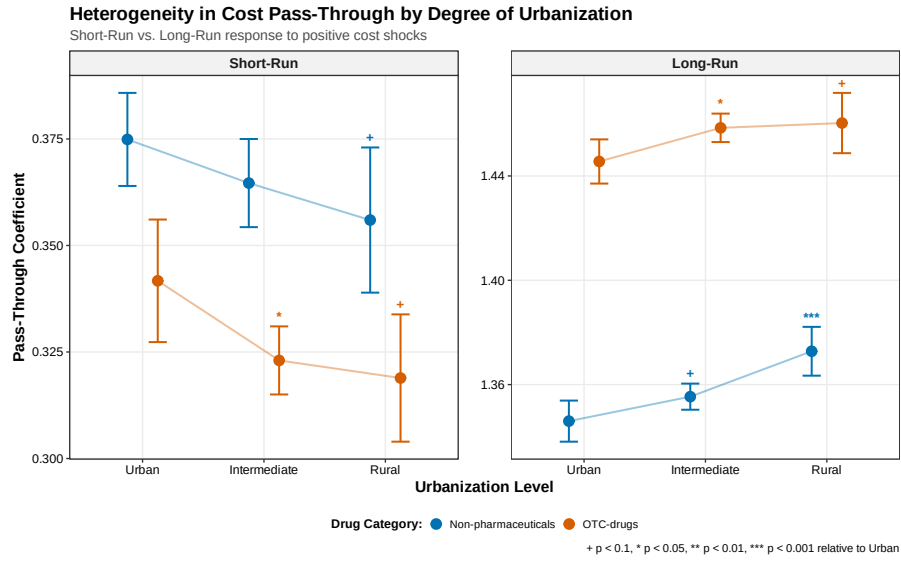
In the short run (left columns), the analysis reveals a distinct pricing inertia in rural markets. Panel (a) shows that for OTC drugs (orange line), the short-run pass-through effect declines significantly as urbanization decreases. While urban pharmacies pass through a substantial portion of cost shocks immediately, rural incumbents exhibit a statistically significant reduction in responsiveness ($p < 0.05$), with magnitudes decreasing to approximately 0.315. Crucially, this rigidity is present across the assortment: non-pharmaceuticals (blue line) exhibit consistently higher short-run pass-through rates than OTC drugs across all spatial categories, indicating that pricing inertia is most acute for health-related products where consumer sensitivity is probably lower. This pattern of stickiness is reinforced in Panel

⁶⁰The coefficients for the year 2022 (H1) are included in the regression to control for the final period of the sample. However, as this indicator covers only the first half of the year, the coefficients are not directly comparable to the full-year estimates of 2018–2021 and are therefore excluded from the interpretation.

⁶¹*Predicted marginal means* are calculated at the average value of all control variables. These values differ from the regression coefficients reported in Table C.1 and C.3, which represent the estimated pass-through for a hypothetical pharmacy with zero income and zero population (marginal effects). By evaluating the model at the sample means, the figure provides a representative estimate for a typical pharmacy within that region.



(a) Aggregation by mode.



(b) Aggregation by area.

Figure 8: Heterogeneity in CPT by degree of urbanization using the aggregation function: Panel (a) mode and by area Panel (b). Note: The points represent predicted marginal means evaluated at the sample average of all control variables (income and demographics). Error bars indicate 95 % confidence intervals. Significance levels relative to Urban: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

(b) using the area-weighted aggregation, where rural pass-through for OTC drugs remains significantly lower than in urban areas ($p < 0.05$), and non-pharmaceuticals exhibit a similar albeit marginally significant drop ($p < 0.1$). This systematic delay suggests that rural pharmacies, shielded by spatial isolation, face less competitive pressure to adjust prices compared to urban ones.

In the long run (right columns), this inertia dissipates and is replaced by substantial markup expansion. Panel (a) demonstrates that for OTC drugs, rural pass-through rates converge to levels significantly exceeding unity (approximately 1.46). Structurally, however, the order reverses compared to the short run: long-run pass-through is consistently higher for OTC drugs than for non-pharmaceuticals across all regions, indicating that the capacity for an overshifting is greatest for OTC drugs. This pattern is reinforced in Panel (b), which further reveals a statistically significant overshifting for non-pharmaceuticals in rural areas relative to urban ones ($p < 0.001$). This stark contrast – low short-run responsiveness followed by aggressive long-run price increases – indicates that rural incumbents possess sufficient spatial market power to smooth short-term volatility while securing permanently expanded margins on their entire assortment.

Finally, it is important to acknowledge that these effects likely constitute conservative lower bounds of the true structural divergence between rural, intermediate and urban markets, as the degree of urbanization is aggregated from the five-digit to the two-digit postal code level to match the definition of regional markets.

6 Discussion

The preceding empirical analyses indicate that B&M pharmacies exercise pricing power in the OTC and non-pharmaceutical segments. Specifically, the ECM estimates reveal a structural dichotomy in cost pass-through: short-run adjustments exhibit rockets & feathers pricing dynamics, whereas the long-run equilibrium is characterized by systematic cost overshifting. Combined with spatial evidence – where structurally lower transaction volumes in rural markets are offset by higher unit markups – and descriptive data on joint purchasing, these dynamics suggest that pharmacies may leverage the regulated Rx segment to exert pricing power over OTC and non-pharmaceuticals. The remainder of this section contextualizes these findings within the broader theoretical literature.

Beginning with the long run CPT rates (≈ 1 -10 months depending on the model, products and pharmacies), the general persistence of overshifting CPT rates indicates that market power in the pharmacy market is not merely transitory but a structural feature of the market. As established by the literature (see e.g., Gabaix et al., 2016; Miravete et al., 2023; Ritz,

2024; Weyl & Fabinger, 2013) such overshifting may be a result of several components such as imperfect competition, log-convexity of demand, the convexity of costs or spatial frictions. Nevertheless, the pervasive finding of overshifting allows to conclude that pharmacies leverage consumer insensitivity to maintain constant percentage markups, effectively amplifying cost shocks to preserve or expand profit margins. Furthermore, the systematic overshifting suggests that competitive pressure in the OTC segment is insufficient to discipline price setting, allowing pharmacies to keep or expand markups beyond the level for non-pharmaceuticals (see next paragraph) and far above a complete pass-through. This contrasts with the interpretation, but not the findings of previous surveys of the German market (Heinsohn & Flessa, 2013). They documented low perceived competitive pressure and argue a lack of price-based strategies. The results presented here highlight that this apparent stability masks a more structural segmentation, where high long-run pass-through may serve as a mechanism for profitability, but price changes are sticky in the short-run.

The analysis further reveals a strategic divergence between OTC drugs and non-pharmaceuticals. While OTC drugs exhibit rockets and feathers in the short-run, non-pharmaceuticals display a reverse asymmetry in the long run, where long-run pass-through rates of cost decreases exceed those of cost increases (see Sections 5.1 and 5.2). This behavior is consistent with a *traffic-builder* strategy; because non-pharmaceuticals face direct external competition from drugstores and supermarkets, pharmacies may be compelled to pass on savings aggressively to defend market share and generate footfall. In contrast, the OTC segment, protected by pharmacy-only dispensing regulations, serves as a profit center. Here, pharmacies exhibit short-run rockets and feathers pricing by swiftly passing on cost increases while delaying price reductions. This asymmetry is likely driven by the urgent nature of acute-care purchases, which inherently depresses consumer search intensity and exacerbates informational frictions – well-established theoretical determinants of asymmetric pass-through (e.g., Tappata, 2009, based on Varian, 1980).

Crucially, pricing power is heterogeneous across products. While best-sellers exhibit lower short-run pass-through rates and symmetric adjustments, their long-run pass-through rates are the highest (see Section 5.3 and Table 5). For instance, the long-run pass-through for cost increases for OTC bestsellers ($\theta^+ \approx 1.67$) exceeds that of niche products ($\theta^+ \approx 1.58$), indicating that pharmacies leverage the high demand for top-selling items to aggressively overshift cost shocks and systematically expand their absolute profit margins. This sustained overshifting could imply that consumers exhibit stronger preferences for leading brands, translating into a more inelastic or log-convex demand curve relative to niche products (see e.g., Gabaix et al., 2016; Ritz, 2024; Weyl & Fabinger, 2013).

In the short run, the empirical findings appear inconsistent with the long-

run equilibrium, as pharmacies exhibit greater pricing inertia among best-sellers. However, this makes sense from a behavioral perspective: because prices for top sellers are probably well known and highly visible, pharmacies may be disciplined by the fear of customer anger (Rotemberg, 2005), employing symmetric competitive pricing strategies to signal a favorable price image to consumers (Lal & Matutes, 1994). Consequently, short-run price adjustments remain small. Furthermore, this short-run behavior may also be consistent with *add-on pricing* strategies, where firms keep prices stable on the visible products (bestsellers) but aggressively manage the margins on the products a consumer does not keep track (niche products) (e.g., Ellison, 2005). This mirrors the findings of Granlund and Bergman (2018), who document that original brand-name drugs exhibit significantly higher price stickiness than generics, with only 20% of price adjustments occurring within three months, whereas generics exhibit 70% of the price adjustment in the same window of time. Consistent with the results of this study, this suggests that for bestselling OTC drugs, the reputational cost of frequent price fluctuations may outweigh the benefits from an immediate price adjustment, whereas for niche products, it allows for a more aggressive and asymmetric margin management. Nevertheless, this dynamic reverses in the long run, where bestsellers exhibit the highest pass-through rates. Ultimately, while strategic arguments and behavioral models provide a rationale for this temporal discrepancy between short-run and long-run pass-through rates, definitively isolating the exact mechanisms remains beyond the scope of the current data.

The overall pricing dynamics are underpinned by spatial differentiation and *one-stop shopping* frictions. Since approximately 75 % to 90 % of new prescriptions are filled within 7 days of a physician visit (Franklin et al., 2018; May et al., 2017 as cited in Albrecht et al., 2020, p. 27) and descriptive evidence indicates that nearly 20 % of OTC and non-pharmaceutical volumes are purchased jointly with Rx drugs (see Section 3.3), this gives rise to the argument that the demand for the unregulated assortment could be anchored to the regulated Rx transaction. This idea is motivated by the hypothesis, that consumers, which have already incurred search and travel costs to fulfill a prescription, exhibit inelastic demand for complementary OTC purchases. This would allow pharmacies to leverage a lock-in and joint search effect (which arise when consumers minimize the total transaction costs of the shopping basket (Florez-Acosta & Herrera-Araujo, 2020) and the marginal search cost for a second product become negligible once the consumer is committed to the store (Zhou, 2014)). Whether stemming from ex-ante planning or in-store convenience, this joint purchasing enables the pharmacy to exercise market power in for OTC drugs and non-pharmaceuticals. In a sense, the consumer is effectively *captive* due to the prescription visit (building on the dichotomy of shoppers versus captive consumers, see, e.g., Baye et al., 2006; Ronayne and Taylor, 2022; Tappata, 2009; Varian, 1980), what

allows the pharmacy to increase margins on unregulated items, without facing the competitive pressure of a planned or standalone transaction. The absolute primacy of the medical or prescription transaction in driving pharmacy footfall is corroborated by recent evidence from Canada. Hoagland and Wang (2025) demonstrate that when Ontario expanded pharmacists' scope of practice to allow prescribing for minor ailments, foot traffic to pharmacies increased by 16%. This confirms that regulatory authority over prescribing and medical treatment is an important catalyst for store visits, creating the captive audience necessary for OTC cross-selling.

Furthermore, interpreting the analyses through the lens of a lock-in effect highlights that rural pharmacies may exploit this on a broader scope: despite facing 7.6% lower transaction volumes than urban competitors, rural incumbents maintain equivalent profitability by setting OTC markups 8.3% higher. In addition, rural pharmacies overshift wholesale prices more strongly than urban ones, and exhibit more inertia in their short-run pass-through behavior. Combined with the previous described findings, this corroborates that the fixed-price Rx segment may serve as an anchor, allowing geographically more isolated pharmacies to cross-subsidize their operations through higher margins on the unregulated segment. However, it is important to note, that due to the aggregation of five-digit zip-codes to two-digit zip codes, these findings likely understate the true market power of rural incumbents.

Moreover, since rural locations typically benefit from lower land and labor costs compared to urban centers, the observation of equivalent *gross* profitability implies that rural pharmacies may generate superior *net* margins (net of fixed costs). This suggests that the elevated rural markups serve not only as a solvency mechanism but also as a vehicle for extracting rents derived from local market power. This pricing behavior aligns with the structural findings of Schaumans and Verboven (2008), who demonstrate that substantial fixed costs in low-density markets compel pharmacies to rely on high unit margins to ensure firm viability. While their analysis focuses on a regime of regulated pricing, the empirical evidence presented in this paper confirms a functional equivalent in the unregulated segment: rural incumbents, shielded from the profit-eroding effects of competition typical in denser markets, leverage local market power to extract rents (potentially necessary to cover fixed costs and pharmacists remuneration). This competition effect is empirically mirrored in the Swedish pharmacy market, where Granlund and Bergman (2018) find that increasing the number of competitors from one to ten reduces generic prices by 81% in the long run. The absence of such competitive pressure in rural German markets may allow incumbent pharmacies to sustain the high-markup equilibrium required to maintain geographic coverage.

These findings have significant implications for the digitization of the healthcare market. The current financial viability of the rural pharmacy network relies partially on this implicit cross-subsidization. However, the

introduction of electronic prescriptions (*E-Rezept*) and the potential liberalization of Rx mail-order bonuses (see Section 2.3) threaten to decouple the Rx transaction from the physical point of sale. Such a decoupling has the potential to systematically favor foreign mail-order and internet pharmacies (see Section 2), thereby eroding the captive footfall required to sustain high-margins on OTC drugs and non-pharmaceutical sales. Consequently, these findings can be interpreted in light of the discussion on the provision of a nationwide dense pharmacy network that is part of public service provision.

To be concrete, the shift towards electronic prescriptions is already well advanced; by early 2025, approximately 90% of all statutory prescriptions were issued digitally.⁶² If digitization reduces the necessity of physical visits for prescriptions, the captive footfall that upholds high OTC margins may erode. This shift may represent a fundamental breakdown of the strategic complementarity traditionally observed between medical practitioners and B&M pharmacies. While Schaumans and Verboven (2008) establish that pharmacy and physician presence are mutually reinforcing due to geographic proximity (strategic complements), the E-Rezept effectuates a digital decoupling. By removing the requirement for a physical stop at a nearby pharmacy immediately following a physician visit, digitization dissolves the primary catalyst for the one-stop shopping mechanism, potentially rendering the current rural financing model unsustainable. The severity of this threat is empirically validated by Anderes (2026), who demonstrates that when Swiss pharmacies lost their local Rx monopoly to dispensing physicians, they suffered a substantial drop in Rx revenue and profit decline, driven by a collapse in acute care sales. Reconciling this with the presented findings: without the ability to cross-subsidize via the one-stop shopping mechanism, the OTC and non-pharmaceutical profits sustaining rural pharmacies may collapse, necessitating a reassessment of how rural pharmaceutical access is financed.

The interpretation of these results is subject to constraints imposed by data privacy. First, the absence of detailed consumer-level panel data and the non-disclosure of pharmacy locations preclude the estimation of a structural demand model. However, the analysis leverages high-frequency transaction data that permits the direct measurement of absolute margins and Lerner Indices, in addition facilitating an ECM estimation at the product-pharmacy level. Although this reduced-form approach abstracts from potential intra-firm portfolio pricing strategies, such as cross-subsidizing cost shocks across complementary goods (Bliss, 1988; Chevalier et al., 2003), it cleanly isolates the pass-through dynamics of products and successfully captures key dimensions of pharmacy pricing behavior directly from the observed transaction data. Second, the pseudonymization of pharmacy identifiers necessitates the

⁶²See https://www.abda.de/fileadmin/user_upload/assets/ZDF/Jahrbuch-ZDF-2025/ZDF_2025_76_77_Eingeloeste_E_Rezepte.pdf.

aggregation of spatial data to the two-digit postal code level. To the extent that this spatial coarsening introduces measurement error by grouping heterogeneous local markets, standard econometric theory implies attenuation bias, driving the coefficients toward zero (Wooldridge, 2010, Chapter 4.4.2). Therefore, the actual differences in pricing power, cost pass-through and volume contraction experienced by isolated rural pharmacies are likely even more pronounced than the aggregated estimates suggest (Section 5.4). Furthermore, the strategic responses to rival costs (Section 5.1) likely represent conservative lower bounds of the true competitive effects.

7 Conclusion

This study provides granular empirical evidence on the competitive conduct of B&M pharmacies, documenting substantial pricing inertia and asymmetric CPT dynamics. The analysis reveals that the market is characterized by the *rockets and feathers* phenomenon, where pharmacies adjust prices upward significantly faster than downward. This asymmetry is structurally distinct across product segments: non-pharmaceuticals and OTC drugs exhibit adjustment dynamics aligning with a 2-to-1 ratio. Crucially, this aggregate rigidity masks significant heterogeneity within the assortment as best-selling products exhibit higher pass-through inertia in the short-run and the observed overall asymmetries are driven by the long-tail of more niche products. In the long run, however, inertia dissipates, and pass-through rates for OTC drugs and non-pharmaceuticals converge to levels significantly exceeding unity. While pass-through rates for OTC drugs converge to higher levels, effectively permanently expanding absolute margins, the non-pharmaceutical segment exhibits minor reverse asymmetry, with higher-long run pass-through rates for cost decreases. Here, pharmacies pass on savings more aggressively, likely to defend market share, consistent with a traffic-builder’ strategy.

A spatial analysis highlights regional differences in transaction volumes and markups. Rural pharmacies exhibit transaction volumes approximately 7.6 % lower than their urban counterparts but compensate for this by setting unit markups on OTC drugs and non-pharmaceuticals roughly 8.3 % higher. This pricing strategy effectively offsets the volume shortfall, generating gross profits that are statistically indistinguishable from high-volume urban locations. This suggests that B&M pharmacies may exercise local market power to neutralize the structural volume disadvantage of rural locations with higher markups. Furthermore, rural pharmacies exhibit more pronounced long-run cost overshifting than their urban counterparts. This market power is likely underpinned by the sector’s structural reliance on the Rx segment, where prices are legally fixed. The urgency and necessity of filling prescriptions could incentivize consumers to choose the nearest provider

to minimize opportunity costs. Consequently, pharmacies may leverage these Rx visits to extract rents from captive consumers via high-margin cross-selling of OTC drugs and non-pharmaceuticals. Whether driven by acute urgency, complementarity (purchasing OTC drugs alongside prescriptions), or informational frictions, such *one-stop shopping* behavior could create a lock-in effect. This may enable pharmacies to exert significant market power over selling OTC drugs and non-pharmaceuticals.

From a health-policy perspective, these findings imply that the stability of pharmaceutical provision, particularly in rural areas, relies partially on this implicit cross-subsidization mechanism. Despite the secondary role of OTC drugs and non-pharmaceuticals in a pharmacy’s overall gross profits, these products constitute a critical financial pillar, effectively financing the infrastructure required for prescription drug provision where Rx volumes alone might be insufficient to cover fixed costs. Consequently, the high degree of cost transmission and markup expansion observed in this sector may not merely be a result of market power, but a structural necessity for maintaining a dense pharmacy network, and thus a part of public service provision.

Looking forward, these dynamics necessitate further investigation at the micro-geographic level to fully verify the relationship between local competitive density and pricing strategies from B&M pharmacies. More critically, future research must monitor the impact of digitization, particularly the introduction of mandatory electronic prescriptions (*E-Rezept*). By reducing the transaction costs associated with forwarding prescriptions to remote providers (e.g., online pharmacies), electronic prescriptions have the potential to erode the transaction volume of Rx-drugs for B&M pharmacies. This, in turn, jeopardizes the revenue stream from OTC drugs and non-pharmaceuticals, as the observed market power in these unregulated segments could be structurally dependent on the footfall generated by Rx-drugs. If this digital decoupling breaks the *one-stop shopping* advantage, the cross-subsidization mechanism may collapse. As recent evidence from Switzerland demonstrates, losing the Rx anchor can disrupt this cross-subsidization mechanism and permanently erodes pharmacy profitability (Anderes, 2026). Consequently, this may force policy makers to reassess how (rural) pharmaceutical access is financed to guarantee an appropriately dense pharmacy network, fulfilling the state’s responsibility for adequate public service provision.

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During the preparation of this work the authors used Google's Gemini in order to improve language. After using this service, the author reviewed and edited the content as needed and takes full responsibility for the content of the publication.

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A Empirical Strategy in Levels

A.1 Strategy

This section outlines the baseline level specification, which serves as a structural precursor to the ECM presented in the main text. While the ECM is preferred for handling non-stationarity and asymmetries, estimating the relationship in levels provides a benchmark for the average pass-through and validates the effectiveness of the high-dimensional fixed effects structure. The specification is defined as follows:

$$p_{jft} = \alpha_{jf} + \eta_t + \delta_{fm} + \rho w_{jft} + \phi p_{jft-\tau} + \beta w_{jft-\tau} + \sigma \bar{w}_{-f,ag,t} + \epsilon_{jft}, \quad (\text{A.1})$$

where p_{jft} and w_{jft} denote the retail and wholesale prices, respectively, of product j (identified by PCN) sold by pharmacy f at calendar day t and calendar month m . The unit of observation is the granular product-pharmacy level (j, f, t) , which inherently avoids composition biases arising from shifts in product mix. The coefficients ρ and β estimate the contemporaneous and lagged direct pass-through of wholesale costs. To account for irregular transaction records, lagged values are defined relative to the last observed transaction τ . The long-run CPT rate can be recovered as $\theta_{level} = (\rho + \beta)/(1 - \phi)$.

The model employs a high-dimensional fixed effects structure to isolate idiosyncratic shocks. α_{jf} (product $\times$ pharmacy fixed effects) absorbs time-invariant characteristics and pharmacy-specific markups; η_t captures common daily market shocks; and δ_{fm} (pharmacy $\times$ month fixed effects) controls for time-varying local demand or labor cost fluctuations. Following standard oligopoly theory (MacKay et al., 2014), the specification controls for strategic interactions using the average competitor wholesale price, $\bar{w}_{-f,ag,t}$, calculated at the chemical substance level within the same two-digit postal code. A positive coefficient σ indicates strategic complementarity (Bulow et al., 1985; Vives, 1990).

Three econometric limitations motivate the use of the ECM in the main text over this level specification. First, including a lagged dependent variable ($p_{jft-\tau}$) with fixed effects introduces Nickell bias (Nickell, 1981), though this is mitigated by the large time dimension ($T > 1,600$). Second, and more critically, retail and wholesale prices may exhibit $I(1)$ non-stationarity. While time fixed effects absorb common trends, they cannot fully correct for idiosyncratic stochastic trends, rendering level regressions susceptible to spurious correlation. Thus, these results should be interpreted as average baseline estimates before accounting for cointegration and asymmetry. Third, the ECM with autoregressive distributed lags can handle dynamics, especially, if parts of the data exhibits non-stationarity (see Table E.1).

A.2 Results

Table A.1 presents the baseline estimates. Columns (1)–(5) report results for OTC drugs, and Columns (6)–(10) for non-pharmaceuticals. The specification is saturated sequentially to demonstrate the necessity of granular controls. Columns (1) and (6) include only time fixed effects (η_t); Columns (2) and (7) introduce separate product and pharmacy fixed effects; Columns (3) and (8) add granular product $\times$ pharmacy interactions; and Columns (4) and (9) incorporate the full set of time-varying pharmacy controls. Finally, Columns (5) and (10) – the preferred specifications within this framework – extend the model dynamically by including the lagged dependent variable to capture price persistence.

The results highlight the severity of composition bias in the naive specifications. A model including only time fixed effects (Columns 1 and 6) yields pass-through coefficients exceeding unity (1.337 and 1.339), suggesting price overshifting.^{A1} However, this finding can be an artifact of composition bias, whereby high-markup pharmacies systematically stock higher-cost product variants. The estimate declines sharply upon the inclusion of separate product and pharmacy fixed effects (Column (2)) and stabilizes at approximately 0.30 once granular pharmacy $\times$ product interactions are introduced (Columns (3) and (4)). This confirms that the α_{jf} term effectively isolates idiosyncratic cost shocks from unobserved, pharmacy-specific pricing strategies.

In the preferred dynamic specifications (Columns (5) and (10)), still evidence of an incomplete pass-through appears. For OTC drugs, the immediate short-run pass-through (ρ) is 0.147. Due to significant price persistence ($\phi = 0.597$), the implied long-run pass-through is $\theta \approx 0.31$, indicating that pharmacies absorb nearly 70% of a (€1) cost shock even in the long run. Non-pharmaceuticals exhibit higher sensitivity, with a short-run coefficient of 0.290 and a long-run pass-through of approximately 0.50. The coefficient on competitors' wholesale prices (σ) is positive and statistically significant across preferred specifications (0.0061 for OTC), supporting the theoretical prediction of strategic complementarity. For non-pharmaceuticals the effect is not significant. This discrepancy could appear due to measurement precision: OTC-drugs are precisely matched via granular ATC5 codes, whereas the non-pharmaceutical segment contains more heterogeneous products. This measurement error introduces attenuation bias, driving the estimated competitive effect toward zero (Wooldridge, 2010, Chapter 4.4.2).

Comparing these baseline estimates with the asymmetric error correc-

^{A1}Simple pooled OLS estimations (see Table A.2, Columns 1 and 4) yield results nearly identical to the time-fixed effects specifications. This composition bias remains evident even when the data are aggregated at coarser unit levels (e.g., by ATC5, brand, or manufacturer) or along coarser temporal dimensions (e.g., quarterly), as detailed in Table A.4.

Dependent Variable:	OTC-drugs					Non-Pharmaceuticals				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ρ_{wjt}	1.337*** (0.0102)	0.4775*** (0.0288)	0.2934*** (0.0143)	0.3004*** (0.0145)	0.1469*** (0.0070)	1.339*** (0.0106)	0.6706*** (0.0431)	0.4679*** (0.0544)	0.4472*** (0.0424)	0.2896*** (0.0203)
Avg. Price of Competitors (σ)	0.0790*** (0.0122)	0.0079* (0.0037)	0.0127*** (0.0027)	0.0126*** (0.0026)	0.0061*** (0.0012)	0.0093 (0.0158)	0.0039** (0.0014)	0.0087*** (0.0015)	0.0088*** (0.0015)	0.0047*** (0.0008)
ϕ_{ijt}					0.5969*** (0.0128)					0.4611*** (0.0175)
β_{ijt}					-0.0217*** (0.0052)					-0.0182 (0.0120)
Fit statistics										
# Year-Days	1,642	1,642	1,642	1,642	1,641	1,642	1,642	1,642	1,642	1,641
# PCN	-	38,100	-	-	-	-	140,367	-	-	-
# Pharmacy ID	-	6,338	-	-	-	-	6,338	-	-	-
# Pharmacy ID $\times$ PCN	-	-	11,083,124	11,083,120	9,025,825	-	-	19,669,497	19,669,489	15,320,263
# Pharmacy ID $\times$ Year-Month	-	-	-	341,995	341,959	-	-	-	341,971	341,942
Observations	493,055,951	493,050,598	487,657,495	487,657,458	474,947,837	400,185,218	400,158,602	389,388,666	389,388,604	365,635,572
Adjusted R ²	0.93955	0.97173	0.98636	0.98681	0.99111	0.93790	0.97449	0.98876	0.98972	0.99281
Within Adjusted R ²	0.93944	0.08881	0.03517	0.03648	0.37069	0.93773	0.20491	0.09806	0.09053	0.29538
Fixed Effects (FEs)										
Year-Days FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
PCN FEs		✓					✓			
Pharmacy ID FEs		✓					✓			
Pharmacy ID $\times$ PCN FEs			✓	✓	✓			✓	✓	✓
Pharmacy ID $\times$ Year-Month FEs				✓	✓				✓	✓

Standard errors (in parenthesis) are clustered on the pharmacy id & PCN.
Differences in the number of observations across models stem from perfect fits and singletons.
Signif. Codes: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A.1: Baseline estimation results from Equation (A.1): Average pass-through of wholesale costs to retail prices for OTC drugs and non-pharmaceuticals.

tion model (Table 3 in the main text) reveals that the level specification captures a weighted average of the underlying short-run pricing dynamics. For OTC drugs, the baseline short-run coefficient of 0.147 masks significant heterogeneity: the asymmetric model decomposes this average into a rapid response to cost increases ($\rho^+ \approx 0.26$) and a sluggish response to cost decreases ($\rho^- \approx 0.12$). A similar pattern emerges for non-pharmaceuticals, where the baseline estimate of 0.290 falls strictly between the estimated upward ($\rho^+ \approx 0.41$) and downward ($\rho^- \approx 0.23$) adjustment parameters.

However, the disparity is most pronounced regarding the long-run equilibrium. While the baseline model implies substantial incomplete pass-through ($\theta_{level} \approx 0.31$ for OTC-drugs), the error correction framework recovers long-run targets exceeding unity ($\theta^+ \approx 1.53, \theta^- \approx 1.54$). This stark contrast indicates that the level specification suffers from severe attenuation bias due to the non-stationarity of the price series. By failing to explicitly model the cointegrating relationship, the level model conflates short-run adjustment frictions with the long-run equilibrium, erroneously suggesting that pharmacies permanently absorb cost shocks when, in reality, they overshift them. A similar pattern is observed for non-pharmaceuticals, where the long-run pass-through estimate rises from approximately 0.50 in the level model to approximately 1.36—1.40 in the error correction specification.

Overall, while these baseline results establish a robust finding of incomplete pass-through, the high persistence of prices and theoretical concerns regarding non-stationarity suggest that the adjustment process is better modeled within the error correction framework presented in Section 4.1.

Dependent Variable:	<i>p_{jft}</i>					
	OTC-drugs			Non-Pharmaceuticals		
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	1.587*** (0.0679)			0.7722*** (0.0681)		
ρw_{jft}	1.337*** (0.0102)	0.5121*** (0.0269)	0.3709*** (0.0183)	1.339*** (0.0106)	0.6783*** (0.0422)	0.4881*** (0.0517)
Avg. Price of Competitors (σ)	0.0790*** (0.0121)	0.0137** (0.0047)	0.0221*** (0.0046)	0.0101 (0.0157)	0.0164*** (0.0019)	0.0225*** (0.0021)
Fit statistics						
# PCN	–	38,100	–	–	140,367	–
# Pharmacy ID	–	6,338	–	–	6,338	–
# Pharmacy ID $\times$ PCN	–	–	11,083,124	–	–	19,669,497
Observations	493,055,951	493,050,598	487,657,495	400,185,218	400,158,602	389,388,666
Adjusted R ²	0.93942	0.97111	0.98562	0.93781	0.97434	0.98858
Within Adjusted R ²		0.10166	0.05622		0.20915	0.10645
Fixed Effects (FEs)						
PCN FEs		✓			✓	
Pharmacy ID FEs		✓			✓	
Pharmacy ID $\times$ PCN FEs			✓			✓
Standard errors (in parenthesis) are clustered on the pharmacy id & PCN. Differences in the number of observations across models stem from perfect fits and singletons. Signif. Codes: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.						

Table A.2: Stepwise estimation results for Equation (A.1). The table reports the average pass-through of wholesale costs to retail prices for OTC drugs and non-pharmaceuticals, sequentially introducing more granular fixed effects to illustrate the magnitude of composition bias.

Dependent Variable:	<i>p_{jft}</i>					
	OTC-drugs			Non-Pharmaceuticals		
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	1.788*** (0.0835)			0.8248*** (0.0519)		
ρw_{jft}	1.356*** (0.0132)	0.5302*** (0.0258)	0.3649*** (0.0183)	1.339*** (0.0094)	0.6755*** (0.0408)	0.4870*** (0.0501)
Fit statistics						
# PCN	–	39,286	–	–	141,633	–
# Pharmacy ID	–	6,338	–	–	6,338	–
# Pharmacy ID $\times$ PCN	–	–	11,888,023	–	–	20,139,641
Observations	504,909,293	504,903,956	499,097,510	408,126,138	408,099,278	397,065,954
Adjusted R ²	0.94210	0.97359	0.98722	0.93829	0.97428	0.98867
Within Adjusted R ²		0.11292	0.05343		0.20722	0.10644
Fixed Effects (FEs)						
PCN FEs		✓			✓	
Pharmacy ID FEs		✓			✓	
Pharmacy ID $\times$ PCN FEs			✓			✓
Standard errors (in parenthesis) are clustered on the pharmacy id & PCN. Differences in the number of observations across models stem from perfect fits and singletons. Signif. Codes: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.						

Table A.3: Stepwise estimation results for Equation (A.1) excluding competitor effects. The table reports the average pass-through of wholesale costs to retail prices for OTC drugs and non-pharmaceuticals, sequentially introducing more granular fixed effects to illustrate the magnitude of composition bias.

Dependent Variable:	OTC-drugs				P_{jft}			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ρw_{jft}	1.173*** (0.0279)	1.186*** (0.0257)	0.5536*** (0.0287)	0.3614*** (0.0164)	1.293*** (0.0216)	1.288*** (0.0195)	0.7256*** (0.0400)	0.6233*** (0.0526)
Avg. Price of Competitors (σ)	-0.0003 (0.0084)	0.0186* (0.0088)	0.0005 (0.0110)	0.0452*** (0.0130)	-0.0117 (0.0080)	0.0038 (0.0067)	0.0029 (0.0048)	0.0388*** (0.0074)
Fit statistics								
# ABFM	4,829	—	—	—	10,645	—	—	—
# Year-Quarter	18	18	18	18	18	18	18	18
# Pharmacy ID	6,338	—	6,338	—	6,338	—	6,338	—
# ABFM $\times$ Pharmacy ID	—	6,525,189	—	—	—	8,885,314	—	—
# PCN	—	—	38,667	—	—	—	139,021	—
# PCN-Pharmacy ID	—	—	—	10,868,768	—	—	—	18,015,070
Standard-Errors	Pharmacy ID & ATC5		Pharmacy ID & PCN		Pharmacy ID & ATC5		Pharmacy ID & PCN	
Observations	64,397,221	62,071,446	97,267,243	90,487,665	73,991,934	69,720,862	132,387,355	119,198,530
Adjusted R ²	0.97523	0.98890	0.98085	0.99266	0.95809	0.97429	0.97863	0.99257
Within Adjusted R ²	0.91252	0.85921	0.13112	0.06971	0.80091	0.76061	0.25461	0.22207
Fixed Effects (FEs)								
ABFM FEs	✓				✓			
Year-Quarter FEs	✓	✓	✓	✓	✓	✓	✓	✓
Pharmacy ID FEs	✓		✓		✓		✓	
ABFM $\times$ Pharmacy ID FEs		✓				✓		
PCN FEs			✓				✓	
PCN-Pharmacy ID FEs				✓				✓

Standard errors (in parenthesis) are clustered on the pharmacy id & ATC5 or PCN.
Differences in the number of observations across models stem from perfect fits and singletons.
Signif. Codes: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A.4: Sensitivity of pass-through estimates to aggregation levels. This table presents estimation results for Equation (A.1) where the time dimension t is aggregated to the quarterly level across all specifications. For columns (1), (2), (5) and (6) the product dimension j is aggregated at the ATC5, format, brand name, and manufacturer.

B Heterogeneity Analysis

B.1 Pharmacy Size

Since the magnitude of CPT can serve as a proxy for market power, using the estimated CPT from Equation (3) allows to test whether firm size plays a relevant role in long-run pricing outcomes. To formally quantify these systematic differences, pharmacies are stratified into deciles based on their total annual revenue. Total annual revenue is calculated as the aggregate of all revenue sources, including OTC-drugs, Rx-drugs, and non-pharmaceutical products. The derived long-run pass-through targets ($\hat{\theta}_{fy}^+$ and $\hat{\theta}_{fy}^-$) are subsequently regressed on these decile indicators using the following specification:

$$\hat{\Psi}_{fy}^k = \sum_{d=2}^{10} \psi_d \mathbf{1}\{\text{Decile}_{fy} = d\} + \kappa_y + \nu_{fy}, \quad (\text{B.1})$$

where the dependent variable $\hat{\Psi}_{fy}^k \in \{\hat{\rho}_{fy}^k, \hat{\theta}_{fy}^k\}$ represents the structural pricing parameter for pharmacy f in year y corresponding to shock direction $k \in \{+, -\}$. Specifically, the model is estimated separately for the long-run equilibrium targets ($\hat{\theta}_{fy}^+$, $\hat{\theta}_{fy}^-$) and by OTC drugs and non-pharmaceuticals. The term $\mathbf{1}\{\text{Decile}_{fy} = d\}$ is an indicator variable equal to 1 if the pharmacy falls into the d -th revenue decile in that year. The summation starts at

$d = 2$ as the first decile serves as the reference category. Consequently, the coefficient ψ_d captures the systematic deviation in pricing power for larger pharmacies relative to the smallest market participants. The term κ_y denotes year fixed effects, absorbing common temporal shifts in the distribution of coefficients, and ν_{fy} is the error term.

Table B.1 reports the results of regressing the pharmacy-specific long-run pass-through estimates ($\hat{\theta}_{fy}$) on revenue decile indicators. The constant term captures the baseline pass-through for the smallest pharmacies (Decile 1) in the reference year (2018).

The results reveal a clear inverse relationship between pharmacy size and pass-through magnitude. For OTC drugs (Columns (3) and (4)), the baseline pass-through for the smallest pharmacies is estimated at 1.474 for cost increases and 1.438 for decreases. In contrast, the coefficients for the upper revenue deciles are consistently negative and statistically significant. The largest pharmacies (Decile 10) exhibit a pass-through rate approximately 0.033 points lower for positive shocks ($p < 0.001$) and 0.038 points lower for negative shocks. This indicates that small pharmacies are the most aggressive in expanding markups, passing on roughly €1.47 of a €1 cost shock in the long run. This systematic difference suggests that scale is associated with differentiated pricing dynamics. While cost overshifting remains a universal feature of the market (with even the largest pharmacies staying well above unity at ≈ 1.44), the statistically distinct reduction for high-revenue pharmacies indicates that they operate with slightly tighter markup constraints.

The year fixed effects reveal distinct reactions to macroeconomic shocks. In the pre-pandemic year of 2019, pricing remained largely comparable to the 2018 baseline, with only minor deviations observed (most notably a slight increase in pass-through for OTC cost decreases, 0.015). The onset of the pandemic in 2020, however, triggered a significant and uniform increase in pass-through rates across all categories (e.g., 0.034 for non-pharmaceutical decreases). Although pharmacies were classified as essential infrastructure and remained open during the national lockdowns,^{B1} the market was characterized by severe volatility, supply chain disruptions, and shifting demand patterns. This uniformity dissipated in 2021. While non-pharmaceuticals remained highly sensitive to cost reductions (+0.032), OTC pass-through rates re-anchored to pre-pandemic levels (−0.006 for increases). Finally, while estimates for 2022 suggest a resurgence in sensitivity for non-pharmaceuticals, conclusive interpretation is constrained as the dataset covers only the first half of the year.

Economically, the finding that smaller pharmacies pass on costs more intensely suggests a link to spatial market power. Small-revenue pharmacies

^{B1}See, https://www.politikwissenschaft.tu-darmstadt.de/media/politikwissenschaft/ifp_dokumente/arbeitsbereiche_dokumente/oeffentliche_verwaltung__public_policy/forschung_2/corona_projekt/2020_03_16_BLK.pdf.

Dependent Variable:	Long-Run CPT Estimate (+)	Long-Run CPT Estimate (-)	Long-Run CPT Estimate (+)	Long-Run CPT Estimate (-)
	Non-Pharmaceuticals		OTC-drugs	
	(1)	(2)	(3)	(4)
Constant	1.359*** (0.0038)	1.366*** (0.0051)	1.474*** (0.0040)	1.438*** (0.0066)
Decile = D02	-0.0023 (0.0036)	-0.0047 (0.0049)	-0.0085* (0.0040)	-0.0177** (0.0066)
Decile = D03	-0.0113** (0.0039)	-0.0204*** (0.0049)	-0.0164*** (0.0046)	-0.0197** (0.0072)
Decile = D04	-0.0106** (0.0039)	-0.0149** (0.0052)	-0.0219*** (0.0046)	-0.0194** (0.0066)
Decile = D05	-0.0027 (0.0038)	-0.0091+ (0.0051)	-0.0144** (0.0048)	-0.0261*** (0.0067)
Decile = D06	-0.0085* (0.0039)	-0.0198** (0.0058)	-0.0251*** (0.0046)	-0.0277*** (0.0063)
Decile = D07	-0.0079+ (0.0040)	-0.0158** (0.0055)	-0.0243*** (0.0048)	-0.0319*** (0.0072)
Decile = D08	-0.0103* (0.0040)	-0.0173** (0.0057)	-0.0272*** (0.0041)	-0.0351*** (0.0070)
Decile = D09	-0.0058 (0.0044)	-0.0220*** (0.0063)	-0.0242*** (0.0052)	-0.0352*** (0.0069)
Decile = D10	-0.0079+ (0.0044)	-0.0225*** (0.0056)	-0.0326*** (0.0057)	-0.0378*** (0.0074)
Year = 2019	0.0033+ (0.0017)	0.0018 (0.0027)	-0.0021 (0.0022)	0.0152*** (0.0034)
Year = 2020	0.0069*** (0.0018)	0.0338*** (0.0028)	0.0087*** (0.0022)	0.0277*** (0.0037)
Year = 2021	-0.0083*** (0.0019)	0.0321*** (0.0028)	-0.0062** (0.0022)	-0.0070+ (0.0038)
Year = 2022(H1)	0.0138*** (0.0021)	0.0442*** (0.0033)	-0.0004 (0.0023)	0.0277*** (0.0041)
Fit statistics				
Observations	26,398	26,398	26,317	26,317
Adjusted R ²	0.00533	0.01536	0.00671	0.00836

Standard errors (in parenthesis) are clustered on the two-digit zip code level.
Signif. Codes: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table B.1: Estimation results from Equation (B.1): Variation in long-run pass-through rates ($\hat{\theta}_{fy}^+$ and $\hat{\theta}_{fy}^-$) by pharmacy revenue decile.

are frequently located in rural or less densely populated areas, where geographical isolation grants them a degree of local market power. This position likely enables them to overshift cost shocks to captive consumers who face high search and travel costs. Conversely, high-revenue pharmacies are typically situated in competitive urban centers where rival pressure constrains their ability to expand markups, forcing them to absorb a marginally larger share of cost fluctuations. The extent to which these pricing dynamics are explicitly driven by local socio-demographic factors is formally examined in the Section 5.4.

B.2 Pass-Through Dynamics

To quantify how this parameter heterogeneity translates into dynamic pricing outcomes, Figure B.1 visualizes the distribution of CIRFs. The colored lines represent the median adjustment path, while the shaded regions (interquartile range (25th–75th), 10th–90th and 2.5th–97.5th percentiles) capture the spread of responses across the market.

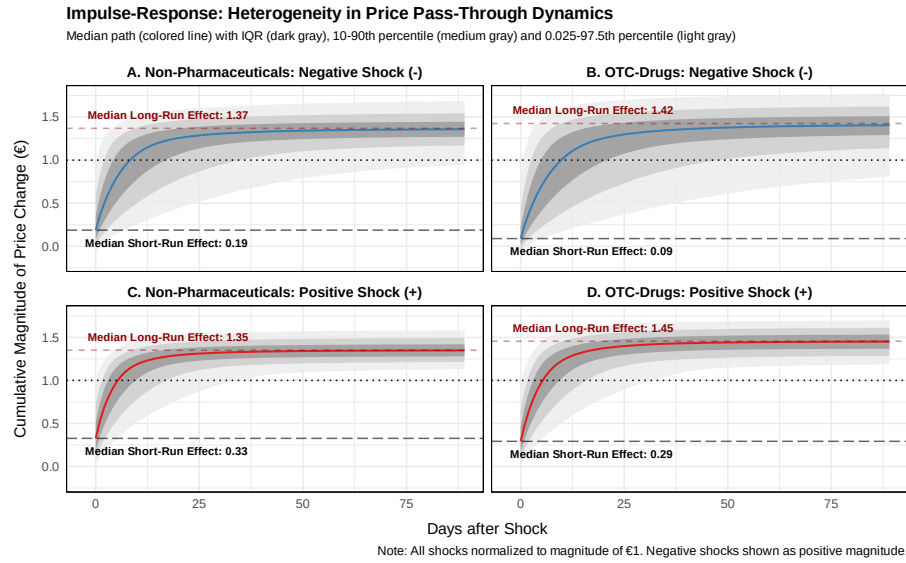


Figure B.1: Heterogeneity in Dynamic Adjustment Paths. The figure plots the median cumulative impulse response (colored line) along with the interquartile range (dark gray), 10th–90th percentile range (medium gray), and 2.5th–97.5th percentile range (light gray). Panels illustrate the dispersion in adjustment dynamics for negative and positive shocks across product segments.

The results exhibit large dispersion in adjustment dynamics, particularly for OTC cost decreases (Panel B). While the median short-run effect is €0.09, the lower tail of the distribution reveals severe rigidity. The bottom 10th percentile shows negligible price adjustments for the first week, and the most rigid 2.5 % of pharmacies do not pass on any portion of the cost saving for

nearly two weeks following the shock. Conversely, for cost increases (Panel D), the entire distribution shifts upward; even the least responsive pharmacies (bottom 10th percentile) implement price increases almost immediately. Furthermore, the widening confidence bands in the long run highlights the diversity in equilibrium strategies. While the median long-run pass-through converges to approximately 1.45, the upper 90th and 97.5th percentile bands extend significantly higher. This suggests that a subset of pharmacies over-shifts cost shocks well beyond the average.

Finally, comparing the paths across segments reveals differences in pricing asymmetry. For OTC drugs, the rockets and feathers effect is strictly a short-run phenomenon driven by extreme stickiness: the median short-run response to a cost increase (€0.29) is more than three times larger than for a decrease (€0.09). However, this asymmetry vanishes after approximately one month in the long run (approx. €1.45 vs. €1.42). In the non-pharmaceutical segment, the short-run asymmetry is less pronounced as increases (€0.33) outpace decreases (€0.19). Uniquely, this segment exhibits a long-run inversion: the cumulative pass-through for cost decreases (€1.37) slightly exceeds that of cost increase (€1.35).

C Socio-Demographic Analysis

Table C.1 examines whether the structural differences in market power identified above translate into differential CPT behavior. The estimated short-run and long-run CPT coefficients from Table 7 are regressed via Equation (4) on the set of regional and socio-demographic variables to test for inertia, asymmetry and differences in magnitudes.

The results reveal that rural markets exhibit pricing inertia, particularly for OTC drugs. In the short run (Columns (1) and (2)), the interaction terms for OTC drugs in rural areas are consistently negative for both cost increases (-0.0178) and decreases (-0.0205). This indicates that rural pharmacies are generally slower to adjust prices in response to immediate shocks compared to their urban counterparts, consistent with lower competitive pressure necessitating less frequent price adjustments.

However, the long-run estimates (Columns (3) and (4)) uncover an asymmetry regarding cost savings. For the baseline non-pharmaceutical category, rural markets are more responsive to cost reductions (0.0218) than they are to cost increases (0.0137). However, this finding vanishes for OTC drugs: the negative interaction term (-0.0278) fully offsets the rural baseline, indicating that rural pharmacies do not pass on OTC cost savings, and the effect is the same for positive pass-through rates from OTC drugs and non-pharmaceuticals (0.0137). In contrast, for cost increases, the interaction is statistically insignificant; consequently, rural pharmacies exhibit a uniform pass-through rate of 0.0137 across both product types.

Dependent Variable:	Negative CPT	Positive CPT	Negative CPT	Positive CPT
	Short-Run		Long-Run	
	(1)	(2)	(3)	(4)
Constant	0.1213 (0.1435)	0.4946*** (0.1377)	1.252*** (0.1526)	1.391*** (0.1191)
Intermediate	-0.0018 (0.0086)	-0.0109 (0.0100)	0.0089 (0.0084)	0.0082 (0.0063)
Rural	-0.0040 (0.0081)	-0.0054 (0.0101)	0.0218** (0.0081)	0.0137* (0.0063)
Intermediate $\times$ OTC drugs	-0.0074 (0.0068)	0.0042 (0.0092)	-0.0011 (0.0067)	0.0031 (0.0055)
Rural $\times$ OTC drugs	-0.0173** (0.0061)	-0.0205* (0.0093)	-0.0278*** (0.0067)	-0.0051 (0.0048)
OTC drugs	-0.0909*** (0.0040)	-0.0301*** (0.0066)	0.0654*** (0.0055)	0.1013*** (0.0039)
year = 2019	0.0150*** (0.0034)	0.0372*** (0.0034)	0.0062* (0.0027)	-0.0003 (0.0021)
year = 2020	0.0203*** (0.0026)	0.1101*** (0.0029)	0.0310*** (0.0025)	0.0081*** (0.0017)
year = 2021	0.0345*** (0.0037)	0.1450*** (0.0043)	0.0116*** (0.0033)	-0.0076** (0.0028)
year = 2022(H1)	0.0469*** (0.0044)	0.1405*** (0.0045)	0.0343*** (0.0036)	0.0061+ (0.0032)
Income per Person (in 10T)	-0.0357** (0.0111)	-0.0275* (0.0126)	0.0270* (0.0121)	0.0225* (0.0089)
Fraction of Population ≥ 65	0.0020 (0.0013)	0.0016 (0.0016)	-0.0040** (0.0015)	-0.0024 (0.0015)
Fraction of Population < 18	0.0014 (0.0028)	-0.0051* (0.0025)	-0.0002 (0.0026)	-0.0033 (0.0024)
Fraction of 1-Person-Households	0.0006 (0.0012)	-0.0019+ (0.0011)	0.0012 (0.0011)	0.0002 (0.0009)
Fraction of 2-Person-Households	0.0026 (0.0023)	-0.0005 (0.0024)	0.0021 (0.0024)	-9.42×10^{-5} (0.0019)
Fit statistics				
Observations	57,245	57,380	57,245	57,380
Adjusted R ²	0.06984	0.07815	0.03170	0.16380

Standard errors (in parenthesis) are clustered on the two-digit zip code.

Signif. Codes: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C.1: Estimation results from Equation (4): Determinants of CPT. *Note:* Outcome variables include short-run positive (ρ_{fy}^+) and negative (ρ_{fy}^-) pass-through coefficients, and the long-run pass-through rate (θ_{fy}^- and θ_{fy}^+). The degree of urbanization is aggregated using the mode method. For results using area-weighted aggregation, see Table C.3.

These results reveal a distinct pricing strategy in rural areas depending on the product type. For non-pharmaceuticals, rural pharmacies actually pass on cost savings more aggressively than the urban baseline. However, this pattern reverses for OTC drugs. Instead of passing on these savings, rural pharmacies retain a larger share of the cost reduction. By shielding OTC drug margins in this way, they might compensate for lower transaction volumes.

Dependent Variable:	log(Wholesale Price) (1)	log(Consumer Price) (2)	log(Markup) (3)	Lerner Index (4)	log(Sales) (5)	log(Gross Profits) (6)
Constant	2.941*** (0.2016)	3.184*** (0.2050)	1.834*** (0.3142)	0.2379*** (0.0641)	5.485*** (0.8169)	7.319*** (0.8656)
Intermediate	-0.0243* (0.0113)	-0.0168 (0.0103)	-0.0035 (0.0135)	0.0046 (0.0028)	0.0216 (0.0366)	0.0181 (0.0390)
Rural	-0.0295* (0.0125)	-0.0068 (0.0125)	0.0378* (0.0181)	0.0147*** (0.0035)	0.0753 (0.0501)	0.1130* (0.0557)
Intermediate $\times$ OTC drugs	0.0366* (0.0163)	0.0382* (0.0168)	0.0419* (0.0181)	0.0013 (0.0019)	-0.0046 (0.0323)	0.0373 (0.0251)
Rural $\times$ OTC drugs	0.0595*** (0.0169)	0.0570** (0.0174)	0.0460* (0.0194)	-0.0023 (0.0024)	-0.0749* (0.0356)	-0.0289 (0.0321)
OTC drugs	-0.2571*** (0.0143)	-0.0833*** (0.0150)	0.2058*** (0.0161)	0.1072*** (0.0014)	0.3286*** (0.0284)	0.5344*** (0.0219)
year = 2019	0.0212*** (0.0031)	0.0209*** (0.0028)	0.0383*** (0.0039)	0.0037*** (0.0009)	-0.0436*** (0.0117)	-0.0053 (0.0120)
year = 2020	0.0425*** (0.0028)	0.0564*** (0.0023)	0.0834*** (0.0028)	0.0090*** (0.0007)	-0.1354*** (0.0077)	-0.0520*** (0.0080)
year = 2021	0.0552*** (0.0042)	0.0642*** (0.0035)	0.0806*** (0.0050)	0.0057*** (0.0013)	-0.0817*** (0.0155)	-0.0011 (0.0162)
year = 2022(H1)	0.0178*** (0.0052)	0.0312*** (0.0041)	0.0544*** (0.0060)	0.0083*** (0.0017)	-0.7621*** (0.0192)	-0.7077*** (0.0205)
Income per Person (in 10T)	0.1742*** (0.0131)	0.1831*** (0.0117)	0.2014*** (0.0201)	0.0061 (0.0051)	0.1691* (0.0666)	0.3705*** (0.0706)
Fraction of Population ≥ 65	0.0048* (0.0021)	0.0030 (0.0020)	-0.0001 (0.0032)	-0.0011 (0.0007)	-0.0188+ (0.0097)	-0.0189+ (0.0101)
Fraction of Population < 18	-0.0156*** (0.0043)	-0.0154*** (0.0044)	-0.0156* (0.0067)	2.14×10^{-5} (0.0013)	0.0842*** (0.0186)	0.0686*** (0.0191)
Fraction of 1-Person-Households	-0.0146*** (0.0016)	-0.0126*** (0.0016)	-0.0097*** (0.0023)	0.0011* (0.0005)	0.0340*** (0.0061)	0.0244*** (0.0064)
Fraction of 2-Person-Households	-0.0260*** (0.0029)	-0.0241*** (0.0030)	-0.0217*** (0.0044)	0.0011 (0.0009)	0.0401** (0.0128)	0.0184 (0.0130)
Fit statistics						
Observations	63,380	63,380	63,380	63,380	63,380	63,380
Adjusted R ²	0.51105	0.27980	0.35351	0.60877	0.21835	0.26893

Standard errors (in parenthesis) are clustered on the two-digit zip code.
Signif. Codes: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C.2: Estimation results from Equation (4) for the outcome variables: wholesale and retail prices, markups, Lerner Indices, sales and gross profits. *Note:* The degree of urbanization is aggregated by area-weights

Dependent Variable:	Negative CPT	Positive CPT	Negative CPT	Positive CPT
	Short-Run		Long-Run	
	(1)	(2)	(3)	(4)
Constant	0.2212 (0.1466)	0.6181*** (0.1587)	1.091*** (0.1635)	1.276*** (0.1196)
Intermediate	-0.0021 (0.0078)	-0.0102 (0.0085)	0.0180* (0.0072)	0.0094+ (0.0052)
Rural	-0.0126 (0.0097)	-0.0189+ (0.0111)	0.0347*** (0.0093)	0.0268*** (0.0068)
Intermediate $\times$ OTC drugs	-0.0143* (0.0057)	-0.0084 (0.0085)	-0.0081 (0.0072)	0.0035 (0.0045)
Rural $\times$ OTC drugs	-0.0117 (0.0076)	-0.0039 (0.0122)	-0.0226** (0.0080)	-0.0120* (0.0055)
OTC drugs	-0.0917*** (0.0035)	-0.0332*** (0.0058)	0.0596*** (0.0062)	0.0996*** (0.0037)
year = 2019	0.0157*** (0.0034)	0.0380*** (0.0034)	0.0051+ (0.0027)	-0.0011 (0.0021)
year = 2020	0.0206*** (0.0027)	0.1105*** (0.0029)	0.0307*** (0.0025)	0.0078*** (0.0017)
year = 2021	0.0350*** (0.0038)	0.1457*** (0.0042)	0.0113*** (0.0032)	-0.0080** (0.0027)
year = 2022(H1)	0.0475*** (0.0046)	0.1413*** (0.0044)	0.0338*** (0.0035)	0.0055+ (0.0030)
Income per Person (in 10T)	-0.0359** (0.0113)	-0.0277* (0.0125)	0.0268* (0.0110)	0.0225* (0.0086)
Fraction of Population ≥ 65	0.0019 (0.0013)	0.0016 (0.0016)	-0.0048** (0.0014)	-0.0027+ (0.0014)
Fraction of Population < 18	-0.0002 (0.0032)	-0.0072* (0.0030)	0.0028 (0.0026)	-0.0011 (0.0023)
Fraction of 1-Person-Households	-0.0002 (0.0012)	-0.0030* (0.0013)	0.0027* (0.0013)	0.0012 (0.0009)
Fraction of 2-Person-Households	0.0016 (0.0020)	-0.0017 (0.0025)	0.0040 (0.0025)	0.0013 (0.0019)
Fit statistics				
Observations	57,245	57,380	57,245	57,380
Adjusted R ²	0.06981	0.07758	0.03135	0.16475

Standard errors (in parenthesis) are clustered on the two-digit zip code.
Signif. Codes: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C.3: Estimation results from Equation (4): Determinants of CPT. *Note:* Outcome variables include short-run positive (ρ_{fy}^+) and negative (ρ_{fy}^-) pass-through coefficients, and the long-run pass-through rate ($\theta_{fy}^+ -$ and θ_{fy}^+). The degree of urbanization is aggregated by area-weights.

D Additional Descriptive Statistics

This appendix complements the empirical analysis by providing granular descriptive evidence on market structure (e.g., Figure D.1) and data composition. It details the classification of therapeutic categories (ATC mappings, Table D.1) and reports the aggregate distribution of sales volume and revenue across these segments (Tables D.2 and D.3). Furthermore, it presents

supplementary visualizations of pharmacy profitability, transaction patterns (including bundle purchases), and the spatial distribution of urbanization used to define local markets (Table D.4 and Figures D.2 to D.9).

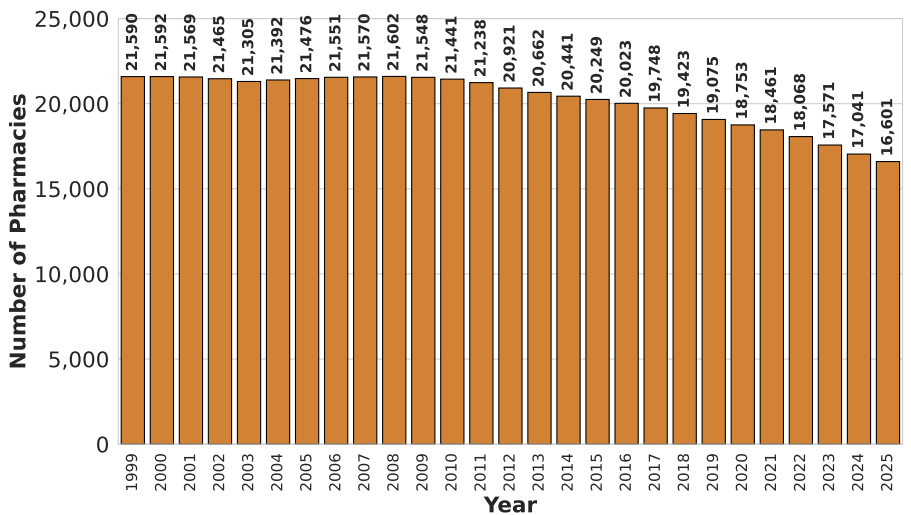


Figure D.1: Evolution of the number of pharmacies in Germany from 1999 to 2025. Sources: ABDA (2024) and Statista (2024b) and <https://www.abda.de/aktuelles-und-presse/pressemitteilungen/detail/2025-apothekezahl-sinkt-auf-16601-betriebsstaetten/>.

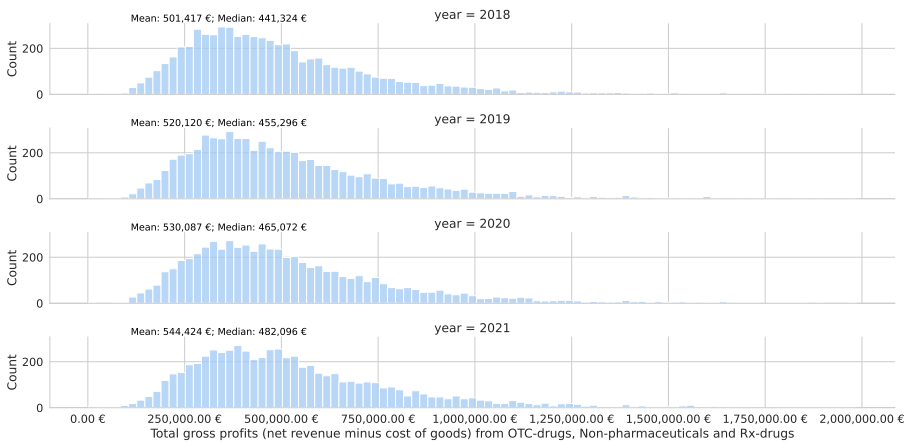


Figure D.2: Distribution of total pharmacy remuneration (gross profit). Note: The distribution represents the aggregated gross profit across all segments.

Therapeutic System Category	ATC2 Codes
Alimentary Tract and Metabolism	A01—A10, A13—A16
Vitamins and Minerals	A11, A12
Cardiovascular and Blood System	B01—B06, C01—C10
Dermatological Preparations	D01—D11
Genito-Urinary System and Sex Hormones	G01—G04
Systemic Hormonal Preparations	H01—H05
Hospital Solutions	K01, K04, K05, K07
Antineoplastics and Immunomodulating Agents	L01—L04
Musculoskeletal System and Pain Management	M01—M05, M09, N02
Nervous System	N01, N03—N07
Antiparasitics, Insecticides and Repellents	P01—P03
Respiratory System	R01—R07
Sensory Organs	S01—S03
Diagnostic Agents	T02, T03, V04, V08, V09
Other Therapeutic Products	V03
Antiseptics for Non-Human Use	V05
General and Nutritional Products	V06
Other Non-Therapeutic Products	V07
Therapeutic Radiopharmaceuticals	V10
Surgical Products	V20
Homeopathic and Anthroposophic Preparations	V60

Table D.1: Mapping of ATC2 codes to broad therapeutic system categories used in the analysis.

Product Type	Drug Market	Sales in Packages in Mio.	Fraction of Sales	
			Within in %	Overall in %
OTC-drugs	Alimentary Tract and Metabolism	98.763	14.231	8.203
	Antineoplastics and Immunomodulating Agents	1.149	0.166	0.095
	Antiparasitics, Insecticides and Repellents	1.032	0.149	0.086
	Cardiovascular and Blood System	29.500	4.251	2.450
	Dermatological Preparations	71.661	10.325	5.952
	Diagnostic Agents	0.092	0.013	0.008
	General and Nutritional Products	0.138	0.020	0.011
	Genito-Urinary System and Sex Hormones	14.394	2.074	1.196
	Homeopathic and Anthroposophic Preparations	21.954	3.163	1.823
	Hospital Solutions	0.080	0.012	0.007
	Musculoskeletal System and Pain Management	161.476	23.267	13.412
	Nervous System	23.587	3.399	1.959
	No ATC Code	1.264	0.182	0.105
	Other Non-Therapeutic Products	1.948	0.281	0.162
	Other Therapeutic Products	0.126	0.018	0.010
	Respiratory System	224.386	32.331	18.637
	Sensory Organs	23.487	3.384	1.951
	Systemic Hormonal Preparations	1.602	0.231	0.133
	Vitamins and Minerals	17.385	2.505	1.444
Non-pharmaceuticals	Alimentary Tract and Metabolism	21.111	4.140	1.753
	Antineoplastics and Immunomodulating Agents	0.003	0.001	0.000
	Antiparasitics, Insecticides and Repellents	1.131	0.222	0.094
	Antiseptics for Non-Human Use	0.126	0.025	0.010
	Cardiovascular and Blood System	5.462	1.071	0.454
	Dermatological Preparations	30.328	5.947	2.519
	Diagnostic Agents	7.055	1.383	0.586
	General and Nutritional Products	27.334	5.360	2.270
	Genito-Urinary System and Sex Hormones	7.527	1.476	0.625
	Homeopathic and Anthroposophic Preparations	0.013	0.003	0.001
	Hospital Solutions	0.004	0.001	0.000
	Musculoskeletal System and Pain Management	6.846	1.342	0.569
	Nervous System	5.359	1.051	0.445
	No ATC Code	304.387	59.689	25.282
	Other Non-Therapeutic Products	33.292	6.528	2.765
	Other Therapeutic Products	0.781	0.153	0.065
	Respiratory System	35.812	7.023	2.974
	Sensory Organs	21.767	4.268	1.808
	Surgical Products	0.020	0.004	0.002
	Systemic Hormonal Preparations	0.000	0.000	0.000
	Vitamins and Minerals	1.599	0.314	0.133

Table D.2: Aggregate market composition by therapeutic category. The table reports the total accumulated sales volume (in millions of packages) for each mapped category (see Table D.1) over the full sample period (2018–2021). The rightmost columns display the relative market share of each category within the total unregulated market.

Product Type	Drug Market	Revenue	Fraction of Revenue	
		in Euro in Mio.	Within in %	Overall in %
OTC-drugs	Alimentary Tract and Metabolism	848.991	15.516	8.536
	Antineoplastics and Immunomodulating Agents	18.581	0.340	0.187
	Antiparasitics, Insecticides and Repellents	18.599	0.340	0.187
	Cardiovascular and Blood System	230.665	4.215	2.319
	Dermatological Preparations	621.685	11.362	6.250
	Diagnostic Agents	4.811	0.088	0.048
	General and Nutritional Products	4.976	0.091	0.050
	Genito-Urinary System and Sex Hormones	206.323	3.771	2.074
	Homeopathic and Anthroposophic Preparations	207.271	3.788	2.084
	Hospital Solutions	0.367	0.007	0.004
	Musculoskeletal System and Pain Management	1,049.568	19.181	10.552
	Nervous System	388.029	7.091	3.901
	No ATC Code	27.603	0.504	0.278
	Other Non-Therapeutic Products	12.181	0.223	0.122
	Other Therapeutic Products	1.939	0.035	0.019
	Respiratory System	1,467.562	26.820	14.755
	Sensory Organs	163.641	2.991	1.645
	Systemic Hormonal Preparations	9.367	0.171	0.094
	Vitamins and Minerals	189.672	3.466	1.907
Non-pharmaceuticals	Alimentary Tract and Metabolism	187.946	4.200	1.890
	Antineoplastics and Immunomodulating Agents	0.031	0.001	0.000
	Antiparasitics, Insecticides and Repellents	16.294	0.364	0.164
	Antiseptics for Non-Human Use	1.191	0.027	0.012
	Cardiovascular and Blood System	93.464	2.089	0.940
	Dermatological Preparations	204.164	4.563	2.053
	Diagnostic Agents	102.192	2.284	1.027
	General and Nutritional Products	518.248	11.582	5.210
	Genito-Urinary System and Sex Hormones	79.632	1.780	0.801
	Homeopathic and Anthroposophic Preparations	0.082	0.002	0.001
	Hospital Solutions	0.092	0.002	0.001
	Musculoskeletal System and Pain Management	58.893	1.316	0.592
	Nervous System	47.147	1.054	0.474
	No ATC Code	2,383.972	53.279	23.968
	Other Non-Therapeutic Products	301.650	6.742	3.033
	Other Therapeutic Products	6.038	0.135	0.061
	Respiratory System	214.450	4.793	2.156
	Sensory Organs	243.438	5.441	2.448
	Surgical Products	1.189	0.027	0.012
	Systemic Hormonal Preparations	0.002	0.000	0.000
	Vitamins and Minerals	14.383	0.321	0.145

Table D.3: Aggregate market composition by therapeutic category. The table reports the total accumulated revenue (in millions of Euro) for each mapped category (see Table D.1) over the full sample period (2018–2021). The rightmost columns display the relative market share of each category within the total unregulated market.

year	Sales Rx-drugs			Sales OTC-drugs & Non-pharmaceuticals			Total Sales		
	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.
2018	47,081.03	42,081	23,911.47	46,888.10	37,397	36,070.82	93,969.13	81,827	53,370.04
2019	47,948.88	42,730	24,534.82	46,347.30	36,841	35,711.38	94,296.17	81,893	53,438.76
2020	47,065.53	42,149	23,846.30	43,791.96	36,029	30,430.21	90,857.49	80,354	48,433.17
2021	48,150.86	43,401	24,025.71	43,702.82	36,067	30,201.34	91,853.68	81,806	48,562.57

(a) Rx- vs. OTC-drug & non-pharmaceutical sales

year	Revenue Rx-drugs			Revenue OTC-drugs & Non-pharmaceuticals			Total Revenue		
	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.
2018	2,641,471.18 €	2,268,976 €	1,670,603.45 €	430,733.16 €	338,304 €	349,899.24 €	3,072,204.34 €	2,657,181 €	1,850,619.18 €
2019	2,817,688.19 €	2,397,699 €	1,810,110.22 €	436,725.19 €	340,879 €	357,250.92 €	3,254,413.38 €	2,794,304 €	1,989,201.52 €
2020	2,958,589.17 €	2,508,114 €	2,041,277.60 €	426,310.39 €	343,027 €	318,571.84 €	3,384,899.57 €	2,893,897 €	2,192,510.82 €
2021	3,253,794.31 €	2,763,038 €	2,425,588.67 €	438,685.79 €	354,983 €	331,880.57 €	3,692,480.10 €	3,156,138 €	2,572,515.15 €

(b) Rx- vs. OTC-drug & non-pharmaceutical revenue

year	Gross Profits Rx-drugs			Gross Profits OTC-drugs & Non-pharmaceuticals			Total Gross Profits		
	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.
2018	368,935.48 €	325,200 €	199,211.51 €	132,481.31 €	103,848 €	108,223.74 €	501,416.79 €	441,324 €	270,792.97 €
2019	384,114.85 €	336,351 €	212,572.20 €	136,005.15 €	106,204 €	111,277.58 €	520,120.00 €	455,296 €	284,697.91 €
2020	394,646.85 €	344,672 €	302,601.62 €	135,439.84 €	108,950 €	114,994.23 €	530,086.68 €	465,072 €	356,220.90 €
2021	407,800.05 €	360,392 €	251,196.59 €	136,624.16 €	109,703 €	135,778.06 €	544,424.21 €	482,096 €	324,405.50 €

(c) Rx- vs. OTC-drug & non-pharmaceutical gross profits

year	Sales to Total Sales			Revenue to Total Revenue			Gross Profits to Total Gross Profits		
	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.
2018	47.94 %	46.60 %	10.62 %	14.52 %	12.83 %	7.62 %	25.75 %	23.83 %	10.25 %
2019	47.24 %	45.93 %	10.64 %	13.98 %	12.32 %	7.53 %	25.55 %	23.65 %	10.25 %
2020	46.85 %	45.59 %	10.33 %	13.42 %	11.98 %	7.12 %	25.53 %	23.87 %	9.88 %
2021	46.20 %	44.91 %	10.03 %	12.65 %	11.30 %	6.70 %	24.74 %	22.98 %	9.57 %

(d) Fraction of OTC drug & non-pharmaceutical sales, revenues and gross profits compared to their total values (including Rx drugs), respectively.

Table D.4: Descriptive statistics for the evolution of sales, revenue and gross profits per year.

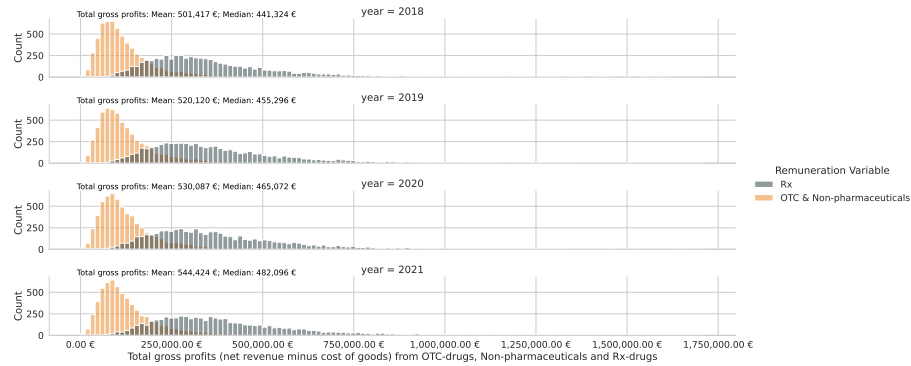


Figure D.3: Distribution of total gross profits by Rx drugs, OTC drugs and non-pharmaceuticals.

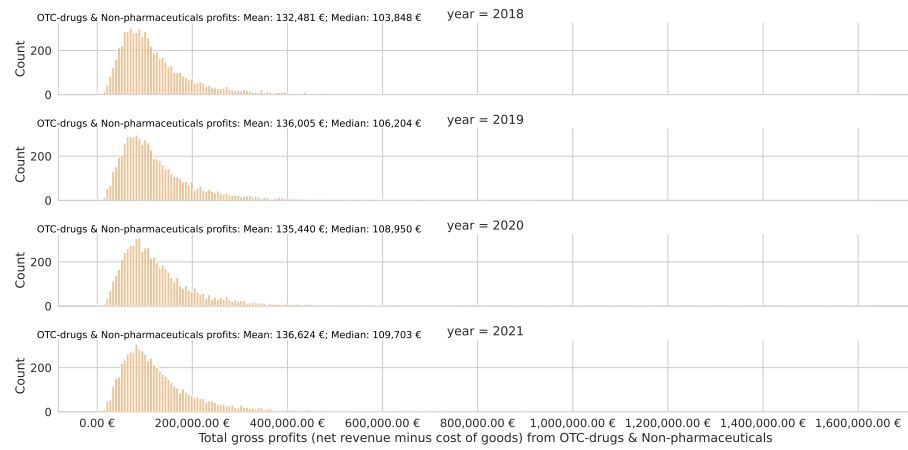


Figure D.4: Distribution of gross profits from OTC drug and non-pharmaceuticals.

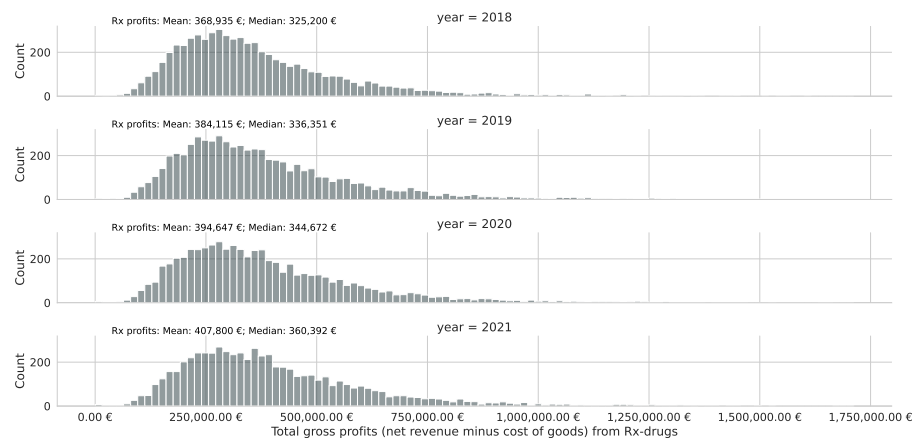


Figure D.5: Distribution of gross profits from Rx drugs.

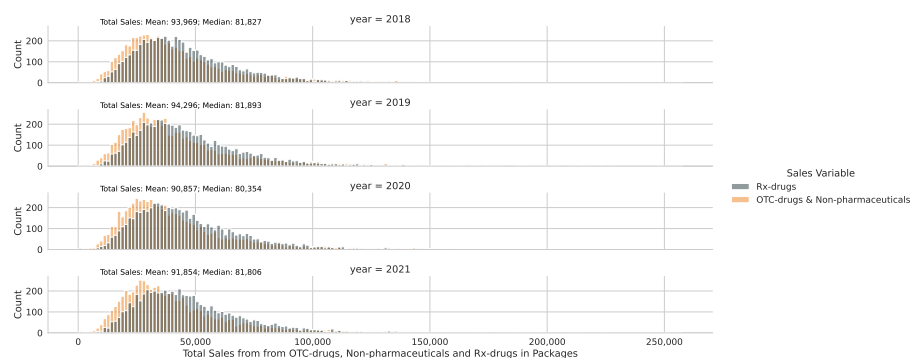


Figure D.6: Distribution of total pharmacy sales by Rx drugs, OTC drugs and non-pharmaceuticals.

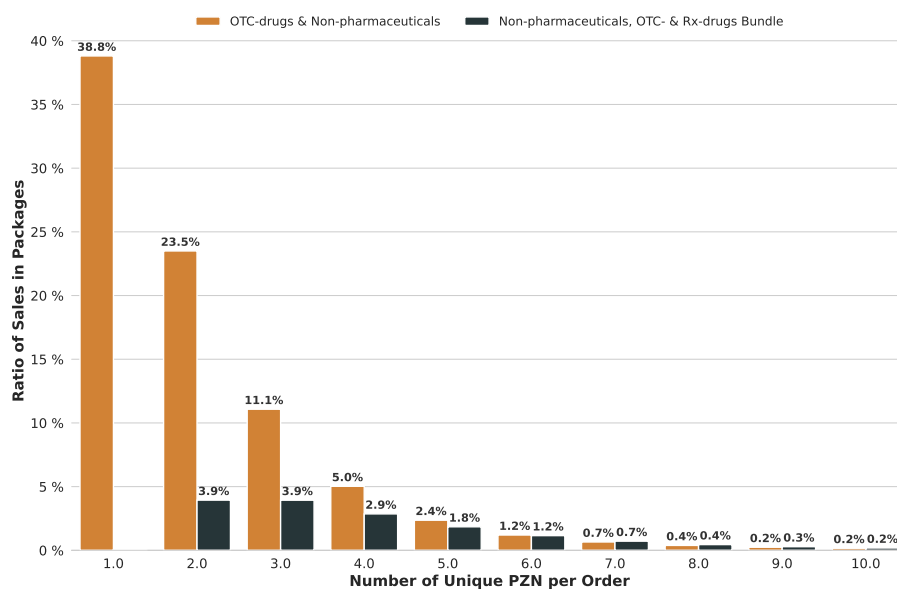


Figure D.7: Sales ratio according to order size and bundle type, focusing on sales containing at least one unique PCN of OTC drugs or non-pharmaceuticals.

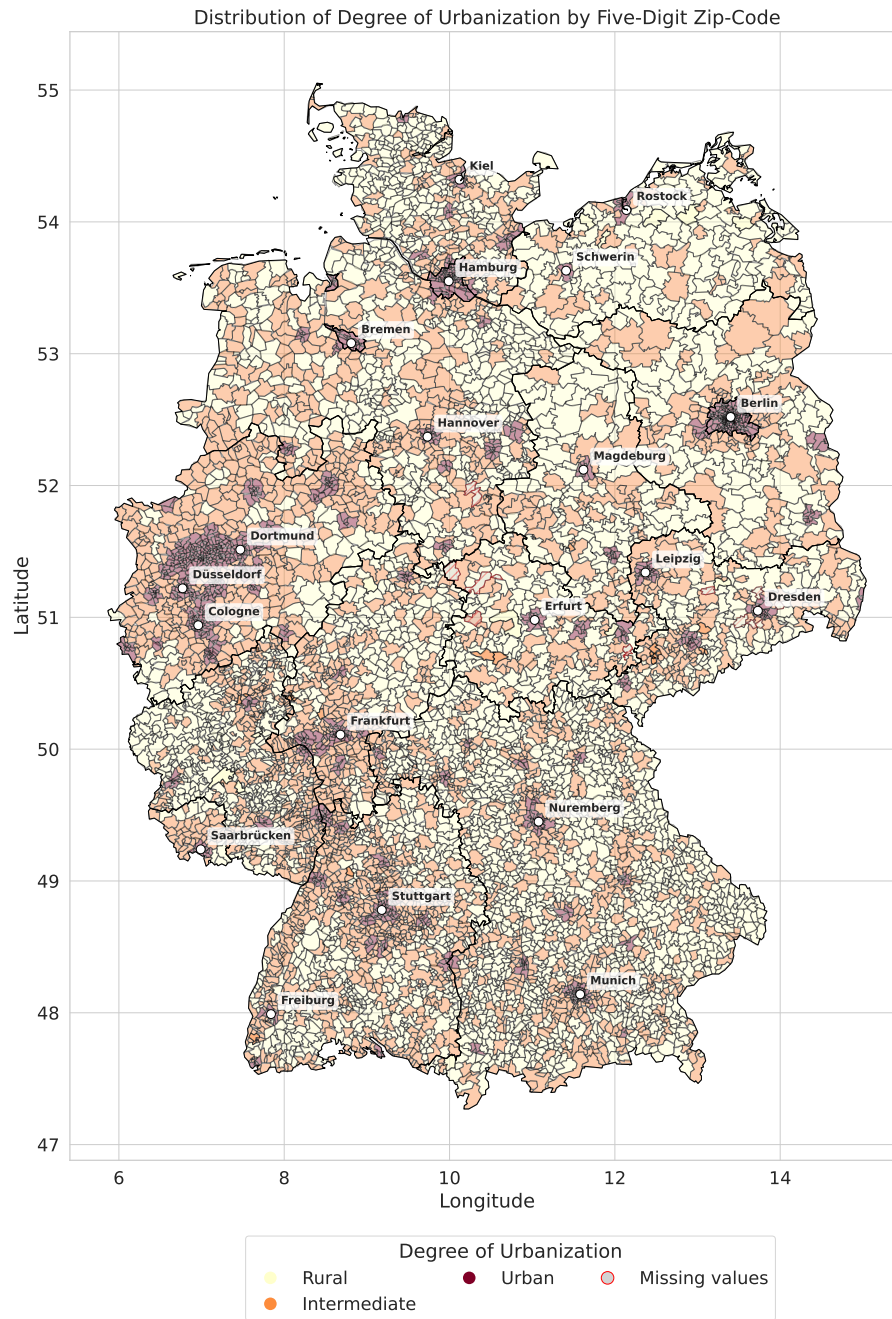


Figure D.8: Degree of urbanization by five-digit zip-code.

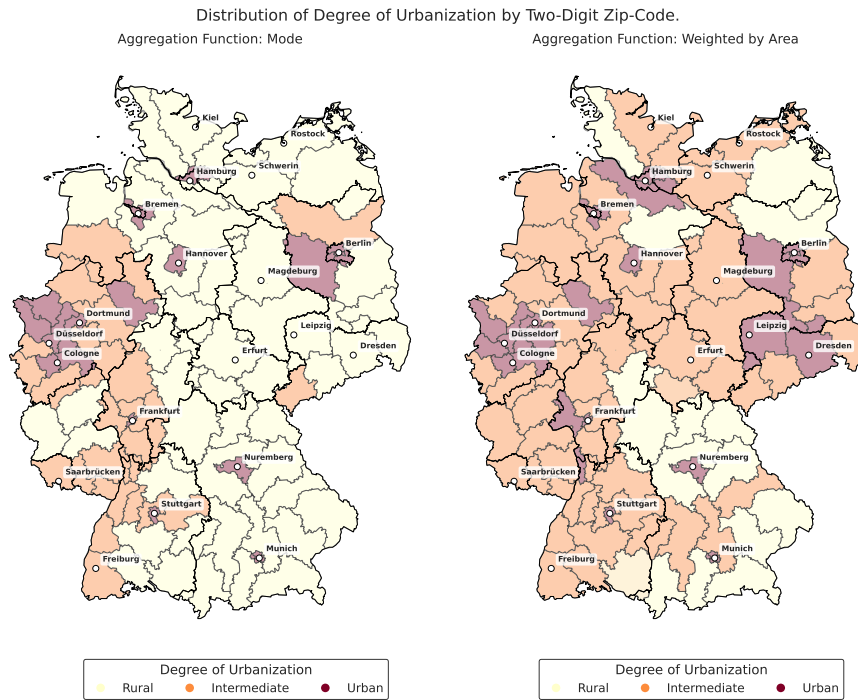


Figure D.9: Degree of urbanization aggregated at the two-digit zip-code. Left panel: Aggregation by mode. Right panel: Aggregation by area.

E Additional Regression Outputs

This appendix presents the full suite of econometric results and robustness checks supporting the main findings. It includes panel unit root tests (Figure E.1) validating the model's stationarity assumptions (i.e., a part is $I(0)$, but all are at least $I(1)$) and reports the complete estimation outputs for the dynamic ECM with extended lag structures (Table E.2). Additionally, it provides disaggregated pass-through estimates for top-ranked product hierarchies (bestsellers vs. niche products, Tables E.3 and E.4).

Test	Integration Order	Global Stat	Total Units	Units p-value below 1%	% Stationary
Consumer Net Price					
IPS (W_{tbar})	Levels - I(0)	-573.7	8859	6225	70.27%
Maddala-Wu (χ^2)	Levels - I(0)	384434.6	8859	6225	70.27%
modified P test (P_m)	Levels - I(0)	1948.1	8859	6225	70.27%
inverse normal test (z-value)	Levels - I(0)	-451.9	8859	6225	70.27%
Logit (L^*)	Levels - I(0)	-1117.6	8859	6225	70.27%
IPS (W_{tbar})	First Diff. - I(1)	1672327.1	8859	8859	100%
Maddala-Wu (χ^2)	First Diff. - I(1)	-3347.6	8859	8859	100%
modified P test (P_m)	First Diff - I(1)	8789.7	8859	8859	100%
inverse normal test (z-value)	First Diff - I(1)	-1258.1	8859	8859	100%
Logit (L^*)	First Diff - I(1)	-4898.0	8859	8859	100%
Wholesale Price (AEP)					
IPS (W_{tbar})	Levels - I(0)	-258.7	8859	4720	53.28%
Maddala-Wu (χ^2)	Levels - I(0)	145841.5	8859	4720	53.28%
modified P test (P_m)	Levels - I(0)	680.6	8859	4720	53.28%
inverse normal test (z-value)	Levels - I(0)	-256.5	8859	4720	53.28%
Logit (L^*)	Levels - I(0)	-413.8	8859	4720	53.28%
IPS (W_{tbar})	First Diff. - I(1)	-2946.6	8859	8859	100%
Maddala-Wu (χ^2)	First Diff. - I(1)	1847641.9	8859	8859	100%
modified P test (P_m)	First Diff - I(1)	9721.0	8859	8859	100%
inverse normal test (z-value)	First Diff - I(1)	-1330.0	8859	8859	100%
Logit (L^*)	First Diff - I(1)	-5411.5	8859	8859	100%

Table E.1: This table reports the results of the panel unit root tests proposed by Im et al. (2003) (IPS), Maddala and Wu (1999) (Maddala-Wu) and Choi (2001) (modified P test, inverse normal test and logit) on units (pharmacy-product ids) of the data that exhibit more than 3 years of observations. The tests are performed on both levels and first-differences of the variables to determine their order of integration. The *Global Stat* represents the W_{tbar} for the IPS test, the χ^2 statistic for the Maddala-Wu test, the P_m , z and L^* statistic for three tests from Choi, all of which strongly reject the null hypothesis of a unit root across the overall panel ($p < 0.01$). To address potential panel heterogeneity, the number and percentage of individual pharmacy-product time series are reported that independently reject the null hypothesis of a unit root at the strict 1 % significance level. The results confirm that while the panel exhibits mixed $I(0)$ and $I(1)$ behavior in levels, all series are stationary in first-differences ($I(1)$), thereby ruling out $I(2)$ non-stationarity.

Dependent Variable:	Δp_{jft}					Non-Pharmaceuticals				
	OTC-drugs									
Coefficients & Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$\rho^- \Delta w_{jft}$	0.0772*** (0.0092)	0.0656*** (0.0067)	0.0643*** (0.0064)	0.0645*** (0.0063)	0.0645*** (0.0062)	0.2226*** (0.0300)	0.1948*** (0.0203)	0.1935*** (0.0162)	0.1980*** (0.0167)	0.1976*** (0.0167)
$\rho^+ \Delta w_{jft}$	0.1882*** (0.0088)	0.1405*** (0.0081)	0.1359*** (0.0078)	0.1343*** (0.0077)	0.1333*** (0.0076)	0.4481*** (0.0749)	0.4176*** (0.0748)	0.4137*** (0.0736)	0.4088*** (0.0714)	0.4044*** (0.0689)
$\gamma^- p_{jft-t-\tau}$	-0.0930*** (0.0062)	-0.0529*** (0.0039)	-0.0474*** (0.0035)	-0.0452*** (0.0034)	-0.0440*** (0.0033)	-0.1298*** (0.0218)	-0.0703*** (0.0140)	-0.0571*** (0.0123)	-0.0514*** (0.0114)	-0.0476*** (0.0109)
$\gamma^+ p_{jft-t-\tau}$	-0.1151*** (0.0057)	-0.0728*** (0.0036)	-0.0668*** (0.0033)	-0.0643*** (0.0031)	-0.0628*** (0.0030)	-0.1622*** (0.0363)	-0.1057*** (0.0290)	-0.0920*** (0.0277)	-0.0850*** (0.0268)	-0.0810*** (0.0262)
$\lambda^- w_{jft-t-\tau}$	0.1470*** (0.0110)	0.0825*** (0.0065)	0.0737*** (0.0059)	0.0701*** (0.0057)	0.0682*** (0.0055)	0.1830*** (0.0315)	0.0984*** (0.0201)	0.0798*** (0.0175)	0.0717*** (0.0164)	0.0664*** (0.0156)
$\lambda^+ w_{jft-t-\tau}$	0.1819*** (0.0101)	0.1142*** (0.0061)	0.1050*** (0.0056)	0.1010*** (0.0054)	0.0988*** (0.0052)	0.2158*** (0.0494)	0.1361*** (0.0391)	0.1168*** (0.0374)	0.1070*** (0.0362)	0.1015*** (0.0355)
$\omega \Delta \text{Days}_{jft}$	0.0005*** (2.41×10^{-5})	0.0005*** (2.32×10^{-5})	0.0005*** (2.36×10^{-5})	0.0005*** (2.39×10^{-5})	0.0005*** (2.41×10^{-5})	0.0004*** (2.41×10^{-5})	0.0004*** (2.08×10^{-5})	0.0004*** (1.97×10^{-5})	0.0004*** (1.91×10^{-5})	0.0004*** (1.86×10^{-5})
$\beta_1^- \Delta w_{jft-1}$		0.0386*** (0.0030)	0.0392*** (0.0031)	0.0406*** (0.0031)	0.0414*** (0.0031)		0.0955*** (0.0089)	0.1067*** (0.0105)	0.1192*** (0.0124)	0.1261*** (0.0136)
$\beta_1^+ \Delta w_{jft-1}$		0.0026 (0.0019)	0.0150*** (0.0021)	0.0196*** (0.0022)	0.0215*** (0.0023)		0.0982*** (0.0235)	0.1470*** (0.0309)	0.1609*** (0.0305)	0.1635*** (0.0294)
$\beta_2^- \Delta w_{jft-2}$			0.0255*** (0.0021)	0.0275*** (0.0022)	0.0286*** (0.0023)			0.0514*** (0.0066)	0.0651*** (0.0078)	0.0748*** (0.0107)
$\beta_2^+ \Delta w_{jft-2}$			0.0007 (0.0022)	0.0080*** (0.0020)	0.0111*** (0.0021)			0.0364* (0.0165)	0.0727** (0.0223)	0.0830*** (0.0261)
$\beta_3^- \Delta w_{jft-3}$				0.0205*** (0.0019)	0.0218*** (0.0020)				0.0467*** (0.0064)	0.0550*** (0.0096)
$\beta_3^+ \Delta w_{jft-3}$				0.0041* (0.0013)	0.0088*** (0.0014)				0.0301* (0.0131)	0.0551*** (0.0169)
$\beta_4^- \Delta w_{jft-4}$					0.0157*** (0.0017)					0.0455*** (0.0066)
$\beta_4^+ \Delta w_{jft-4}$					0.0014 (0.0013)					0.0096 (0.0250)
$\phi_1^- \Delta p_{jft-1}$		-0.6667*** (0.0146)	-0.6795*** (0.0140)	-0.6876*** (0.0140)	-0.6905*** (0.0139)		-0.6886*** (0.0187)	-0.7353*** (0.0182)	-0.7543*** (0.0182)	-0.7633*** (0.0180)
$\phi_1^+ \Delta p_{jft-1}$		-0.1173*** (0.0078)	-0.2560*** (0.0095)	-0.2765*** (0.0108)	-0.2843*** (0.0114)		-0.1877*** (0.0165)	-0.3198*** (0.0195)	-0.3551*** (0.0220)	-0.3741*** (0.0238)
$\phi_2^- \Delta p_{jft-2}$			-0.2184*** (0.0080)	-0.2382*** (0.0094)	-0.2482*** (0.0102)			-0.2985*** (0.0139)	-0.3526*** (0.0139)	-0.3787*** (0.0154)
$\phi_2^+ \Delta p_{jft-2}$			-0.0815*** (0.0048)	-0.1608*** (0.0067)	-0.1757*** (0.0078)			-0.0940** (0.0304)	-0.1790*** (0.0387)	-0.2086*** (0.0429)
$\phi_3^- \Delta p_{jft-3}$				-0.1217*** (0.0059)	-0.1366*** (0.0070)				-0.1733*** (0.0166)	-0.2148*** (0.0202)
$\phi_3^+ \Delta p_{jft-3}$				-0.0471*** (0.0035)	-0.0976*** (0.0050)				-0.0872*** (0.0072)	-0.1501*** (0.0121)
$\phi_4^- \Delta p_{jft-4}$					-0.0755*** (0.0046)					-0.1282*** (0.0114)
$\phi_4^+ \Delta p_{jft-4}$					-0.0378*** (0.0027)					-0.0647*** (0.0072)
Fit statistics										
# PCN-Pharmacy ID	2,491,778	2,491,778	2,491,778	2,491,778	2,491,778	6,078,232	6,078,232	6,078,232	6,078,232	6,078,232
# Year-Days	1,637	1,637	1,637	1,637	1,637	1,637	1,637	1,637	1,637	1,637
# Pharmacy-ID $\times$ Year-Month	341,772	341,772	341,772	341,772	341,772	341,307	341,307	341,307	341,307	341,307
Observations	231,400,516	231,400,516	231,400,516	231,400,516	231,400,516	172,288,976	172,288,976	172,288,976	172,288,976	172,288,976
Adjusted R ²	0.03062	0.23224	0.24879	0.25385	0.25603	0.13450	0.31951	0.34682	0.35781	0.36392
Within Adjusted R ²	0.01031	0.21615	0.23305	0.23821	0.24044	0.03726	0.24306	0.27343	0.28566	0.29245
Long-run Pass-Through θ^+	1.580	1.569	1.572	1.572	1.572	1.330	1.287	1.270	1.259	1.254
Long-run Pass-Through θ^-	1.581	1.560	1.554	1.552	1.550	1.410	1.401	1.398	1.396	1.395
Z-test $\theta^+ = \theta^-$	-0.119	0.630	1.151	1.288	1.350	-3.386***	-3.096**	-2.791**	-2.754**	-2.729**
K Lags	0	1	2	3	4	0	1	2	3	4
AIC	377,566,880.9	314,690,484.8	308,763,817.1	306,907,315.1	306,142,571.7	429,071,416.9	387,977,778.5	379,255,256.0	375,814,942.5	374,094,333.3
BIC	426,501,332.6	363,625,005.6	357,698,406.8	355,841,973.8	355,077,299.6	538,004,721.7	496,911,151.1	488,188,696.5	484,748,450.8	483,027,909.5
LogLik	-185,948,248.5	-154,510,046.4	-151,546,708.5	-150,618,453.5	-150,236,077.9	-208,114,527.4	-187,567,704.2	-183,206,439.0	-181,486,278.2	-180,625,969.7
Wald test $\rho^+ = \rho^-$	114.768***	69.185***	69.116***	68.646***	68.589***	6.796**	7.670**	8.517**	8.476**	8.880**
Wald test $\gamma^+ = \gamma^-$	83.788***	108.474***	107.367***	110.624***	109.617***	1.754	2.758 ⁺	2.943 ⁺	2.867 ⁺	3.049 ⁺
Wald test $\lambda^+ = \lambda^-$	70.682***	70.682***	70.682***	70.682***	70.682***	70.682***	70.682***	70.682***	70.682***	70.682***
Wald test $\gamma^+ = \gamma^- = \lambda^+ = \lambda^- = 0$	140.523***	129.992***	130.068***	132.341***	134.152***	10.738***	9.343***	8.532***	9.243***	9.191***
Fixed Effects (FEs)										
PCN-Pharmacy ID FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year-Days FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Pharmacy-ID $\times$ Year-Month FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Standard errors (in parenthesis) are clustered on the pharmacy id & PCN. Wald tests show F-statistics
Signif. Codes: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table E.2: Dynamic asymmetric estimation results (Equation (1) with up to $K = 4$ lags): Short-run and long-run pass-through estimates for high-turnover OTC drugs (*Respiratory System, Musculoskeletal System and Pain Management*) and non-pharmaceuticals (*No ATC Code*).

Dependent Variable:	(1)	(2)	(3)	(4)	Δp_{jft} (5)	(6)	(7)	(8)	(9)
$\rho^- \Delta w_{jft}$	0.0659** (0.0138)	0.1054*** (0.0108)	0.1219*** (0.0186)	0.1403*** (0.0279)	0.0915*** (0.0138)	0.1208*** (0.0142)	0.1265** (0.0313)	0.0994*** (0.0120)	0.1209*** (0.0144)
$\rho^+ \Delta w_{jft}$	0.0689* (0.0226)	0.1156*** (0.0088)	0.2135*** (0.0146)	0.0963** (0.0206)	0.1423*** (0.0116)	0.2205*** (0.0121)	0.1034** (0.0219)	0.1367*** (0.0088)	0.2222*** (0.0123)
$\omega \Delta Days_{jft}$	0.0002 (0.0001)	0.0003*** (4.06×10^{-5})	0.0006*** (2.28×10^{-5})	5.19×10^{-5} (0.0002)	0.0006*** (7.67×10^{-5})	0.0007*** (2.75×10^{-5})	6.2×10^{-5} (0.0002)	0.0006*** (5.79×10^{-5})	0.0007*** (2.75×10^{-5})
$\gamma^- p_{jft-\tau}$	-0.0756*** (0.0155)	-0.0989*** (0.0051)	-0.1136*** (0.0052)	-0.1143*** (0.0114)	-0.1066*** (0.0086)	-0.1162*** (0.0044)	-0.1101*** (0.0134)	-0.1113*** (0.0056)	-0.1152*** (0.0045)
$\gamma^+ p_{jft-\tau}$	-0.0879*** (0.0146)	-0.1076*** (0.0047)	-0.1408*** (0.0072)	-0.1209*** (0.0115)	-0.1205*** (0.0077)	-0.1441*** (0.0059)	-0.1172*** (0.0128)	-0.1209*** (0.0055)	-0.1432*** (0.0060)
$\lambda^- w_{jft-\tau}$	0.1296*** (0.0258)	0.1619*** (0.0096)	0.1796*** (0.0092)	0.1781*** (0.0151)	0.1636*** (0.0143)	0.1785*** (0.0072)	0.1697*** (0.0180)	0.1771*** (0.0086)	0.1777*** (0.0073)
$\lambda^+ w_{jft-\tau}$	0.1470*** (0.0239)	0.1757*** (0.0085)	0.2226*** (0.0116)	0.1884*** (0.0152)	0.1875*** (0.0126)	0.2211*** (0.0089)	0.1814*** (0.0169)	0.1931*** (0.0085)	0.2205*** (0.0092)
Fit statistics									
# Pharmacy ID $\times$ PCN	62,652	604,653	4,586,395	62,564	616,835	4,557,057	62,496	615,238	4,583,496
# Year-Days	1,641	1,641	1,641	1,641	1,641	1,641	1,641	1,641	1,641
# Pharmacy ID $\times$ Year-Month	341,660	341,820	341,940	341,653	341,820	341,951	341,681	341,815	341,951
Observations	37,531,035	180,112,138	422,089,152	30,265,844	167,176,855	409,592,325	32,517,007	171,686,780	413,605,018
Adjusted R ²	0.03715	0.03413	0.02923	0.04239	0.02919	0.03591	0.04085	0.03031	0.03414
Within Adjusted R ²	0.00405	0.00672	0.01524	0.00753	0.00959	0.01793	0.00724	0.00869	0.01756
# Top PCN	10	100	1000	10	100	1000	10	100	1000
Top PZN by	Sales	Sales	Sales	Revenue	Revenue	Revenue	Profits	Profits	Profits
Fraction of Total in %	13.1520	44.2220	87.3466	9.2187	37.9892	83.1285	9.0191	38.7750	84.4188
Long-run Pass-Through θ^+	1.672	1.633	1.580	1.558	1.556	1.535	1.547	1.597	1.540
Long-run Pass-Through θ^-	1.715	1.636	1.581	1.557	1.534	1.536	1.542	1.591	1.542
Z-test $\theta^+ = \theta^-$	-1.794 ⁺	-0.435	-0.100	0.131	2.613**	-0.233	0.485	0.815	-0.417
Wald test $\rho^+ = \rho^-$	0.025	0.621	107.109***	3.521 ⁺	9.813**	140.860***	0.763	5.532*	139.894***
Wald test $\gamma^+ = \gamma^-$	20.606***	15.735***	43.765***	4.767*	32.525***	102.880***	4.015*	21.213***	95.340***
Wald test $\lambda^+ = \lambda^-$	21.404***	14.278***	49.337***	4.476*	34.539***	121.419***	4.235*	21.971***	110.915***
Wald test $\gamma^+ = \gamma^- = \lambda^+ = \lambda^- = 0$	31.039***	138.006***	195.660***	56.118***	91.221***	269.226***	49.640***	128.911***	248.821***
Fixed Effects (FEs)									
Pharmacy ID $\times$ PCN FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year-Days FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Pharmacy ID $\times$ Year-Month FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓

Standard errors (in parenthesis) are clustered on the pharmacy id & PCN. Wald tests show F-statistics.

Signif. Codes: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table E.3: OTC drugs: Top 10 vs. Top 100 and Top 1000. Full table of estimation results for top-ranked products by sales, revenue and gross profits.

Dependent Variable:	(1)	(2)	(3)	(4)	Δp_{jft} (5)	(6)	(7)	(8)	(9)
$\rho^- \Delta w_{jft}$	0.1235*** (0.0109)	0.1275*** (0.0127)	0.1824*** (0.0124)	0.1593** (0.0352)	0.1792*** (0.0184)	0.2098*** (0.0110)	0.1272** (0.0270)	0.1718*** (0.0193)	0.1982*** (0.0126)
$\rho^+ \Delta w_{jft}$	0.1317*** (0.0226)	0.2356*** (0.0236)	0.2704*** (0.0132)	0.2348*** (0.0267)	0.2629*** (0.0118)	0.2789*** (0.0077)	0.2364*** (0.0263)	0.2554*** (0.0110)	0.2500*** (0.0213)
$\omega \Delta Days_{jft}$	0.0002* (7.48×10^{-5})	0.0004*** (6.59×10^{-5})	0.0004*** (2.38×10^{-5})	0.0014* (0.0005)	0.0010*** (0.0001)	0.0006*** (3.02×10^{-5})	0.0014** (0.0004)	0.0010*** (0.0001)	0.0006*** (3.16×10^{-5})
$\gamma^- p_{jft-\tau}$	-0.1105*** (0.0155)	-0.1053*** (0.0090)	-0.1483*** (0.0093)	-0.1429*** (0.0260)	-0.1498*** (0.0153)	-0.1720*** (0.0108)	-0.1694*** (0.0174)	-0.1339*** (0.0142)	-0.1503*** (0.0154)
$\gamma^+ p_{jft-\tau}$	-0.1233*** (0.0197)	-0.1283*** (0.0080)	-0.1784*** (0.0095)	-0.1674*** (0.0288)	-0.1962*** (0.0138)	-0.2087*** (0.0070)	-0.1882*** (0.0196)	-0.1812*** (0.0135)	-0.1686*** (0.0257)
$\lambda^- w_{jft-\tau}$	0.1624*** (0.0231)	0.1418*** (0.0159)	0.2071*** (0.0146)	0.1839*** (0.0356)	0.1999*** (0.0227)	0.2365*** (0.0163)	0.2490*** (0.0256)	0.1795*** (0.0214)	0.2080*** (0.0220)
$\lambda^+ w_{jft-\tau}$	0.1828*** (0.0297)	0.1730*** (0.0146)	0.2471*** (0.0150)	0.2159*** (0.0382)	0.2633*** (0.0195)	0.2866*** (0.0108)	0.2777*** (0.0301)	0.2443*** (0.0191)	0.2328*** (0.0353)
Fit statistics									
# Pharmacy ID $\times$ PCN	61,419	561,720	3,771,609	61,190	564,193	3,709,785	61,973	569,763	3,712,649
# Year-Days	1,641	1,641	1,641	1,641	1,641	1,641	1,641	1,641	1,641
# Pharmacy ID $\times$ Year-Month	341,462	341,685	341,808	341,227	341,681	341,798	341,242	341,681	341,809
Observations	14,012,783	65,992,694	207,336,289	10,709,504	52,381,667	177,019,976	11,482,192	54,224,648	180,881,482
Adjusted R ²	0.04635	0.05226	0.07846	0.05001	0.07388	0.07572	0.04365	0.07525	0.09061
Within Adjusted R ²	0.01014	0.01699	0.02665	0.02277	0.02815	0.03322	0.02043	0.02578	0.02683
# Top PCN	10	100	1000	10	100	1000	10	100	1000
Top PZN by	Sales	Sales	Sales	Revenue	Revenue	Revenue	Profits	Profits	Profits
Fraction of Total in %	3.8819	16.8351	52.5124	4.2602	18.1514	50.9730	3.9915	51.2765	51.2765
Long-run Pass-Through θ^+	1.482	1.348	1.385	1.290	1.342	1.373	1.475	1.348	1.381
Long-run Pass-Through θ^-	1.470	1.347	1.397	1.287	1.334	1.375	1.470	1.341	1.384
Z-test $\theta^+ = \theta^-$	2.755**	0.155	-2.505*	0.298	1.443	-0.471	1.442	1.103	-0.601
Wald test $\rho^+ = \rho^-$	0.184	16.972***	28.569***	3.121+	33.704***	36.934***	7.360**	27.826***	8.534**
Wald test $\gamma^+ = \gamma^-$	6.140*	53.663***	164.596***	25.788***	9.381**	18.306***	9.673**	10.432**	1.310
Wald test $\lambda^+ = \lambda^-$	6.618*	62.560***	156.984***	31.610***	9.660**	17.095***	9.469**	10.525**	1.253
Wald test $\gamma^+ = \gamma^- = \lambda^+ = \lambda^- = 0$	19.062***	137.082***	139.536***	39.520***	69.139***	264.872***	40.470***	59.679***	36.030***
Fixed Effects (FEs)									
Pharmacy ID $\times$ PCN FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year-Days FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Pharmacy ID $\times$ Year-Month FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓

Standard errors (in parentheses) are clustered on the pharmacy ID and PCN. Wald tests show F-statistics.
Signif. Codes: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table E.4: Non-pharmaceuticals: Top 10 vs. Top 100 and Top 1000. Full table of estimation results for top-ranked products by sales, revenue and gross profits.

3 Retail Markets

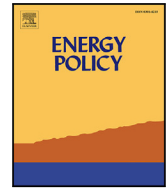
3.1 Pass-through of temporary fuel tax reductions: Evidence from Europe

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Pass-through of temporary fuel tax reductions: Evidence from Europe

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ABSTRACT

Several European countries have implemented temporary fuel tax reductions in 2022 to relieve the financial burden on their citizens. This paper is the first to provide estimates of the pass-through rates as well as the effect on retail margins for France and Italy. Further, it contributes to the recent literature on the fuel tax reduction in Germany. Using a unique data set containing daily consumer prices at service station chain level for gasoline and diesel, we employ a staggered Difference-in-Differences (DiD) design. Our main results imply that in the aggregate there was a full-shifting of the fuel tax reductions in all three countries. Nevertheless, in an event study design we find that the pass-through rates over time are heterogeneous between the countries and types of fuel. Depending on time, heterogeneous effects imply a full-shifting up to a minor over-shifting of the pass-through rates. These findings also have important implications for the effective design of unconventional fiscal policy as well as for competition policy in the fuel market.

1. Introduction

More than two years after the beginning of the COVID-19 pandemic many countries worldwide exhibited very high inflation rates. The reasons are a recovering demand in combination with ongoing supply chain problems as well as the war of aggression in the Ukraine. In April 2022, the inflation rate of Germany reached 7.4%, the highest rate since 1981. Other western countries have similar rates: the inflation rate of the whole EU has been 8.1%, the US even had a rate of 8.3% in April 2022. In autumn 2022, inflation in some countries has already risen to around 10%.¹

In this situation several governments tried to relieve their citizens with tax reductions or transfer payments. On April 27, 2022, the German government announced a (second) stimulus package worth 14–16 billion Euro.² Beside new transfer payments and a cheap, nationwide public transport ticket (*€9 ticket*), it also included a temporary reduction of the energy tax rate from June 1 to August 31, 2022 at an estimated cost of 3.15 billion Euro.³ Since the energy tax is levied on fuel products in Germany, this might also have had an effect on retail fuel prices. However, consumers only benefit from this regulation if the petroleum companies pass-through the tax reduction sufficiently.

Also other countries in the EU, such as France or Italy have implemented temporary measures in 2022. In France, the government introduced a fixed fuel discount between April 1 and August 31, which later has been extended until the end of the year 2022. The Italian government had already approved a subsidy program in March including a fuel tax reduction from March 22 until the end of April. Also this intervention has later been extended until December 31, 2022. Those government actions provide us with ideal exogenous shocks, which we can use as a natural experiment.⁴

In this paper, we estimate the pass-through rate and the effect on the retail margins of the temporary fuel tax reductions in the three largest countries of continental Europe (measured by GDP), namely France, Germany and Italy. Austria, Estonia, Lithuania, and Latvia are being selected as appropriate control countries for the purpose of this analysis because these nations did not introduce any comparable measures during the year 2022. Using a unique panel data set containing daily consumer prices for gasoline and diesel on service station chain level, we compute the pass-through rates and changes in the margins by employing a staggered Difference-in-Differences (DiD) approach.

Our results imply a heterogeneous passing over time of the fuel tax reductions depending on the country as well as on the type of fuel.

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¹ See <https://www.global-rates.com/de/wirtschaftsstatistiken/inflation/inflation.aspx>. (Last accessed: October 19, 2022).

² See <https://www.bundesfinanzministerium.de/Content/DE/Pressemitteilungen/Finanzpolitik/2022/04/2022-04-27-zweites-entlastungspaket.html>. (Last accessed: October 19, 2022). The package was approved by the German parliament on May 13.

³ See <https://www.bundestag.de/dokumente/textarchiv/2022/kw20-de-energiesteuersenkungsgesetz-894664>. (Last accessed: October 19, 2022).

⁴ Due to the circumstance that the interventions were introduced to ease the burden to consumers and with the intention to lower consumer prices, potential endogeneity issues will be discussed at the end of Section 5.

However, we find the following two key results. First, the average pass-through rates are very high so that there is a full-shifting of the temporary tax reductions, indicating highly competitive markets. Second, the estimated pass-through rates are on average higher for gasoline than for diesel,⁵ which may result from the special situation on the European energy markets in 2022 following the Russian invasion of Ukraine and a relating high demand of (heating) diesel.

The results of our paper have implications for the effective design of unconventional fiscal policy and are also relevant for competition policy. Unconventional fiscal policy can only be effective in stimulating demand if consumers expect tax reductions to be passed through by firms. Besides, such fuel tax reductions also have distributional- and climate-economical effects. While the discount may act like a redistribution from bottom to top as particularly high-income consumers with large cars are benefiting, it is generally questionable whether subsidizing fossil fuels is a good idea in times of climate change.

The rest of the paper is structured as follows. Section 2 presents related literature, followed by a description of retail fuel markets in Section 3. We present our data set and descriptive statistics in Section 4. In Section 5, we explain our empirical strategy and then present the estimation results in Section 6. We conclude in Section 7 by discussing policy implications and limitations.

2. Related literature

Since gasoline markets are typically characterized by a very specific cyclical pricing pattern, academia as well as competition authorities are highly interested in analyzing this industry sector. The leading theory to explain price cycles in gasoline markets are Edgeworth price cycles. This theory has been formalized by Maskin and Tirole (1988) and assumes a dynamic oligopoly game where firms compete in prices and sell homogeneous goods. Starting at a supra-competitive price, firms undercut each other until the price reaches marginal costs. Given that there is no gain to lowering prices further, firms play a war of attrition. After one firm relents the price back to a high level, the other follow and the cycle begins anew (see Noel et al. (2011)).

In contrast to the literature mentioned above, other authors discuss the possibility of tacit collusion in gasoline markets. Since petrol stations can easily observe and monitor price changes as well as learn the price setting behavior of their competitors, an explicit agreement is not necessary to establish such a behavior. Evidence for collusion in gasoline markets has been found for Australia (Byrne and De Roos, 2019) and Norway (Fors and Steen, 2013). With respect to Germany, Dewenter et al. (2017) show that the introduction of the 'Markttransparenzstelle für Kraftstoffe' (market transparency unit for fuels, MTS-K)⁶ in 2013 has increased both gasoline and diesel prices. Clark et al. (2023) find that algorithmic pricing has a significant effect on competition in the German gasoline market.

Another strand of the literature analyzes the effects of changes in the crude oil price on refined petroleum products. Here, most of the papers are focused on the oil-gasoline relationship. It has been shown that downstream prices seem to respond to increases in upstream prices more rapidly than their responses to decreases in upstream prices, so that there is a potentially asymmetric pass-through of increasing and decreasing costs ('rockets and feathers') (e.g., Grasso and Manera (2007) and Noel (2009, 2015)). In this context, similar studies explore the causes for this asymmetric relation between crude oil and gasoline. They identify refinery utilization rates and inventories as a main

driver of those asymmetries (e.g., Kaufmann and Laskowski (2005) and Perdiguer-Garcia (2013)).

Recent papers also analyze the pass-through of taxes and excise duties on fuel prices. In general, pass-through rates depend on consumer behavior as well as on competition parameters (e.g., Montag et al. (2021), Genakos and Pagliero (2022) and Harju et al. (2022)). The effect of tax changes on market prices primarily depends on supply and demand elasticities (Edgeworth, 1897). In a perfectly competitive market, the pass-through rate increases in the elasticity of supply and decreases in the elasticity of demand. However, if competition is not perfectly competitive, the pattern of tax incidence becomes more complex and several degrees of tax shifting are possible: under-, full- and over-shifting to consumers (see Appendix A). Besides, not only the horizontal market structure but also vertical market power has to be considered (Fuest et al., 2020).

Some empirical results indicate that the coefficient associated with taxes on gasoline prices is not statistically different from one (or slightly less than one) (e.g., Marion and Muehlegger (2011), Bello and Contín-Pilart (2012) and Li et al. (2014)). In contrast, other studies find that a higher percentage of a tax increase is passed to consumers than a tax reduction (Doyle Jr. and Samphantharak, 2008; Silvia and Taylor, 2014) or identify state-specific rates of pass-through (Kaufmann, 2019). Regarding the fuel tax reduction in Germany in 2022, results range between a partial pass-through to a full-pass-through (e.g., Dovern et al., 2023; Schmerer and Hansen, 2023; Kahl, 2023; Bernhardt et al., 2023; Fuest et al., 2022; Seiler and Stöckmann, 2023). Here we explicitly contribute for the case of Germany but also other not yet examined countries (France and Italy) utilizing a staggered difference-in-differences design.

3. The retail fuel market

The fuel market is characterized by a vertical structure, with refineries producing fuels from crude oil in the upstream market and selling them to fuel stations, which in turn distribute the fuels to end customers (downstream market). In our study, we focus on the analysis of retail prices on the service station chain level, however, an understanding of the upstream sector is still relevant, especially for the calculation of margins. During fuel production a barrel (42 gallons) of crude oil can be refined into 19 gallons of gasoline, 12 gallons of diesel and 13 gallons of other products.⁷ In addition to crude oil, refineries also add other oils and liquids to the finished products that are sold to the petrol stations.

After significant increases in the European retail fuel prices at the beginning of 2022, several countries adopted measures with the aim of relieving consumers. For our analysis, we focus on the three largest economies in continental Europe that have introduced reductions of excise duties on fuel or similar measures, explicitly Germany, France, and Italy. To choose appropriate control countries for our staggered DiD approach, we need to find countries of the European Union (EU) which have not implemented any regulations in the fuel market in 2022. Table 1 presents an overview of policies introduced in all member states of the EU. It is obvious, that there are numerous overlaps in timing (i.e., measures came into force on the same day), which prevent a comparison. Apart from that, there are several countries that have chosen VAT reductions or price caps as policy measures, which also reduces comparability (due to varying magnitude of actual discounts over time). The consideration of all countries shows that by these criteria the majority of all EU countries are not eligible as control countries for our analysis.⁸ Yet, Austria, Estonia, Latvia, and Lithuania, as countries that have not introduced any regulations, are considered suitable for comparison.

⁵ Note: Our hypothesis tests indicate that there is no statistically significant difference between them (see Section 6.1).

⁶ The MTS-K is an independent unit of the German competition authority. All petrol stations in Germany are legally bound to inform the MTS-K about price changes in real time (see https://www.bundeskartellamt.de/EN/EconomicSectors/MineralOil/MTU-Fuels/mtufuels_node.html?sessionid=0E947D4936B3B12872C630A4005CED95.2_cid378).

⁷ See <https://www.eia.gov/energyexplained/oil-and-petroleum-products/refining-crude-oil-inputs-and-outputs.php> (Last accessed: October 19, 2022).

⁸ Belgium, Croatia, Hungary, Poland, Portugal, and Slovenia have introduced regulations other than a fixed tax reduction/discount. Bulgaria, Czech

Table 1

Overview of fuel tax reductions in all EU member states. In the case of fuel tax reductions given values are excl. associated VAT reduction.

Sources: <https://www.bruegel.org/dataset/national-policies-shield-consumers-rising-energy-prices> (Last accessed: August 7, 2023).

Country	Country code	Type of measure	Date (mm/dd/yy)	Tax reduction		In sample?
				€5	Diesel	
Austria	AT	–	–	–	–	Control
Belgium	BE	VAT reduction + Fuel tax reduction	02/01 + 03/19/22	15% + 17.5ct/l	15% + 17.5ct/l	–
Bulgaria	BG	Fixed discount	07/09/22	13ct/l	13ct/l	–
Croatia	HR	Price cap	10/17/21	–	–	–
Cyprus	CY	–	–	–	–	–
Czech Republic	CZ	Fuel tax reduction	06/01/22	1.5CZK/l	1.5CZK/l	–
Denmark	DK	–	–	–	–	–
Estonia	EE	–	–	–	–	Control
Finland	FI	–	–	–	–	–
France	FR	Fixed discount	04/01/22	15ct/l	15ct/l	Treatment
Germany	DE	Fuel tax reduction	06/01/22	29.55ct/l	14.04ct/l	Treatment
Greece	GR	–	–	–	–	–
Hungary	HU	Price cap	11/11/21	–	–	–
Ireland	IE	Fuel tax reduction	03/10/22	20ct/l	15ct/l	–
Italy	IT	Fuel tax reduction	03/22/22	25ct/l	25ct/l	Treatment
Latvia	LV	–	–	–	–	Control
Lithuania	LT	–	–	–	–	Control
Luxembourg	LU	Fuel tax reduction	03/31/22	7.5ct/l	7.5ct/l	–
Malta	MT	–	–	–	–	–
Netherlands	NL	Fuel tax reduction	04/01/22	17.3ct/l	11.1ct/l	–
Poland	PL	VAT reduction	02/01/22	15%	15%	–
Portugal	PT	“Autovoucher” (limited to 50l/month)	11/01/21	10ct/l	10ct/l	–
Romania	RO	–	–	–	–	–
Slovenia	SI	Price cap	03/15/22	–	–	–
Slovakia	SK	–	–	–	–	–
Spain	ES	Fixed discount	04/01/22	20ct/l	20ct/l	–
Sweden	SE	Fuel tax reduction	06/01/22	17ct/l	17ct/l	–

The retail fuel markets in all countries of our sample are characterized by an oligopoly. Those oligopolists operate nationwide, while there are also smaller suppliers with a single or small number of service stations that operate on a regional basis. For instance, in Germany five firms (Shell, BP/Aral, Esso, Total, and Jet) combine for a market share of 67%. In the other countries, the market shares of the oligopolists are within a comparable range (see Table 2). Differences in the total number of service stations are primarily related to country size and population.

In contrast, the upstream markets in the individual countries of our sample have larger differences. In Austria, for example, there is only one refinery, and the majority of fuel is imported from Germany. France also has a relatively small number of refineries and refining capacity in relation to the market size, resulting in a more inelastic supply side compared to Germany and Italy. Estonia, Latvia, and Lithuania do not have any (or only one) refinery and are therefore also strongly dependent on fuel imports. However, we incorporate these observable differences between countries by including the refinery utilization, imports of crude oil and petroleum products, and the number of gas stations per chain as control variables into our empirical analysis (see Sections 4 and 5).

Considering retail fuel prices, it becomes clear that the price of crude oil accounts for an important share of prices and their fluctuations. Yet, taxes and other duties account for the largest share. Table 3 summarizes the excise duties on gasoline and diesel for the countries in our data set. All countries levy a lower excise duty on diesel than on gasoline, with Germany having the largest diesel privilege (at least without taking into account the temporary fuel tax reductions). Without considering any temporary tax reductions, Austria has the lowest excise duties for fuel and Italy has the highest ones. All countries also levy an

additional value-added tax (VAT) on gasoline and diesel.⁹ In Germany, an additional fuel carbon tax of 7.2 cents (8.03 cents) on gasoline (diesel) and an additional fuel storage fee of 0.27 cents (0.30 cents) on gasoline (diesel) are levied.

In Germany, the excise duty on fuel (“energy tax”) has been lowered by 29.55 cents per liter for gasoline (35.20ct incl. VAT) and by 14.04 cents per liter for diesel (16.70ct incl. VAT) for the period between June 1 and August 31, 2022.¹⁰ With this reduction, Germany has lowered the excise duty on fuel to the minimum level permitted in the EU. In Italy, a reduction of the excise duty on gasoline and diesel by 25 cents per liter (30.50ct incl. VAT) has been introduced from March 22, 2022 on.¹¹ This measure was initially limited until April 30, but was extended shortly after and ultimately lasted until the end of 2022. The French government has passed a law that introduced a discount for all important fuel products by 15 cents per liter (18.00ct incl. VAT) from April 1, 2022 on.¹² On September 1, the fuel discount has even been increased from 15 to 25 cents per liter and in addition has been extended until December 31, 2022.¹³ This discount was paid as a subsidy for quantities sold to the distributor at the second-last distribution level. Based on the termination of the tax reduction in Germany on August 31, 2022 and the simultaneous change of the

⁹ VAT rates are as follows: 19% in Germany, 20% in Austria, Estonia, and France, 21% in Latvia and Lithuania, and 22% in Italy. To calculate margins and pass-through rates we include VAT reductions associated with the energy tax reductions/discounts to consider the overall reductions.

¹⁰ See: <https://www.bundestag.de/dokumente/textarchiv/2022/kw20-de-energiesteuersenkungsgesetz-894664> (Last accessed: July 10, 2023).

¹¹ See: <https://www.gazzettaufficiale.it/eli/id/2022/03/21/22G00032/sg> and <https://www.loc.gov/item/global-legal-monitor/2022-05-31/italy-new-law-reduces-excise-taxes-and-vat-on-fuels-to-ameliorate-financial-crisis-caused-by-war-in-ukraine/> (Last accessed: July 10, 2023).

¹² See: https://www.legifrance.gouv.fr/download/pdf?id=Yhu9yt95Vn93t_PqwVky79FdAV-pqnpuwM5LZBeTr90 (Last accessed: July 10, 2023).

¹³ See: <https://www.connexionfrance.com/article/French-news/How-the-French-government-fuel-discount-will-change-from-September-1> (Last accessed: July 12, 2023).

Republic, Ireland, the Netherlands, Spain, and Sweden were excluded as additional treated countries for timing reasons. Due to their specific geographical location, data unavailability and/or a currency other than the Euro we decided not to consider Cyprus, Denmark, Finland, Greece, Malta, Romania, and Slovakia.

Table 2

Overview of relevant market characteristics in all countries considered. When market share values were not publicly available, they were approximated based on the stations in our dataset relative to all stations (denoted by $\approx$).

Sources: Statistical Report 2023, FuelsEurope, <https://www.fuelsEurope.eu/publications/publications/statistical-report-2023> (Last accessed: August 7, 2023).

		Austria	Estonia	France	Germany	Italy	Latvia	Lithuania
Downstream	Number of fuel stations	2759	515	11,040	14,452	21,700	600	765
	Oligopoly members	BP, ENI, Jet, OMV, Shell	Alexela Oil, Neste, Circle K, Olerex, Saare Kütus	Shell, Aral, Esso, Total, Jet	Shell, Aral, Esso, Total, Jet	Eni, Q8, Esso, Tamoil	Circle K, Neste, Viada, Virsi-A	Viada, Circle K, Neste, Baltic Petroleum
	Market share of oligopolists	67%	$\approx$ 54%	62%	67%	49%	$\approx$ 52%	$\approx$ 51%
Upstream	Number of refineries	1	0	6	11	10	0	1
	Refinery capacity (in Mt/a)	9.80	0	58.20	100.90	83.90	0	9.60

Table 3

Excise Taxes on Gasoline (E5) and Diesel in cents per liter.

Source: https://ec.europa.eu/taxation_customs/cedb/ (Last accessed: August 7, 2023).

Country	Treatment	Gasoline (E5)				Diesel			
		Pre	Post	Difference	Diff. incl. VAT	Pre	Post	Difference	Diff. incl. VAT
Austria	–	48.00	–	–	–	40.00	–	–	–
Estonia	–	42.277	–	–	–	39.292	–	–	–
France	04/01/2022	68.29	53.29	–15.00	–18.00	59.40	44.40	–15.00	–18.00
Germany	06/01/2022	65.45	35.90	–29.55	–35.20	47.04	33.00	–14.04	–16.70
Italy	03/22/2022	72.84	47.84	–25.00	–30.50	61.74	36.74	–25.00	–30.50
Latvia	–	41.121	–	–	–	33.295	–	–	–
Lithuania	–	43.443	–	–	–	33.017	–	–	–

discount in France, we have chosen an observation period until August 31, 2022.

Even though technically the introduced discount in France is different compared to the tax reductions in Germany and Italy, basically it has a similar effect on the costs of the retailers (i.e., service stations). For this reason, it is referred to as a reduction of excise duties paid on the retail-level in the following. In our empirical analysis (see Sections 5 and 6), we compare our estimated coefficients with the overall tax reductions (also including the associated VAT changes), which are also given in Table 3. With regard to the implemented measures, it is important to note that these represented a one-time reduction in all treated countries. In Austria, Estonia, Latvia, and Lithuania, the tax rate remained constant throughout the whole observation period (see Fig. 8 in Appendix C).

4. Data and descriptive statistics

4.1. Data collection

Our analysis is based on several different data sources. First, we scraped data on daily average gasoline (E5) and diesel consumer prices on a service station chain level from the information platform *Fuelo*. These prices on *Fuelo* are the basis of our analysis. *Fuelo* uses official sources as well as information from consumers, publishes this on its website and displays historical information on a daily average level. Real-time price updates are not considered relevant for our analysis as the platform only provides the historical price averages at the service station chain level.¹⁴ The data from *Fuelo* also provides information on the number of fuel stations per service chain. Incorporating this measure serves a dual purpose. Firstly, it helps control for variations

in the number of stations across different countries. Secondly, it takes into account the fact that the average fuel prices displayed on the *Fuelo* website are constructed based on different numbers of chain stations in each country.¹⁵

Second, we use data on the crude oil price Brent from *Onvista* and exchange rates from Dollar into Euro by the Federal Reserve Bank of St. Louis (FRED) to highlight the relation between consumer prices and the Brent price.¹⁶ The Brent price is also a crucial part to determine retail margins.

Third, we incorporate data on refinery capacities and convert them into a measure of refinery utilization, which indicates how efficiently the refineries are utilizing their maximum capacity. It is crucial to control for refinery utilization in our analysis, since gasoline and diesel can either be produced domestically within the country or imported from other countries. To assess this, we utilize data from both *Concawe* and *Eurostat*.¹⁷ *Concawe* provides annual national-level data on refinery capacities, measured in mega tonnes per annum (Mt/a). With the assistance of *Eurostat* data on the supply (and transformation) of oil and petroleum products, we convert these capacities into a measure of refinery utilization.

Exact calculation of the utilization rate needs some clarification. Before the utilization rate can be determined the domestic production must be calculated from several variables, i.e. the stream of raw oil, loss from refining the crude oil, changes in stock, releases of strategic

¹⁴ Example for German prices from February 2, 2022, <https://de.fuelo.net/prices/date/2022-2-2?lang=en>. (Last accessed: July 11, 2023). Statement from *Fuelo* on their sources: https://de.fuelo.net/prices/last_updated?lang=de. (Last accessed: July 11, 2023).

¹⁵ See, Appendix C, Table 9 for the distribution of number of service chain stations in the data. Note: Data from *Fuelo* has a large market coverage. Example: Germany had 14,452 stations in 2022. Our scraped data covers 10,600 stations.

¹⁶ See historical Brent prices, <https://www.onvista.de/rohstoffe/db-Oelpreis-Brent-26262975>, and exchange rates from FRED, <https://fred.stlouisfed.org/series/DEXUSEU>. (Last accessed: July 11, 2023).

¹⁷ See information from *Concawe*, <https://www.concawe.eu/refineries-map/>, and *Eurostat* https://ec.europa.eu/eurostat/databrowser/view/nrg_cb_oilm/default/table?lang=en. (Last accessed: July 11, 2023).

reserve or inflow from marine bunkers.¹⁸ The capacities provided by *Concawe* and *Eurostat* are available on a yearly basis, while the data for refinery utilization is required on a monthly level. To bridge this gap, the yearly capacities are converted into monthly capacities by dividing them by 12. Dividing the monthly domestic production by monthly available capacity determines the utilization of the refinery on a monthly level. Controlling for these refinery utilization possesses the opportunity to rule out differences at the supply side from the local refinery level, i.e. from breakdown in the refinery or loss of access to crude oil.

Fourth, to account for variations in the total imports of oil and petroleum products, we incorporate national-level data from *Eurostat* specifically related to the imports of these products. These imports are measured in thousands of tonnes. By controlling for changes in imports, we aim to capture another aspect of supply-side changes that are likely to be significantly influenced by the outbreak and ongoing war in Ukraine.¹⁹

4.2. Descriptive statistics

Our final data set includes consumer price data in Euro per liter for the seven European countries Germany, France, Italy, Austria, Lithuania, Estonia and Latvia on a service station chain level during the period from January 3 to August 31, 2022.

Table 8 in Appendix C presents the summary statistics divided by countries. To calculate the margins, we simply subtract taxes and duties as well as the share of the crude oil price (Brent price) attributable to the production of diesel and gasoline from the gross consumer prices.²⁰ Even though these margins still contain different cost types (e.g., cost of refining, transportation costs), with the crude oil price we can eliminate the main source of input cost variation.

The data set contains 12,515 observations on a service station chain level, i.e. we have a panel data set including price information on 52 unique country-service-station-chain-pairs for 241 days.²¹ For each country we observe a different number of chains present in the data (see Table 9 in Appendix C).²² For instance, Germany has 15 different service station chains present in the data, whereas Austria

has six. Overall, there are 30 chains in the treated country's data and 22 chains in the non-treated data.²³ Countries display variations in terms of refinery utilization²⁴, the number of stations per chain, and total imports of oil and petroleum products. To illustrate this point, a comparison of Germany and Austria serves as an examples. When comparing these two countries, we observe differences in the magnitude of imports of oil and petroleum products (mean: 10,191 v. 987; measured in thousand tonnes), refinery utilization (mean: 0.91 v. 0.55; represented by decimal units) and the number of fuel stations operated per chain (mean: 706 vs. 196). By incorporating these covariates into our analysis, we aim to account for and capture differences in the pre-existing trends and characteristics across countries. These factors help us consider the unique features and dynamics of each country's fuel market.²⁵

Despite these differences across countries and service station chains, Table 4 reports that the average price level of diesel and gasoline is very similar across the seven countries, although prices are smaller in Austria, which is mainly driven by the low fuel taxes in this country. Concentrating on the treated countries (France, Germany, Italy), we mostly observe higher average consumer prices after the fuel tax reductions (compare *Pre* and *Post* in Table 4).²⁶ Even though this seems to be counterintuitive at first glance, this is mainly driven by the increasing price for crude oil during our observation period (see development of the Brent prices in Fig. 1), which has mostly overcompensated the decreased fuel taxes. Table 4 also shows that the absolute as well as the relative retail margins for diesel and gasoline have increased in Italy and Germany after the fuel tax reductions, while they started to decrease in France after the introduction (on average).²⁷

Fig. 1 visualizes the development of the average median consumer prices and Fig. 2 shows the daily average retail margins. The figures are divided into sub figures to point out the development of gasoline (upper) and diesel (middle) of the seven European countries during our observation period. The Brent price (lower) is also depicted to highlight the strong link to the market price for crude oil. The vertical lines reflect the introduction of the respective tax reductions in Italy (March 22, yellow), France (April 1, blue), and Germany (June 1, red). In fact, the prices as well as the margins in the seven countries tend to follow the same trend before the policy changes. In all countries, there is also a noticeable increase in both, prices and margins, at the end of February when the war in the Ukraine has started.

With respect to the diesel and gasoline consumer prices, Fig. 1 shows that both have decreased in the first phase after the respective fuel tax cuts in the three treated countries. However, they tend to increase again after a while which is mainly driven by the price increase for crude oil (depicted in dashed gray in the lower sub figure).²⁸

Simultaneously, the absolute (and also the relative) effect on retail margins exhibits similar trends between the three treated countries (see

¹⁸ *Eurostat* provides information from the Monthly Oil and Gas questionnaire at page 10, No. 11 on how the gross inland deliveries are determined and we will use this to rearrange this equation for domestic production ([https://ec.europa.eu/eurostat/documents/38154/42198/MOS_v2012.1.pdf/f4a7a75c-b0d1-4370-802a-560ca5f86f4d#:~:text=Gross%20inland%20deliveries%20\(Observed\)%3A,\)%20to%20the%20inland%20market](https://ec.europa.eu/eurostat/documents/38154/42198/MOS_v2012.1.pdf/f4a7a75c-b0d1-4370-802a-560ca5f86f4d#:~:text=Gross%20inland%20deliveries%20(Observed)%3A,)%20to%20the%20inland%20market)). (Last accessed: July 11, 2023). From the data of *Eurostat* we calculate domestic production for petroleum products with the formula: Domestic Production = Gross inland deliveries – Primary product receipts – Recycled products + Refinery fuel – Imports + Exports + International marine bunkers – Interproduct transfers + Products transferred + Stock changes. Note: Refinery gross output denotes what we call domestic production. Statistics from *Eurostat* regarding what they refer to as *indigenous production* is not available.

¹⁹ See: *Eurostat*, imports of oil and petroleum products by partner country, https://ec.europa.eu/eurostat/databrowser/view/NRG_TI_OILM_custom_6837161/default/table?lang=en. (Last accessed: July 11, 2023).

²⁰ An important note is that our measure of retail margins includes the refinery margin, the station margin, as well as different cost types such as the cost of refining or the cost of transportation. For a detailed description on the calculation of margins see Appendix B.

²¹ The data set is slightly unbalanced.

²² Fuel's market coverage per service station chain varies and total market coverage is different across countries. However, it is worth mentioning that the geographic coverage within countries comprises almost their entirety, which can be substantiated through visual inspection. Nevertheless, the goal of this study is to analyze the overall pass-through rate. In this respect, our identification strategy relies on the comparison of the evolution of country-wide large chains average prices, such as most important players Shell, Esso, or Total rather than analyzing the entire market.

²³ The estimation will utilize the not-yet-treated characteristics of the data. During the time periods when part of the data is not yet treated these chains will be used as a comparison group.

²⁴ See Fig. 9 in Appendix C.

²⁵ Estonia and Latvia do not have a refinery. For regression purpose the values are set to zero. For E5 in Lithuania (Estonia) there are 9 (2) observations missing which are filled by the last available value of the respective chain to complete the series. For some months in Lithuania refinery utilization is sometimes slightly larger than 1. This probably comes from data accuracy and calculations from an annual capacity to a monthly levels.

²⁶ It is worth mentioning that the definition of the *Pre* and *Post* periods is distinct for the three treated countries due to the different implementation dates of the fuel tax reductions.

²⁷ Relative retail margin reflects the simple Lerner-Indices formula, dividing the absolute margins by net prices.

²⁸ To have a better understanding of the individual consumer price curves, we additionally present the gasoline and diesel price development for the seven countries separately in Fig. 7 of Appendix C.

Table 4

Summary statistics of fuel prices and margins by country before (pre) and after (post) the tax decrease (Numbers in Euro per liter, except the relative margins.). Averages are based on the country and service station chain pairs.

	Country	Austria	Estonia	France		Germany		Italy		Latvia	Lithuania
				Pre	Post	Pre	Post	Pre	Post		
Fuel price	E5	1.783	1.857	1.916	2.038	1.947	1.861	1.948	1.983	1.803	1.738
	Diesel	1.815	1.759	1.886	2.065	1.881	1.968	1.840	1.960	1.753	1.726
Fuel margin	E5	0.273	0.392	0.257	0.392	0.204	0.352	0.228	0.372	0.347	0.270
	Diesel	0.386	0.342	0.323	0.505	0.326	0.465	0.253	0.467	0.387	0.367
Relative fuel margin/Lerner-index	E5	0.260	0.345	0.278	0.330	0.220	0.310	0.260	0.320	0.318	0.262
	Diesel	0.337	0.310	0.324	0.392	0.309	0.374	0.277	0.374	0.338	0.329

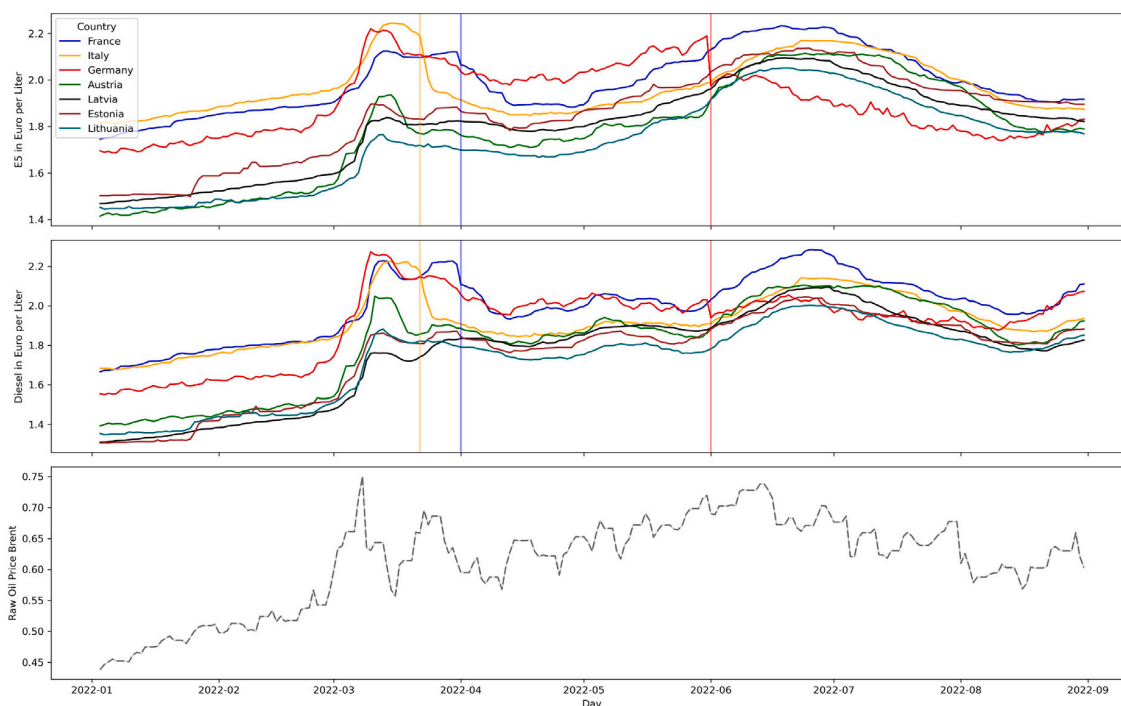


Fig. 1. Development of average consumer prices for gasoline (upper) and diesel (middle). The vertical lines reflect the introduction of the respective tax reductions in Italy (March 22, yellow), France (April 1, blue), and Germany (June 1, red). Brent prices (lower) in Euro per Liter is denoted in dashed gray.

Fig. 2). In addition, the margins reveal a slight difference between the countries. While the margins have increased in Germany for diesel and gasoline, in France and Italy they immediately started to decrease after the temporary tax reduction. In relative terms these margins (see Fig. 10 in Appendix C) reflect the Lerner-Indices (Lerner, 1934; Giocoli, 2012) which range from 0 (no market power) to 1 (monopoly market power). Interpretation of this crude measure of market power is problematic, especially without deep knowledge about the exact cost structure on all parts of the vertical chain within the fuel market and should be carried out with caution (Elzinga and Mills, 2011). Therefore, the large increase and long-term shift in the margins seen in the mid of March 2022 may be prominent but the exact cause cannot be determined without detailed market information on costs and is not part of this paper. In this regard, our empirical analysis will show that these changes in margins are on average not affected by the tax reductions.

5. Methodology

In our empirical analysis, we estimate the impact of the temporary fuel tax reductions on fuel prices and retail margins. In order to do this, we compare the evolution of consumer prices and retail margins at fuel stations in Germany, France, Italy, Austria and the Baltic States, before and after the reductions of the fuel taxes.

We apply a staggered Difference-in-Differences (DiD) design to causally estimate the effect of the temporary fuel tax reductions on fuel prices and retail margins. In contrast to the canonical DiD setup, the staggered design allows to estimate the unbiased average treatment effect on the treated (ATT) when there are more than two time periods and variation in timing of the treatment. This design is more credible and robust than the canonical DiD with a single treatment period because including multiple treatments plausibly alleviates concerns that contemporaneous trends drive the observed treatment effects (see, e.g., Baker et al., 2022). Goodman-Bacon (2021) shows that time-varying treatment effects can create a bias in the static two-way fixed effects (TWFE) DiD estimate since earlier-treated units act as effective controls for later-treated units so that the resultant DiD estimates could reflect differences in treatment effects over time between different treatment groups.

Hence, more recent papers propose alternative DiD estimators that do not suffer from the pitfalls associated with TWFE described above (De Chaisemartin and d'Haultfoeuille, 2020; Sun and Abraham, 2021; Callaway and Sant'Anna, 2021b). We follow the recent DiD methodology developed by Callaway and Sant'Anna (2021b) as it allows to estimate a time-varying and cohort-specific ATT using not-yet-treated or never-treated as clean controls. Specifically, the estimation strategy

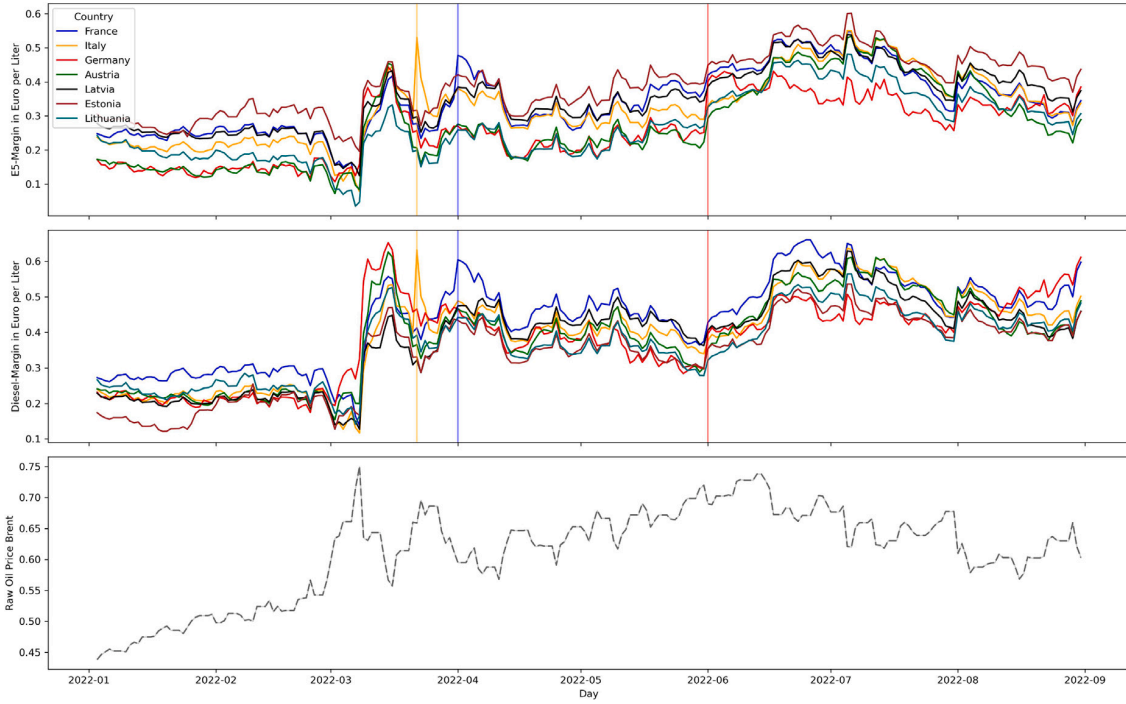


Fig. 2. Development of average retail margins for gasoline (upper) and diesel (middle). The vertical lines reflect the introduction of the respective tax reductions in Italy (March 22, yellow), France (April 1, blue), and Germany (June 1, red). Brent prices (lower) in Euro per Liter is denoted in dashed gray. See relative retail margins in Fig. 10 in Appendix B.

follows the stylized regression²⁹:

$$y_{ijt} = X'_{it}\beta + \tau_{it} \cdot TAX_{it} + \eta_{ij} + \lambda_t + \epsilon_{ijt}, \quad (1)$$

where y_{ijt} denotes the consumer price (or retail margin) of gasoline or diesel sold by gas station chain j in country i at date t , and TAX_{it} is a dummy variable that equals one when country i implements a temporary fuel tax reduction at date t (note that France, Italy and Germany implemented there reductions at different dates, see Section 3). The vector X' contains our control variables refinery utilization rate, total imports of oil and petroleum imports and number of gas stations.³⁰ The variable η_{ij} corresponds to country service station chain fixed effects and controls for any time-invariant differences between the countries in our dataset. Finally, λ_t gives the day fixed effects, which capture the transitory shocks that identically affect the individual countries, such as fluctuations in the price of crude oil or the conflict in the Ukraine.

Let us further assume that G_i contains i different states treated at different points of time and C_i is a set of never treated states. Then, under the parallel trend and anticipation assumptions (Wooldridge, 2021) we can estimate the ATT for a treatment-timing group g at a point in time as the group-time average treatment effect using never-treated (2) or not-yet-treated (3) units as controls by using the R package as provided by Callaway and Sant'Anna (2021a)³¹:

$$ATT(g, t) = E[Y_t - Y_{g-1} | G = g] - E[Y_t - Y_{g-1} | C = 1]. \quad (2)$$

$$ATT(g, t) = E[Y_t - Y_{g-1} | G = g] - E[Y_t - Y_{g-1} | D_t = 0, G \neq g]. \quad (3)$$

In the Eqs. (2) and (3), t indexes the time in days, g gives the period in which country i is treated and Y_{it} is the fuel price or retail margin of country i .

Finally, we can average the $ATT(g, t)$ over all countries:

$$\Theta_S(g) = \frac{1}{T - g + 1} \sum_{t=2}^T 1\{g \leq t\} ATT(g, t). \quad (4)$$

Eq. (4) then gives the time-average for each group and the overall average respectively. As already mentioned above, we use the fuel prices of seven different European countries to causally identify the effect of the temporary fuel tax reductions. Thereby, Germany, Italy, and France are the treated countries and Austria as well as the Baltic States are the never-treated countries in our staggered DiD approach.

We also want to estimate the treatment effect heterogeneity over time as the effect of the temporary fuel tax reductions on the retail prices might be dynamic. Using an event study design we can prove the process of tax pass-through over time to check whether there is an effect, how many periods it takes to have an effect, and how long it lasts. Moreover, we can test the parallel trend assumption checking the pre-treatment estimators. Hence, based on Eqs. (2) and (3) we provide an event study including pseudo-ATTs for the pre-period and ATTs for the post-period.

To perform the described analysis, various requirements for an unbiased and exogenous estimation have to be satisfied. In general, we can assume that the countries in our data set are very comparable. They are all members of the European Single Market, which implies harmonized border checks, common customs policy, and identical regulatory procedures on the movement of goods within the European Union (EU). Beyond, the seven countries are relatively similar in their geographic location and have highly correlated public and school holidays. In our observation period, also the travel restrictions put in place due to the COVID-19 crisis were similar and no major reforms, which could also affect fuel prices, were implemented.

²⁹ Depicting this equation's purpose is to intuitively highlight the estimation strategy. Exact estimation will rely on Eqs. (2) and (3) as well as on Footnote 30.

³⁰ Note that we can add time-invariant variables when using the approach from Callaway and Sant'Anna (2021b) because those variables are interacted with the day fixed effects. Thus, the covariates are not collinear with our state fixed effects, but act more like state-specific time trends. Technically, we use an inverse probability weighting (IPW) to rebalance the distribution of covariates and estimate reweighted ATTs (Abadie, 2005).

³¹ See <https://bcallaway11.github.io/did/index.html>.

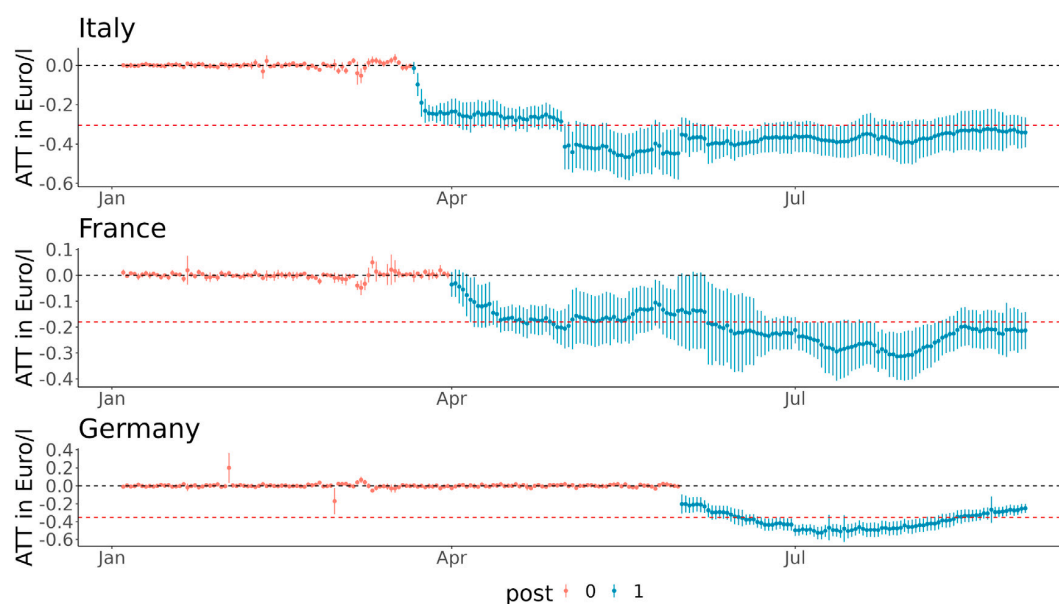


Fig. 3. Event Study of prices with gasoline (E5) and all covariates. Bootstrapped (robust) standard errors are clustered on the country and service station chain level. Error bars represent 95% confidence intervals. Red dashed horizontal line depicts the value for a full-pass-through.

Furthermore, to causally identify an unbiased ATT of the temporary fuel tax reductions on fuel prices, there should also be no other transitory shocks that would differently affect fuel prices in the individual countries before and after the tax reduction. Due to their geographic proximity the petroleum companies in the seven countries procure most of their crude oil from similar sources. Finally, we also focus on a relatively narrow window around the tax reductions, which should alleviate concerns on transitory shocks differently affecting the seven countries.

Moreover, requirements have to be considered in the context of government actions tackling high energy prices observed due to the start of the Ukraine war (see Section 1). The fuel tax reductions were implemented as a measure to counter high inflation rates which may induce the idea of potential endogeneity between tax reductions and price changes, i.e. price changes are affected by the tax reduction and vice versa. There are two major arguments why endogeneity does not pose a problem in our analysis. First, depending on time intervals of tax changes, i.e. tax changes every month, frequent changes might pose a problem for identifying an unbiased effect, especially if long-run relationships are examined (Kaufmann, 2019). However, excise duty reductions by the European governments do not occur on a regular basis,³² and the interventions in 2022 were implemented at short notice. The energy taxation in Europe is more rigid compared with e.g. the taxation in the US (Kaufmann, 2019) and the interventions in 2022 were a reaction on an exogenous shock, namely the war in the Ukraine. Second, we utilize a panel data set on a service station chain level and a (staggered) DiD design to compare the decisions of countries that implemented a tax reduction to countries that decided against introducing such an intervention. All European countries were equally affected by high fluctuations of the crude oil price (see Fig. 1), which is accounted for by time fixed effects. This design mitigates a bias for the ATT as long as the control groups are not affected by a similar intervention and generally provide a reasonable comparison group to construct

an appropriate counterfactual, i.e. the parallel trend assumption must be satisfied (Wooldridge, 2021). Our analysis meets this requirement, especially because the never-treated part of the control countries did not receive any fuel tax reductions during our observation period. Figures presented in Section 6.2 (Figs. 3 and 4) provide evidence in form of event studies, highlighting that the assumption of a common trend can be assumed to be satisfied.

6. Results

6.1. Baseline results

Table 5 presents the results of estimating regression Eq. (4) using the consumer price for gasoline and diesel as outcome variables. The coefficients in columns (I) and (II) correspond to the average treatment effect of the temporary fuel tax reductions on gasoline and diesel in France, Italy and Germany without any other control variables. Columns (III) and (IV) show the effect on consumer prices when we additionally control for the supply side parameters refinery utilization, number of gas stations and the total imports of oil and petroleum products.

In general, the results in Table 5 show that the fuel tax reductions led to a statistically significant decline in the average consumer prices of both fuel types for all three countries treated ($p < 0.001$). For our model without any covariates, in Germany the average price for diesel decreases by 15 cents per liter after the fuel tax reduction (column (II)), whilst the average price for gasoline decreases by about 36 cents per liter (column (I)). Also for France the price decrease for diesel (−18 cents per liter) is slightly lower compared to the one for gasoline (−19 cents per liter). With a price drop of more than 30 cents per liter for diesel and 32 cents per liter for gasoline, also the estimated pass-through rates in Italy are very high. Including additional control variables (columns (III) and (IV)) only quantitatively changes our estimation results, even though we apparently underestimate the average treatment effects without controlling for the supply side.

In a next step, we can calculate the average pass-through rates of the fuel tax reductions. Therefore, we divide the estimated coefficients by

³² Excise duties on gasoline and diesel have not been changed in Germany since 2006, in Italy since (at least) 2016, and in France since 2018 (prior to the reduction in 2022). For the legal basis, see: https://ec.europa.eu/taxation_customs/tedb/index.html. See also Fig. 8 in Appendix C that highlights exact tax changes in 2022.

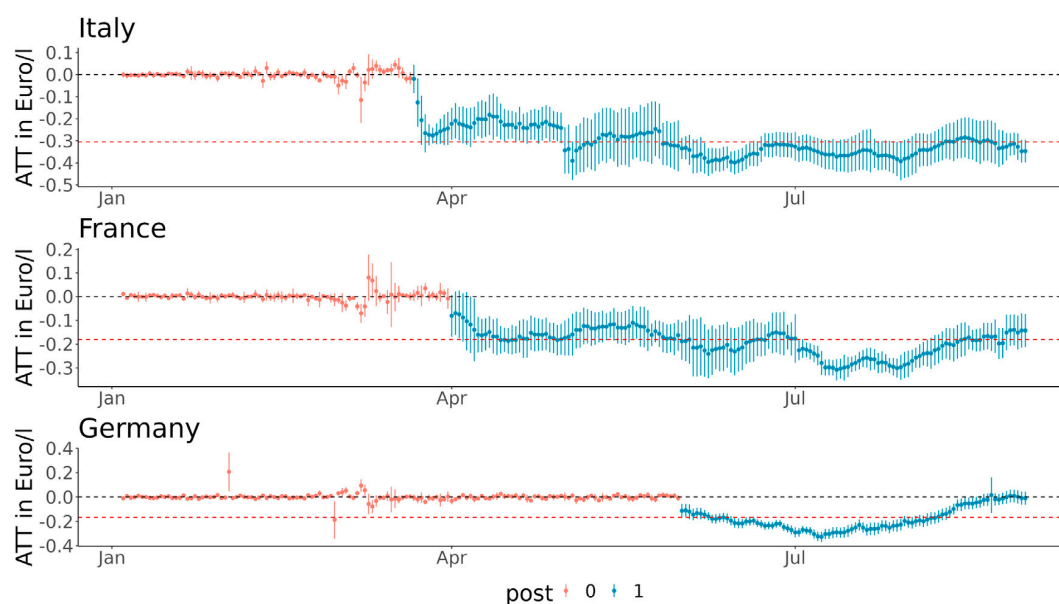


Fig. 4. Event Study of prices with diesel and all covariates. Bootstrapped (robust) standard errors are clustered on the country and service station chain level. Error bars represent 95% confidence intervals. Red dashed horizontal line depicts the value for a full-pass-through.

Table 5

Staggered DiD, referring to estimates of Eqs. (2) and (3) that are averaged by means of Eq. (4). Approach with consumer prices as outcome variable. Bootstrapped (robust) standard errors provided in parentheses are clustered on the country and service station chain level.

	(I) Gasoline	(II) Diesel	(III) Gasoline	(IV) Diesel
Italy	−0.32*** (0.02)	−0.30*** (0.02)	−0.35*** (0.02)	−0.30*** (0.02)
France	−0.19*** (0.02)	−0.18*** (0.01)	−0.20*** (0.02)	−0.19*** (0.01)
Germany	−0.36*** (0.02)	−0.15*** (0.01)	−0.39*** (0.02)	−0.18*** (0.01)
Simple weighted average	−0.31*** (0.01)	−0.20*** (0.01)	−0.33*** (0.01)	−0.21*** (0.01)
Pass-through Italy	106.09%	98.67%	114.27%†	100.00%
Pass-through France	104.65%	99.91%	111.37%	105.99%
Pass-through Germany	103.52%	89.73%	110.32%	106.69%
Time FE	Yes	Yes	Yes	Yes
Country and chain FE	Yes	Yes	Yes	Yes
Supply parameters	No	No	Yes	Yes
Observations	12,515	12,515	12,515	12,515

H0: No effect, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. H0: 100% Pass-Through, † $p < 0.05$, †† $p < 0.01$.

the actual tax reductions in the three countries.³³ The estimated pass-through rates in Table 5 mostly imply a full- or even an over-shifting of the temporary fuel tax reductions. In our baseline estimations (columns (I) and (II) in Table 5), there is an over-shifting for gasoline and almost a full-shifting for diesel. With an estimated pass-through rate of approx. 106%, Italy has the highest passing on of the temporary fuel tax reduction for gasoline and France the highest one for diesel (approx. 100%). Overall, the estimated rates are very similar in the three countries, even though the estimated pass-through rate for diesel is slightly lower in Germany (approx. 90%). Including the control variables for the supply side (columns (III) and (IV) in Table 5) into our regression model generally increases the estimated pass-through rates so that we also find a full- or over-shifting for diesel now. In general, the high pass-through rates might be explained by the inelastic demand for fuel products and particularly by the high public awareness as well as the threat of policymakers to pursue antitrust measures. The 2022 fuel tax reductions had great political and economic implications so that there was a high attention in the public debate (Kahl, 2023).

³³ For instance, in our baseline estimation for diesel (column (II) of Table 5) the pass-through rate for Germany can be calculated as follows: $passthrough = \frac{EstCoeff}{TaxReduction} = \frac{-15}{-16.7} = 0.8973 = 89.73\%$.

However, testing whether the average pass-through rates are statistically significant different from a full pass-through (100%) shows that all but one are not different to 100% (see Table 5). Only the pass-through rate of gasoline in Italy in column (III) is statistically significantly higher than 100% ($p < 0.05$).

Beside the general high average pass-through rates, a second interesting finding is that the effects of the tax reductions are mostly higher for gasoline compared to diesel in our estimations. This is in sharp contrast to the literature that finds a more inelastic demand for diesel compared to gasoline (Ajanovic et al., 2012; Karagiannis et al., 2011; Fridström and Østli, 2021). However, the Russian invasion of Ukraine led to a high uncertainty of consumers in the energy markets in 2022, which was combined by an unusually high demand for heating diesel in spring and summer 2022. Households increased their heating diesel stocks out of fear of continuously rising prices, because they expected even higher prices in the future. This phenomena was particularly present in Germany.³⁴ As heating diesel is a close substitute for diesel (whereas gasoline is not), this might explain the lower pass-through rates for diesel in our results.

³⁴ See <https://www.dw.com/en/german-residents-make-plans-amid-fears-of-a-winter-gas-shortage/a-62482737>.

Table 6

T-test of gasoline versus diesel pass-through rates of Table 5. Depicted values correspond to p-values. See Footnote 35 for the exact construction of the hypothesis test.

	H0: (I) = (II)	H0: (III) = (IV)
Italy	0.4451	0.2392
France	0.7679	0.7403
Germany	0.2582	0.7104

However, scrutinizing differences of the average gasoline estimates against average diesel estimates in each country by means of a hypothesis test reveals no statistically significant differences. Table 6 shows the p-values for each of the tests, with none rejecting the null hypothesis based on common significance levels.³⁵

6.2. Pre-treatment trends and dynamic effects

To check whether the estimated results are causal effects and to highlight evolution of pass-through rates over time, we will present an event study design next. The crucial assumption to interpret the results as causally is the parallel trends assumption. Even though this assumption is not directly testable, the event study design does lead to a formal test of pre-treatment trends. With this approach, we can also observe the treatment effects of the fuel tax reductions over time.

Fig. 3 presents the group-time average treatment effects from Eq. (3) for gasoline in the three treated countries.³⁶ We use the regression model including all control variables and compute bootstrapped 95% confidence intervals. Moreover, we apply a varying base period which means that a pseudo-ATT is computed in each treatment period by comparing the changes in outcomes for a particular group relative to the comparison group in the pre-treatment periods.³⁷ This just means that we compute changes in the pre-treatment periods from period $t - 1$ to period t , but repeatedly change the value of t (Callaway and Sant'Anna, 2021b). The pre-treatment coefficients are close to zero and mostly insignificant in all three countries providing supportive evidence for the common trend assumption. An exception is the time of the beginning of Russia's invasion in the Ukraine, which leads to a short divergence in the pre-trends for Italy and France. However, the pre-trends converge back to the zero line and are statistically insignificant shortly before the exogenous shocks of the tax cuts in all three countries.

In Fig. 3 we can also observe that the treatment effects over time are negative and mostly statistically different from zero. In Germany, there is an immediate drop at the day of the fuel tax reduction with almost full pass-through (red dashed horizontal line). There is a similar

³⁵ Hypothesis test is constructed by each model and country where the t-statistic is then given by $t = \frac{EstCoef_{Gasoline} - EstCoef_{Diesel}}{\sqrt{\frac{StdError_{Gasoline}^2}{TaxReduction_{Gasoline}} + \frac{StdError_{Diesel}^2}{TaxReduction_{Diesel}}}}$, following Wooldridge (2015, Chapter. 4-4). Note: The test usually contains a covariance term in the denominator between the two variables. In our comparison the pass-through rates are independent because the underlying estimation is run separately, thus there is no covariance term between gasoline and diesel. Hence, from an econometricians' viewpoint most average pass-through rates are neither statistically different from a complete pass-through nor do the average pass-through rates differ between gasoline and diesel. Overall, the average pass-through rates are mostly in line with findings of the literature on Germany 2022 which imply a full pass-through (Dovern et al., 2023; Schmerer and Hansen, 2023; Kahl, 2023; Bernhardt et al., 2023; Seiler and Stöckmann, 2023).

³⁶ In Fig. 11 of Appendix C, we present the same dynamic analysis but without including any covariates. The results there are qualitatively very similar.

³⁷ Pseudo-ATT means that we estimate the effect of participating in the treatment if the treatment had occurred in that period (instead of when it actually occurred).

Table 7

Staggered DiD, referring to estimates of Eqs. (2) and (3) that are averaged by means of Eq. (4). Approach with retail margins as outcome variable. Bootstrapped (robust) standard errors provided in parentheses are clustered on the country and service station chain level.

	(I) Gasoline	(II) Diesel	(III) Gasoline	(IV) Diesel
Italy	−0.02 (0.01)	0.00 (0.01)	−0.01 (0.02)	0.02 (0.01)
France	−0.01 (0.02)	0.00 (0.01)	−0.01 (0.02)	−0.00 (0.01)
Germany	−0.01 (0.02)	0.02 (0.01)	−0.02 (0.02)	0.00 (0.01)
Simple weighted average	−0.01 (0.01)	0.01 (0.01)	−0.01 (0.01)	0.00 (0.01)
Time FE	Yes	Yes	Yes	Yes
Country and chain FE	Yes	Yes	Yes	Yes
Supply parameters	No	No	Yes	Yes
Observations	12,515	12,515	12,515	12,515

H0: No effect, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

development in Italy, where full pass-through is already reached three days after the tax cut. In the following, there is an over-shifting in both countries. On the contrary, in France it takes almost two weeks until there is a full pass-through. This is in line with our theoretical predictions, since France has a more inelastic supply side compared to the two other treated countries (see Section 3). Overall, Fig. 3 suggests that the treatment effects are relatively stable over time in all three countries.

Fig. 4 shows the group-time average treatment effects for diesel from Eq. (3).³⁸ The pre-treatment coefficients are again close to zero and mostly insignificant (except during the start of the Ukraine conflict for Italy and France). The pattern of the treatment effects over time is very similar compared to the event study for gasoline in Fig. 3. While Italy has a relatively fast full pass-through again, it takes some time in France until there is a significant effect and even longer for a full-pass through. In Germany, we again observe an immediate drop in the treatment effects at the day of the fuel tax reduction. Again, the treatment effects are relatively stable over time, even though the effects get insignificant for Germany in the end of August. This can be explained by the drought in Germany throughout the summer of 2022. The very high temperatures led to exceptional low water levels in German rivers which, in turn, raised the transportation costs for diesel imports (Dovern et al., 2023).

6.3. Retail margins

Table 7 shows the results of estimating regression Eq. (3) averaged w.r.t Eq. (4) using the retail margins for gasoline and diesel as outcome variables. The results indicate that the reduction in fuel taxes had no significant effect on the average margins in the three countries. This is in line with our results from Section 6.1 as we mostly find a full-shifting of the temporary fuel tax reductions which, on average, should not have an effect on the retail margins.

However, performing an event study design for the outcome variable retail margins implies that there are some positive margins for diesel as well as gasoline in the first days after the fuel tax reductions. Fig. 5 presents the event study results for gasoline in the three treated countries. The margins are significantly positive in the first days after the tax cuts for Italy and France, but insignificantly for Germany. This is in line with our findings in Fig. 3 because it takes some days in Italy and France until the tax reduction is passed through to consumers. Since those daily average treatment effects get insignificantly after a few days, the overall average treatment effects in Table 7 are still

³⁸ In Fig. 12 of Appendix C, we present the same dynamic analysis but without including any covariates. The results there are again qualitatively very similar.

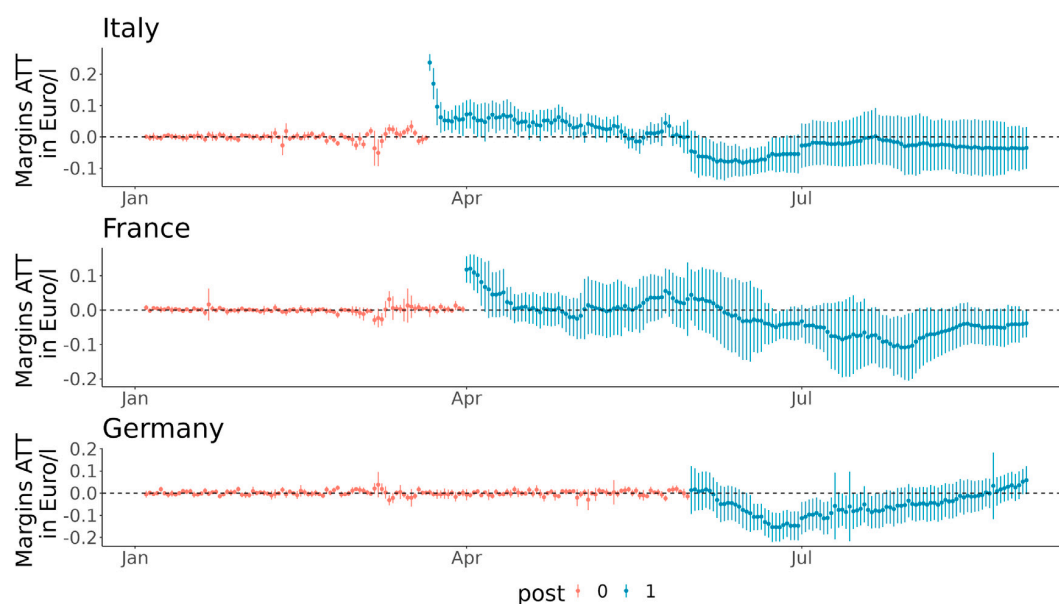


Fig. 5. Event Study of Margins with gasoline (E5) and all covariates. Bootstrapped (robust) standard errors are clustered on the country and service station chain level. Error bars represent 95% confidence intervals.

insignificant. In contrast, there is an immediate drop in Germany in the gasoline prices, which results in the insignificant margins also in the first days after the tax cut there.

In the following weeks, we even find some negative average treatment effects in Fig. 5. For instance, in Germany there is a drop in the estimated ATTs in June and July. Again, this corresponds to our estimated pass-through rates for gasoline (see Fig. 3) because for these months we find an over-shifting of the tax reduction in Germany. Since this means that the German petroleum companies passed through more than 100% of the temporary tax reduction to the consumers at this time, their retail margins are lower compared to the counterfactual scenario where there had been no tax cut.

Fig. 6 shows the equivalent results for the retail margins of diesel. Beside the positive retail margins in Italy and France, we also find some positive diesel margins in the first days after the tax cut for Germany now. During the time of the over-shifting of the fuel tax reductions (see Fig. 4), we even find some significant negative average treatment effects here. This is similar to the estimated retail margins for gasoline (see Fig. 5) and again relates to the fact that the petroleum companies passed through more than 100% of the temporary fuel tax reductions to the consumers at these times leading to lower retail margins compared to the counterfactual scenarios. Overall, those effects over time cancel each other out so that we have no significant average effect in Table 7. This also relates to our main estimations in Table 5 where we find that the temporary tax reductions are mostly passed to consumers on a one-to-one basis on average, which should not lead to any significant changes in the average retail margins.

7. Conclusion and policy implications

This paper provides empirical evidence on the pass-through of temporary fuel tax reductions in the three largest European economies. The governments in Italy, France and Germany introduced relief packages to mitigate the effects of increasing energy prices in the course of post-pandemic economic recovery and the Russian aggression towards the Ukraine. As a part of those packages, the three countries reduced the fuel taxes (introduced a discount on fuel) for several months in 2022. Since the individual measures have taken place at different points of

time, we apply a staggered DiD design to causally estimate pass-through rates as well as changes in retail margins.

Our results imply a heterogeneous pass-through over time of the fuel tax reductions depending on the country and type of fuel. Nevertheless, we mostly find a full-shifting of the temporary fuel tax reductions meaning the estimated average pass-through rates are close to 100%. This identifies the fuel markets in the three countries as highly competitive, where the consumers enjoy all of the tax reliefs. High pass-through rates can be explained by the general inelastic demand for fuel products and particularly by the high public awareness as well as the threat of policymakers to pursue antitrust measures during the 2022 tax cuts.

A second finding of our paper is that the average pass-through rates are generally higher for gasoline compared to diesel. However, hypothesis tests indicate that, from a statistical perspective, there is no discernible distinction between the pass-through rates for gasoline and diesel. Besides this statistical perspective, the average findings are in contrast to the previous literature, which finds a more inelastic demand for diesel compared to gasoline. This might be explained by the unusual market situation in 2022. The Russian invasion of Ukraine led to a high uncertainty of consumers in the European energy markets, which (among others) resulted in a higher demand for heating diesel, a close substitute for diesel.

Analyzing dynamics associated with time within the framework of an event study reveals differences with regard to the development of the ATTs between countries and types of fuel. The period of time until a full pass-through is reached for the first time differs, and, in addition, different periods of over- and full-shifting are observable.

With respect to the margins, we find no significant effect different from zero on the average retail margins in the three countries. This is in line with the estimated pass-through rates because the tax reductions were mostly passed on to the consumers one-for-one, which does not change the retail margins of the petroleum companies. However, performing an event study design suggests that the petroleum companies have made some positive retail margins at least in the first days after the fuel tax reductions as it has taken some days until the tax reductions have been fully passed-through to the consumers.

A key takeaway from our paper for policymakers is that temporary fuel tax reductions seem to be a suitable measure to lower consumer

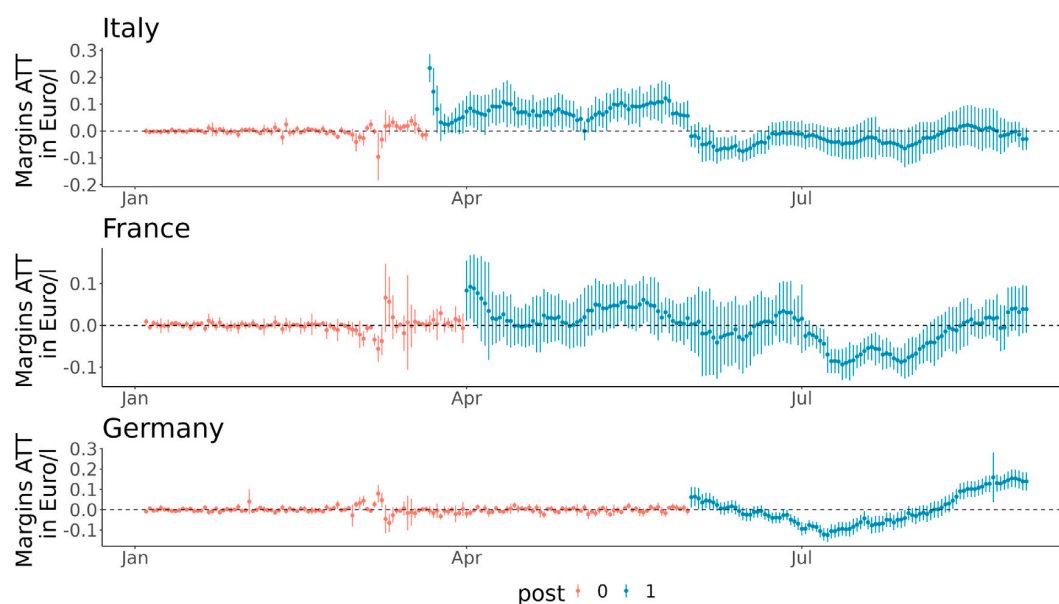


Fig. 6. Event Study of Margins with diesel and all covariates. Bootstrapped (robust) standard errors are clustered on the country and service station chain level. Error bars represent 95% confidence intervals.

prices for diesel and gasoline, even though it may take some time until full pass-through is reached. Hence, the primary goal of the governments to relieve their citizens by achieving lower consumer prices for petroleum products has been met. Whether the corrective goal of a Pigouvian tax or subsidy can be achieved generally depends on whether the consumers also bear the incidence of the measure. In this context, the fuel markets in the three countries seem to be competitive enough so that environmental taxes are passed on to the consumers. However, due to the distributional- and climate-economical shortcomings as well as the relatively high fiscal burden of fuel tax reductions it is debatable whether a temporary fuel tax reduction is a suitable intervention at all.

From a competition policy perspective, our results hardly allow any conclusions to be drawn about whether there are competition restrictions in the fuel market at all. However, the estimated pass-through rates in the three countries imply that the alleged restrictions can at least not hinder a high pass-through of the tax reductions. In general, comprehensive sector analyses by the competition authorities to find the mildest means of competition policy seem to be more appropriate than short-term government interventions in the fuel market.

Apart from already mentioned limitations regarding policy implications, data limitations do not allow to make any statements on welfare effects, as we cannot observe the traded volumes. Furthermore, due to the aggregated price data at service chain level, it is not possible to look at regional effects within individual countries. However, the geographic location of the service stations included in the dataset shows that we observe a balanced geographic coverage, which implies that the average effects within countries are robust. With regard to our observation period and the design of the measures studied, only temporary effects are analyzable. For further studies, it would be interesting to extend the period and also examine the end of the measures and the associated tax increases under the subject of asymmetric pass-through of increasing and decreasing costs, i.e. rockets and feathers. Overall, it is crucial to emphasize that the obtained results are not readily transferable or applicable to other industries. The retail fuel market (and any other market) is characterized by unique features and therefore an own empirical assessment of the pass-through of tax reductions in other industries would be necessary.

Nevertheless, our work provides new and important insights into the transmission of tax reductions in a dynamic and much studied industry, using the most recent methods.

CRedit authorship contribution statement

Chiara Patricia Drolsbach: Software, Validation, Investigation, Data curation, Writing – review & editing. **Maximilian Maurice Gail:** Methodology, Software, Validation, Formal analysis, Investigation, Data curation, Visualization. **Phil-Adrian Klotz:** Conceptualization, Methodology, Validation, Formal analysis, Writing – original draft, Supervision.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Data will be made available on request.

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Appendix A

Economic theory implies that the elasticities of demand and supply as well as the competitive situation in a market determine the level of pass-through. Following [Weyl and Fabinger \(2013\)](#), we denote p as the retail price and t as the quantity tax rate, so that the pass-through rate is given by $\rho = \frac{dp}{dt}$. We further define the elasticity of demand ($\epsilon_D \equiv -(D'p/Q)$) and supply ($\epsilon_S \equiv S'p/Q$). In this framework, [Weyl and Fabinger \(2013\)](#) postulate that the solution of the firm maximization problem can be described by the conduct parameter $\theta = (p - mc(q))/pe_D$. θ maps the degree of competition in a market. For instance, θ is equal to 0 in perfect and Bertrand competition, equal to 1 in a monopolistic

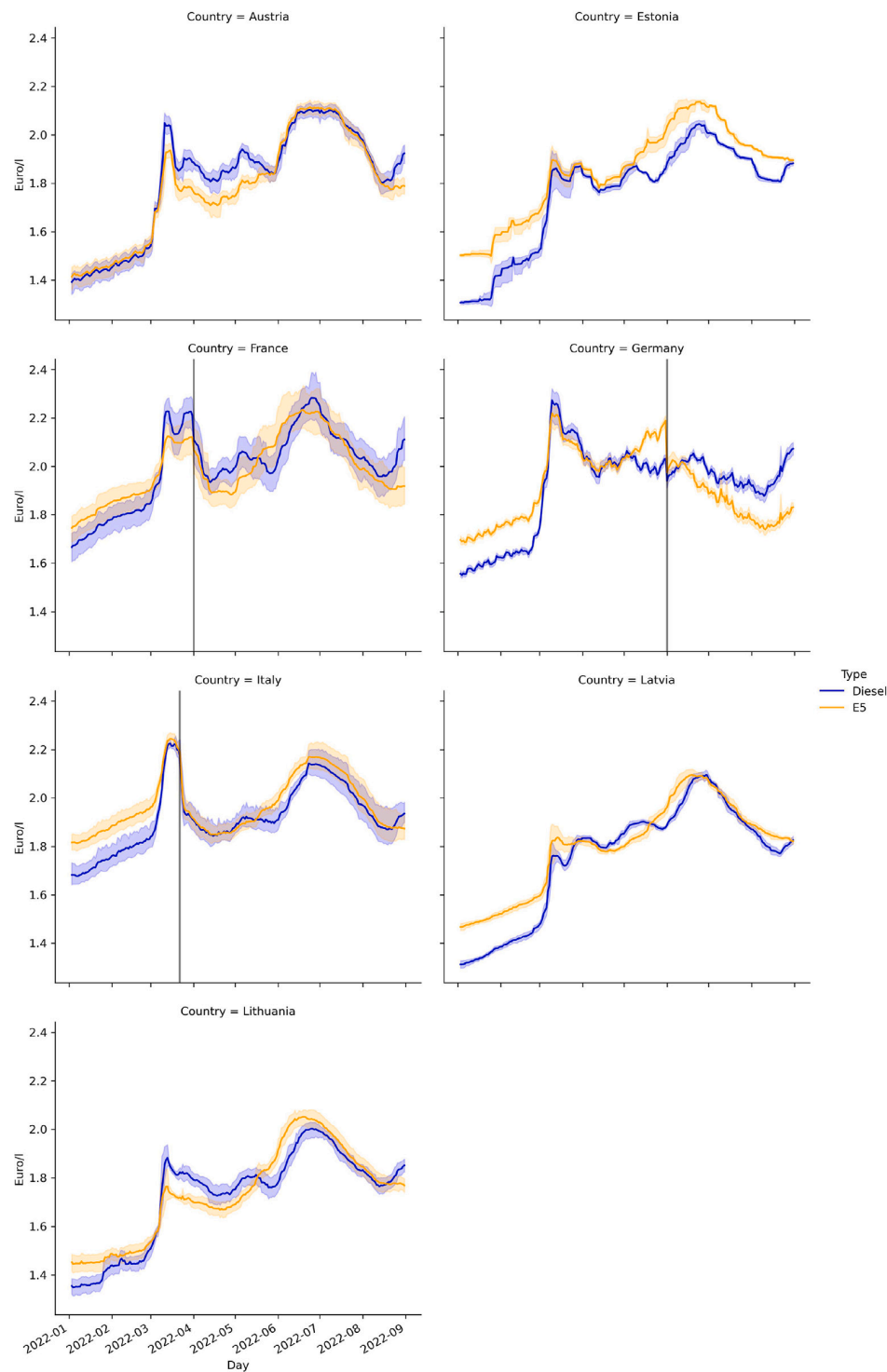


Fig. 7. Development of gasoline and diesel consumer prices for the seven countries in our data set. The vertical lines reflect the introduction of the respective tax reductions. Confidence band is shown to highlight that data varies on the service station chain level.

Table 8

Summary statistics of Austria, France, Germany, Italy, Latvia, Lithuania and Estonia on the service station chain level.

Country	Statistic variable	Count	mean	std	min	25%	50%	75%	max
Austria	Diesel	1446	1.81	0.23	1.28	1.67	1.87	1.99	2.15
	E5	1446	1.78	0.22	1.29	1.61	1.79	1.95	2.16
	Margin of Diesel in Euro/l	1446	0.39	0.13	0.06	0.26	0.40	0.48	0.67
	Margin of E5 in Euro/l	1446	0.27	0.13	−0.01	0.18	0.25	0.37	0.56
	Number of Stations per Chain	1446	196.17	104.28	8.00	117.00	229.50	281.00	312.00
	Relative Margin/Lerner-Index of Diesel	1446	0.34	0.07	0.08	0.29	0.34	0.39	0.50
	Relative Margin/Lerner-Index of E5	1446	0.26	0.08	−0.01	0.21	0.25	0.32	0.43
	Total Imports of Oil and Petroleum Products	1446	987.21	212.86	745.05	779.82	887.39	1,156.51	1,328.22
	Utilization of Capacity	1446	0.55	0.27	0.25	0.26	0.37	0.78	0.93
Estonia	Diesel	1190	1.76	0.22	1.26	1.67	1.83	1.90	2.06
	E5	1188	1.86	0.19	1.45	1.74	1.90	1.98	2.16
	Margin of Diesel in Euro/l	1190	0.34	0.11	0.06	0.24	0.37	0.43	0.55
	Margin of E5 in Euro/l	1188	0.39	0.09	0.13	0.32	0.40	0.46	0.61
	Number of Stations per Chain	1190	55.78	30.58	9.00	34.00	59.00	79.00	95.00
	Relative Margin/Lerner-Index of Diesel	1190	0.31	0.07	0.09	0.27	0.32	0.36	0.43
	Relative Margin/Lerner-Index of E5	1188	0.35	0.05	0.13	0.32	0.35	0.38	0.45
	Total Imports of Oil and Petroleum Products	1190	135.61	24.04	103.00	120.00	131.00	147.00	190.00
	Utilization of Capacity	0							
France	Diesel	1687	2.00	0.18	1.52	1.86	2.01	2.13	2.51
	E5	1687	1.99	0.16	1.60	1.88	1.97	2.11	2.50
	Margin of Diesel in Euro/l	1687	0.44	0.14	0.05	0.34	0.45	0.54	0.84
	Margin of E5 in Euro/l	1687	0.34	0.12	0.00	0.25	0.33	0.42	0.74
	Number of Stations per Chain	1687	426.29	216.77	97.00	197.00	419.00	685.00	736.00
	Relative Margin/Lerner-Index of Diesel	1687	0.37	0.07	0.06	0.33	0.37	0.42	0.52
	Relative Margin/Lerner-Index of E5	1687	0.31	0.07	0.00	0.27	0.31	0.36	0.49
	Total Imports of Oil and Petroleum Products	1687	6,749.68	454.79	6041.00	6,395.00	6,965.00	6,987.00	7,486.00
	Utilization of Capacity	1687	0.73	0.09	0.60	0.69	0.71	0.85	0.90
Germany	Diesel	3615	1.91	0.19	1.51	1.81	1.97	2.03	2.49
	E5	3615	1.91	0.16	1.65	1.77	1.90	2.04	2.40
	Margin of Diesel in Euro/l	3615	0.38	0.12	0.01	0.26	0.40	0.47	0.96
	Margin of E5 in Euro/l	3615	0.26	0.10	−0.07	0.17	0.26	0.34	0.84
	Number of Stations per Chain	3615	706.67	683.04	13.00	188.00	458.00	980.00	2,597.00
	Relative Margin/Lerner-Index of Diesel	3615	0.33	0.06	0.01	0.28	0.33	0.38	0.57
	Relative Margin/Lerner-Index of E5	3615	0.25	0.06	−0.09	0.20	0.25	0.31	0.53
	Total Imports of Oil and Petroleum Products	3615	10,191.52	321.37	9391.28	10,161.25	10,305.77	10,483.69	10,507.72
	Utilization of Capacity	3615	0.91	0.04	0.84	0.89	0.92	0.95	0.96
Italy	Diesel	1928	1.92	0.15	1.62	1.82	1.89	2.03	2.37
	E5	1928	1.97	0.13	1.75	1.86	1.94	2.07	2.35
	Margin of Diesel in Euro/l	1928	0.40	0.14	0.07	0.31	0.40	0.50	0.80
	Margin of E5 in Euro/l	1928	0.33	0.11	0.04	0.24	0.31	0.39	0.71
	Number of Stations per Chain	1928	2,109.50	1511.06	176.00	1,017.50	1,973.00	3,054.25	4,437.00
	Relative Margin/Lerner-Index of Diesel	1928	0.34	0.07	0.07	0.30	0.35	0.39	0.52
	Relative Margin/Lerner-Index of E5	1928	0.30	0.07	0.04	0.26	0.29	0.35	0.49
	Total Imports of Oil and Petroleum Products	1928	6,446.63	540.48	5701.19	5,993.60	6,614.20	6,986.43	7,182.83
	Utilization of Capacity	1928	0.86	0.09	0.70	0.82	0.92	0.93	0.97
Latvia	Diesel	1444	1.75	0.23	1.28	1.51	1.82	1.90	2.13
	E5	1444	1.80	0.19	1.44	1.61	1.83	1.93	2.13
	Margin of Diesel in Euro/l	1444	0.39	0.13	0.06	0.24	0.42	0.47	0.64
	Margin of E5 in Euro/l	1444	0.35	0.09	0.03	0.27	0.35	0.41	0.57
	Number of Stations per Chain	1444	64.63	22.20	30.00	41.00	70.00	88.00	89.00
	Relative Margin/Lerner-Index of Diesel	1444	0.34	0.07	0.06	0.28	0.35	0.39	0.47
	Relative Margin/Lerner-Index of E5	1444	0.32	0.05	0.03	0.29	0.32	0.35	0.44
	Total Imports of Oil and Petroleum Products	1444	184.64	49.18	125.43	154.16	180.90	202.69	298.02
	Utilization of Capacity	0							
Lithuania	Diesel	1205	1.73	0.20	1.29	1.55	1.79	1.85	2.04
	E5	1196	1.74	0.19	1.38	1.58	1.74	1.88	2.10
	Margin of Diesel in Euro/l	1205	0.37	0.10	0.10	0.28	0.38	0.45	0.59
	Margin of E5 in Euro/l	1196	0.27	0.10	−0.02	0.19	0.25	0.34	0.53
	Number of Stations per Chain	1205	81.40	25.40	44.00	74.00	76.00	91.00	122.00
	Relative Margin/Lerner-Index of Diesel	1205	0.33	0.06	0.10	0.29	0.33	0.38	0.45
	Relative Margin/Lerner-Index of E5	1196	0.26	0.07	−0.02	0.22	0.26	0.31	0.41
	Total Imports of Oil and Petroleum Products	1205	747.42	245.31	327.60	438.80	764.90	1,059.60	1,061.10
	Utilization of Capacity	1205	0.81	0.35	0.20	0.24	1.02	1.06	1.07

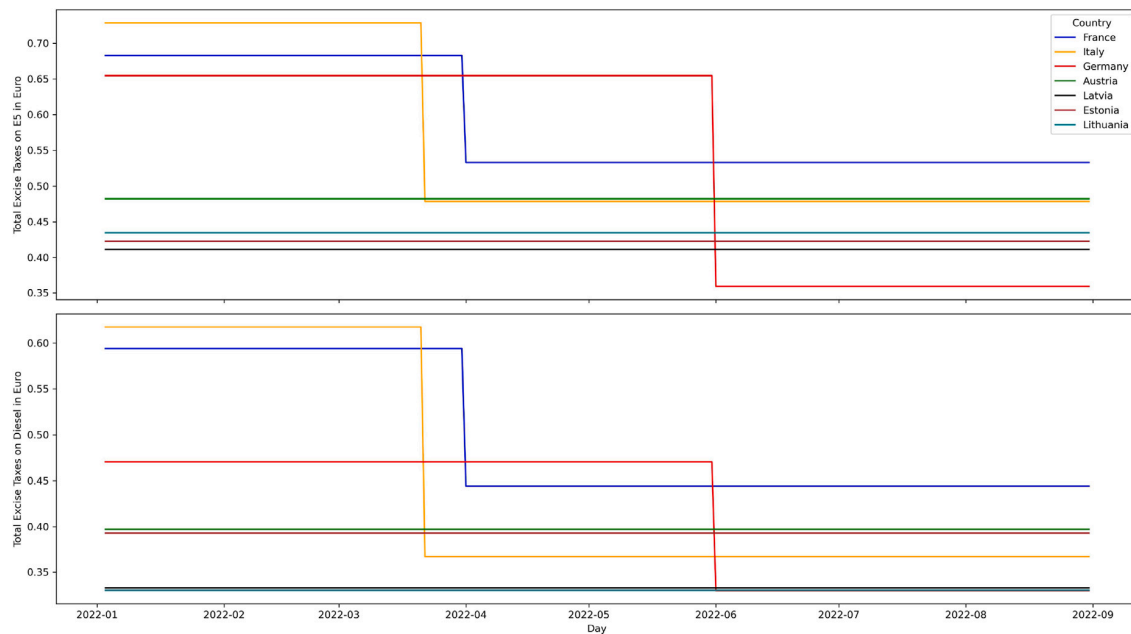


Fig. 8. Development of gasoline and diesel excise duties (inclusive of further duties) for the seven countries in our data set.

market, and equal to $1/n$ in Cournot competition. Then, the pass-through rate ρ is independently of the specific model given by

$$\rho = \frac{1}{1 + \frac{\theta}{\epsilon_D} + \frac{\epsilon_D - \theta}{\epsilon_S} + \frac{\theta}{\epsilon_{ms}}} \quad (\text{A.1})$$

Aside from the conduct parameter θ , formula (A.1) implies that the pass-through of a marginal cost increase also depends on the elasticity of demand ϵ_D , the elasticity of the inverse marginal cost curve (the elasticity of supply)³⁹ ϵ_S , the curvature of the demand function ϵ_{ms} ,⁴⁰ and the variation of θ in changes of production ϵ_θ .⁴¹

Even though formula (A.1) suggests that the sign and magnitude of the pass-through is ambiguous, we can simplify the expression for ρ in some special cases. If there is perfect competition in a market ($\theta = 0$), then $\rho = \frac{1}{1 + (\epsilon_D/\epsilon_S)}$ so that the pass-through only depends on the ratio of demand and supply elasticity. More generally, if the marginal cost were constant, demand were linear, and θ were constant, expression (A.1) would simplify to $\rho = 1/(1 + \theta)$. A rise in the conduct parameter θ (less competition) would lead to lower pass-through in this situation (Genakos and Paglierio, 2022). For instance, in a monopolistic market ($\theta = 1$) the pass-through would be lower ($\rho = 0.5$) compared to a market with perfect competition ($\theta = 0$) where we would have full pass-through ($\rho = 1$).

However, in general, the sign of the effect of an increase in the conduct parameter θ on the pass-through remains ambiguous. This is

³⁹ The monopolist determines the price based on demand and its costs, there is, just like in an oligopoly, no supply curve and accordingly, no supply elasticity in the sense of perfect competition.

⁴⁰ Given by $\epsilon_{ms} = \frac{ms}{ms'q}$, where ms is the negative of the marginal consumer surplus ($ms = -p'q$). If demand is linear, then $\epsilon_{ms} = 1$, if concave, $\epsilon_{ms} < 1$, and if convex, $\epsilon_{ms} > 1$ (and the opposite is also true) (Genakos and Paglierio, 2022).

⁴¹ Given by $\epsilon_\theta = (\theta/q)(d\theta/dq)$.

especially the case for an oligopolistic market, which should be the most appropriate market form to model the fuel industry in Europe. The impact of the conduct parameter on the pass-through can either be positive or negative, depending on the actual market situation. Under certain assumptions also pass-through rates larger than one are possible. Hence, the impact of the intensity of competition on the pass-through rate in an oligopolistic market remains an empirical problem (Genakos and Paglierio, 2022).

Appendix B

To compute the daily average retail margins for the seven countries in our data set, we subtract a fuel share of the crude oil price (major input cost) as well as the country-specific taxes and duties (see Montag et al. (2021)). For each country in our raw data set, we observe a daily average gross consumer price. In a first calculation step, we calculate the average consumer prices without VAT taxes for every day and country (see Footnote 9). To get the daily average net price, we then also subtract the excise duties for the individual countries (see Table 3). Thereby, for the treated countries we have to differentiate between the period before and after the fuel tax reductions.

In a final step, we have to subtract the input cost of crude oil (Brent) from the daily net price. Therefore, we use the information that around 54% of the Brent oil price per barrel corresponds to the production of 19 gallons of gasoline and around 34% to the production of 12 gallons of diesel.⁴² We further transform these measures into the input cost per liter of gasoline and diesel. The retail margins of gasoline and diesel are then computed as the average gross consumer price per liter adjusted to VAT taxes and excise duties minus the share of crude oil price per liter of a corresponding fuel product.

⁴² See

<https://www.eia.gov/energyexplained/oil-and-petroleum-products/refining-crude-oil-inputs-and-outputs.php>. (Last accessed: October 19, 2022).

Appendix C

Tables 8 and 9 and Figs. 7–12 provide additional summary statistics, illustration of relevant variables and event studies used in our analysis.

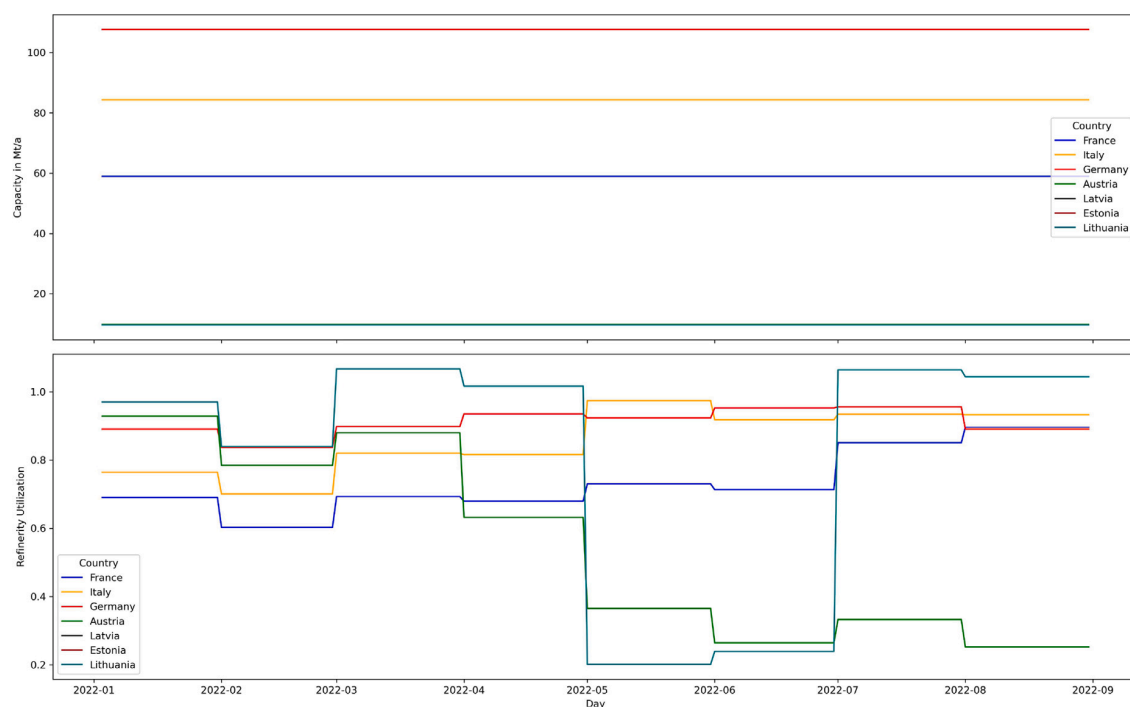


Fig. 9. Development of Capacity and Refinery Utilization.

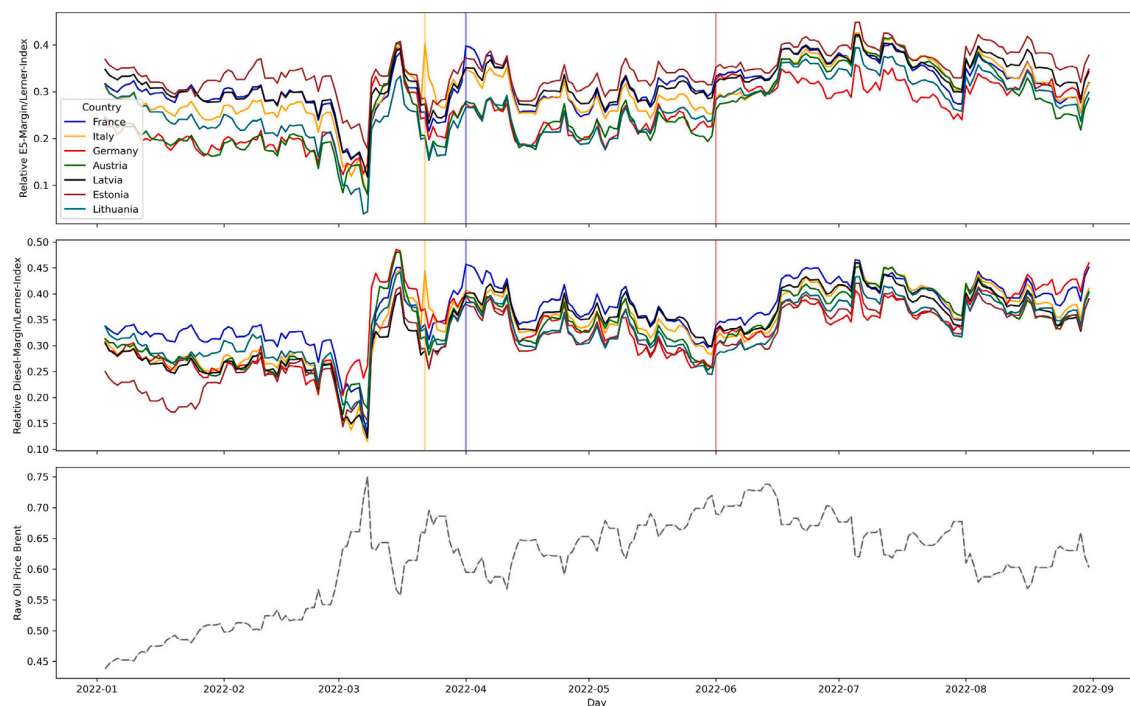


Fig. 10. Development of relative retail margins for gasoline (upper) and diesel (middle). The vertical lines reflect the introduction of the respective tax reductions in Italy (March 22, yellow), France (April 1, blue), and Germany (June 1, red). Brent prices (lower) in Euro per Liter is denoted in dashed gray.

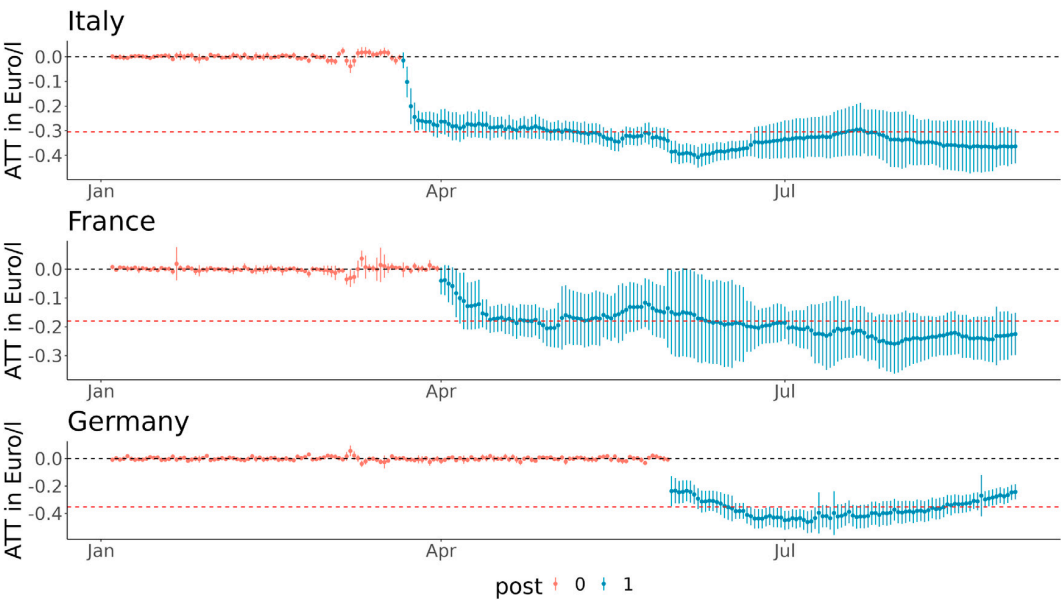


Fig. 11. Event Study of prices with gasoline (E5) and no covariates. Bootstrapped (robust) standard errors are clustered on the country and service station chain level. Error bars represent 95% confidence intervals.

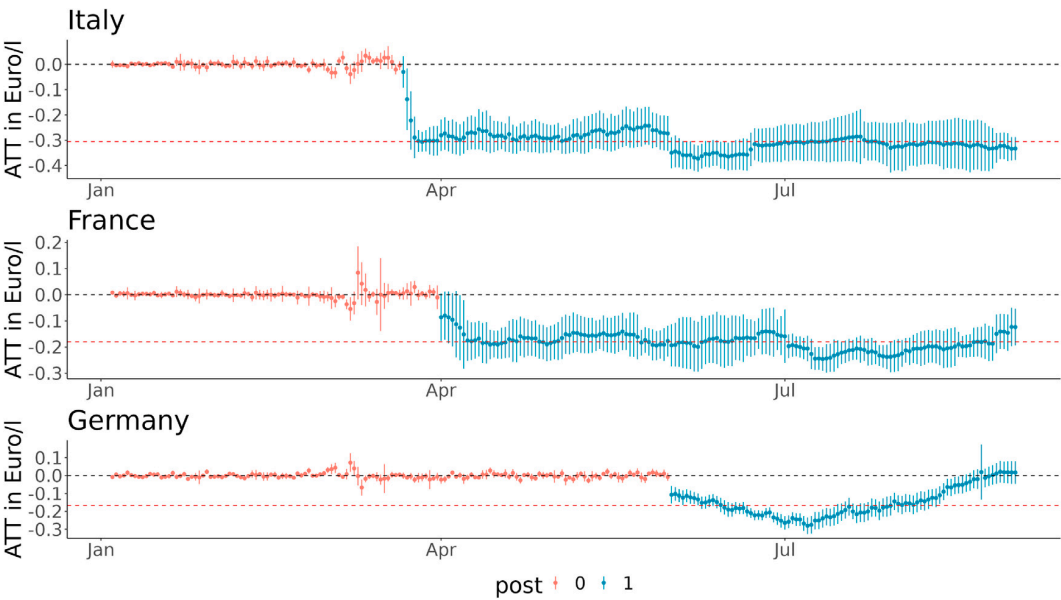


Fig. 12. Event Study of prices with diesel and no covariates. Bootstrapped (robust) standard errors are clustered on the country and service station chain level. Error bars represent 95% confidence intervals.

Table 9

Summary statistics for number of service stations per chain present in the data. Overall coverage: Austria 1177/2748 $\approx$ 43%, Germany 10600/14500 $\approx$ 73%, France 2984/11151 $\approx$ 27%, Italy 16876/21700 $\approx$ 78%, Estonia 276/491 $\approx$ 56%, Latvia 388/605 $\approx$ 64% and Lithuania 407/718 $\approx$ 56%. Visual inspection of the stations displayed on the map provided by Fuelo reveals the extent of geographical coverage within the national markets.

Source: Fuelo.net, <https://de.fuelo.net/gasstations?lang=en>.

Country	Provider	Number of stations per chain
Austria	Eni	312
	BP	281
	Shell	248
	OMV	211
	AVIA	117
	Mol	8
Estonia	Terminal	34
	Premium7	9
	NESTE	59
	Circle K	79
	Olerex	95
France	Esso	474
	Shell	97
	Eni	197
	BP	376
	Total	419
	Avia	685
	E.Leclerc	736
Germany	Aral	2597
	Shell	1791
	Esso	1112
	Total	951
	Avia	980
	Gulf	47
	ED	107
	Tamoil	13
	OMV	332
	HEM	383
	agip	437
	Tankpool24	458
	Star	572
	Jet	632
	Westfalen	188
Italy	Esso	2394
	Repsol	176
	IES	206
	TotalErg	1288
	Tamoil	1552
	Q8	2697
	Eni	4437
	IP	4126
Latvia	VIRŠI	70
	Viada	89
	Circle K	88
	NESTE	70
	LN	41
	Astarte	30
Lithuania	Viada	122
	Circle K	91
	Baltic Petroleum	74
	EMSI	44
	NESTE	76

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3.2 E-book Pricing Under the Agency Model: Lessons from the UK

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- 15th Annual CRESSE Conference on Competition and Regulation, 2021, Heraklion-Crete, Greece.
- 48th Annual Conference of the European Association for Research in Industrial Economics (EARIE), 2021, Bergen, Norway.



E-book Pricing Under the Agency Model: Lessons from the UK

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Abstract

This paper empirically analyzes the relation between the widely used agency model and retail prices of e-books sold in the UK. Using a unique cross-sectional data set of e-book prices for a large number of book titles across all major publishing houses, we exploit cross-genre and cross-publisher variation to examine the interplay between the agency model and e-book prices. Since the genre information is ambiguous and even missing for some titles in our original data set, we also apply a latent Dirichlet allocation (LDA) approach to determine detailed book genres based on the book's descriptions. Using propensity score matching, we find that retail prices for e-books sold under the agency model tend to be systematically lower than book titles with similar characteristics sold under the wholesale model, approximately 20%. This result varies with the exact sales rank of a book and is driven by the so-called *long tail* books. Our results are robust across various regression specifications and double machine learning techniques.

Keywords E-books · Agency agreements · Vertical restraints · Amazon · Propensity score matching

JEL Classification D12 · D22 · L42 · L82 · Z11

1 Introduction

The rise of the Internet—and platform markets more specifically—has accelerated the adoption of so-called agency arrangements. Under this model, suppliers pay retailers sales royalties, and crucially, suppliers determine the final retail prices. On the contrary, in many traditional retail markets, suppliers charge retailers wholesale prices and retailers set final prices to consumers (*wholesale model*). Even though agency arrangements are also used in some of these conventional markets (e.g., newspapers sold at kiosks), this form of a vertical contract is especially prevalent in online markets (e.g., Amazon Marketplace, Apple App

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Store, eBay Buy It Now). It is also frequently employed in the e-book market, which is the focus of this paper.

In general, books are considered experience goods, as consumers can ascertain their quality only after reading them (Nelson 1970; Reimers and Waldfogel 2021). While some countries, such as Germany, France, or Japan, implement fixed book price (FBP) systems, others, including the UK and the USA, do not. Fixed book prices represent a form of resale price maintenance (RPM) where publishers set retail prices, thereby restricting or eliminating price competition among retailers.¹ The primary motivation for introducing FBP systems is typically to ensure a broad and diverse supply of books, available through a geographically wide network of bookstores.

With the advent of e-books, countries that maintain FBP systems for print books faced the decision of whether to extend this existing legislation to e-books. The applicability of the same cultural policy arguments and legal considerations is debatable, especially given that a geographically wide network of bookstores is irrelevant for e-books. Nevertheless, eight OECD countries with fixed prices for printed books have also extended these fixed prices to e-books, and no country is known to have RPM regulations solely for e-books without corresponding rules for print books (Poort and van Eijk 2017). However, in many countries without fixed prices for e-books (such as the UK), this digital product is partly sold under the agency model (Gilbert 2015), which has similar effects like RPM between a manufacturer and a retailer.²

In 2010, Apple, in cooperation with the six largest publishing houses, was the first to adopt the agency model for e-books. This adoption occurred in response to Amazon's aggressive pricing strategy to gain market share. In April 2012, the Department of Justice (DOJ) sued Apple and five of these six publishing houses for conspiring to raise e-book prices by using the agency model in conjunction with most-favored nation (MFN) clauses.³ Three of the publishers settled shortly after the antitrust case was filed, with the other two settling later the same year. These settlements stipulated that the five publishers could not restrict retailers' ability to set e-book prices for a period of 2 years. Furthermore, the settlements prohibited the use of MFN clauses for 5 years.

Empirical evidence on the price effects of RPM, fixed book prices, and the agency model is scarce. While systematic empirical evidence on RPM is limited to case studies (MacKay and Smith 2017; Ippolito 1991), the only study empirically investigating the agency model's price effect is De los Santos and Wildenbeest (2017). They utilized data on e-book prices for bestselling titles from 2012 and 2013, exploiting the Apple antitrust case as an exogenous shock. Their findings indicate that the agency model, in combination with MFN clauses, led to an average price increase of 8–18% (depending on the retailer).⁴ However, De los Santos

¹ Presently, 15 OECD countries have a regulation for fixing the prices of printed books. The fixed prices for printed books typically last 18–24 months after a book has been published.

² In particular in combination with most-favored nation (MFN) clauses, the agency model can have economically similar effects where upstream firms control the retail prices (see Sect. 2).

³ See *USA v. Apple Inc.*, 12 Civ. 2826 (DLC). MFN clauses determine that the retail price set by a publisher of a given product through one retailer must be no higher than the retail price set by this publisher through a competing retailer. Consequently, a MFN clause ensures that if a retailer increases the commission for one publisher, the retail price remains consistent across other retailers. This can incentivize retailers to demand higher fees, ultimately leading to increased retail prices (Johnson 2017).

⁴ In a subsequent paper, De los Santos et al. (2021) analyze the reversion from wholesale to agency pricing (following the two-year ban imposed by the Apple case) and report qualitatively similar results for the Amazon platform.

and Wildenbeest (2017) could not disentangle the price effect of the agency model from that of the MFN clauses, as the Apple settlement simultaneously banned both. This limitation represents a key difference from our analysis, as we are able to estimate the price effect of the agency model in the absence of MFN clauses.

The goal of this study is to analyze the price effect of the agency model using a larger and more detailed data set, particularly by not solely incorporating bestselling book titles. We also examine whether similar effects emerge in the absence of an alleged conspiracy, as observed in the Apple case. The internet provides consumers access to a broader range of book titles beyond just the popular ones (bestselling titles), and several authors have demonstrated the importance of so-called *long tail* titles in markets for creative products (e.g., Aguiar and Waldfogel 2018; Brynjolfsson et al. 2003).⁵ While a unanimous general definition for these goods is lacking, Brynjolfsson et al. (2011) define the *long tail* as the 50% least sold products. With respect to books, Brynjolfsson et al. (2003) introduced 100,000 book titles as an absolute cutoff point for long tail, corresponding to the typical inventory and store size of large brick-and-mortar (B&M) bookstores at that time. We adopt this definition and supplement it by also considering 200,000 book titles, which represents the current inventory and store size of the largest bookstore in London today.⁶

Our cross-sectional data set comprises prices for 12,001 e-books published on Amazon UK between 2010 and 2020. Utilizing data from Amazon ensures an extensive market coverage, given that Amazon accounted for approximately 50% of overall UK book sales (and 87% of UK e-book sales) in 2018.⁷ We exploit publisher and book genre variation to estimate the effect of the agency model, beginning with a standard OLS regression approach. We further estimate this effect for different subsets of e-book sales ranks to differentiate between best-selling, middle tail, and long tail books. Subsequently, we apply various robustness checks, notably employing a propensity score matching (PSM) design. Given that the assignment of vertical contract types for e-books was very likely not a random process, PSM helps mitigate selection bias arising from observed book characteristics. This method enables us to match e-books sold under the agency model with comparable e-books sold under the wholesale model based on a range of book characteristics.

The main results from our standard OLS regression approach and the PSM design indicate that e-books sold under the agency model on *Amazon.co.uk* are, on average, approximately 20% cheaper than e-books with very similar characteristics sold under the wholesale model. This effect is heterogeneous, varying with a book's sales rank, i.e., whether a book title is a bestselling or a *long tail* book, and is, according to our findings, almost exclusively driven by *long tail* books. Various robustness checks qualitatively confirm this main finding. This result contradicts the empirical outcome reported by De los Santos and Wildenbeest (2017), but aligns with explanations proposed by the theoretical literature on agency versus wholesale models (Johnson 2020; Foros et al. 2017; Gaudin and White 2014). We discuss the theoretical mechanisms of the agency model and the key differences between our analysis and that of De los Santos and Wildenbeest (2017) in the next section.

The remainder of the paper is structured as follows. Section 2 discusses the related literature. Section 3.1 describes the data, followed by Sect. 3.2 presenting descriptive statistics. Section 3.3 outlines our main estimation strategy. Our primary results are presented in Sect. 4,

⁵ For instance, Brynjolfsson et al. (2003) estimated that the benefit consumers obtain from access to *long tail* book titles may be as high as \$1.03 billion alone in the year 2000.

⁶ The Waterstones Piccadilly Bookshop in London stocks over 200,000 books at any time (see <https://www.timeout.com/london/shopping/waterstones-piccadilly-1>, last accessed July 15, 2025).

⁷ See Nielsen (2018), "Books & Consumers - UK Industry Standard Report Q4 2018," p. 13.

with extensions provided in Sect. 5. Section 6 details our robustness checks. Finally, 7 concludes and outlines the paper's contribution.

2 Related Literature

Our article contributes to several strands of literature. First and foremost, it relates to studies investigating the competitive effects of the agency model. While the empirical literature on the agency model's economic effects remains rather scarce (apart from the two studies by De los Santos and Wildenbeest 2017 and De los Santos et al. 2021), several recent theoretical papers have analyzed differences in retail prices between the agency and the wholesale model.

Our paper differs from the study by De los Santos and Wildenbeest (2017) in three important ways. First, while De los Santos and Wildenbeest (2017) utilize the court decision in the Apple antitrust case (see Footnote 3) as an exogenous shock in their approach, our findings do not rely on an alleged conspiracy. Second, our empirical analysis incorporates not only bestselling but also *long tail* book titles. We show that the agency effect is heterogeneous with respect to the book sales rank. Third, De los Santos and Wildenbeest (2017) could not isolate the *pure* price effect stemming from agency agreements, as these were implemented in conjunction with MFN clauses during their sample period. In contrast, our analysis can disentangle the effect of the agency model from that of MFN clauses. This is possible because Amazon settled with the EU Commission in 2017, agreeing not to include MFN clauses for any e-book distributed in the European Economic Area (EEA) for the subsequent five years (see AT.40153 E-book MFNs and related matters (Amazon), Decision dated May 4, 2017. We have scraped the Amazon data in 2020).

Our empirical analysis is supported by several theoretical papers analyzing the price effect of the agency model across different market designs. Foros et al. (2017), for instance, model imperfect competition at both the upstream and downstream levels, finding that the agency model's price effect hinges on the relative intensity of competition between these two market levels. Specifically, retail prices will be lower under agency agreements if consumers' price sensitivity between goods (e-books) exceeds their price sensitivity between platforms. This theoretical insight provides an important foundation for our empirical results, particularly given the assumption that competition among publishers should be greater than among retailers—an assumption supported by Amazon's quasi-monopolistic power in the UK e-book market (see Footnote 7). Furthermore, our findings suggest that the negative price effect of the agency model is driven by *long tail* e-book titles, which typically exhibit higher consumer price sensitivity compared to bestselling titles. Nevertheless, Foros et al. (2017) also demonstrate that the agency model can be anti-competitive (leading to higher retail prices) when adopted by platforms on a market-by-market basis. They note, however, that widespread adoption may not always occur due to a prisoner's dilemma among platforms. In this context, MFN clauses can circumvent this prisoner's dilemma by inducing industry-wide adoption of agency pricing, which, in their model, would not otherwise occur, thus rendering them anti-competitive.

Johnson (2020) employs a similar model featuring imperfect competition at upstream and downstream levels, but further assumes that consumers are locked into a single platform. The author finds that when publishers set consumer prices instead of retailers (agency model), prices might be higher initially but lower in later periods. This outcome contrasts with the wholesale model, where retailers strategically set low initial prices to attract and lock in consumers, only to raise them once a sufficient consumers base is established. Lu (2017) utilizes

a bilateral duopoly model with product differentiation in both the upstream and downstream markets to demonstrate that the agency model can benefit consumers—relative to the wholesale model—through lower retail prices due to the elimination of double marginalization. Condorelli et al. (2018) present a theory that endogenizes the choice between agency and wholesale models in an environment where the retailer possesses privileged information about consumer valuations. Their theoretical welfare analysis suggests that the agency model can be welfare enhancing, implying that a ban on agency agreements might decrease welfare. Collectively, these three papers provide robust theoretical foundations that align with our empirical findings.

Another strand of the theoretical literature on agency models focuses on situations where complementary devices are necessary for consuming the main products (e.g., an e-book reader for e-books). Abhishek et al. (2016) find that agency selling is more efficient than the wholesale model, leading to lower retail prices when complementary devices are absent. However, prices under the agency model can sometimes be higher if positive externalities arise from sales of associated products (such as e-readers). Gaudin and White (2014) highlight that a retailer's incentive to set high prices increases when they possess monopolistic control over a complementary device. This was initially relevant in the e-book market when Amazon e-books were exclusively readable on Kindle devices. However, they suggest these effects would have largely dissipated once Amazon released Kindle apps for other platforms in 2009, which is within the relevant period for our empirical analysis.

Our paper also relates to the literature on best price clauses (BPCs) at online platforms. For instance, such BPCs are frequently used by online travel agencies (OTAs), obliging hotels not to offer lower prices on distribution channels other than the respective OTA (narrow BPCs typically cover only the direct channel, wide BPCs extend to other OTAs) (Hunold et al. 2018). Several national competition authorities in Europe have intervened against these clauses, ultimately reaching diverse decisions (Hunold 2017). Most recent empirical literature on this topic indicates that the elimination of (narrow) BPCs primarily incentivized chain hotels to charge the lowest price on the direct channels and hotels also began expanding their room availability on OTAs such as *Booking.com* (Hunold et al. 2018; Mantovani et al. 2021; Ennis et al. 2023).

More generally, our article also relates to the broader literature on RPM. RPM can lead to lower retail prices by internalizing vertical externalities, such as double marginalization (Spengler 1950; Tirole 1988), and RPM regimes can also lead to a larger number of B&M stores compared to regimes with free prices (Dearnley and Feather 2002; Davies et al. 2004). In contrast, Rey and Stiglitz (1988, 1995) show that vertical restraints eliminating intra-brand competition can also serve to mitigate inter-brand competition, thereby becoming anti-competitive. This perspective aligns with Rey and Vergé (2010), who argue that RPM can facilitate industry-wide monopoly pricing, contingent on the extent of potential competition and firms' influence in contractual negotiations. Moreover, several studies find that RPM can be utilized to correct for service externalities (Mathewson and Winter 1984; Perry and Porter 1986; Winter 1993). However, Hunold and Muthers (2017) point out that minimum RPM may distort the allocation of services toward the high-price products of the manufacturer with greater market power.

Finally, our article also contributes to the nascent literature on machine learning (ML) and text mining approaches in economics. Varian (2014) and Athey and Imbens (2019) provide overviews of important ML methods. Wang et al. (2019) employ the *Learning to Place* ML approach to predict book sales, identifying the publishing house as a strong driving factor across all genres. We utilize double machine learning (DML) techniques, similar to the approach of Knaus (2021), who applied DML to examine the impact of children's musical

practice on cognitive skills and school performance. For a broad overview on text mining approaches, see Gentzkow et al. (2019). In this article, we apply a latent Dirichlet allocation (LDA) model to determine book genres by analyzing book descriptions and expert reviews (e.g., Larsen and Thorsrud 2019).

3 Data and Methodology

3.1 Data Set Construction

The data generating process is structured as follows. We scraped the *Amazon.co.uk* webpage for book prices and other book characteristics, with data collection starting in mid-February 2020 taking 2 weeks. To do so, we utilized an a priori list of publishing houses, publishers, and imprints derived from a historical *Sunday Times* bestseller list. This procedure ensures that our sample predominantly comprises books from publishers with a relatively high market share and allows us to cover a broad range of titles.⁸

Our raw data set comprises approximately one million observations, with each containing nearly all available details on the Amazon website, such as prices, formats, descriptions, ratings, and reviews. For each unique book title, there are up to three entries if all formats (hardcover, paperback, and e-book) are available. However, this raw data set includes numerous entries that are either duplicates or irrelevant to our analysis, such as outdated editions or those with incomplete information on specific book characteristics essential for our empirical analysis. Thus, we cleanse the data by retaining only the latest revised version of each title and including only those with available retail prices from our scraped data. Following this cleansing process, our working data set consists of 77,629 observations (encompassing all three book formats), respectively 47,161 unique book titles. For our empirical analysis (see Sect. 4), we include only e-book titles for which all explanatory variables (book characteristics) are available (see Table 8 in Appendix 1), resulting in a final working dataset of 12,001 e-book titles.

Our variables of interest are the retail price, which is the price a consumer must pay for a given book, and the treatment variable *Agency*. The latter is coded as one if the Amazon webpage for a book title displays a text field containing “This price was set by the publisher” and zero otherwise.⁹ Additionally, our data set contains several control variables for the empirical analysis. These include various book characteristics such as book format, genre, and an e-book size (in KB); book review metrics, including star rating and the number of consumer and expert reviews; information on the author and publisher; and other attributes like the publication date and the recommended retail price (RRP). Table 8 in Appendix 1 provides descriptions for all variables included in our data set.

We also matched the data set obtained from Amazon with a historical *Sunday Times* bestseller list to identify authors who have previously written a bestselling book. This variable is important for our empirical analysis, as a bestselling author’s name serves as an important quality signal for the readers.

Each book is a unique product, typically written by a single author and published by a single publisher. Consequently, books are heterogeneous goods, making direct value comparisons between individual titles unfeasible. To appropriately account for this heterogeneity, it is therefore essential to control for book genres in our empirical analysis. However, genre

⁸ This bestseller list contains entries from January 2006 until the end of March 2019.

⁹ See Appendix Fig. 6 for an example from the Amazon webpage.

information on the Amazon webpage is often ambiguous or entirely unavailable for some titles.¹⁰ Thus, we employ a Latent Dirichlet Allocation (LDA) approach to derive book genres from the descriptions and reviews of the individual books available on the Amazon webpage. This control variable should be appropriate to capture specific effects across genres. We describe this text mining approach in Appendix 2.

3.2 Descriptive Statistics

Our sample comprises 47,161 unique book titles published on *Amazon.co.uk* by Bloomsbury, Faber, Hachette, HarperCollins, Oxford, Pan Macmillan, Penguin Random House, Scholastic, Simon & Schuster, and a group of smaller publishers from 2010 to 2020. While the full data set contains 77,629 observations—reflecting the availability of up to three formats per title—our empirical analysis (detailed in Sect. 4) is based on 12,001 e-book titles. This subset specifically excludes titles with missing book characteristics, as discussed in Sect. 3.1. Although the primary focus of our empirical analysis is on the price of e-books, this section also presents descriptive statistics for hardcover and paperback formats to illustrate the relationships among all three.

Table 9 in Appendix 1 offers descriptive statistics for the variables used in our empirical analysis, summarized by publisher. Beyond e-book retail prices and the RRP, we observe several characteristics for each book title, including its Amazon sales rank, customer ratings, number of customer and expert reviews, and page count. As Table 9 illustrates, e-books from Scholastic exhibit the lowest average retail price, while those e-books from Bloomsbury have the highest mean prices. Additionally, Hachette titles show the lowest average sales rank, and the books published by Simon & Schuster exhibit the highest average number of customer reviews. Most other book characteristics are largely similar across publishers.

Figure 5 in Appendix 1 depicts the frequency distribution of the retail prices for e-books (top), paperbacks (center) and hardcover books (bottom) below £100. Apparently, e-book prices range from £0.25 and £10; paperback prices primarily concentrate in the £10–£20 interval; and hardcover prices are considerably higher. While the distributions of e-books and paperbacks are relatively more compressed, hardcover prices exhibit higher volatility. Finally, all three price distributions exhibit significant mass points at candidate focal points (e.g., £0.49 (e-books), £9.99 (paperback), and £15.99 (hardcover)).

The e-books of individual publishers are sold under different pricing arrangements. Amazon states on its product pages whether the respective publisher has set the price of an e-book. Figures 6 and 7 in Appendix 1 provide examples of this through screenshots of the e-book *Elon Musk: How the Billionaire CEO of SpaceX and Tesla is Shaping our Future* and the e-book *Pulse*. In Fig. 6, the first box on the right-hand side of the Amazon webpage displays the text “price was set by the publisher,” indicating an example for the usage of the agency model. In contrast, this information is missing in Fig. 7, meaning Amazon sets the retail price for this e-book, which represents an example of the wholesale model.

Figure 1 visualizes the distribution of e-book retail prices across publishers. Prices are clearly more dispersed for book titles published by Bloomsbury and Oxford, whereas the other major publishers predominantly have books in the range up to £20. The group of smaller publishers has a significantly larger fraction of e-books in the cheapest price range (around £0.49). In Appendix 2, this figure is presented with assigned book genres (see Fig. 13).

Table 1 illustrates the distribution of the pricing arrangements used by individual publishers for the e-books in our sample. In this table, a value of one in the *Agency* column indicates

¹⁰ In Sect. 4, we also present a specification using Amazon’s reported genre information as a control variable.

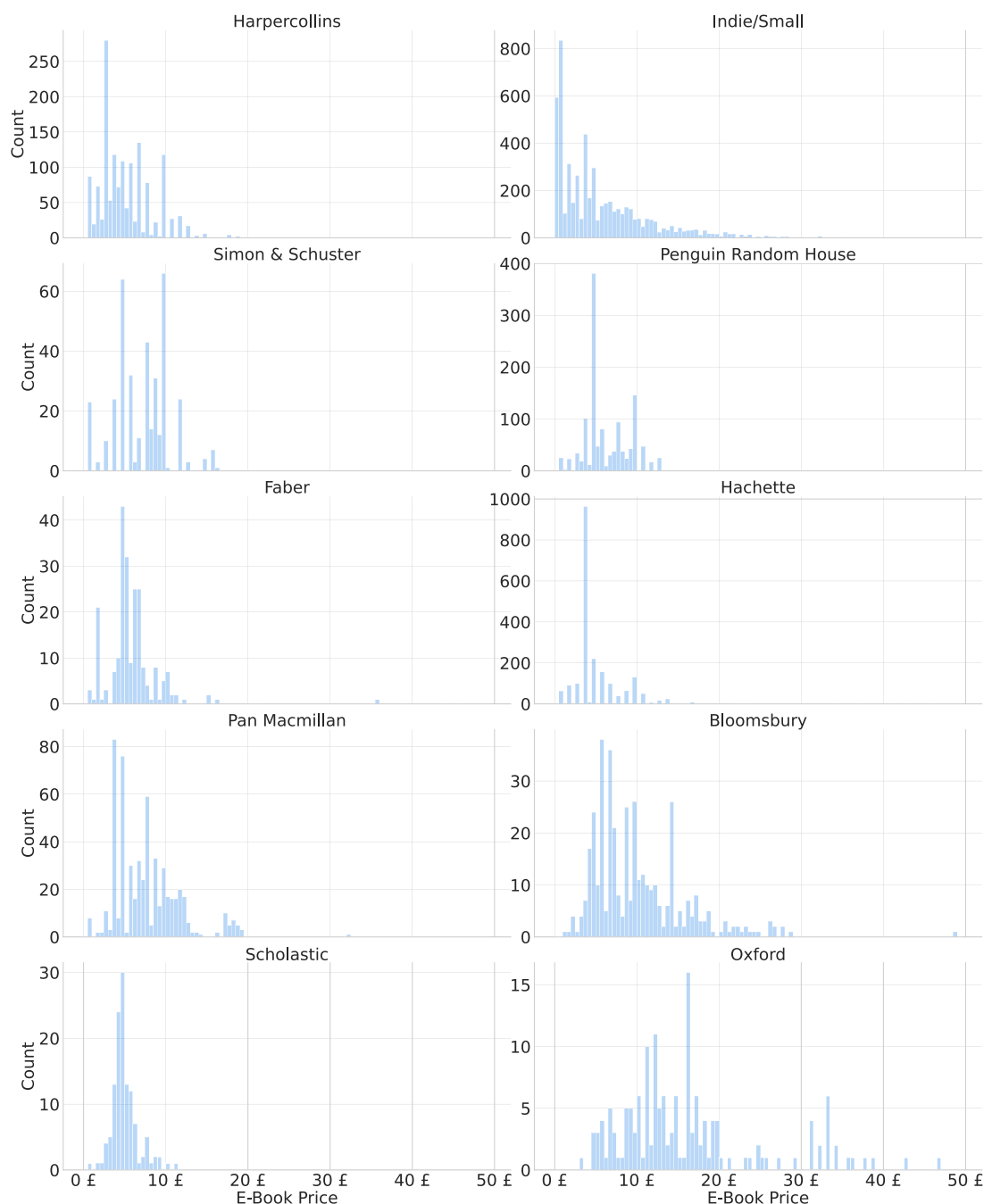


Fig. 1 Prices for e-books grouped by publishers. The interval size for each bar is 1 Pound. For illustration purposes the figures are censored at 50 Pound

that book titles are sold under the agency model, while zero denotes the wholesale model. The table shows that all e-books in our data set published by the large publishing houses Hachette, HarperCollins, Penguin Random House and Simon & Schuster are sold under the agency model on Amazon. Pan Macmillan, however, uses the wholesale model for some of its American imprints' e-books. For e-books in our sample published by Bloomsbury, Faber, Oxford, Scholastic and most smaller publishers, only the wholesale model is used.

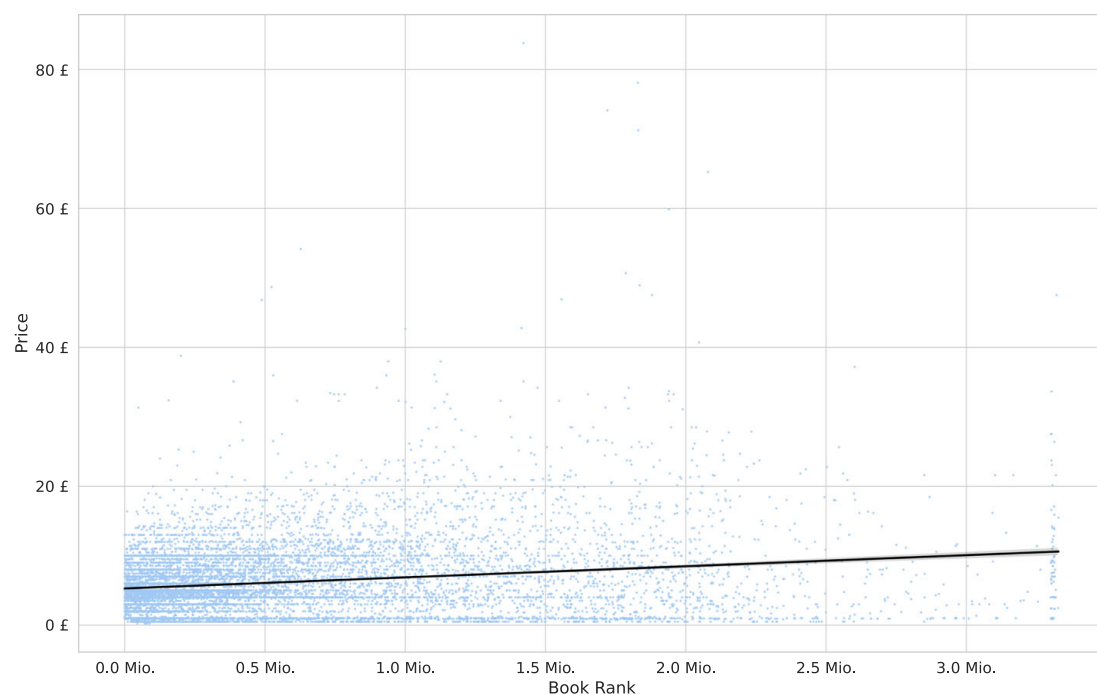
Finally, we illustrate the relationship between retail prices for e-books and their book sales rank on Amazon, as depicted in Fig. 2 via a scatter plot with a simple regression line. Notably,

Table 1 Distribution of the agency variable by publishers

Publisher	Agency	Amount	Percentage	Mean price
Bloomsbury	0	397	100%	10.37
Faber	0	223	100%	5.84
Hachette	1	2,073	100%	5.31
Harper Collins	1	1,469	100%	5.42
Small Pub.	0	4,155	77.37%	6.81
	1	1,215	22.63%	2.32
Oxford	0	161	100%	16.71
Pan Macmillan	0	285	50.35%	9.75
	1	281	49.65%	5.81
Penguin Random House	1	1,240	100%	6.56
Scholastic	0	126	100%	5.01
Simon & Schuster	1	376	100%	7.45

a positive relation exists between the retail price and the e-book's sales rank in our sample, as indicated by the positive slope of the regression line. This finding in our sample aligns with the study by Fishwick (2008), who notes that “substantial discounts” (p. 370) have become prominent for bestselling books in the British book market after the abandonment of the Net Book Agreement in 1997.

Regarding book sales rank, it is important to clarify how Amazon determines this metric. According to Amazon, the ranks are internally updated hourly, though they do not appear immediately. Moreover, the rank includes current and *all* prior sales, with higher weight

**Fig. 2** Relation of Amazon book ranks for e-books and retail prices

given to more recent sales.¹¹ This information on the definition of book ranks on Amazon will be important for our estimation approach as today's price might be affected by recent or prior sales, but not necessarily the reverse.

3.3 Methodology

The objective of our study is to analyze the relationship between the pricing arrangement and the retail prices of e-books sold on *Amazon.co.uk*. Therefore, we exploit publisher and book genre variation to estimate the price effect of the agency model on e-books. Before turning to the presentation of our estimations, we formalize the hypothesis to be tested. If there were no difference between the two types of vertical contracts (agency and wholesale) with respect to the retail price of an e-book, the publisher's ability to set the final consumer price should (*ceteris paribus*) have no effect on these prices. Hence, the hypothesis to be tested is as follows:

Hypothesis H_0 . *The retail price of an e-book is independent of the used vertical contract.*

If a positive correlation is found between the agency model and the price of e-books, Hypothesis H_0 can be falsified, indicating that e-books sold under the agency model are, on average, more expensive. Conversely, observing a negative correlation would also falsify Hypothesis H_0 , implying e-books sold under the agency model would be cheaper on average.

In our baseline estimation approach, we employ the standard hedonic modeling approach in the spirit of Rosen (1974), which relies on observing differences in market prices to infer the value or implicit price of underlying characteristics. Thus, we estimate the following log-log OLS model with heteroskedasticity-consistent (HC) standard errors¹²:

$$p_i = \alpha_0 + \alpha_1 A_i + \alpha_2 G_i + \alpha_3 R_i + \alpha_4 RRP_i + W\theta + \eta_i. \quad (1)$$

In Eq. (1), the dependent variable p_i is the logarithm of the retail price for e-book i sold on *Amazon.co.uk*. The treatment variable A_i is a dummy variable, which takes the value one if an e-book i is sold under the agency model and zero otherwise. The variable G_i is a categorical variable representing book genres, and R_i is a continuous variable representing the logarithm of e-book sales ranks on Amazon. The variable RRP_i represents the logarithm of the recommended retail price for book title i . All other book-specific covariates are included in the matrix W (see Table 8 in Appendix 1).

However, two potential endogeneity issues may arise in regression Eq. (1), which might distort our estimated coefficients. First and foremost, our treatment variable A_i may be endogenous due to selection bias. Specifically, e-books sold under agency contracts may differ from e-books sold under wholesale contracts in unobserved ways related to their retail prices. Hence, the assignment of an e-book to either the agency or wholesale model was likely not a random process. To mitigate this potential selection bias, we apply a matching procedure in Sect. 5.1.

Second, a bidirectional relationship likely exists between an e-book's rank R_i and its retail price p_i : specifically, beyond rank affecting price, the retail price also influences sales, thereby contributing to rank determination. As already explained, e-book ranks on *Amazon.co.uk* are internally determined by weighted overall sales. Thus, ranks may be driven by the current quantities, but this relationship is ambiguous since the rank also incorporates past sales

¹¹ See https://kdp.amazon.com/en_US/help/topic/G201648140. (Last accessed: July 15, 2025)

¹² We use the terms heteroskedasticity-consistent standard errors, heteroskedasticity-robust standard errors, and robust standard errors interchangeably; see White (1980) on the concept and Kleiber and Zeileis (2008, Ch. 4.3) for the implementation in R.

quantities. E-book prices can be affected by current and past sales, but the impact of prices on total (current and past) sales is less clear. Nevertheless, we cannot conclusively rule out endogeneity due to a potential reverse causality. To address this potential source of endogeneity between the e-book price and its sales rank, we also present an instrumental variable (IV) approach in Sect. 5.2.

4 OLS Estimation Results

The results of our OLS regression model are outlined in Table 2. We estimate four different specifications in this estimation approach: Column 1 of Table 2 presents a naive OLS regression model without genre effects; column 2 includes LDA genre fixed effects¹³; and column 3 uses the Amazon genre information as a control variable. Column 4 focuses solely on e-books from Pan Macmillan, as this publisher is the only one in our sample (apart from the heterogeneous group of smaller publishers) selling e-books under both vertical contracts types: the agency and wholesale models (see Table 1).

There is a clear and statistically significant negative relationship between the agency pricing arrangement and the retail price of e-books across all four specifications. This provides strong evidence against Hypothesis H_0 . In terms of magnitude, the effect ranges between 18.29 and 22.04%, depending on the exact specification. For the regression in column 1, an e-book which is sold under the agency model on *Amazon.co.uk* is approximately 18.53% cheaper, on average, than one sold under the wholesale model.¹⁴ Including LDA genre fixed effects (see column 2 in Table 2) increases the agency effect to 18.94% in magnitude, while the effect is slightly lower when incorporating Amazon genre information (see column 3).¹⁵

The estimated coefficients for the other explanatory variables shown in Table 2 are highly consistent across the first three specifications. The sign of the variable *log sales rank* indicates that e-books with higher sales ranks are sold at higher prices. This observation was previously demonstrated descriptively in Fig. 2 (Sect. 3.2) and confirms the results of Fishwick (2008), who notes that “substantial discounts” became prominent for bestsellers in the UK. However, we discuss potential endogeneity issues concerning this control variable in Sect. 5.2. Moreover, a significant and positive relationship between the RRP of an e-book and its retail price is observable in our results (see variable *log RRP*).¹⁶ We also observe that an e-book’s file size (given in KB) has a positive and significant price effect (see variable *log Kindle Size*).¹⁷

¹³ We describe the process to generate book genres using an LDA approach in Appendix 2. Thus, we identify a topic for every e-book title based on the largest probability assigned by the LDA.

¹⁴ To calculate the exact effect of the dummy variable *Agency* on the dependent price variable, the formula $100 \times (e^{\beta} - 1)\%$ must be used (Wooldridge 2013, Section 2.4).

¹⁵ Table 12 in Appendix 2 illustrates that varying the number of topics derived from our LDA text mining approach does not qualitatively change our estimation results.

¹⁶ Theoretically, manufacturers might use the RRP as a focal point to influence retailer pricing decisions (e.g., Motta 2004, Ch. 6.1). An RRP stands out among all possible prices a retailer can choose, potentially coordinating the prices set by individual retailers. Recent empirical studies on this topic have also been conducted (e.g., Faber and Janssen 2019).

¹⁷ Including e-book file size as a control variable allows to account for latent factors that may influence the perceived quality of the e-book (e.g., number of illustrations or image quality), as well as production costs (based on the text length, including typesetting, professional editing, and figure design). Furthermore, file size may indirectly capture the author’s opportunity costs, which should be *ceteris paribus* higher for more extensive works.

Table 2 OLS estimation approach. Dependent variable is the logarithm of the retail price for e-books sold on Amazon

	Dependent variable: log Price			
	(1)	(2)	(3)	(4)
Agency	−0.205*** (0.011)	−0.210*** (0.011)	−0.202*** (0.011)	−0.249*** (0.059)
log sales rank	0.053*** (0.004)	0.059*** (0.004)	0.051*** (0.004)	0.059*** (0.013)
log Kindle Size	0.045*** (0.004)	0.050*** (0.004)	0.027*** (0.004)	−0.004 (0.016)
log star rating	0.531*** (0.049)	0.484*** (0.048)	0.436*** (0.048)	0.199 (0.138)
No. expert reviews	0.015*** (0.004)	0.015*** (0.004)	0.018*** (0.004)	0.028* (0.017)
log RRP	1.091*** (0.011)	1.071*** (0.012)	1.080*** (0.012)	0.807*** (0.045)
Date Retail	0.010*** (0.003)	0.012*** (0.003)	0.008*** (0.003)	0.001 (0.007)
Bestsellers	0.001*** (0.0004)	0.001*** (0.0004)	0.001*** (0.0004)	0.001 (0.001)
WeekInChart	0.007*** (0.002)	0.007*** (0.002)	0.007*** (0.002)	0.007 (0.006)
Constant	−1.856*** (0.066)	−2.204*** (0.084)	−1.745*** (0.074)	−0.876*** (0.238)
Genre	No	Yes	Amazon Genres	Yes
Inclusion	All	All	All	Pan Macmillan
Robust <i>F</i> Statistic	1285.3022	662.2041	627.4533	67.7247
Observations	12,001	12,001	12,001	566
Adjusted <i>R</i> ²	0.602	0.610	0.615	0.683
Robust standard errors in parentheses	* <i>p</i> < 0.1; ** <i>p</i> < 0.05; *** <i>p</i> < 0.01			

The results presented in Table 2 also indicate that consumer and expert recommendations seem to drive the price of an e-book, as both the consumer star rating (*log star rating*) and the number of expert reviews (*No. expert reviews*) exhibit a positive and significant effect in our regressions. The time since a book's publication (in years, variable *Date Retail*) also appears to have a positive effect on e-book prices.

Lastly, two covariates related to author quality remain in Table 2. The explanatory variable *WeekInChart* reflects the average number of weeks an author's former bestsellers remained on the *Sunday Times* bestseller charts, and the continuous variable *Bestsellers* represents the historical number of bestselling book titles an author has written. As expected, both variables show a positive and significant effect on e-book retail prices across all three specifications, which can be interpreted as quality signals that increase the price of a book title.¹⁸

¹⁸ The variables *WeekInChart* and *Bestsellers* are based on a historical *Sunday Times Bestseller* list. The matching process was conducted via Python's Fuzzy Matching.

Column 4 of Table 2, which incorporates only e-books sold by Pan Macmillan, qualitatively confirms our baseline results. Furthermore, focusing solely on e-books from Pan Macmillan in a separate OLS regression can be informative, as it can reduce the selection bias of our treatment variable A_i to some extent (see Sect. 4). In particular, focusing on a single publisher that sells e-books under both vertical contract types allows us to rule out that the agency effect might be confounded with publisher-specific effects (e.g., the relative publisher-platform bargaining power).

The agency effect may vary with the sales rank of a book title, i.e., whether we observe a bestselling title or a *long tail* book (those with higher sales rank). To illustrate this relationship, we re-estimate Eq. (1), starting with a small subset of the data and progressively expanding the sample to include books up to a certain sales rank. For instance, we progressively include up to rank 1000, then up to rank 2000, and so forth, until we reach the largest sales rank. Thus, we obtain a series of estimated agency coefficients, each based on a different e-book subset. Figure 3 illustrates how the coefficients of the agency variable change as higher book sales ranks are gradually included. The black line represents the point estimate, while the shaded area (bounded by red lines) represents the 95% confidence interval. For the lowest range of sales ranks (bestselling books), the coefficient is not statistically different from zero. As book titles from higher sales ranks (*long tail* books) are included, the coefficients become statistically significant and converge to -0.21 , consistent with the outcome from column 2 of Table 2. Overall, Fig. 3 implies that the agency effect is strongly dependent on the book sales rank, which can partly explain the divergence between the results of De los Santos and Wildenbeest (2017) (only based on bestselling titles) and our findings.

To emphasize the distinctions among bestsellers (top sales ranks), mid-tail books (medium sales ranks), and *long tail* books (highest sales ranks), we utilize the absolute definition of the 100,000th book rank as the *long tail* threshold (Brynjolfsson et al. 2010). According to Brynjolfsson et al. (2003), a typical large local bookstore stocks between 40,000 to 100,000

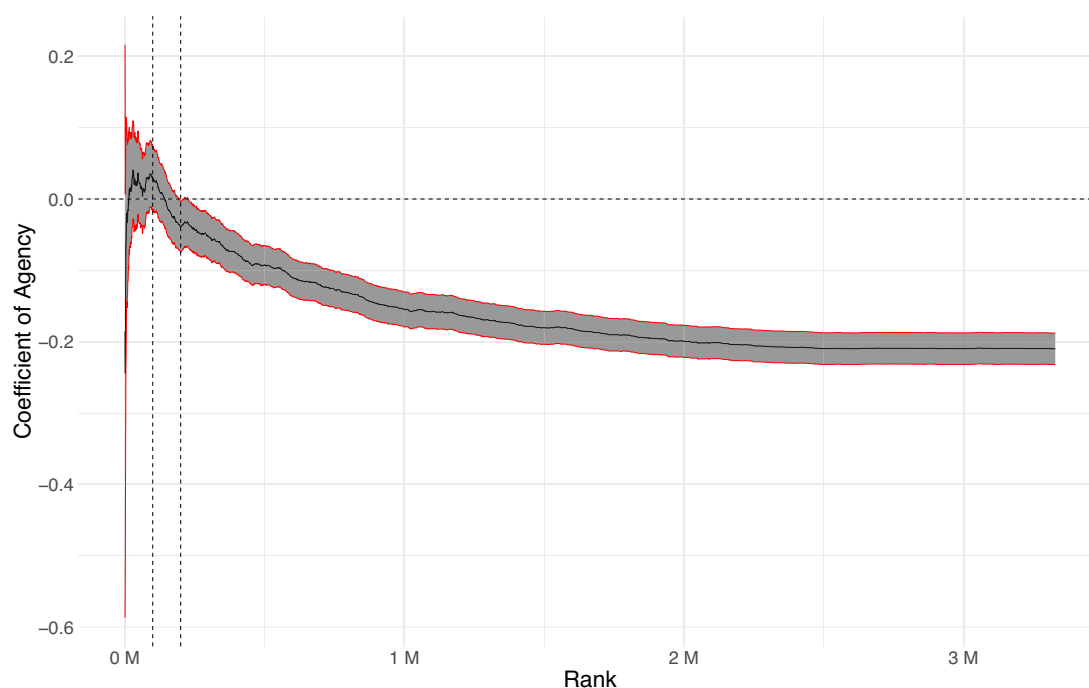


Fig. 3 Results from Eq. (1) if higher sales ranks are gradually included. This starts at sales rank of 1000 comprising approximately 100 observations. The vertical dashed lines denote the book rank of 100,000 and 200,000, representing the potential *long tail* thresholds

unique book titles. Therefore, we classify books with a sales rank exceeding 100,000 as belonging to the *long tail*,¹⁹.

Based on the results presented in Fig. 3, we calculate the mean coefficients by partitioning them at the vertical dashed lines. On average, books in the *long tail* category show an agency coefficient of -15.34% , while those with ranks below 100,000 exhibit an average effect of approximately 1% .²⁰

5 Extensions

As discussed in Sect. 4, the results of our OLS estimation approach may be biased and inconsistent due to endogeneity issues regarding our treatment variable *Agency*. Hence, we apply a matching procedure in Sect. 5.1 to reduce this potential selection bias. Furthermore, we employ an instrumental variable approach in Sect. 5.2 to address the potential endogeneity of our control variable, book sales rank.

5.1 Propensity Score Matching

A simple OLS estimation would allow us to identify the effect of different pricing arrangements if the treatment were random. However, the assignment of e-books to either the agency or wholesale model was likely not a random process, leading to selection bias when using a standard OLS estimation approach. Hence, as a first step, we seek to reduce this selection bias by relying on propensity score matching (Rubin 1977; Rosenbaum and Rubin 1983). Through this, we identify appropriate treated and control e-books through a matching procedure.

In particular, we identify control e-books (not sold under the agency model) that share the same ex-ante probability of being under the agency model as the treated e-books. To implement the PSM, we first estimate a logistic regression to recover the likelihood that an e-book is sold under the agency model based on its observable characteristics and use the predicted values from this estimation to summarize the covariates as a single scalar, the propensity score. Second, we match each e-book sold under the agency model to the most similar e-book sold under the wholesale model, based on this propensity score.

The propensity score e for e-book i is defined as the probability of receiving treatment conditional on the confounding variables X_i : $e(X_i) = Pr(A_i = 1|X_i)$. The identification relies on two key assumptions. The first is the conditional independence assumption, which requires that the outcome variable is independent of the treatment, conditional on the propensity score. This implies that only variables simultaneously influencing both treatment and outcome should be included to ensure unconfoundness. The second assumption, known as the common support (or overlap) assumption, requires that for any given propensity score value, there must be units in both the treatment and the control groups (Rubin 1977). These assumptions are essential for identification, and if both hold, treatment assignment can be considered *strongly ignorable*. Consequently, this allows the analysis to offer unbiased estimates of the treatment effect (Rosenbaum and Rubin 1983; Rubin 1978). The first assumption

¹⁹ In Fig. 3, a vertical dashed line indicates the threshold of 100,000, denoting the point at which the agency coefficient begins to decline and the *long tail* definition applies. Furthermore, for comparative analysis and due to the potential uncertainty regarding the absolute definition, we additionally define the *long tail* using a more recent threshold of 200,000 (see Footnote 6)

²⁰ Using a threshold of 200,000 yields comparable findings, with an average coefficient of -15.57% for the *long tail* and approximately zero for ranks below the threshold.

cannot be tested empirically, whereas the second assumption can be evaluated by assessing pre-treatment characteristics and scrutinizing propensity scores once they are estimated. Regarding the latter, we demonstrate below that the common support assumption presents no issues. Concerning the first assumption, we present regression analyses incorporating all covariates previously used from Sect. 4, along with a more limited subset of covariates likely determined prior to the book's sale or the selection of the sales model. This approach is crucial for comparing books under the agency model only with those under the wholesale model that are a highly comparable in their features. We show that estimating the model using PSM can at least substantially decrease bias.

Table 3 Logistic regression with *Agency* as the dependent variable

	Dependent variable: <i>Agency</i>	
	(1)	(2)
log RRP		−0.143*** (0.041)
log sales rank		−0.175*** (0.015)
log star rating		−0.184 (0.134)
No. expert reviews		0.370*** (0.021)
log kindle size	0.053*** (0.016)	0.119*** (0.017)
WeekInChart		0.011 (0.009)
DSeries	−0.422*** (0.068)	−0.363*** (0.069)
TtS	0.082 (0.117)	0.109 (0.124)
SReader	−0.099* (0.054)	−0.139** (0.056)
XR	0.535*** (0.047)	0.286*** (0.050)
Date Retail		−0.005 (0.010)
Bestsellers		0.019** (0.007)
Constant	−0.088 (0.193)	1.471*** (0.295)
Genre topics	LDA	LDA
Pseudo R2 (McFadden)	0.043	0.103
Adj.Pseudo R2 (McFadden)	0.041	0.099
Observations	12,001	12,001
Log likelihood	−7,891.200	−7,401.500
Robust standard errors in parentheses	* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$	

As a first step of the matching procedure, we estimate the following logit regression:

$$A_i = \alpha + X_i\beta + \eta_i, \quad (2)$$

where A_i is a binary indicator, with $A_i = 1$ for the agency model and $A_i = 0$ for the wholesale model. Hence, $e(X_i) = \Pr(A_i = 1|X_i)$ represents the probability that e-book i is sold under the agency model, and X_i is a vector of e-book characteristics (consistent with the explanatory variables in Eq. (1)).

In Table 3, we report the estimates for Eq. (2) from which the propensity scores are estimated.²¹ We present results from two PSM specifications: Column 1 includes only a subset of book characteristics as covariates, while column 2 incorporates all explanatory variables introduced in Sect. 4. Specifically, column 1 excludes variables presumed to be realized after the model decision (agency or wholesale) has been made, such as book sales rank, star rating, and expert reviews.

The frequency distributions of the propensity scores based on a 10-nearest-neighbor matching for the treated and control e-books are presented in Fig. 4a and b depicts the restricted model, while Fig. 4b illustrates the full model. The propensity score distributions of the agency (blue) and wholesale e-books (red) are notably different before the matching procedure (*Unadjusted Sample* in Fig. 4). Following the matching procedure, the propensity score distributions for the two e-book groups are highly similar (see *Adjusted Sample* in Fig. 4), indicating that most wholesale e-books can be matched with agency e-books based on their characteristics. Only a few e-book titles are excluded from the sample after the matching process. Although excluding some of the covariates in this approach clearly alters the distributions between Fig. 4a and b for the treated and untreated e-books, we are able to find a comparable wholesale e-book for nearly every agency e-book in our sample.

In a second step, we use the estimated propensity scores $e(X_i)$ to construct a matched sample comprising only paired e-books to estimate the average treatment effect (ATE) of the agency model on the retail price of e-books. To estimate the ATE, it is essential to impute the counterfactual outcome for each observation i , which is conditional on the applied matching methodology. It is also recommended to incorporate a bias adjustment using the covariates (Abadie and Imbens 2011). Consequently, a regression model is fitted to the outcome variable, specifically the logarithm of prices p , utilizing all previously delineated covariates. This estimation is conducted individually for both the agency and wholesale samples:

$$\begin{aligned} p_i &= W_0\gamma + \varepsilon_i \text{ if } A_i = 0 \\ p_i &= W_1\delta + \mu_i \text{ if } A_i = 1. \end{aligned} \quad (3)$$

From the estimated coefficients, γ and δ , and the values of covariates, W_0 and W_1 , we construct bias-adjusted counterfactual values for each missing values $i = 1, \dots, N$. Specifically, for each observation i with $A_i = 0$, the imputation algorithm follows:

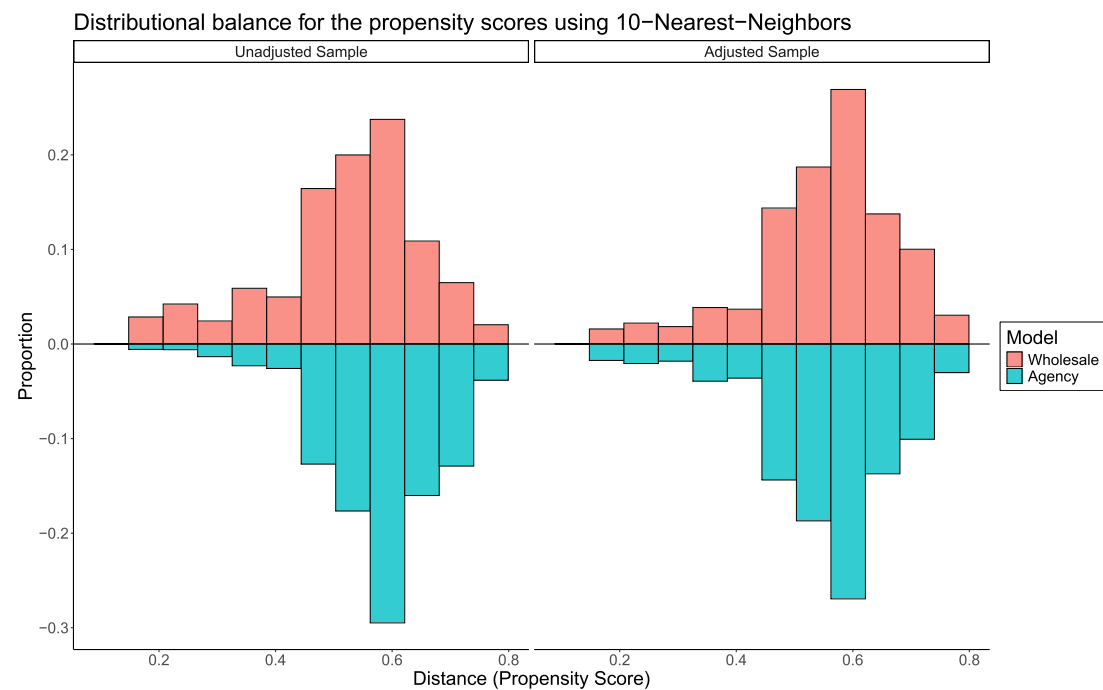
$$\hat{p}_i(A_i = 0) = \begin{cases} p_i & \text{if } A_i = 0 \\ \frac{1}{k} \sum_{j \in N_k(j)} [p_j + (W_{0,i} - W_{0,j})\gamma] & \text{if } A_i = 1. \end{cases} \quad (4)$$

Similarly, for $A_i = 1$, we obtain the following:

$$\hat{p}_i(A_i = 1) = \begin{cases} \frac{1}{k} \sum_{j \in N_k(j)} [p_j + (W_{1,i} - W_{1,j})\delta] & \text{if } A_i = 0 \\ p_i & \text{if } A_i = 1. \end{cases} \quad (5)$$

²¹ Matching algorithms rely on the R Matching package (Sekhon 2011). Covariate balancing (see Fig. 9) and the propensity score distribution are assessed by using the R package cobalt (Greifer 2024).

(a)



(b)

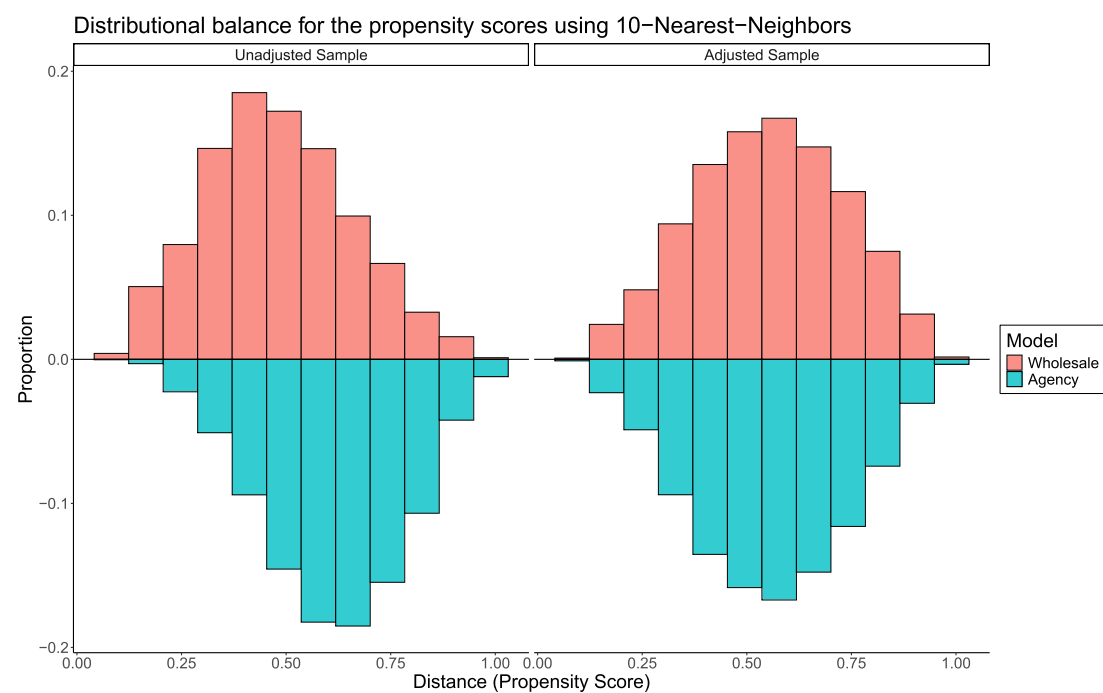


Fig. 4 Frequency distributions of propensity scores (from a logistic model with 10-nearest-neighbors matching): unadjusted (left) and adjusted (right) samples

Table 4 Propensity score matching results for the adjusted subset of covariates, estimated following Eq. (6). Standard errors are Abadie-Imbens heteroskedastic standard errors (except from column 1 of each sub-table, see Abadie and Imbens (2006)). The models in columns 1 to 5 each correspond to a different number of kNN. The caliper radius is set by 0.2 standard deviation of the propensity scores derived from the logistic regression (see Austin 2011, for optimal caliper length)

(a) Restricted model: Based on the propensity scores estimated from the logistic regression in column (1) of Table 3.

	(1)	(2)	(3)	(4)	(5)
τ_{ATE}	-0.231	-0.286	-0.304	-0.300	-0.299
Standard error	0.012	0.017	0.016	0.016	0.016
<i>t</i> -statistic	-18.800	-17.212	-18.576	-19.025	-19.071
Number of nearest neighbor1 (<i>k</i>)		1	1	5	10
Replace	No	Yes	Yes	Yes	Yes
Debiased	No	No	Yes	Yes	Yes
Observations	9438	12,000	12,000	12,000	11,999

(b) Full model: Based on the propensity scores estimated from the logistic regression in column 2 of Table 3

	(1)	(2)	(3)	(4)	(5)
τ_{ATE}	-0.195	-0.226	-0.207	-0.207	-0.207
Standard error	0.014	0.020	0.019	0.018	0.017
<i>t</i> -statistic	-13.822	-11.104	-10.853	-11.663	-11.854
Number of nearest neighbor1 (<i>k</i>)		1	1	5	10
Replace	No	Yes	Yes	Yes	Yes
Debiased	No	No	Yes	Yes	Yes
Observations	7946	12,001	12,001	11,961	11,941

Here, *k* denotes the number of matches in our *k*-nearest-neighbor (kNN) matching, and $N_{k(j)}$ refers to the set of *k* matches relevant for each observation *i*. From the imputation algorithms in Eqs. (4) and (5), the final estimator for the ATE is given by (Abadie and Imbens 2011, Section 2.2):

$$\hat{\tau}_{ATE} = \frac{1}{N} \sum_i^N [\hat{p}_i(A_i = 1) - \hat{p}_i(A_i = 0)]. \quad (6)$$

Table 4 reports the ATE as estimated from Eq. (6), for the restricted and full model, along with the standard errors, *t*-statistics, and the count of nearest neighbors employed for each model. In general, we apply nearest neighbor matching methods with a caliper of 0.2 standard deviations, adhering to the common support requirement and addressing the potential non-randomness in the treatment assignment. The agency model continues to show a significant and negative effect on the retail price of e-books. The exact effect ranges from 20.6 to 26.21% (Table 4a) and from 17.7 to 20.2% (Table 4b) depending on the matching procedure used.²²

²² Although this may seem like a relatively large effect at first glance, it can be put into perspective by considering an example in absolute terms. The median retail price of an e-book in our data set is £4.99. Assuming that this median e-book is sold under the wholesale model (at £4.99), a 20% negative agency effect in our estimation implies that the same e-books would be sold for £3.99 under the agency pricing model in a counterfactual scenario.

This continues to provide strong evidence against Hypothesis H_0 . Moreover, the results from the restricted subset of covariates (Table 4a) indicate that excluding variables presumed to be realized post-selection of the sales model does not qualitatively alter our findings, although the coefficient magnitudes are more pronounced.

5.2 IV Estimation

The results of our OLS estimation in Sect. 4 might be biased and inconsistent because our important control variable, book sales rank, may be endogenous. Hence, we employ an instrumental variable approach in this section to address this potential source of endogeneity. We utilize the logarithmized number of customer reviews (*log no. customer reviews*) as an instrument for the book sales rank to avoid inconsistent estimates due to reverse causality.

Our instrumental variable *log no. customer reviews* is highly correlated with our endogenous regressor, book sales rank (relevance condition), but should have no partial effect on the price of an e-book (orthogonality assumption). Customer reviews can enhance the consumer awareness and information quality, thereby influencing the tendency for a consumer to purchase a book. However, the absolute number of customer reviews does not directly affect a consumer's purchasing decision for a book title; only surprisingly positive (negative) reviews can increase (decrease) the consumption of a given good (Reimers and Waldfogel 2021). Hence, the absolute number of customer reviews should also have no partial effect on e-book prices, even though our instrument is highly correlated with the book sales rank (as it is an indicator for past sales).

Following the approach explained above, the linear projection in the first-stage regression of our 2SLS estimation can be formalized as follows:

$$R_i = \beta_0 + \beta_1 A_i + \beta_2 P_i + \beta_3 G_i + \beta_4 P_i \times G_i + \beta_5 RRP_i + \beta_6 CR_i + W\theta + \xi_i. \quad (7)$$

In Eq. (7), the dependent variable R_i refers to the sales rank of book title i on *Amazon.co.uk*. Most covariates used here were introduced in the baseline estimation (Eq. 1). Our instrumental variable *log no. customer reviews* is denoted by CR_i .

The structural equation of our basic model takes the following form:

$$p_i = \gamma_0 + \gamma_1 A_i + \gamma_2 P_i + \gamma_3 G_i + \gamma_4 P_i \times G_i + \gamma_5 RRP_i + \gamma_6 \hat{R}_i + W\theta + \varepsilon_i, \quad (8)$$

where the dependent variable p_i is the logarithm of the retail price of e-book i and the fitted values from the first-stage are captured by $\hat{R}_i$.

The regression results based on Eq. (8) are presented in columns 3 and 4 of Table 5. Column 1 present OLS regression results without including sales rank as a control variable. Column 2 replicates the results of a (naive) OLS estimation from Sect. 4 that includes book sales rank on the right-hand side of the regression. The two IV specifications in Table 5 differ by the type of genre control variable included (LDA-generated or Amazon book genres). The results of our IV approach confirm that e-books sold under the agency model on *Amazon.co.uk* are, on average, significantly cheaper than book titles sold under the wholesale model. Compared to the OLS estimation result in column 2, the estimated coefficients for the variable *Agency* only differ in magnitude, as we find a negative effect of agency pricing ranging between 17.4 and 18%. Hence, we slightly overestimate the absolute effect of agency pricing when the endogeneity of the variable book sales rank is ignored. The effects of the other explanatory variables also barely differ between the OLS and the IV estimation approaches, although the effect of book sales rank has become larger in the IV regressions.

Table 5 IV estimation results

	Dependent variable: log Price			
	(1)	(2)	(3)	(4)
log sales rank		0.059*** (0.004)	0.090*** (0.008)	0.076*** (0.007)
Agency	−0.233*** (0.012)	−0.210*** (0.011)	−0.198*** (0.012)	−0.191*** (0.012)
log Kindle Size	0.053*** (0.004)	0.050*** (0.004)	0.049*** (0.004)	0.026*** (0.004)
log star rating	0.445*** (0.048)	0.484*** (0.048)	0.505*** (0.048)	0.456*** (0.048)
No. expert reviews	0.004 (0.004)	0.015*** (0.004)	0.021*** (0.005)	0.023*** (0.004)
log RRP	1.067*** (0.012)	1.071*** (0.012)	1.073*** (0.012)	1.082*** (0.012)
Date Retail	0.013*** (0.003)	0.012*** (0.003)	0.011*** (0.003)	0.008*** (0.003)
Bestsellers	0.001*** (0.0004)	0.001*** (0.0004)	0.002*** (0.0004)	0.002*** (0.0004)
WeekInChart	0.003 (0.002)	0.007*** (0.002)	0.009*** (0.003)	0.009*** (0.002)
Constant	−1.421*** (0.068)	−2.204*** (0.084)	−2.612*** (0.123)	−2.076*** (0.111)
Genre	LDA	LDA	LDA	Amazon Genres
Instrument	No	No	Reviews	Reviews
First stage <i>F</i> -statistic	No	No	3663.8841	3915.4707
Wu-Hausman statistic	No	No	23.179	17.0578
Observations	12,001	12,001	12,001	12,001
Adjusted <i>R</i> ²	0.601	0.610	0.608	0.614
Robust standard errors in parentheses	* <i>p</i> < 0.1; ** <i>p</i> < 0.05; *** <i>p</i> < 0.01			

6 Robustness Checks

To check the robustness of our results presented in the previous section, we apply two additional estimation approaches. In Sect. 6.1, we utilize data on the paperback format to conduct an alternative OLS estimation procedure. Subsequently, we present a double machine learning (DML) approach in Sect. 6.2.

6.1 Format Estimation

The coefficient for the agency variable in our OLS estimation approach might be biased since we compare different book titles with each other. An ideal analysis would compare the same title offered to similar consumers under two separate vertical contracts. However,

a specific e-book title is sold under either the agency or the wholesale model, rendering the counterfactual scenario unobservable. As an alternative, we conduct a robustness check wherein we compare price differences between digital and paperback formats for the same book title. Since print books are generally sold under wholesale contracts in the UK, this allows us to identify the price effect of the agency model on e-book prices (by also controlling for format differences).

To implement this robustness analysis, we retain only e-books sold under the agency model that also have an equivalent paperback version in our sample, which reduces our data set to 7446 observations. Then, we estimate the following OLS model:

$$p_{i,j} = \alpha_0 + \alpha_1 A_{i,j} + \alpha_2 F_{i,j} + W\theta + \epsilon_{i,j}. \quad (9)$$

The dependent variable $p_{i,j}$ in Eq. (9) is the logarithm of the retail price for a book title i in format j . Again, the treatment variable $A_{i,j}$ is a dummy variable, equal to one if a book is sold under the agency model and zero for the wholesale contract. The format fixed effect $F_{i,j}$ is equal to one for the paperback version and zero for the digital version. All other covariates are captured by the matrix W .

The results of Eq. (9) are reported in Table 6, controlling for genre fixed effects (column 1), publisher fixed effects (column 2), and both types of fixed effects (column 3). Column 4 estimates Eq. (1) by incorporating only the e-book format so that all the observations for the

Table 6 Robustness check with paperback books (log-log OLS). Dependent variable is the logarithm of the retail price for digital and paperback versions of book titles sold on Amazon

	Dependent variable: log Price			
	(1)	(2)	(3)	(4)
Agency	−0.174*** (0.017)	−0.261*** (0.021)	−0.236*** (0.021)	−0.166*** (0.010)
Paperback	0.485*** (0.014)	0.439*** (0.015)	0.447*** (0.015)	
log star rating	0.219*** (0.045)	0.215*** (0.046)	0.182*** (0.045)	0.281*** (0.043)
No. expert reviews	0.038*** (0.005)	0.032*** (0.006)	0.031*** (0.006)	0.039*** (0.004)
log RRP	0.888*** (0.014)	0.931*** (0.013)	0.888*** (0.014)	0.839*** (0.011)
Date Retail	0.004 (0.004)	0.005 (0.004)	0.003 (0.004)	0.014*** (0.003)
Bestsellers	−0.0002 (0.0002)	−0.001*** (0.0002)	−0.001** (0.0002)	−0.00002 (0.0003)
Constant	−0.479*** (0.058)	−0.398*** (0.037)	−0.257*** (0.059)	−0.462*** (0.064)
Publisher	No	Yes	Yes	No
Genre	Yes	No	Yes	Yes
Robust F Statistic	497.1137	627.141	381.3814	29.6844
Observations	7,446	7,446	7,446	3,723
Adjusted R^2	0.606	0.609	0.622	0.479
Robust standard errors in parentheses	* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$			

paperback format are excluded (see the number of observations and the empty cell for the paperback coefficient in column 4). Comparing the estimation results to those reported in Table 4b, we observe that the agency effect is highly similar, although its magnitude is partly smaller here (ranging between 15.3 and 23%). The estimated coefficients for the variable *Format* imply that the paperback versions are significantly more expensive than the digital ones, confirming our descriptive statistics in Sect. 3.2 (see Fig. 5). The coefficients of the remaining covariates in Table 6 are largely consistent with our main estimations in Sect. 4.

The results of a slightly modified regression model based on Eq. (9) are presented in Table 10 of Appendix 1. Here, the dependent variable is the difference in absolute prices between digital and paperback formats for a given book title. This makes the interpretation for the agency effect more intuitive, although the treatment effect must now be understood as an absolute effect (in British pounds) rather than a percentage.²³ In Table 10, the constant of the regression line represents the mean price difference between e-books and paperbacks, indicating that the print format is more expensive than the digital format, on average. The significant negative coefficient for the variable *Agency* implies that this price difference is more pronounced for agency compared to wholesale e-books. This further suggests that e-books sold under the agency model are cheaper than e-books sold under the wholesale model, which confirms our main results in Sect. 4.

6.2 Double Machine Learning (DML)

Beyond established approaches in the standard econometric analysis for causal inference, further methods exist. We have already applied standard econometric methods as the OLS estimation and the propensity score matching to deal with econometric issues. Recent advances in machine learning approaches also provide an expanded toolbox for empirical analyses in economics (e.g., for a broad overview, see Athey and Imbens 2019; Athey 2018).

Recent developments in the machine learning literature have yielded many approaches, such as the DML technique, that enable addressing common econometric issues (including confounding variables or variable selection) through the use of cross-validation and non-parametric models. This technique also allows non-standard modeling of relationships between variables, unlike conventional approaches where independent variables often assume specific, linear or quadratic effects on the dependent variable. It permits the use of any arbitrary machine learning technique, which relies on algorithms to find a suitable model for some chosen score functions, such as the mean squared error.

The reason for relying on further non-parametric/semi-parametric regressions is to circumvent the imposition of a specific model structure. The algorithms of the machine learning models will select the best-fitting model under given restrictions or parametrization. We then employ regularized linear regression techniques, such as the least absolute shrinkage and selection operator (Lasso), or regression trees/forests. These methods facilitate comparing our standard econometric approaches with models that are capable to disregarding irrelevant variables or incorporating non-linear effects (Athey and Imbens 2019). Moreover, this methods can assist in a similar manner to propensity score matching in addressing the underlying selection issue (e.g., Lee et al. 2010; Westreich et al. 2010; Knaus 2021).

Therefore, we apply DML techniques and compare the results to our previous estimations to provide additional robustness checks. From a predictive perspective, the estimations are split into three distinct regression models, as proposed by Chernozhukov et al. (2018). These

²³ Appendix 1 Table 11 also presents the results in percentages, derived using log ratios (differences in logs) to compare digital and paperback formats.

models utilize the DML framework to address challenges such as high-dimensional variables, non-parametric functional forms, robust variable selection, or unobserved confounding variables. While the DML framework can be extended to handle endogeneity arising from unobserved confounding variables (the latter typically requiring instrumental variable methods), our current application focuses on handling observed covariates. The analysis is based on the *DML-Conditional-Average-Treatment-Effect-Estimator*.²⁴

The equation system we estimate takes the following general form (dropping the individual index i for each book), which is based on the partial linear regression model of Robinson (1988):

$$\begin{aligned} p &= \theta A + q(W) + \varepsilon \\ A &= f(W) + \eta \\ \text{s.t. } \mathbf{E}[\varepsilon|W] &= \mathbf{E}[\eta|W] = \mathbf{E}[\varepsilon \cdot \eta|W] = 0, \end{aligned} \quad (10)$$

where p denotes the price of an e-book, which depends on the agency dummy variable A and $q(W)$ is a function of covariates W . The agency dummy variable A is modeled by a function $f(W)$ of these covariates W (similar to the logit specification in Eq. (2)). The variables η and ε represent stochastic error terms. These conditional expectation functions can then be estimated by using non-parametric regressions:

$$\begin{aligned} q(W) &= \mathbf{E}[p|W] \\ f(W) &= \mathbf{E}[A|W]. \end{aligned} \quad (11)$$

Next, the residuals for the price, $\tilde{p}$, and the residuals of the agency dummy variable, $\tilde{A}$, are computed by subtracting the fitted values (obtained from the regression tasks in (11)) from the actual price p and the actual agency dummy variable A :

$$\begin{aligned} \tilde{p} &= p - q(W) \\ \tilde{A} &= A - f(W) \end{aligned} \quad (12)$$

Finally, we use these residuals (for prices $\tilde{p}$ and the agency dummy variable $\tilde{A}$) to estimate a linear treatment effect θ which is unbiased based under the assumptions of Chernozhukov et al. (2018):

$$\tilde{p} = \theta \tilde{A} + \epsilon. \quad (13)$$

In Table 7, the column *Model* represents the applied functional form. The first entry in this column refers to the functional form used for computing and predicting $\tilde{p}$, and the second for classifying $\tilde{A}$. Specifically, *Lin-Logit* relies on OLS and a logistic regression; *Lin-Lasso* relies on an OLS and a logistic regression including L_1 penalty (called Lasso); *Lasso-ElasticNet* employs a Lasso and a logistic regression with a combination of L_1 and L_2 penalties (*Elastic Net*); *Lasso-RFC* refers to a Lasso regression and a random forest classifier; *RFR-ElasticNet* combines a random forest and an *Elastic Net*; *RFR-RFC* utilizes a random forest for both stages; and *XGBoost* refers to Extreme Gradient Boost for both stages.²⁵

The column *Score* in Table 7 displays the mean squared error from the final stage. In the final stage, a simple linear regression is used to estimate the conditional average treatment

²⁴ For the estimation implementation, we follow the Python module *econml* provided by Battocchi et al. (2019) (see also <https://econml.azurewebsites.net/spec/api.html#linear-in-treatment-cate-estimators>, last accessed, July 15, 2025).

²⁵ While many other estimation techniques are possible, these are sufficient to highlight the stability of our results.

Table 7 Double machine learning approach. The dependent variable is the logarithm of the e-book retail price and the treatment variable is *Agency*. The price effect of the agency arrangement for each model is given in the column *Agency* and represents the ATE. Column *Score* refers to the mean squared error. Note: The data has been mean centered with unit variance, as Lasso requires this normalization

	Agency	Std. Error	<i>p</i> -value	Score	Perc. Change
Lin-Logit	−0.2138	0.0115	0.0000	0.3263	−19.2519
Lin-Lasso	−0.2138	0.0115	0.0000	0.3263	−19.2485
Lasso-ElasticNet	−0.2137	0.0115	0.0000	0.3264	−19.2443
Lasso-RFC	−0.2417	0.0132	0.0000	0.3266	−21.4719
RFR-ElasticNet	−0.1714	0.0094	0.0000	0.2160	−15.7488
RFR-RFC	−0.2057	0.0114	0.0000	0.2153	−18.5962
XGBoost	−0.1688	0.0119	0.0000	0.2314	−15.5280

effect. The hyperparameters for each model are selected from a reasonable set and then we use 3 – 5 cross-fold validation within Python’s Sklearn GridSearch. Additionally, an outer 5-fold splitting is also applied in each estimation. The presented results outline the best estimate (lowest score) for each model class.

The point estimates of the individual regression models are given in the column *Agency*, and the relative percentage changes (in relation to the intercept) are presented in the column *Perc. Change* of Table 7.²⁶ Overall, the DML techniques confirm the results of our main estimations presented in Sect. 4, reaffirm strong evidence against Hypothesis H_0 , and demonstrate the robustness of our regressions, even when employing more flexible methods. For instance, e-books sold under the agency model are, on average, 18.6 % cheaper than digital books sold under the wholesale model when using the regression model *RFR-RFC*.

7 Conclusion

In this paper, we provide evidence that e-books sold under the agency model on *Amazon.co.uk* are, on average, significantly cheaper than e-books sold under the wholesale model. Our results are based on a unique data set encompassing numerous characteristics of an e-book. To quantify the relationship between the retail price of an e-book and pricing arrangement used, we rely on classical econometric techniques as well as on newer methods, such as the DML approach. We find a robust and statistically significant effect that e-books sold under the agency model are approximately 20% cheaper than e-books books sold under the wholesale model. The exact effect depends on the sales rank of a book, that is, whether a book title is a bestselling or a *long tail* book. Moreover, our findings indicate that this effect is driven almost exclusively by *long tail* books.

The results of our empirical analysis align with many theoretical papers analyzing the price effect of the agency model (cf. Sect. 2). These theoretical papers find that retail prices for e-books sold under the agency model are lower due to the elimination of double marginalization (Lu 2017; Condorelli et al. 2018), a lock-in effect exploited by retailers (Johnson 2020), or because agency selling is more efficient than the wholesale model and leads to lower retail prices (Abhishek et al. 2016). Additionally, our results align with the theoretical analysis by

²⁶ The exact percentage change can be calculated using the formula $100 \times (e^{Agency} - 1)\%$ (Wooldridge 2013, Section 2.4).

Foros et al. (2017) if one assumes that competition is greater among publishers than among retailers, which most likely is the case given Amazon's quasi-monopolistic power in the UK e-book market (see Footnote 7). Our finding that the negative price effect of the agency model is driven by *long tail* e-book titles, which exhibit higher consumer price sensitivity compared to bestselling titles, is theoretically supported by Foros et al. (2017).

To the best of our knowledge, this paper is the first empirical analysis to estimate the price effect of the agency model for e-books in the absence of MFN clauses and by incorporating not only bestselling titles but also *long tail* book titles. Besides, we also apply an LDA approach to determine book genres. Nevertheless, a limitation of our approach is its reliance on cross-sectional data instead of panel data, which prevents us from controlling for dynamic effects on e-book retail prices. Moreover, we include only one online platform, namely Amazon, instead of comparing various online retailers. Although Amazon has a relatively high market share for e-books in the UK (see Footnote 7), competition between the individual retailers very likely also affects e-book prices.

The dynamic effect of the agency model on e-book prices, including bestselling as well as *long tail* book titles, remains an open question. Future research should concentrate on panel data to address such dynamic effects of different vertical contracts. Furthermore, other online platforms selling e-books should also be included in such an analysis to identify effects between the online retailers. Finally, the long-run effect of the agency model on consumer welfare presents an interesting research area. Consumer welfare depends not only on e-book prices, but also on other factors such as the number, variety, and quality of book titles written and published.

Appendix 1: Additional figures and tables

Table 8 Relevant variables per book title and the information content they provide

Variables	Information
Price	Retail price from the upper right <i>Buy-Box</i>
Format	Hardcover, paperback, Kindle
Star rating	Average rating normalized to be between 0 and 1
No. customer reviews	Number of consumer reviews
No. expert reviews	Number of expert reviews on Amazon
DSeries	Dummy variable whether book is part of a series
Description and reviews	Detailed text-information on the book and by different reviewers
Genre	Constructed by LDA from the descriptions and reviews (see Appendix 2)
RRP	Recommended retail price which is the print RRP. For Kindle it is either related to the hardcover or paperback RRP
Agency	Dummy variable to be one if the price was set by the publisher and zero otherwise. Only possible for e-books
Seller	Sample is restricted to be sold by Amazon
Author	Information on the author of a book
Title	Information on the title of a book
Kindle_Size	Kindle file size (in KB)

Table 8 continued

Variables	Information
Publisher	Name of the publisher. We have different levels of aggregation (Imprint, Publisher, Publishing House)
Amazon rank	Uncategorized Amazon bestseller rank for either print books or e-books
Bestsellers	Number of bestsellers in the Sunday Times Bestseller List conditional on the Author's name
WeekInChart	Average number of weeks in the bestseller charts conditional on the Author's name
Identifier	Aggregation of ASINs to verify the books
Date Retail	Period of time since the publication of a book title (in years)
TtS	Dummy variable whether Text-to-Speech is enabled for an e-book
SReader	Dummy variable whether Screen Reader is supported for an e-book
XR	Dummy variable whether enhanced typesetting is enabled for an e-book

Table 9 Summary statistics

	Total	Bloomsbury	Faber	Hachette	Harpercollins	Indie/Small	Oxford	Pan Macmillan	Penguin Random House	Scholastic	Simon & Schuster
Retail Price	mean	6.18	10.37	5.84	5.31	5.42	5.80	7.79	6.56	5.01	7.45
	std	5.17	7.02	3.21	2.86	3.20	6.07	3.86	2.70	1.58	3.27
Sales Rank	mean	567929.29	554409.87	326982.37	262402.04	427905.75	778429.39	497575.36	365604.06	597692.75	555114.93
	std	650399.53	594258.21	432500.62	383524.36	514257.98	728316.54	551601.82	533573.80	655036.93	649582.93
Star Rating	mean	0.88	0.90	0.85	0.89	0.89	0.86	0.89	0.88	0.93	0.90
	std	0.10	0.10	0.13	0.08	0.09	0.12	0.10	0.08	0.07	0.08
No. Customer Reviews	mean	97.92	51.10	60.80	119.85	106.88	80.50	124.70	134.59	76.33	142.89
	std	171.91	110.04	113.83	178.01	168.94	161.48	210.71	195.05	138.88	210.61
Pages	mean	320.98	292.57	280.60	388.78	342.59	286.86	336.34	319.01	231.84	367.91
	std	841.07	124.35	191.93	1949.17	330.58	212.83	121.63	140.63	113.11	256.70
Kindle Size	mean	11470.46	13078.55	2849.00	14134.92	8288.26	8626.52	12118.73	20466.37	35684.97	15118.97
	std	35870.99	27346.77	10529.46	46813.98	28600.24	30096.99	37011.08	48498.42	42097.93	27885.95
RRP	mean	12.98	15.50	10.28	13.07	12.04	12.08	13.61	14.59	8.31	13.85
	std	9.26	9.76	4.88	5.32	5.90	10.22	5.68	5.91	3.15	5.29
Date Retail	mean	1.96	1.64	2.41	1.81	2.09	1.93	1.66	2.18	2.11	2.13
	std	2.06	1.76	2.62	2.06	2.36	1.91	1.78	2.25	2.11	2.29
No. Expert Reviews	mean	1.60	1.74	2.43	2.33	1.25	1.34	2.24	1.71	0.65	1.35
	std	1.40	0.96	1.24	1.06	1.12	1.51	1.57	1.40	0.85	1.04

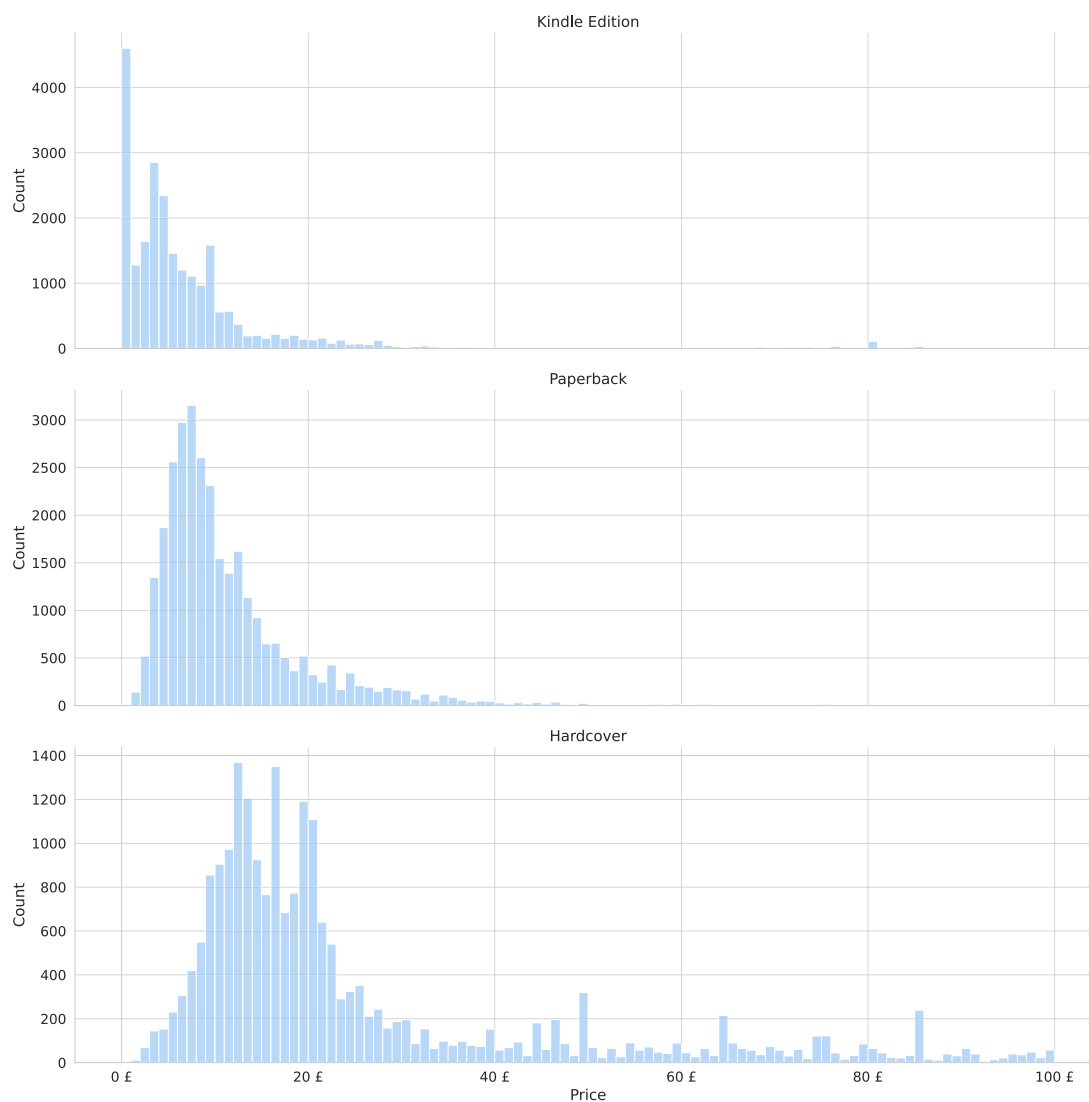
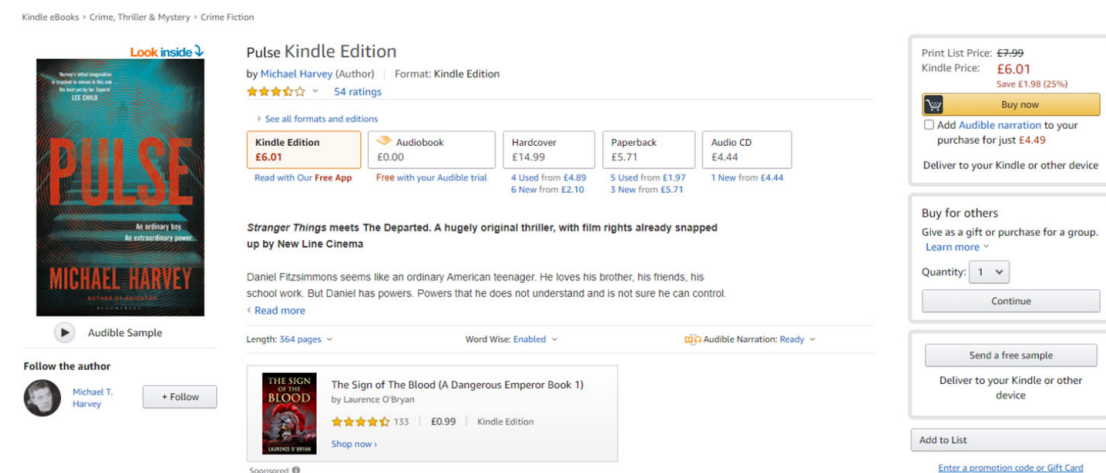


Fig. 5 Distribution of retail prices by book format

The screenshot shows the Amazon.co.uk product page for the book 'Elon Musk: How the Billionaire CEO of SpaceX and Tesla is Shaping our Future' by Ashlee Vance. The page layout includes:

- Navigation Bar:** Links to Kindle Books, Kindle Unlimited, Prime Reading, Kindle Book Deals, Best Sellers & more, Free Reading Apps, Buy A Kindle, Newsstand, and Audible Audiobooks.
- Product Header:** The book title, author name, and a 'Look inside' button.
- Book Cover:** A portrait of Elon Musk with the title 'ELON MUSK' and subtitle 'HOW THE BILLIONAIRE CEO OF SPACEX AND TESLA IS SHAPING OUR FUTURE' by Ashlee Vance.
- Rating and Reviews:** A 5-star rating with 6,985 ratings.
- Format Selection:** Buttons for Kindle Edition (£5.49), Audiobook (£0.00), Hardcover (£13.92), Paperback (£8.19), and Audio CD (£64.95). Each button includes a 'Read with our Free App' or 'Free with your Audible trial' label.
- Description:** A paragraph about Elon Musk's achievements and the book's content, followed by a 'Read more' link.
- Author Information:** A section for 'Follow the author' with a link to Ashlee Vance's profile.
- Purchase Options:** A 'Buy now' button and a 'Deliver to your Kindle or other device' option.
- Additional Features:** A 'Buy for others' section, a 'Send a free sample' button, and a 'Deliver to your Kindle or other device' option.
- Sponsored Section:** A promotional banner for 'The 1-Page Marketing Plan' by Allan Dib, priced at £1.99.

Fig. 6 Screenshot of *Elon Musk: How the Billionaire CEO of SpaceX and Tesla is Shaping our Future* (Amazon.co.uk)

Fig. 7 Screenshot of *Pulse* (Amazon.co.uk)**Table 10** Robustness check with paperback books. Dependent variable is the difference in the retail price between digital and paperback versions of book titles sold on Amazon

	Dependent variable: Kindle Price - Paperback Price		
	(1)	(2)	(3)
Agency	−0.385*** (0.111)	−0.265** (0.114)	−0.436*** (0.113)
log star rating	1.811*** (0.671)	1.597** (0.667)	1.039 (0.676)
No. expert reviews	−0.012 (0.042)	−0.011 (0.042)	0.024 (0.039)
log RRP	0.378*** (0.140)	0.330** (0.158)	0.714*** (0.151)
Date Retail	−0.038 (0.029)	−0.047* (0.029)	−0.038 (0.028)
Bestsellers	0.007*** (0.002)	0.007*** (0.002)	0.008*** (0.002)
Constant	−3.218*** (0.381)	−3.333*** (0.713)	−4.461*** (0.463)
Genre	No	LDA	Amazon
Robust <i>F</i> Statistic	8.0357	6.7923	19.4451
Observations	3,723	3,723	3,723
Adjusted <i>R</i> ²	0.016	0.032	0.059
Robust standard errors in parentheses	* <i>p</i> < 0.1; ** <i>p</i> < 0.05; *** <i>p</i> < 0.01		

Table 11 Robustness check with paperback books. Dependent variable is the difference in the log retail price between digital and paperback versions of book titles sold on Amazon

	Dependent variable: log(Kindle Price) - log(Paperback Price)		
	(1)	(2)	(3)
Agency	−0.099*** (0.020)	−0.077*** (0.020)	−0.106*** (0.020)
log star rating	0.300*** (0.104)	0.243** (0.102)	0.142 (0.103)
No. expert reviews	0.011 (0.008)	0.013 (0.008)	0.020*** (0.007)
log RRP	0.316*** (0.021)	0.309*** (0.023)	0.369*** (0.023)
Date Retail	0.002 (0.005)	−0.001 (0.005)	0.001 (0.005)
Bestsellers	0.001** (0.001)	0.001** (0.001)	0.002*** (0.001)
Constant	−1.195*** (0.061)	−1.279*** (0.126)	−1.373*** (0.072)
Genre	No	LDA	Amazon
Robust <i>F</i> Statistic	38.1151	27.3776	25.7949
Observations	3,723	3,723	3,723
Adjusted <i>R</i> ²	0.092	0.117	0.141
Robust standard errors in parentheses	* <i>p</i> < 0.1; ** <i>p</i> < 0.05; *** <i>p</i> < 0.01		

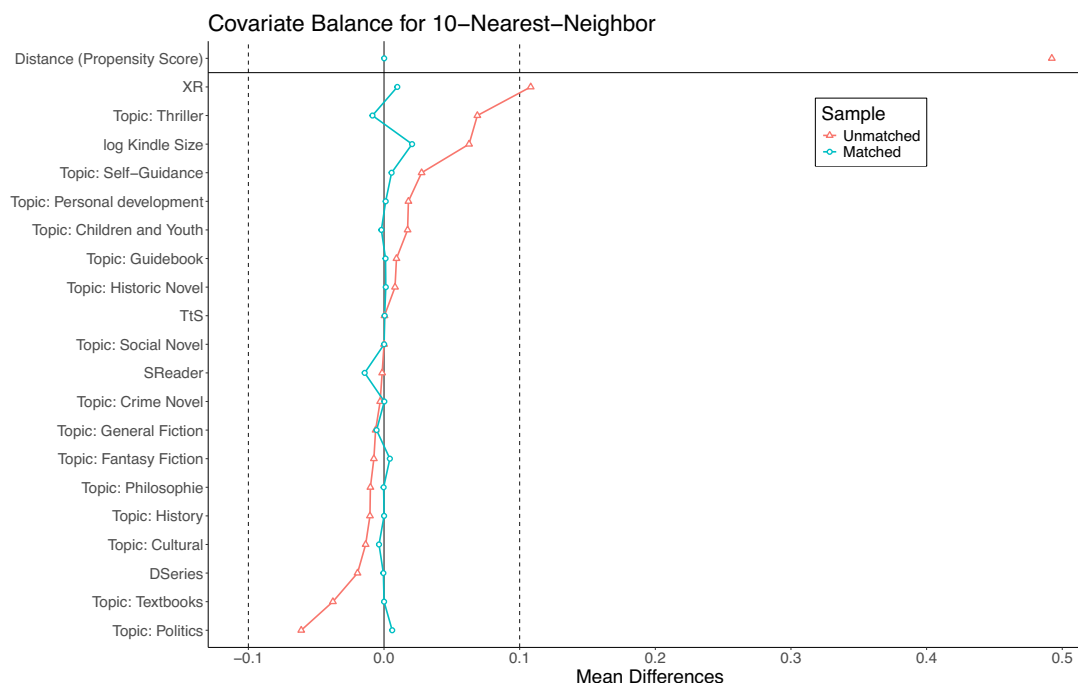


Fig. 8 Covariate balance for 10-nearest-neighbors (see column 5 of Table 4a in Sect. 5.1). The x -axis represents standardized mean differences, while the y -axis lists the covariates introduced in Sect. 3.3. The figure compares the balance between unmatched (red line) and matched (blue line) samples. The goal of matching is to reduce imbalances between the treatment and control groups, a goal reflected by the blue line moving closer to zero relative to the red line. Dashed vertical lines indicate a common balance thresholds (e.g., 0.1), which assist in assessing whether covariates are sufficiently balanced post-matching. The results suggest that the matching procedure significantly reduces imbalances for all covariates

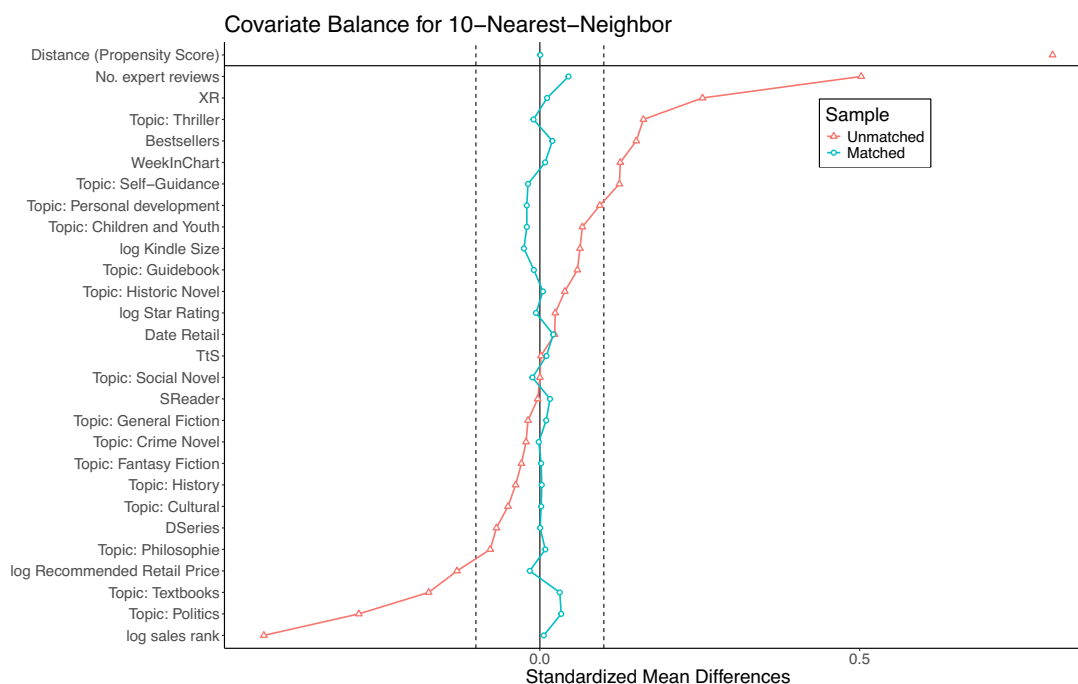


Fig. 9 Covariate balance for 10-nearest-neighbors (see column 5 of Table 4b in Sect. 5.1). Refer to Fig. 8 for interpretation details

Appendix 2: Latent Dirichlet allocation (LDA)

In the recent past, new technologies have enabled the use of text as data, thereby, integrating it as an input to economic research. Text data, which is inherently high-dimensional, can capture relevant economic concepts not covered by *hard* economic data. In the recent years, there has been an explosion of empirical economics research using text as data (e.g., see Larsen and Thorsrud (2019) for a latent Dirichlet allocation (LDA) approach or Lenz and Winker (2020) for paragraph vector topic modeling). We have decided to use an LDA approach to generate book genres and to assign each single book title from our data set into one of these genres. Such a text mining approach is necessary because on the Amazon webpage, genre information is ambiguous or even unavailable for some book titles. For this purpose, we use the descriptions and expert reviews from individual books in our data set as text data input. We further rely on natural language processing (NLP) to extract the relevant information.

We apply several Python modules to clean and prepare the raw data set.²⁷ This process involves removing common words and surnames, eliminate stop words, stripping punctuation and pronouns, and reducing all words to their respective word stems. After this filtering process, we retain approximately 45,819 unique tokens.

This cleaned descriptions corpus is decomposed into book genres using the previously mentioned LDA model. LDA provides a statistical framework for generating documents based on topics. It is an unsupervised topic model that clusters words into topics (or genres), which are distributions over words, and classifies descriptions as mixtures of these topics/genres. The term *latent* is used because the words are intended to communicate an underlying structure, namely, the subject matter (topic) of the description. The term *Dirichlet* is used because the topic mixture is drawn from a conjugate Dirichlet prior (Thorsrud 2020).

The structure of the LDA model is as follows: the whole corpus is represented by M distinct documents (descriptions) and $N = \sum_{m=1}^M N_m$ is the total number of words in all documents. Assuming K latent topics/ genres, each topic is given by a probability vector $\phi_k = (\phi_{k,1}, \dots, \phi_{k,N})$ with $\sum_{n=1}^N \phi_{k,n} = 1$ indicating the probability that each word shows up in this topic. Further, each document $m \in \{1, \dots, M\}$ contains all topics with different probabilities (weights) $\theta_m = (\theta_{m,1}, \dots, \theta_{m,K})$ with $\sum_{k=1}^K \theta_{m,k} = 1$. Both ϕ_k and θ_m are assumed to have conjugate Dirichlet distributions with hyperparameters (vectors) α and β , respectively.

Given ϕ_k and θ_m , a document is generated by drawing for each word a topic $k \in \{1, \dots, K\}$ according to the probabilities θ_m and one word from the selected topic according to its distribution ϕ_k . This procedure is repeated until the length of the document is reached. To solve the LDA model, we a priori set $\alpha = 50$ and $\beta = 0.01$. The hyperparameter optimization is executed by using Gibbs simulations. Gibbs sampling (also known as alternating conditional sampling) is a specific form of Markov chain Monte Carlo and simulates a high-dimensional distribution by sampling on lower-dimensional subsets of variables where each subset is conditioned on the value of all others (e.g., Steyvers and Griffiths 2007).

The sampling is performed sequentially and proceeds until the sampled values approximate the target distribution. We set the number of sampling iterations equal to 1000. Then, based on different coherence values across the estimated LDA models with varying numbers of genres, we found that 15 topics/genres provide an optimal statistical decomposition of our book description corpus. This number also provides a comprehensible set of topics that can

²⁷ The base module is gensim by Řehůřek and Sojka (2010), an open-source NLP text analytics tool.

Table 12 OLS estimation approach for different genres generated by LDA or given by Amazon. Dependent variable is the logarithm of the retail price for e-books sold on Amazon

	Dependent variable: log Price				
	(1)	(2)	(3)	(4)	(5)
Agency	−0.204*** (0.011)	−0.210*** (0.011)	−0.210*** (0.011)	−0.210*** (0.011)	−0.202*** (0.011)
log sales rank	0.056*** (0.004)	0.055*** (0.004)	0.060*** (0.004)	0.059*** (0.004)	0.051*** (0.004)
log Kindle Size	0.045*** (0.004)	0.045*** (0.004)	0.050*** (0.004)	0.050*** (0.004)	0.027*** (0.004)
log star rating	0.502*** (0.048)	0.492*** (0.049)	0.491*** (0.048)	0.484*** (0.048)	0.436*** (0.048)
No. expert reviews	0.013*** (0.004)	0.014*** (0.004)	0.015*** (0.004)	0.015*** (0.004)	0.018*** (0.004)
log RRP	1.090*** (0.011)	1.094*** (0.011)	1.076*** (0.011)	1.071*** (0.012)	1.080*** (0.012)
Date Retail	0.011*** (0.003)	0.011*** (0.003)	0.013*** (0.003)	0.012*** (0.003)	0.008*** (0.003)
Bestsellers	0.001*** (0.0004)	0.001*** (0.0004)	0.001*** (0.0004)	0.001*** (0.0004)	0.001*** (0.0004)
WeekInChart	0.007*** (0.002)	0.007*** (0.002)	0.007*** (0.002)	0.007*** (0.002)	0.007*** (0.002)
Constant	−1.987*** (0.074)	−1.960*** (0.074)	−1.970*** (0.075)	−2.204*** (0.084)	−1.745*** (0.074)
No. Genre Topics	9	10	12	15	Amazon Genres
Observations	12,001	12,001	12,001	12,001	12,001
Adjusted R^2	0.608	0.608	0.610	0.610	0.615
Robust standard errors in parentheses	* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$				

be attributed to officially available genres.²⁸ A detailed list of all 15 genres is presented in Table 13.

A caveat of the LDA estimation procedure is that it does not assign names or labels to the topics/genres. Thus, labels are subjectively assigned to each genre based on the most important words associated with each topic. In most cases, classifying the genres is conceptually straightforward. Besides, the exact labeling plays no material role in our empirical approach; it is merely a convenient way of referring to the different topics instead of solely using topic numbers.

It is more important that the LDA decomposition yields a meaningful and easily interpretable genre classification of the book descriptions. Our LDA approach achieves this by identifying all important book genres and clearly delineating the topics. This is demonstrated in Figs. 10, 11, and 12, which illustrate three examples of our 15 word clouds. These clouds

²⁸ For 15 genres, the coherence value exhibits a local optimum when the elbow method is applied (Thorndike 1953). We also considered 9, 10, and 12 genres, but the agency effect remained robust when controlling for a different number of genres (see Table 12).

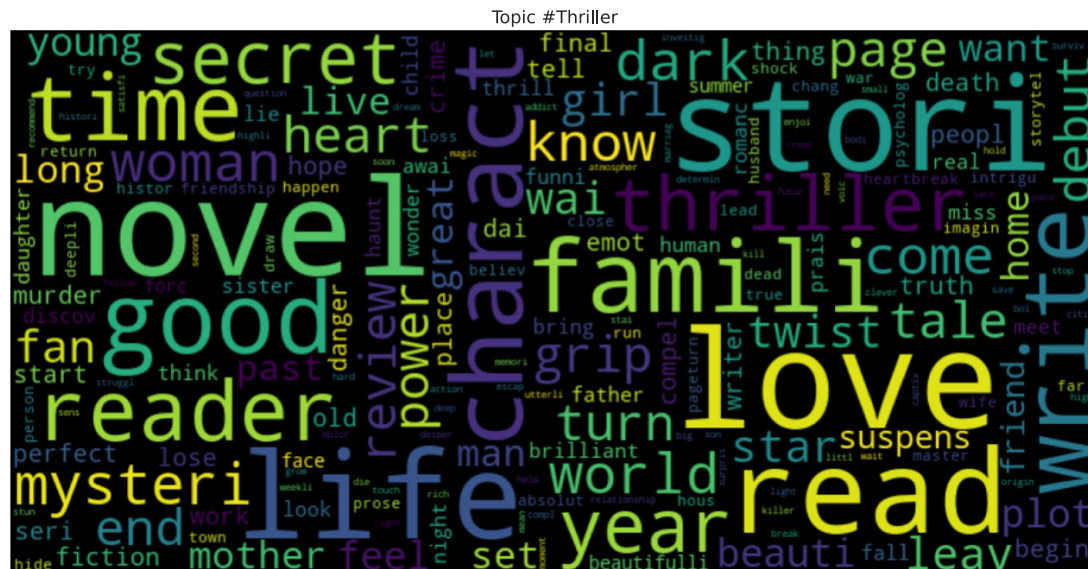


Fig. 11 Topic Thriller from the LDA. Size is according to weight of word within the topic

certain topics, but all publishers sell book titles from different genres. This is an advantage for our empirical approach, as it mitigates potential multicollinearity issues. From an economic perspective, it ensures we can meaningfully compare publishers, avoiding a scenario where specific topics were exclusively from specific publishers (which would preclude comparison).

However, it is evident that individual publishers exhibit distinct primary topics. For instance, Pan Macmillan primarily publishes fiction titles like crime novels, thrillers, or society novels, whereas HarperCollins focuses on family novels and drama genres. However, it is important not to take these topics at face value, as the LDA assigns a probability to each topic.



Fig. 12 Topic Politics from the LDA. Size is according to weight of word within the topic

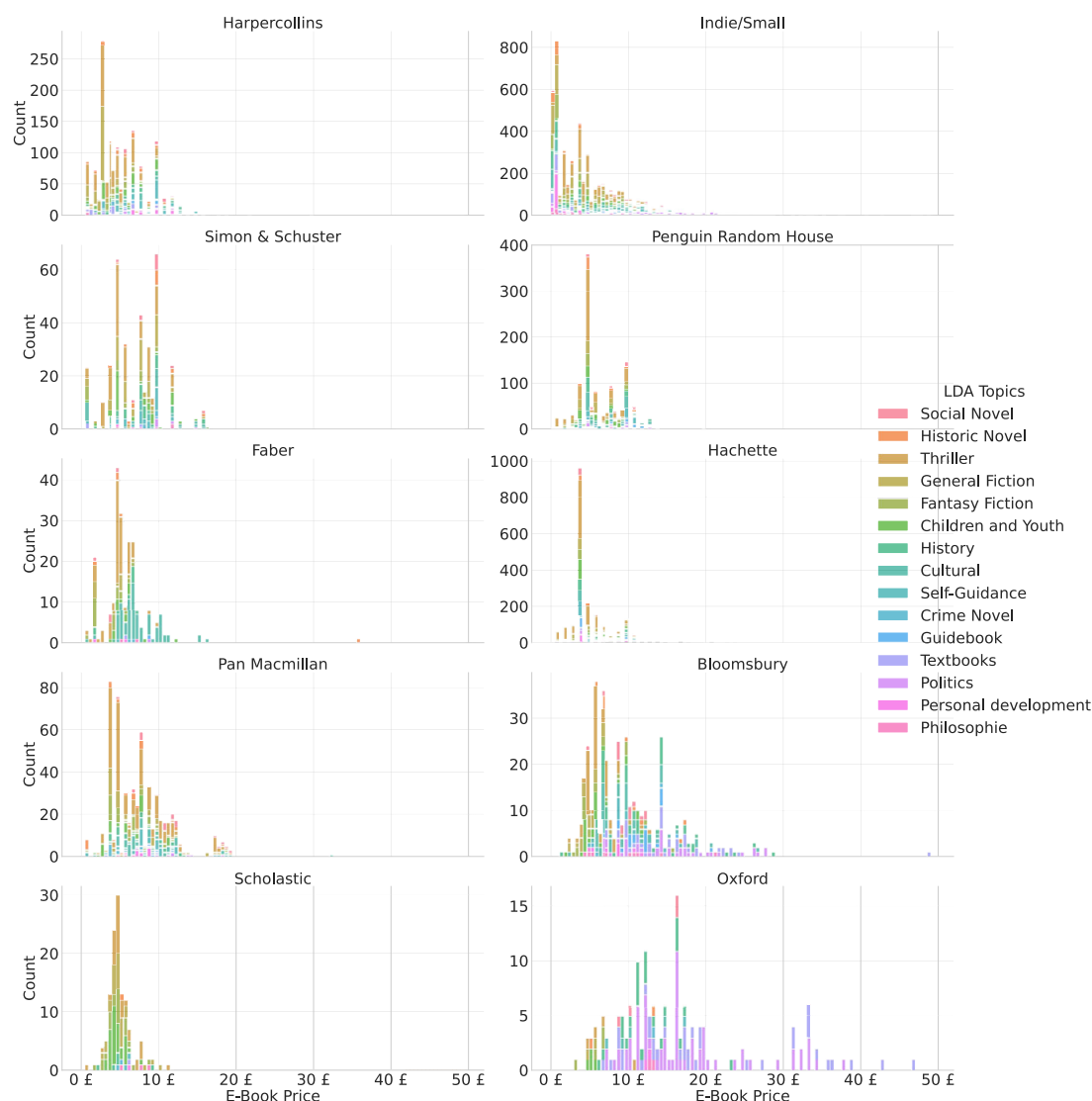


Fig. 13 Prices for e-books grouped by publisher and genre. The ordinate is scaled differently for each subplot

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Declarations

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Informed Consent N/A

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4 Transport Market

Derailing the Car? The Effect of Germany's Deutschlandticket on Road Traffic

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Derailing the Car? The Effect of Germany's Deutschlandticket on Road Traffic*

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Abstract

This paper examines the causal impact of Germany's *Deutschlandticket*, a flat rate nationwide public transport ticket introduced in May 2023, on extra-urban passenger traffic. Using a synthetic difference-in-differences approach, we construct a counterfactual for German traffic volumes utilizing automatic counting station data from Austria, Switzerland, Finland, and Great Britain. Contrary to the policy's objectives, we find that this drastic fare reduction failed to induce a lasting modal shift away from private cars. While a temporal analysis reveals a transient traffic reduction during the initial summer months of the policy (approximately 7.5%), this substitution effect vanishes rapidly and does not persist into the subsequent year. These findings are robust to the spatial composition of the donor pool, as well as to road-type and day-of-week heterogeneity. Our results are consistent with recent literature suggesting that standalone demand-side subsidies are insufficient to overcome non-monetary generalized costs, such as the reliability issues and capacity constraints currently affecting the German rail network. Consequently, the efficacy and efficiency of the *Deutschlandticket* as a climate policy instrument remains highly questionable.

Keywords: Flat-Rate Public Transport Policy, Modal Shift, Deutschlandticket, Synthetic Difference-in-Differences.

JEL Codes: R48, R41, L91.

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1 Introduction

In May 2023, the German government has introduced the *Deutschlandticket* as a permanent nationwide local public transport (PT) ticket.¹ This ticket is a subscription model and is valid on all local and regional buses and trains throughout Germany. The initial monthly price was €49, with a price increase to €58 at the beginning of 2025 and a further price increase to €63 in 2026. At the end of 2025, approximately 14.6 million people used the ticket². The *Deutschlandticket* is the successor ticket of the *9-Euro-Ticket*, which provided travelers the unlimited usage of local PT (e.g., regional trains, subways, and buses) for €9 per month between June and August 2022.³ This pricing strategy provides travelers unlimited access to local PT at a fraction of conventional fare costs. Because it includes the entire suburban and regional rail as well as bus network, the ticket heavily targets short- and medium-distance commuters traveling between suburban areas and urban centers. To contextualize the financial relief for travelers, under the standard 2024 tariff structure of the Rhine-Main Transport Association, a standard monthly ticket for a major urban center like Frankfurt (fare zone 3) cost €106.20, while a single daily ticket cost €8.10.⁴ Consequently, even with increased prices up to €63 in 2026, the *Deutschlandticket* undercuts usual standard regional pricing substantially, aiming to incentivize a modal shift toward PT, reduce fuel consumption, and hence decrease greenhouse gas emissions.

The direct costs for the *Deutschlandticket* amount to 3 billion Euros per year, divided equally between the German government and the federal states.⁵ Although its predecessor ticket was primarily intended to relieve the financial burden on citizens, both interventions simultaneously simplified the highly fragmented tariff structures of Germany's local transport associations. Additionally, the government aimed to incentivize a modal shift towards PT

¹See <https://dserver.bundestag.de/btd/20/055/2005548.pdf>.

²See <https://www.vdv.de/deutschlandticket.aspx>.

³See <https://www.bmdv.bund.de/SharedDocs/DE/Artikel/K/9-euro-ticket-beschlossen.html>.

⁴See https://www.vgf-ffm.de/fileadmin/VGF/Tickets__Tarife__Plaene/Fahrtpreise/Documents/RMV-Preisliste_2024.pdf.

⁵See *Regionalisierungsgesetz* - RegG, § 9.

to reduce fuel consumption.⁶ Economically, the *Deutschlandticket* represents a lasting nationwide introduction of a uniform price setting for local PT. This could eliminate regional price signals that previously captured the value of services, potentially obscuring demand information necessary for optimal capacity planning. As a consequence, this may contribute to capacity challenges and variations in service quality (Liebensteiner et al., 2024).

Governments frequently intervene in the market by means of subsidies to decrease fares. Such interventions – in the form of subsidies – might be reasonable and justified based on two economic grounds: first, to utilize positive externalities through higher PT frequency and lower opportunity costs for the passengers (Mohring, 1972; Button, 2010); second, to internalize the negative externality of motorized individual traffic by making PT traffic relatively more appealing (Basso and Silva, 2014; Parry and Small, 2009). However, the efficacy and efficiency of this second channel relies heavily on the cross-price elasticity between car and PT usage. Studies consistently demonstrate that car demand is highly inelastic to PT fares, with cross-elasticities often close to zero (e.g., Fearnley et al., 2017)). Consequently, empirical evaluations of drastic price reductions, such as fare-free public transport (FFPT) schemes, typically find that ridership gains stem from induced demand or shifts from active modes like walking, rather than a meaningful substitution of car trips (e.g., Cats et al., 2017).

In this paper, we evaluate the causal effect of the *Deutschlandticket* on extra-urban passenger traffic in Germany. Specifically, we analyze whether the drastic, nationwide fare reduction induced a significant decrease in traffic outcomes, serving as a proxy for a modal shift from private cars to PT. Such substantial fare reductions might impact individuals’ decisions to lower their car usage, whether by substituting daily trips or even reducing vehicle ownership. To do so, we compile a comprehensive dataset of traffic volumes from automatic counting stations covering the German highway network and employ the synthetic difference-in-differences (SDiD) approach proposed by

⁶See, e.g., for the *9-Euro-Ticket* <https://www.bundestag.de/dokumente/textarchiv/2022/kw20-de-neun-euro-ticket-894660>, and the *Deutschlandticket* <https://www.bundesregierung.de/breg-de/aktuelles/deutschlandticket-2134074>.

Arkhangelsky et al. (2021). We construct a valid counterfactual using traffic data derived from automatic counting stations in four European countries: Austria, Switzerland, Finland, and Great Britain.

Our results indicate that the introduction of the ticket did not lead to a lasting reduction in aggregate traffic volumes. While we identify a transient, statistically significant reduction of 7.5 % during the first summer of implementation (June to August 2023), this effect dissipates rapidly. The aggregate average treatment effect on the treated (ATT) remains economically negligible and statistically indistinguishable from zero, suggesting that the fare reduction failed to alter driving outcomes and induce a modal shift to PT.

The remainder of the paper is structured as follows: Section 2 reviews the related literature regarding subsidies in PT, modal shift and uniform fare strategies, often introduced through FFPT. Section 3 presents the data and descriptive statistics. Section 4 outlines the empirical strategy, followed by the main estimation results in Section 5. Section 6 provides robustness and sensitivity analyses, Section 7 discusses policy implications, and Section 8 concludes the paper.

2 Related Literature

The literature on pricing and subsidies in public transport (PT) demonstrates that while fare reductions reliably increase transit ridership, their capacity to induce a modal shift from private cars is structurally limited. This limitation is grounded in the underlying structure of demand. While PT is broadly own-price inelastic among existing users – as lower fares rarely induce them to travel more frequently – policies targeting negative externalities rely on attracting new users from other modes. Their success depends on mode substitution rather than generating new mobility, making the cross-price elasticity of private car use with respect to transit fares the critical parameter. Empirical evidence demonstrates that this cross-price elasticity is consistently

inelastic and close to zero.⁷ Consequently, even substantial fare reductions are expected to induce only marginal decreases in car usage (Paulley et al., 2006; Litman, 2004). Although there is some evidence that ticket price reductions can cause a demand increase in PT usage (Wallimann et al., 2023; Storchmann, 2003), empirical evidence for the presence of a modal shift away from private cars remains scarce.

Several structural factors drive this low cross-mode substitutability. First, demand elasticities vary by trip purpose and time, with peak-hour commuting exhibiting lower price sensitivity than off-peak leisure travel (e.g., Horn af Rantzien and Rude, 2014; Paulley et al., 2006). Second, intertemporal dynamics matter: short-run elasticities are minimal, whereas long-run elasticities increase as permanent price changes influence discrete household choices, such as vehicle ownership and residential location (Litman, 2004; Wardman et al., 2018). Third, the elasticity may also differ with respect to urban or rural areas, since PT availability is lower in rural areas, making individuals more reliant on private cars (Paulley et al., 2006). According to Cools et al. (2016), a zero fare policy (such as an FFPT) could have a much greater effect compared to a pure fare reduction. Finally, because motorized individual traffic often accounts for the vast majority of the passenger mode share, even a marginal percentage reduction in car usage requires a massive proportional surge in PT ridership (Fearnley et al., 2017; Paulley et al., 2006).

Furthermore, endogenous quality degradation serves as a critical barrier to cross-mode substitution. In the presence of capacity constraints, drastic fare reductions induce significant new transit demand, which subsequently exacerbates station crowding, reduces in-vehicle comfort, and increases delays (Lu et al., 2024; Liebensteiner et al., 2024). As crowding externalities strictly increase the non-monetary generalized cost of travel, car drivers –

⁷A broad review of the empirical literature confirms that the own-price elasticity of PT demand is generally inelastic (e.g., Holmgren, 2014; Paulley et al., 2006; Litman, 2004; Wallimann et al., 2023; Fearnley et al., 2017; Horn af Rantzien and Rude, 2014; Holmgren, 2007; Kholodov et al., 2021). More importantly for modal shift dynamics, cross-price elasticity estimates of car demand with respect to transit fares consistently cluster near zero (e.g., Glaister, 2001; Litman, 2004; Wardman et al., 2018; Fearnley et al., 2017).

who typically exhibit high values of travel time savings and lower tolerances for congestion – are deterred from switching modes (Tirachini et al., 2013; Basso and Silva, 2014).

As summarized in Table 1, a growing body of empirical literature evaluates various fare subsidies and FFPT implementations. The evidence spans multiple geographic contexts and methodological approaches, consistently highlighting the friction between ridership gains and cross-mode substitution.

A vast body of literature deals with the effect of FFPT policies on travel demand. Fare reductions are generally able to increase ridership (e.g., Cats et al., 2014; Mendez, 2025). However, substantial surges in FFPT ridership are predominantly driven by induced demand and mode shifts from active transport (walking and cycling), rather than from private cars (e.g., Cats et al., 2017). Evaluations of the 2013 FFPT introduction in Tallinn identify mobility improvements primarily for low-income residents, with minimal uptake among working-age commuters (Cats et al., 2014; Cats et al., 2017). Similar substitution patterns emerge in Hesse, Germany (Busch-Geertsema et al., 2021), and in randomized field experiments in Santiago, Chile, where free transit passes increased off-peak subway utilization by 23% without inducing substitution away from cars (Bull et al., 2021). Although a fare abolition in Templin, Germany, substantially increased transit ridership, a mode shift was found for pedestrians and bicycles only, but not for cars (Storchmann, 2003). The assessment of Austria’s *Klimaticket*, a nationwide public transport pass offering users unlimited access to PT services throughout Austria for an annual fee of €1,095, did not identify any statistically significant increase in passenger volumes (Wallimann, 2024). While experimental evidence suggests targeted, temporary free tickets might induce marginal shifts (Fujii and Kitamura, 2003), the literature broadly concludes that standalone fare subsidies fail to reduce car usage efficiently. Effective internalization requires pairing subsidies with policies that directly increase the generalized cost of driving (Parry and Small, 2009; Cats et al., 2017; Fearnley et al., 2017).

Recent literature examining the German *9-Euro-Ticket* and the subsequent *Deutschlandticket* – both weak forms of a FFPT policy – predomi-

Reference	Setup	Method	Data	(Main) Outcome Vari- ables	(Main) Result
Fujii and Kitamura (2003)	One-month experiment of free bus tickets in Kyoto (Japan)	Before-and-after comparison (immediately after and one month after experiment)	Survey of participants	Attitudes towards, habits of, and frequency of using private vehicles and bus	(+) attitudes towards bus, (+) frequency of bus use, (-) habits of using automobile
Storchmann (2003)	Introduction of FFPT in Templin (Germany)	Before-and-after comparison and evaluation of survey data	Ridership data provided by the city of Templin & survey of PT users	Transit ridership, modal shift	(+) use of PT, (+) shift from pedestrians and bicycles to PT, (+) minimal shift cars to PT
Cats et al. (2014)	Introduction of FFPT to Tallinn’s residents in January 2013	Before-and-after comparison (3 months after the introduction)	Automated vehicle location and automatic passenger count data	Passenger demand (measured by number of boarding passengers)	(+) passenger demand, when accounting for supply variables (+1.2%)
Cats et al. (2017)	Introduction of FFPT to Tallinn’s residents in January 2013	Before-and-after comparison (1 year after the introduction)	Interviews and travel diaries of 1,500 random households	Modal shift, travel attitudes and satisfaction, trip destination choices	(+) PT usage (+14%), (-) walking trips (-40%), (-) car trips and share (-10% and -5%), (+) car vehicle-km (+31%)
Bull et al. (2021)	Random assignment of free public transport tickets in Santiago (Chile)	Randomized Control Trial	Trip diary	Number and time of trips by different modes	(+) overall travel (12%)
Busch-Geertsema et al. (2021)	Introduction of FFPT for state employees in Hesse (Germany)	Before-and-after comparison	two-wave survey at Goethe University Frankfurt	Public transport use, car use, car availability	(+) use of public transport
Gohl and Schrauth (2022)	Introduction of <i>9-Euro-Ticket</i> in Germany	Difference-in-differences	Air pollution data	Air pollution index	(-) air pollution (-8%)
Wallimann et al. (2023)	Reduction of public transport ticket prices in Geneva (Switzerland)	Synthetic control method	Transport companies’ annual reports	Public transport use (passenger trips per vehicle kilometer)	(+) public transport demand (+10.6%)
Andor et al. (2023)	Introduction of <i>9-Euro-Ticket</i> in Germany	Difference-in-differences	Surveys on mobility data	Sum of trips and distances by mode; cost-benefit analysis FFPT	(-) car use (-10%), (+) public transport use
Aydin and Kürschner Rauck (2023)	Introduction of <i>9-Euro-Ticket</i> in Germany	Difference-in-differences	Air pollution data	Particulate matter	(-) air pollution
Liebensteiner et al. (2024)	Introduction of <i>9-Euro-Ticket</i> in Germany	Difference-in-differences, Event study design	mobile network-based mobility, traffic volume, and rail traffic data	number, purpose and mode choice of trips, train delay	(+) train trips (+34%), (-) number of passenger cars (-1.4%), (+) train delays (+4%p)
Loder et al. (2024)	Introduction of <i>9-Euro-Ticket</i> in Germany	Before-during-after comparison	Mobilität.Leben study	public transport trips, private transport trips	(+) public transport use, (-) car use
Wallimann (2024)	Introduction of the KlimaTicket in Austria	Synthetic control method & synthetic difference-in-differences	Yearly data of PT users provided by PT companies	Rail travel demand	No short-run effect on passenger growth
Guajardo Ortega and Link (2025)	Introduction of <i>9-Euro-Ticket</i> in Germany	Mixed logit model	GPS-tracked trips	Mode inertia	(+) positive inertia for car
Schlett and Loder (2025)	Introduction of <i>Deutschlandticket</i> in Germany (focusing on the Munich metropolitan region)	Before-and-after comparison	Multimonth, semipassive, smartphone-based GPS tracking data (Mobilität.Leben study)	Cost of travel, travel distance, activity time, and travel direction	(+) PT travel distance, (-) car travel distance, (+) monetary savings for ticket holders
Waldorf et al. (2025)	Introduction of <i>9-Euro-Ticket</i> & <i>Deutschlandticket</i> in Germany	Propensity score matching	Mobilität.Leben study	activity participation, mode use, financial relief, and attitudes	(+) increase in PT demand, (-) car use

Table 1: Overview of empirical evaluations regarding FFPT schemes and extreme fare reductions.

nantly relies on survey and descriptive data (e.g., Rozynek, 2024), though recent structural and quasi-experimental approaches provide more robust causal estimates. Applying a dynamic difference-in-differences (DiD) framework, Liebensteiner et al. (2024) identify a strong increase in leisure rail travel coupled with notable adverse effects on rail infrastructure quality, but find limited substitution between transportation modes. Using GPS-tracked trip data, Guajardo Ortega and Link (2025) document significant inertia effects among car users, resulting in negligible and small modal shifts from car to public transport. Applying a propensity score matching method, Waldorf et al. (2025) similarly present an increase in PT trips that is, however, paired with only a small decrease in car trips, suggesting that fare reductions alone cannot stimulate a mode shift.

Survey-based and descriptive evaluations yield heterogeneous, albeit slightly more optimistic, estimates of mode substitution. Concerning the *9-Euro-Ticket*, on the one hand, aggregate mobility data indicates a 44% increase in PT demand alongside minimal reductions in motorized private transport.⁸ On the other hand, survey analyses estimate that 10% to 20% of ticket holders substituted at least one car trip, corresponding to an approximate 10% reduction in aggregate vehicle kilometers traveled (Loder et al., 2024; Andor et al., 2023).⁹

Furthermore, early evidence explores the environmental externalities of the *9-Euro-Ticket*. Utilizing a DiD design, Gohl and Schrauth (2022) demonstrate that the intervention reduced a benchmark air pollution index by over 6%. Similarly, Aydin and Kürschner Rauck (2023) identify a significant short-term reduction in particulate matter emissions, attributing this decline to reductions in car usage.

Finally, expanding on spatial and temporal dimensions, Schlett and Loder (2025) analyze the *Deutschlandticket* using multi-month, smartphone-based GPS tracking. They reveal that the ticket’s effects extend beyond pure monetary relief, varying significantly across fare zones and weekdays. Notably, the

⁸See the Federal Statistical Office https://www.destatis.de/DE/Presse/Pressemitteilungen/2022/09/PD22_377_12.html.

⁹Industry reports echo these survey findings; see the Association of German Transport Companies (VDV) evaluation at <https://www.vdv.de/bilanz-9-euro-ticket.aspx>.

fare innovation induced a spatial substitution effect: ticket holders increased their public transit travel primarily within the city center while decreasing their car travel in suburban zones.

3 Data and Descriptive Statistics

3.1 Data

Our data set covers extra-urban, non-local motor vehicle traffic volumes for Germany and the control countries – Austria, Switzerland, Finland, and Great Britain – from January 2018 to December 2024.¹⁰ For each country, our data set includes observations of automatic and permanent traffic counting stations in the respective road network. To harmonize reporting frequencies, station-level data are aggregated to monthly traffic counts for cars, vans and motorcycles (vehicles weighing < 3.5 tons), pooling all lanes and directions.¹¹ The panel is balanced by restricting the sample to stations continuously active from January 2018 through December 2024, dropping any unit with missing monthly observations. However, we retain stations where daily reporting gaps were bridged by the data providers’ estimation procedures, provided a valid monthly aggregate is available.¹² Finally, we align heterogeneous national road definitions into two categories: motorways (such

¹⁰Data sources: Germany: *Federal Highway Research Institute (BAST)*, Austria: (*Motorway and Expressway Financing Joint-Stock Company (ASFiNAG)*), Switzerland: *Swiss Federal Roads Authority (ASTRA)*, Finland: *Fintraffic*, and Great Britain: Department for Transport.

¹¹We harmonize the reporting frequencies by aggregating the raw data to a monthly resolution, a step that smooths daily and weekly fluctuations. For Germany, Finland, Switzerland, and Great Britain, this involves aggregating raw hourly data. However, in the case of Austria, the data is reported as monthly daily averages separated by day type (weekdays vs. weekends). To ensure comparability, we reconstruct the total monthly flow for Austria by projecting these average counts into a single aggregate figure.

¹²A balanced panel is a prerequisite for the SDiD estimator used in this analysis. Furthermore, restricting the sample prevents compositional bias arising from differential station entry or exit within countries.

as German Autobahn) and main roads (such as German Bundesstraße).¹³

While our counting station data primarily captures extra-urban highway traffic (motorways and main roads), these networks constitute critical commuting corridors connecting suburban and rural populations to major urban centers. In Germany, a substantial share of the workforce commutes across municipal boundaries, heavily relying on highway roads and the rail network. According to the Federal Institute for Research on Building, Urban Affairs and Spatial Research, in 2024 approximately 20.5 million employees in Germany (approximately 60% of the total) commuted to a different municipality for work. The average one-way commute distance was 17.2 kilometers, with 7.23 million employees commuting more than 30 kilometers.¹⁴ Crucially, the *Deutschlandticket* encompasses all regional rail, suburban rail, and bus networks, which run parallel to these exact motorways and serve as the primary public transport substitutes for medium-distance car commuters. Since approximately 65% of commuters rely on private cars,¹⁵ our dataset on highways and federal roads captures flows targeted by the policy.

The control group is constructed to ensure socio-economic comparability with independence from local spillovers. The DACH-region (Germany, Austria, Switzerland), which encompasses more than 100 million individuals who largely exhibit similar socio-economic conditions, despite regional variations within and across each country¹⁶, shares high economic integration and contiguous road networks, ensuring the control group is exposed to similar

¹³We define motorways and main roads for each country, categorizing country-specific road types into these two classes. Broadly, we adhere to the European Commission report on motorways and the Vienna Convention on Road Signs and Signals (<https://road-safety.transport.ec.europa.eu/system/files/2021-07/ersosynthesis2018-motorways.pdf>). Here, a motorway/highway is a high-capacity road designed exclusively for motor traffic, featuring separate carriageways, no at-grade crossings, and controlled access. In contrast, a main road (such as a Bundesstraße in Germany) is a major road connecting cities and regions, allowing mixed traffic, including slower vehicles, and often featuring intersections. While motorways ensure uninterrupted high-speed travel, main roads can accommodate both regional and local traffic with varying access controls.

¹⁴See <https://www.bbsr.bund.de/BBSR/DE/startseite/topmeldungen/pendeln-in-deutschland-2024.html>.

¹⁵See https://www.destatis.de/DE/Presse/Pressemitteilungen/2025/05/PD25_N027_13.html.

¹⁶See <https://www.statista.com/topics/4623/dach-countries/>.

regional demand shocks. Conversely, Finland and Great Britain are included to introduce geographically distinct observations. While these markets may differ in specific weather, institutional or socio-economic factors, their lack of physical road connectivity to Germany ensures that traffic volumes are independent of local cross-border diversion effects (i.e., thereby mitigating spatial spillover effects).

Furthermore, with the exception of Austria, none of the control countries introduced a comparable nationwide PT fare policy during the observation period. The Austrian government introduced a comparable PT ticket, the *Klimaticket* in the end of October 2021. Similar to the *Deutschlandticket*, it gives ticket holders unlimited and nationwide access to PT for an annual price of €1,095¹⁷. This development may affect the suitability of using Austrian traffic volume data in our empirical analysis. However, empirical evidence of the *Klimaticket* indicates that there is no statistically significant increase in PT passengers (Wallimann, 2024, p. 206-207), therefore we argue that Austrian car traffic is not affected by the policy. To further underline the robustness of our result, we will evaluate a leave-one-out analysis excluding Austrian counting points from our dataset (see Table 5), which does not alter our results.

Table 2 summarizes the different types of roads that we include in our data set and our categorization, as well as the number of available counting stations in our sample. Since the Finnish road type *Valtatie* (which translates to highway) is very heterogeneous compared to a German *Autobahn*, we use the number of lanes for each counting station in order to make road types more comparable. Each *Valtatie* with four or more lanes is categorized in the motorway category, otherwise in the main road category.

¹⁷See <https://www.bmimi.gv.at/themen/mobilitaet/1-2-3-ticket/fakten.html>, last accessed on February 19, 2026. By 2026, the price has increased to €1,400 for the standard ticket, see <https://www.klimaticket.at/#kosten>, last accessed on February 19, 2026.

Country	Motorway	Main road	Counting stations	Data source
Germany	Autobahn	Bundesstraße	777	<i>BASt</i>
Austria	Autobahn, Schnellstraße	-	104	<i>ASFiNAG</i>
Switzerland	Autobahn	Hauptstraße	50	<i>ASTRA</i>
Finland	Valtatie*	Kantatie, Valtatie*	309	Fintraffic
Great Britain	Motorway, road with motorway regulations	primary and secondary roads	132	Department for Transport
Total			1,372	

Table 2: Road types in the different countries and categorization in road types motorway or main road. Also displayed is the number of counting stations in our data set as well as data sources. The full data for Austria, Switzerland and Great Britain was available upon request. In order to make Finland’s road types more comparable to Germany’s classification, we split the road type Valtatie by the number of lanes. A Valtatie road with four or more lanes is categorized as motorway, with less than four lanes as main road.

Figure 1 illustrates the spatial coverage of counting stations across Great Britain, Finland, and the DACH-region. The counting stations are differentiated by road type, with dark blue markers representing locations on main roads and light blue markers indicating points on motorways, all overlaid against the road network in grey, where white lines delineating national and regional administrative boundaries. The distribution patterns reveal distinct regional characteristics: Great Britain and Germany exhibit high-density coverage corresponding to their complex road networks and population centers. In contrast, Finland displays a clear north-south divide, where monitoring is concentrated in the populous southern regions (in the vicinity of major cities such as Helsinki) and becomes sparser in the north. Similarly, the network in Austria and Switzerland reflects the constraints of alpine topography, with stations aligned primarily along major valleys and transit corridors. The Austrian sample, however, is restricted exclusively to motorways (Table 2). Overall, the visualization demonstrates that the dataset provides robust coverage of strategic transport arteries, capturing both high-speed motorways and essential main roads across diverse geographic landscapes.

The sample period (January 2018–December 2024) encompasses two dis-

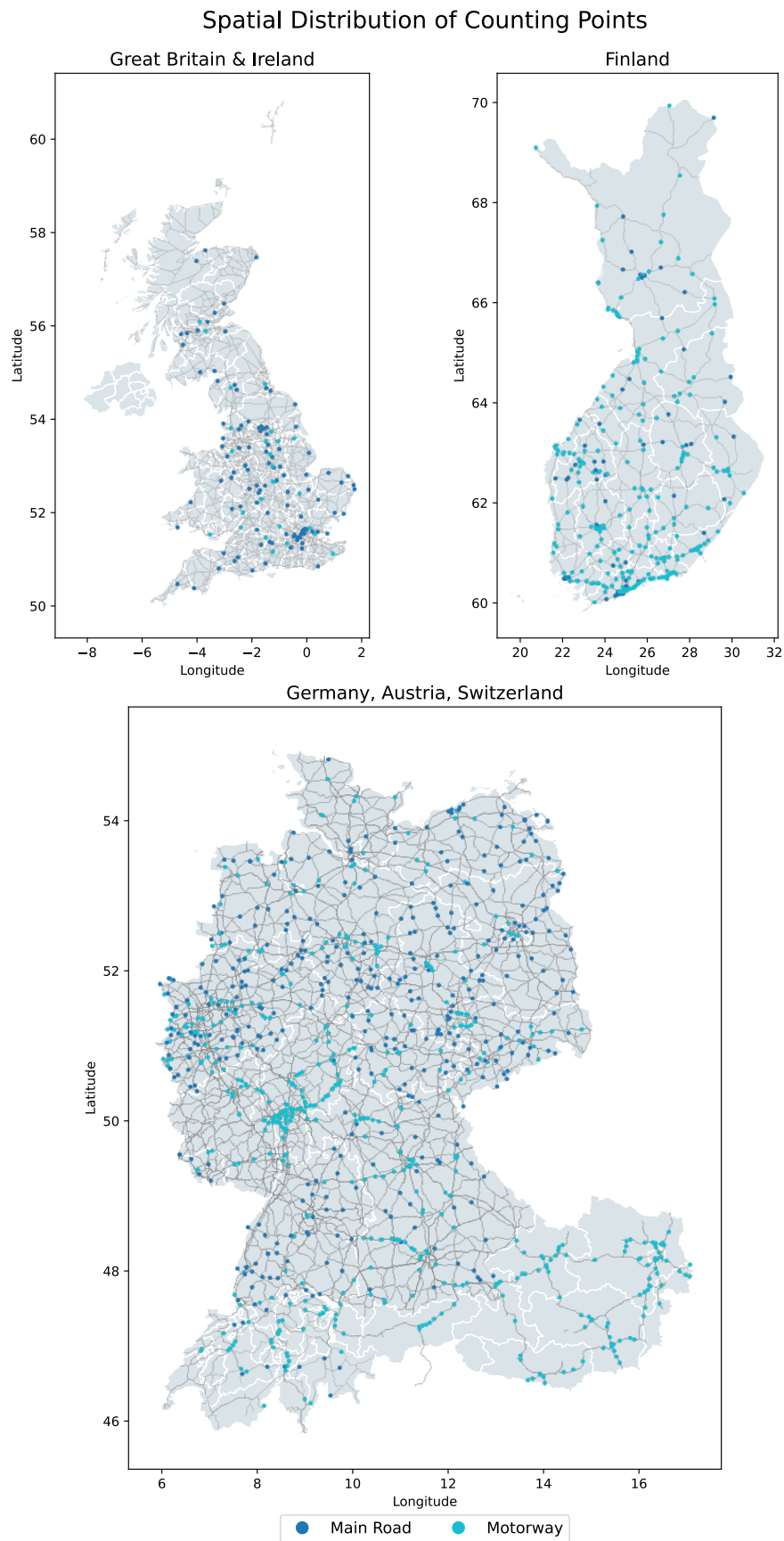


Figure 1: Spatial distribution of traffic counting stations on main roads and motorways in Great Britain, Finland, and the DACH-region.

tinct structural shocks to traffic outcomes. First, the COVID-19 pandemic profoundly impacted mobility. Arguably, not all of the countries in our sample are affected to the same extent. With Germany, Austria and Switzerland being transit countries for inner-European travel, traffic volumes might have decreased more heavily compared to traffic in Finland or Great Britain. Second, in response to the energy crisis following the Russian invasion of Ukraine, the German government introduced a temporarily available monthly PT ticket from June to August 2022 (*9-Euro-Ticket*). The ticket’s conditions were very similar to those of the *Deutschlandticket* with one major exception: the price. As the name suggests, the *9-Euro-Ticket* was available for €9 per month, while the price of its successor, the *Deutschlandticket*, increased to €49 a month in the relevant time period. During that same period of the *9-Euro-Ticket*, the German government also decreased the fuel tax to the European minimum (Drolsbach et al., 2023, p. 3). Both of these policies could possibly affect travel patterns in Germany, acting as potential confounders for identifying an (unbiased) effect of the *Deutschlandticket* on traffic volumes.

To mitigate bias from these structural breaks, the estimation excludes the period from January 2020 through August 2022. This removes periods affected by the pandemic that might have influenced the traffic volumes heterogeneously across countries (e.g. home office policies), the *9-Euro-Ticket*, or the fuel tax reduction. While one might consider restricting the sample solely to the periods after the expiration of the *9-Euro-Ticket*, such a shortened panel lacks a pre-treatment summer baseline, raising concerns regarding seasonal bias, since travel patterns exhibit strong seasonality.¹⁸

To account for exogenous variation in travel conditions, we augment the panel with station-level weather data sourced from the *European Climate Assessment & Dataset* (ECA&D) project (Klein Tank et al., 2002). For every traffic counting station, we assign the nearest (available) weather station and utilize monthly average temperature, average wind speed, and the to-

¹⁸See Figure 2.

tal number of ice days.¹⁹ Including these covariates is necessary to control for spatially idiosyncratic meteorological shocks – such as freezing events – which can affect traffic volume, speed, and accident risk in different locations heterogeneously (Cools et al., 2010).

3.2 Descriptive Statistics

Our main data set contains traffic data for Germany (GER), Austria (AUT), Switzerland (CHE), Finland (FIN), and Great Britain (GBR). Table 3 presents descriptive statistics for the final panel, stratified by road type, and control and treatment group. The final sample comprises 1,372 counting stations observed over 52 periods ($N = 71,344$). Three structural features of the data are noteworthy. First, there are crucial level differences in traffic intensity across regions. German motorways record the highest average monthly volume (1.45 million vehicles), exceeding the AUT/CHE (1.26 million) and FIN/GBR (0.78 million) control groups. Second, the table quantifies the data availability constraints discussed in Section 3.1. While the motorway sample is geographically well-represented across all regions, the main road control group for Austria and Switzerland is limited (10 stations, only provided by Switzerland). Consequently, the donor pool, and thus the identification of treatment effects for main roads, is heavily based on the denser network of stations provided by Finland and Great Britain (300 stations). Third, meteorological covariates exhibit expected geographic heterogeneity. The FIN/GB control group is characterized by lower average temperatures and a higher frequency of ice days compared to the DACH region, validating the inclusion of weather controls to adjust for spatially asymmetric shocks.

¹⁹Traffic stations are matched to the nearest weather station; missing daily observations are imputed using the next-nearest active station. We utilize ECA&D data series TG (daily mean temperature), FG (daily mean wind speed), and TX (daily maximum temperature), see: <https://www.ecad.eu/dailydata/predefinedseries.php>. The latter is used to compute the monthly count of *Ice Days*, defined as days where the maximum temperature remains below freezing ($T_{max} < 0^\circ\text{C}$).

Road Type	Country Period Variable	AUT		CHE		FIN		GBR		GER	
		Pre	Post	Pre	Post	Pre	Post	Pre	Post	Pre	Post
Motorway	Traffic Volume in Mio. Counts	1.24 (0.77)	1.30 (0.77)	1.32 (0.86)	1.36 (0.87)	0.63 (0.39)	0.63 (0.39)	1.49 (0.61)	1.36 (0.51)	1.45 (0.80)	1.44 (0.79)
	Avg. Daily Tmp. in °C	10.22 (7.42)	12.92 (7.32)	9.45 (7.16)	11.86 (7.03)	4.61 (8.22)	7.27 (9.00)	9.76 (4.74)	11.68 (4.34)	9.96 (6.46)	12.35 (6.03)
	WindSpeed in km/h	2.32 (1.27)	2.36 (1.29)	2.05 (0.96)	2.03 (0.90)	4.46 (1.75)	4.45 (1.75)	4.51 (0.79)	4.59 (0.85)	3.46 (1.23)	3.41 (1.22)
	Number of Icedays	1.23 (2.98)	0.63 (2.11)	1.68 (4.65)	1.30 (3.86)	6.39 (8.35)	4.81 (7.69)	0.11 (0.47)	0.00 (0.04)	0.90 (2.32)	0.59 (1.75)
	Unique Counting Points	104		40		116		25		382	
	Observations	3,328	2,080	1,280	800	3,712	2,320	800	500	12,224	7,640
Main Road	Traffic Volume in Mio. Counts			0.22 (0.19)	0.23 (0.20)	0.19 (0.25)	0.19 (0.26)	0.43 (0.30)	0.41 (0.27)	0.32 (0.28)	0.31 (0.27)
	Avg. Daily Tmp. in °C			9.04 (7.45)	11.33 (7.23)	3.25 (8.57)	5.94 (9.49)	9.82 (4.74)	11.71 (4.37)	9.69 (6.45)	12.10 (5.96)
	WindSpeed in km/h			2.69 (2.30)	2.75 (2.24)	3.45 (1.31)	3.51 (1.34)	4.52 (0.82)	4.61 (0.92)	3.58 (1.29)	3.49 (1.27)
	Number of Icedays			2.12 (5.50)	1.46 (4.28)	8.15 (9.78)	6.39 (9.00)	0.08 (0.40)	0.00 (0.06)	1.04 (2.63)	0.62 (1.87)
	Unique Counting Points			10		193		107		395	
	Observations			320	200	6,176	3,860	3,424	2,140	12,640	7,900
Total Counting Points		104		105		309		132		777	
Total Observations		3,328	2,080	1,600	1,000	9,888	6,180	4,224	2,640	24,864	15,540

Table 3: Summary statistics from January 2018 to December 2019, as well as September 2022 to December 2024. Note: Austria (AUT), Switzerland (CHE), Finland (FIN), Great Britain (GBR) and Germany (GER); standard deviations in parentheses.

Figure 2 shows the average monthly traffic volumes per counting station recorded on motorways and main roads for all five countries in our sample. Notably, traffic volumes in all chosen countries fluctuate seasonally with higher traffic counts during summer months. The level differences that can be seen in Figure 2 can be explained by differing characteristics of the specific road network (e.g., number of lanes), as well as differences in the location of the counting station (e.g., commuter belts vs. rural transit). As explained (and shown in Tables 2 and 3), observations for Austria only include motorway equivalents which measure a greater number of vehicles compared to main roads. Therefore, the average number of passenger vehicles depicted in Figure 2 is highest for Austria. Conversely, Finland’s and Great Britain’s road networks cover fewer relative motorway counting stations than main roads. Nevertheless, the development of passenger vehicle traffic over time is very similar between the five countries in our observation period. The grey shaded area marks the time period that will be excluded in the following estimations. As can be seen, the COVID-19 pandemic temporarily decreased traffic counts in spring 2020 for all five countries, but to different extents. The

traffic outcomes have returned gradually to pre-pandemic levels by the end of 2021, however we further exclude observations until August 2022 because traffic volumes might be affected by other policies such as the *9-Euro-Ticket* and the fuel tax cut. The red shaded area highlights the period since the *Deutschlandticket* has been in place.

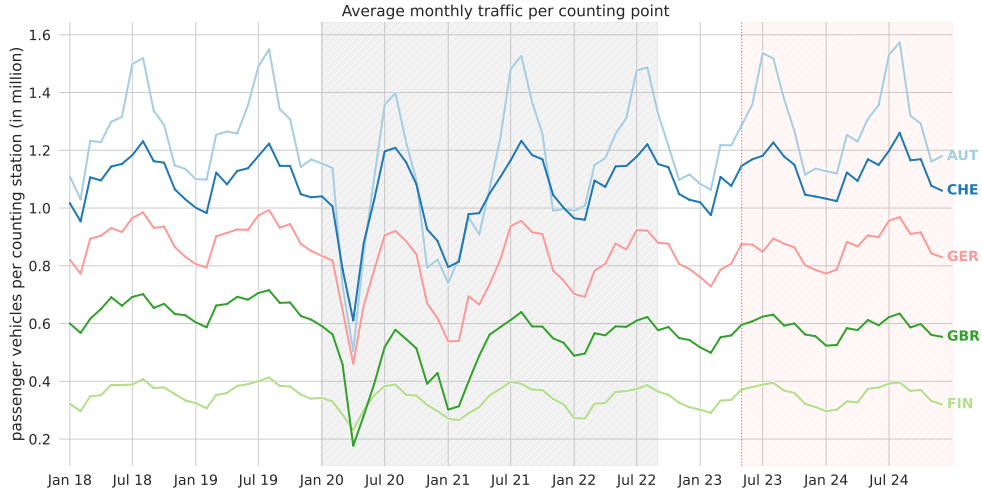


Figure 2: Monthly mean traffic of passenger vehicles per counting point in Austria (AUT), Switzerland (CHE), Germany (GER), Great Britain (GBR), and Finland (FIN) from January 2018 to December 2024. The grey shaded area represents the excluded period from January 2020 to August 2022. The red shaded area marks the treatment period of the *Deutschlandticket* with its introduction in May 2023.

4 Methodology

To assess whether the introduction of the *Deutschlandticket* exerted a statistically significant impact on passenger traffic in Germany, we employ the synthetic difference-in-differences (SDiD) estimation framework introduced by Arkhangelsky et al. (2021). We compare changes in extra-urban traffic in Germany to a weighted counterfactual drawn from our four European control countries. The SDiD method combines features of synthetic control (SC) methods and difference-in-differences (DiD). It reweights and matches on pre-exposure trends to weaken the reliance on parallel trends like SC while simultaneously being invariant to additive unit-level shifts and allowing for valid large-panel inference like DiD.

The average causal effect of the *Deutschlandticket* is estimated as follows:

$$(\hat{\tau}^{sdid}, \hat{\mu}, \hat{\alpha}, \hat{\beta}) = \arg \min_{\tau, \mu, \alpha, \beta} \left\{ \sum_{i=1}^N \sum_{t=1}^T (Y_{it} - \mu - \alpha_i - \beta_t - Ticket_{it}\tau)^2 \hat{\omega}_i^{sdid} \hat{\lambda}_t^{sdid} \right\}. \quad (1)$$

Including counting points from Germany, Austria, Switzerland, Finland, and Great Britain we obtain a balanced panel with 1,372 counting points and $T = 52$ months (total $N \times T = 71,344$ observations).²⁰ The variable Y_{it} in Equation (1) measures log traffic volume for counting point i (unique to each country) and month t , while the variables α_i and β_t denote counting point and time fixed effects, respectively, and μ is an intercept. The binary treatment indicator $Ticket_{it} \in \{0, 1\}$ indicates the introduction of the ticket in May 2023. The ATT is then given by τ . In addition, the weights $\hat{\omega}_i^{sdid}$ and $\hat{\lambda}_t^{sdid}$ denote unit and time weights, respectively. The unit weights $\hat{\omega}_i^{sdid}$ are chosen in a way that the trend for traffic volumes of control units aligns with that of treated units in the pre-treatment period. Similarly, the time weights $\hat{\lambda}_t^{sdid}$ are calculated to emphasize pre-treatment periods that are similar to treatment periods for units that are never treated.²¹

Finally, as we aim to control for time-varying exogenous covariates, we follow the adjustment described in Arkhangelsky et al. (2021, Footnote 4) by using the residuals $Y_{it}^{res} = Y_{it} - X_{it}'\hat{\gamma}$ from the regression of Y_{it} on the vector X_{it} , where X_{it} contains the weather covariates – logarithmic daily mean temperature, wind speed, and the count of ice days (see Section 3) – to control for exogenous meteorological conditions affecting modal choice and traffic patterns.²²

Since our data set includes more control unit observations than pre-

²⁰This time period includes January 2018 to December 2019 as well as September 2022 to December 2024. For an explanation see Section 3.1.

²¹See Equations (4) and (6) in Arkhangelsky et al. (2021).

²²To capture potential non-linearities, temperature is entered in logarithmic form. Since the natural logarithm is undefined for negative values (in Celsius readings), we transform all temperatures to the Kelvin scale ($K = ^\circ\text{C} + 273.15$). This affine transformation ensures strict positivity while preserving the ordinal rank of observations.

treatment periods, the weighting algorithm does not necessarily lead to unique values for unit weights $\hat{\omega}_i^{sdid}$ (Doudchenko and Imbens, 2016). Arkhangelsky et al. (2021) address this by including a penalty term to increase dispersion. We depart from this dispersion-based approach by configuring the weighting algorithm to use the default specifications of the standard synthetic control method (Abadie et al., 2010), relying instead on (i) bootstrapped and (ii) placebo variance estimation to ensure stability and robust inference. Specifically, by omitting the intercept and setting the penalty term to zero (i.e., close to zero), we force the algorithm to build a counterfactual in levels rather than changes, effectively reducing the unit-weighting step closer to the SC method (Arkhangelsky et al., 2021, p. 4092).²³ While the standard SDiD ridge penalty successfully distributes weights across donor pools in aggregate, macro-level panels, applying it to our highly granular, heterogeneous station-level dataset risks severe interpolation bias (e.g., Abadie, 2021).²⁴ Standard SDiD tends to disperse weights across hundreds of dissimilar rural and urban counting stations simply to match an average trend (see Section 5). As established in the broader synthetic control literature, dispersing weights across a large number of heterogeneous control units can severely compromise the validity of the counterfactual when pre-treatment fit is noisy (Doudchenko and Imbens, 2016; Ferman and Pinto, 2021). Our approach enforces sparsity instead, shifting the focus to a selective set of control units that are structurally similar to treated units in absolute levels. As demon-

²³Formally, the unit weights $\hat{\omega}^{sdid}$ in Arkhangelsky et al. (2021) are obtained by solving the regularized minimization problem: $(\hat{\omega}_0, \hat{\omega}) = \arg \min_{\omega_0, \omega} \left\{ \sum_{t=1}^{T_{pre}} \left(\omega_0 + \sum_{i=1}^{N_{co}} \omega_i Y_{it} - \frac{1}{N_{tr}} \sum_{i=N_{co}+1}^N Y_{it} \right)^2 + \zeta^2 T_{pre} \|\omega\|_2^2 \right\}$ where ω_0 is an intercept term allowing for level differences and ζ is a regularization parameter that penalizes weight concentration. To combine the behavior of the SDiD with the default values of the SC, we restrict this minimization by fixing $\omega_0 = 0$ and setting the penalty $\zeta \approx 0$ (effectively removing the ridge penalty). This reduces the objective function to the constrained least squares problem of Abadie et al. (2010), forcing the algorithm to match outcome levels rather than parallel trends and prioritizing the selection of a sparse set of control units (best fit) over the variance-reduction benefits of dispersed weights.

²⁴Interpolation bias occurs when an algorithm assigns weights to control units with fundamentally different structural characteristics than the treated unit, simply because their weighted average coincidentally matches the treated unit's pre-treatment trends. In highly heterogeneous datasets, this creates a synthetic counterfactual that overfits to idiosyncratic noise rather than identifying truly comparable peers (Abadie, 2021).

strated by Ferman (2021), enforcing these strict SC-style constraints acts as a necessary regularization mechanism that prevents spurious fits when the number of control units is large relative to the pre-treatment periods. By adopting this hybrid SDiD specification, we ensure a rigorous pre-treatment fit via SC-style unit weights, while retaining the advantages of SDiD regarding time-weighting and robust large-panel inference. Results using this specification will be labeled *SDID-SCM*.²⁵

5 Main Results

The estimation results from Equation (1) are outlined in Table 4. Columns (1) and (2) show our estimation results for the SDiD estimation with a regularized SC weighting algorithm (*SDID-SCM*), with and without the inclusion of covariates. We further subsample the estimation by weekend (Friday-Sunday), during the week (Tuesday-Thursday) and road types. The dependent variable in all four columns is the natural logarithm of monthly traffic volume; thus, coefficients are interpreted as semi-elasticities (e.g., a value of -0.01 implies a reduction of approximately 1 %). The ATT $\hat{\tau}^{sdid}$ is shown together with the standard error in parentheses below each ATT.

We find no evidence of a systematic long-lasting reduction in traffic volumes. The point estimate including all countries, road types and covariates (Column (2)) is economically negligible ($\hat{\tau} = -0.005$, s.e. 0.011) and statistically indistinguishable from zero. This null result holds regardless of the inclusion of meteorological covariates, suggesting that the policy intervention cannot induce a measurable modal shift at the aggregate level. Stratifying the sample by road type and time of week confirms the absence of overall average treatment effects across all subsamples.

²⁵All estimations have been conducted based on R *synthdid* package from Arkhangelsky et al. (2021): see <https://synth-inference.github.io/synthdid/index.html>.

Countries	Road Type/Covariates	(1)	(2)
		No	Yes
All		−0.015 (0.016)	−0.005 (0.014)
All (Friday - Sunday)	Both	0.018 (0.020)	0.011 (0.018)
All (Tuesday - Thursday)		−0.016 (0.010)	−0.010 (0.011)
All	Main Roads	−0.009 (0.013)	−0.013 (0.014)
	Motorways	−0.009 (0.013)	−0.006 (0.012)

Table 4: Estimation results from equation (1) for the ATT measured by $\hat{\tau}^{sdid}$. Covariates include mean temperature, mean wind speed and the number of ice days. Columns (1)-(2) shows estimation results for *SDID-SCM* without and with covariates included. Note: Standard errors (in parentheses) are computed using a block bootstrap stratified by treatment status with 200 replications

To evaluate the temporal dynamics of the policy, we extract the time-varying treatment effect derived from the SDiD estimation procedure. While Equation (1) calculates a single average treatment effect ($\hat{\tau}^{sdid}$) across the entire post-intervention period, the underlying algorithm constructs a counterfactual that can be decomposed over time. Specifically, the estimation procedure first determines the optimal unit weights ($\hat{\omega}_i^{sdid}$) and time weights ($\hat{\lambda}_t^{sdid}$). To isolate the effect for any specific month t , we apply these fixed unit weights to the control pool to compute the synthetic spatial match for each month, and adjust it by time weights. The difference between the actual treated outcome and this adjusted counterfactual yields the month-specific ATT in the post-treatment phase, as well as a pseudo-ATT for each pre-treatment month (serving as a visual diagnostic for pre-trend fit). Figure 3 presents these time-varying estimates for the specification in Table 4, column (2), including covariates for all countries and all road types. The upper panel displays the curve for each point in time together with the respective 95%

bootstrapped confidence interval.²⁶ The black dotted line marks the introduction of the *Deutschlandticket*, while the red dashed line shows the ATT, averaged over all post periods. The lower panel displays time weights $\hat{\lambda}_t^{sdid}$ for every month in the pre-treatment period. Although the ATT of -0.005 (as can be seen in Table 4 and displayed by the red dashed line) is not statistically different from zero, the effect over time is heterogeneous. We observe a significant, transient reduction in traffic volumes in June, July, and August 2023, peaking with a magnitude of approximately 0.075 in July. However, this effect dissipates rapidly; after September 2023, estimates are statistically indistinguishable from zero. Even the same summer months (June, July and August) in 2024 show no evidence of a deviation in travel patterns.

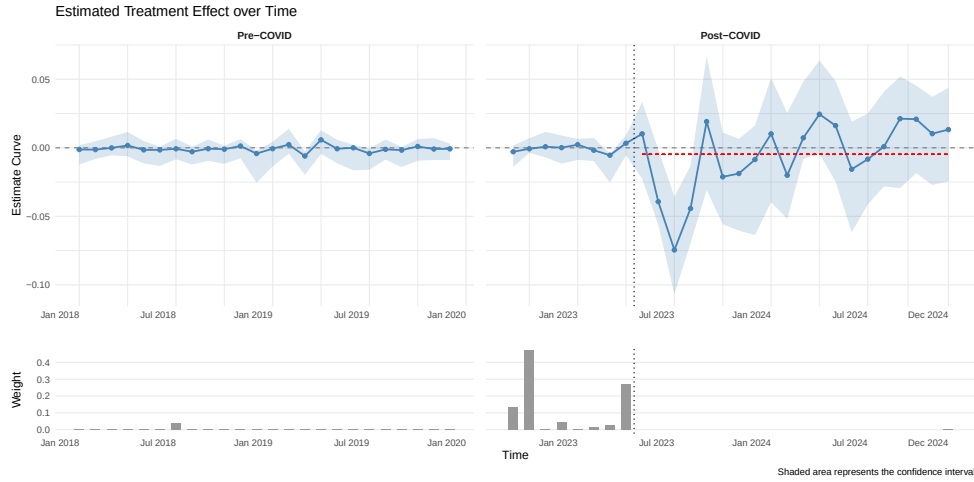


Figure 3: Estimation of the monthly treatment effect (blue marks) using the *SDID-SCM* approach with corresponding bootstrapped 95% confidence interval (blue shaded area) is displayed in the upper panel. The black dotted line marks the introduction of the *Deutschlandticket* in May 2023. The red dashed line represents the average treatment effect after treatment was introduced. The lower panel shows the weighting of time periods ($\hat{\lambda}_t^{sdid}$). Results correspond to Table A.1, *SDID-SCM* with covariates using all countries and road types. Note: Confidence intervals are computed using a block bootstrap stratified by treatment status with 200 replications

To validate our findings, we also estimate the model using the standard *SDID* weighting algorithm proposed by Arkhangelsky et al. (2021). Although

²⁶The original *synthdid* R package from Arkhangelsky et al. (2021) implementation does not provide functionality for bootstrapping the time-varying treatment effect curve. We therefore extended the code to compute pointwise 95% confidence intervals using the block bootstrap procedure.

this specification yields a statistically significant but small reduction in traffic ($\hat{\tau} = -0.015$), diagnostics reveal that it fails to construct a valid counterfactual in our setting, i.e., the pre-trend deviations are substantial and significantly different from zero, violating the parallel trends assumption (see Appendix Table A.1 and Figure A.4). The discrepancy stems from the standard algorithm’s reliance on trend-matching via unit fixed effects and a strong emphasis on the ridge penalty (see Section 4), which in our high-dimensional sample leads to a spurious fit by averaging hundreds of heterogeneous, low-quality control units (see Appendix Figure A.1) to minimize variance at the cost of bias. Because traffic volumes vary drastically between major transit corridors and minor regional roads, constructing a synthetic German highway out of hundreds of fractionally weighted, low-volume foreign roads empirically produces an invalid counterfactual. In contrast, our preferred *SDID-SCM* specification restricts the weighting algorithm to match on traffic levels and enforces sparsity (see Figures A.2 and A.3 which highlight that 31 counting points make 90% of the synthetic control weights with a strong reliance on Great Britain) by reducing the regularization penalty (Arkhangelsky et al., 2021, p. 4092). As evidenced by the superior pre-treatment fit in Figure 3 (compared to Figure A.4), this selectivity minimizes bias effectively by filtering out irrelevant variation from dissimilar roads and irrelevant control stations. Consequently, we interpret the significant estimates from the standard algorithm as artifacts of poor pre-treatment fit rather than evidence of a structural modal shift.²⁷

6 Robustness and Sensitivity Analyses

This section assesses the robustness of our main findings along two dimensions. First, we examine the sensitivity of the estimates to the composition

²⁷Furthermore, we conducted a placebo randomization where we bootstrapped a 50/50 split of the control units 200 times (see Figures A.5 and A.6). In each iteration, half of the donor pool was randomly assigned as a placebo treated unit while the remaining half served as the control group, allowing us to construct an empirical distribution of treatment effects under the null hypothesis of no effect. The results confirm the standard bootstrap analysis.

of the donor pool by systematically excluding individual control countries (a “leave-one-out” analysis). Second, we calculate the Minimum Detectable Effect (MDE) to determine whether our statistically insignificant results reflect a true absence of treatment effects or merely a lack of statistical power.

Table 5 presents the results of the leave-one-out analysis, designed to verify that the baseline estimates are not driven by idiosyncratic trends in any single control country. We re-estimate the model by systematically excluding individual countries from the donor pool (rows 1–4) and restricting the donor pool to specific sub-regions (rows 5–6). This leave-one-out approach verifies whether the inclusion of a specific country (e.g., Great Britain or Switzerland) is disproportionately influencing the construction of the synthetic control.

The results demonstrate high stability in non-significance even when we systematically exclude entire countries from the donor pool. Across all permutations – whether excluding individual countries or restricting the sample to sub-region – the point estimates remain statistically indistinguishable from zero, fluctuating between -0.002 and -0.021 . Even the model estimation that yields the largest coefficient (-0.021) — achieved by excluding Finland — remains statistically insignificant. Similarly, restricting the sample to the DACH region or to Finland and Great Britain, yields estimates close to zero (-0.007 and -0.009 , respectively). This consistency confirms that the null result is not driven by idiosyncratic trends in the Nordic or British road networks, but reflects a robust absence of modal shift in the treated German units.

	(1)	(2)
Countries/Covariates	No	Yes
Without Austria	−0.011 (0.016)	−0.002 (0.012)
Without Switzerland	−0.017 (0.018)	−0.016 (0.014)
Without Finland	−0.016 (0.011)	−0.021 (0.012)
Without Great Britain	−0.010 (0.015)	−0.010 (0.015)
Only DACH	−0.019 (0.014)	−0.007 (0.012)
Only Finland and Great Britain	−0.012 (0.015)	−0.009 (0.013)

Table 5: Sensitivity analysis results from Equation (1). Columns (1) and (2) display estimates for the *SDID-SCM* algorithm (with and without covariates). Rows represent samples with specific control countries excluded (leave-one-out) or regional subsamples (DACH, Finland/GB). Note: Standard errors (in parentheses) are computed using a block bootstrap stratified by treatment status with 200 replications.

To formally distinguish whether our statistically insignificant estimates reflect a true absence of effect or merely a lack of statistical power, we calculate the Minimum Detectable Effect (MDE). Following the standard framework (see e.g., Duflo et al., 2007), the MDE is defined as the smallest true effect size that our specific research design has a specified probability (power) of detecting at a given significance level.

Technically, the MDE is calculated $(t_{1-\alpha/2} + t_{1-\beta}) \times SE(\hat{\tau}^{sdid})$, where $SE(\hat{\tau}^{sdid})$ is the standard error of the point estimate, $t_{1-\alpha/2}$ is the critical value for the significance level α and $t_{1-\beta}$ is the critical value for the desired statistical power $(1 - \beta)$. For a standard two-sided test at $\alpha = 0.05$, $t_{1-\alpha/2}$ is 1.96, testing against Type I errors (false positives) and for the standard power benchmark of 80 %, $t_{1-\beta}$ is 0.84, testing against Type II errors (false negatives). The sum of these thresholds (roughly 2.8 for 80 % power) effectively determines how many standard errors away from zero the true effect must be for us to reliably detect it.

Covariates	Target Statistical Power for MDE		
	50% Power	80% Power	90% Power
No	0.0311	0.0445	0.0515
Yes	0.0271	0.0387	0.0448

Note: Minimum Detectable Effect (MDE) calculated as $(t_{\alpha/2} + t_{1-\beta}) \times SE$. Significance level α set to 0.05.

Table 6: Results for the MDE.

Table 6 presents the MDE sensitivity analysis. First, the inclusion of meteorological covariates significantly improves precision; adding weather controls reduces the MDE at 50 % power from 3.11 % to 2.71 % (at 80 % power from 4.45 % to 3.87 %.) Consequently, we can confidently rule out the existence of treatment effects larger than approximately 3-4 % – magnitudes typically required for an effect to be considered *economically* meaningful (as discussed in Section 7). Crucially, this degree of statistical power is driven by our rich dataset, which features a large number of cross-sectional units (1,372 counting stations) and an effective 50/50 split between treated and control stations. These results confirm that the failure to find a significant average treatment effect is not driven by a lack of statistical power, but rather provides robust evidence that the policy did not generate a lasting modal shift of relevant magnitude, but only a short-run effect (see Figure 3).

7 Discussion

Our primary empirical finding – a statistically insignificant effect of the *Deutschlandticket* on highway traffic volumes – warrants careful economic interpretation. Although point estimates are negative, suggesting a reduction in traffic volume, they are economically negligible and statistically indistinguishable from zero. A statistically significant temporary reduction of up to 7.5 % is detected only in July 2023, indicating a causal yet strictly temporary effect of the *Deutschlandticket*.

To put the effect of 7.5 % in July 2023 into perspective, Figure 4 translates the estimated relative treatment effect over time into *absolute* vehicle counts per counting station (utilizing the percentage effects of Figure 3). The top panel demonstrates that the synthetic counterfactual accurately tracks the seasonal fluctuations of average German traffic volumes during the pre-treatment period. Following the introduction of the *Deutschlandticket* in May 2023, the distinct divergence emerges. As explicitly detailed in the bottom panel, the peak relative reduction in July 2023 corresponds to an absolute decrease of approximately 68,000 passenger vehicles per counting point (approximately 2267 vehicles per day), relative to a synthetic baseline of roughly 916,000 vehicles. The visualization reinforces the strictly transient nature of this shift. The absolute difference rapidly reverts toward zero by late autumn of 2023, exhibits no significant structural or lasting divergence (see Figure 3) in the subsequent summer of 2024.

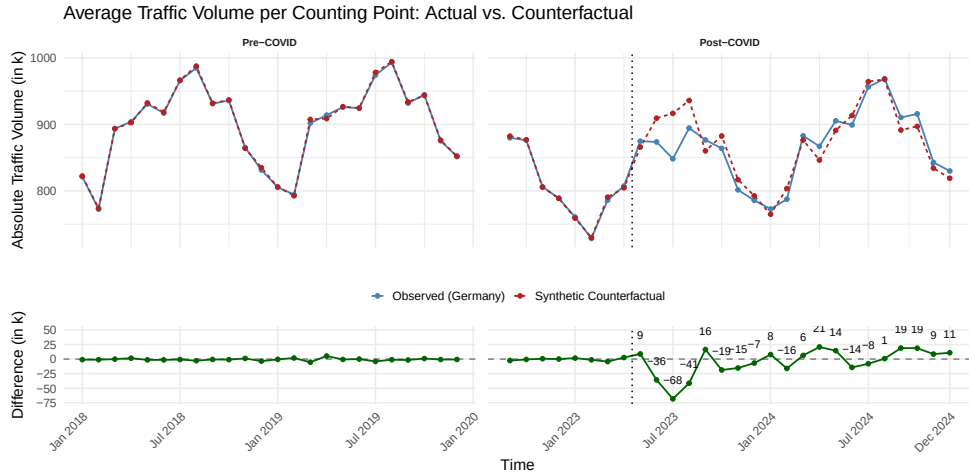


Figure 4: Average monthly traffic volume per counting station (in thousands) comparing the observed average German traffic volume to the synthetic counterfactual. The top panel displays the absolute volume levels, demonstrating the pre-treatment fit. The bottom panel illustrates the absolute difference (Observed minus Counterfactual), with numeric labels indicating the exact deviation in the post-treatment period. The vertical dotted line marks the introduction of the *Deutschlandticket* in May 2023.

As demonstrated by the MDE analysis, the null result is not an artifact of insufficient statistical power; rather, aggregate *average* traffic reductions larger than 3% can be confidently ruled out. In line with findings from the literature (see Section 2) this suggests that the (short-run) cross-price elas-

ticity between private driving and PT is extremely low, even in the face of a drastic price reduction for the substitute good. As the literature on cross-mode substitutability demonstrates, policies exclusively targeting public transit fares are structurally ill-suited to reduce car usage. Cats et al. (2017) establish that travelers are substantially more responsive to policies that increase the generalized cost of driving than to those that decrease the monetary cost of PT. This asymmetry aligns with recent evidence evaluating the *9-Euro-Ticket*, where strong mode choice inertia prevented significant substitution away from private vehicles (Guajardo Ortega and Link, 2025). Ultimately, the anticipated modal shift is bounded by non-monetary costs – specifically travel time, reliability, and convenience – which a pure fare reduction intrinsically fails to mitigate.

Although the aggregate effect is not evident, the short-run reduction in traffic volumes in the first summer of implementation (June–August 2023) likely reflects a novelty effect or follow-the-crowd behavior during the vacation season (similar to the *9-Euro-Ticket*), where leisure travel is more flexible than commuting. However, this effect dissipates rapidly, and does not occur in 2024. The lack of persistence suggests that while the ticket may induce experimentation or occasional leisure usage, it has failed to alter the habitual commuting patterns that drive baseline traffic volumes from private vehicles. This contrasts with the hypothesis that a permanent ticket would allow households to make long-term adjustments (e.g., reducing vehicle ownership), suggesting that reliability concerns may outweigh the financial savings for daily commuters.

We interpret this lack of substitution in the context of binding supply-side constraints, a mechanism well-documented in recent evaluations of the German rail network. As demonstrated by Liebensteiner et al. (2024) and Lu et al. (2024), the massive demand shock induced by fare reductions significantly exacerbated station crowding and train delays. If perceived transit quality degrades due to these external capacity constraints, the cross-price elasticity of demand effectively drops to zero, particularly for car commuters with a high value of travel time savings. Consequently, injecting a substantial demand-side subsidy without simultaneous supply-side capacity expansion –

in a network already facing severe investment gaps (Krebs and Steitz, 2021) – results in a structural mismatch. Hence, in light of the findings of this paper, it may be economically more meaningful to allocate funds toward supply-side investments in the public transport sector, rather than subsidizing a uniform ticket price, particularly if the goal is to induce a modal shift.

Our analysis is subject to specific data limitations. First, our dataset covers extra-urban highways and main roads. While this captures the bulk of suburban-to-urban and regional commuting we cannot observe changes in purely inner-city traffic volumes. It is plausible that the *Deutschlandticket* induced stronger spatial substitution effects within short-distance urban transit networks – a dynamic documented by Schlett and Loder (2025) – which remains unobservable by highway counters. Second, we lack observations of rail journeys. Because we only observe that private car volumes remained statistically unchanged, we cannot determine if rail journeys increased due to new, induced demand (trips that would not have been undertaken otherwise). If the ticket primarily subsidized existing rail passengers and induced new mobility from other modes (e.g. by foot or bike) rather than reducing car trips, the net environmental benefit of the policy remains highly ambiguous.

The absence of a significant reduction in traffic volume has profound implications for the cost-effectiveness of the *Deutschlandticket* as a climate policy instrument. While we cannot rule out modal shifts within purely inner-urban transit networks, our findings indicate that the policy failed to decarbonize the energy-intensive regional commuting sector. Given that medium-to-long distance commuting accounts for a disproportionate large share of transport emissions compared to short inner-city trips (see, e.g., Wadud et al., 2024; Leroutier and Quirion, 2022), the lack of response on these roads suggests that the aggregate environmental impact of the ticket is likely lower than anticipated by policy makers. Although the policy succeeds as a social transfer by relieving the financial burden on existing passengers, its efficacy and efficiency as a tool to reduce high-mileage private transport appear limited. With annual direct costs of approximately 3 billion Euros – shared equally between the federal government and the states – the implicit cost per ton of CO₂ abated is likely prohibitively high given the negligible

decrease in vehicle kilometers traveled. While the policy succeeds as a social transfer by relieving the financial burden on existing passengers, its efficacy as an environmental policy is limited.

Furthermore, the fiscal sustainability of such demand-side subsidies is questionable. The subsidy of 3 billion Euros annually absorbs substantial public funds. Simultaneously, the cap on ticket prices restricts the ability of transport operators to generate the revenue necessary for maintaining service quality, and distorts the price signal required for efficient capacity allocation. Given that Krebs and Steitz (2021) estimate an investment gap of 50 billion Euros for the digitization and expansion of the German rail network by 2030 and considering that German rail infrastructure is almost entirely dependent on public grants to fund network development (Götz and Schäfer, 2020), our findings suggest that reallocating these resources toward supply-side infrastructure improvements – thereby reducing non-monetary costs such as delays and overcrowding – may yield higher long-term economic returns than subsidizing fares. This is corroborated by empirical evidence from (Schäfer, 2026), who demonstrates that railway funding structures achieve higher efficiency when public funds are prioritized for upstream infrastructure management rather than downstream operational subsidies.

Finally, the temporal horizon and ongoing policy uncertainty play a crucial role in shaping the observed demand responses. Although the *Deutschlandticket* was introduced as a permanent intervention, continuous political debates regarding future price increases and funding availability generate substantial uncertainty for consumers. As established in the literature, PT demand is significantly more elastic in the long run than in the short run; long-run elasticities are typically two to three times larger because permanent price signals are required to induce discrete, structural household adjustments, such as modifying vehicle ownership or residential location (Holmgren, 2007; Litman, 2004; Wardman et al., 2018). Consequently, if consumers cannot reliably anticipate the long-term persistence and price stability of the ticket, cross-mode substitution away from private cars will remain bounded by the rigid, short-run elasticity estimates.

8 Conclusion

In this paper, we evaluate the impact of the *Deutschlandticket* on passenger vehicle traffic using a SDiD approach by Arkhangelsky et al. (2021). Our central empirical finding is that this nationwide fare reduction failed to induce a lasting modal shift away from private cars. While we identify a short-term reduction in traffic volumes during the initial three months (June to August 2023) of the policy, this substitution effect rapidly dissipates and does not reappear in the subsequent year, effectively ruling out purely seasonal fluctuations.

Ultimately, these results highlight the limits of price instruments in transport markets. Our findings are consistent with the hypothesis that in capacity-constrained networks, non-monetary costs – such as reliability, punctuality, and comfort – act as binding barriers to substitution. When public transit quality is degraded by infrastructure deficits, even drastic price reductions fail to outweigh the generalized costs of switching from a private car.

From a policy perspective, this suggests that the current strategy of subsidizing demand is insufficient to decarbonize the regional commuting sector. Rather than indefinitely funding fare subsidies that function primarily as social transfers, our analysis suggests that long-term environmental goals would be better served by redirecting public funds toward supply-side infrastructure investments. Future research should leverage granular mobility data (e.g., Schlett and Loder, 2025) to explore potential heterogeneity between rural and urban areas and the general long-term effects of the *Deutschlandticket*, but the aggregate evidence indicates that without providing sufficient funds for rail infrastructure, cheaper tickets will fail to lower traffic volume on roads, let alone induce to a modal shift necessary to *derail* the car.

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Declarations of competing interests

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Declaration of generative AI and AI-assisted technologies in the writing process

During the preparation of this work the authors used OpenAI's ChatGPT, Google's Gemini and Writefull in order to improve language. After using these services, the authors reviewed and edited the content as needed and take full responsibility for the content of the publication.

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A Appendix: Additional Figures and Tables

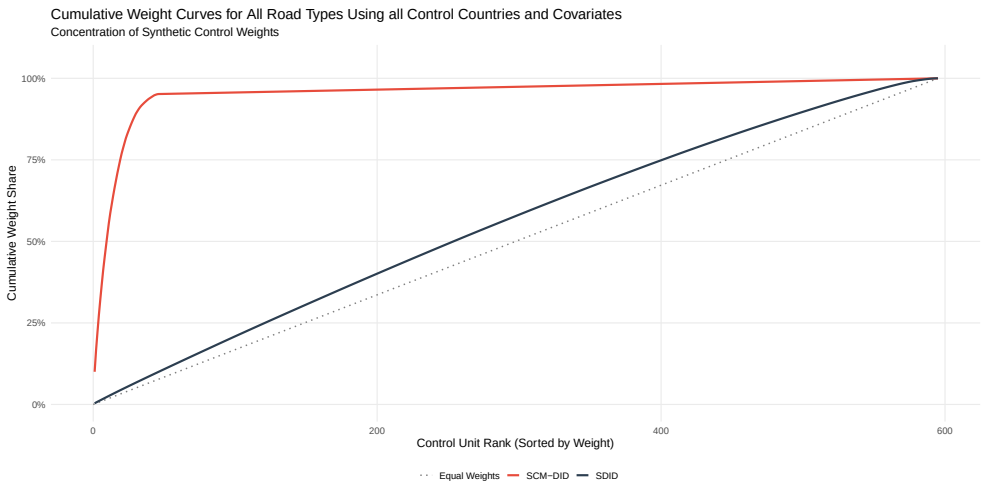


Figure A.1: Cumulative share of weights assigned to control units, sorted by weight magnitude. The red curve refers to the *SDID-SCM* model with covariates (Table 4, Column (2)). The black curve refers to the standard *SDID* with covariates (Table A.1, Column (2)). The dotted line represents the case of equal weights per unit.

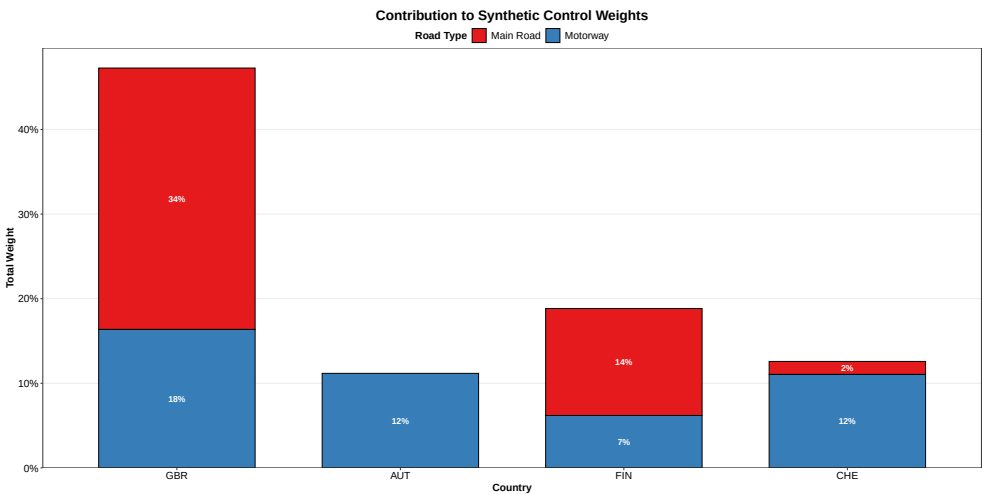


Figure A.2: Distribution of the top 90 % of synthetic control weights by country and road type from the *SDID-SCM* model (Figure A.1).

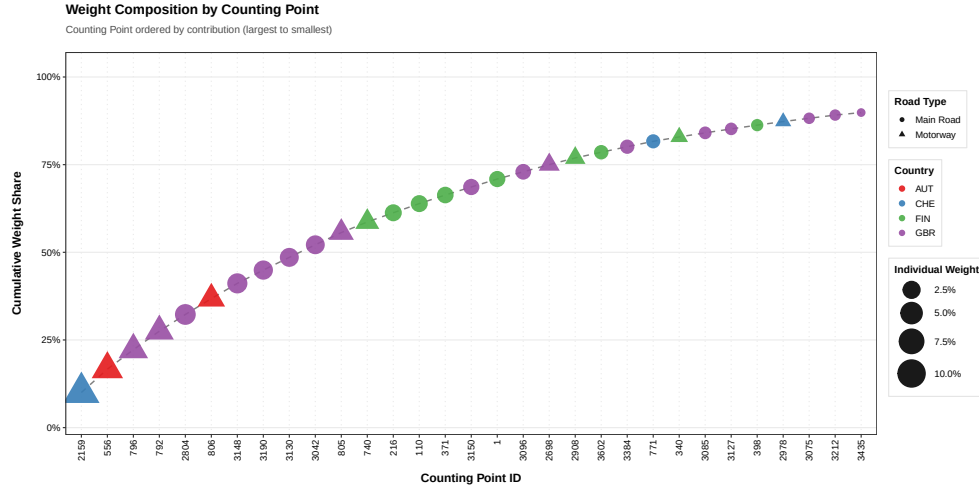


Figure A.3: Concentration of the top 90 % of weights among individual counting points (31 points), ordered by weight, from the *SDID-SCM* model (Figure A.1).

Countries	Road Type/Covariates	(1)	(2)
		No	Yes
All		−0.007 (0.005)	−0.015 (0.006)
All (Friday - Sunday)	Both	−0.002 (0.006)	−0.010 (0.008)
All (Tuesday - Thursday)		−0.009 (0.005)	−0.022 (0.006)
All	Main Roads	−0.009 (0.010)	−0.020 (0.011)
	Motorways	0.007 (0.010)	0.007 (0.009)

Table A.1: Estimation results from equation (1) for the ATT measured by $\hat{\tau}^{sdid}$. Covariates include mean temperature, mean wind speed and the number of ice days. Columns (1)-(2) shows estimation results for SDiD standard values. Note: Standard errors (in parentheses) are computed using a block bootstrap stratified by treatment status with 200 replications

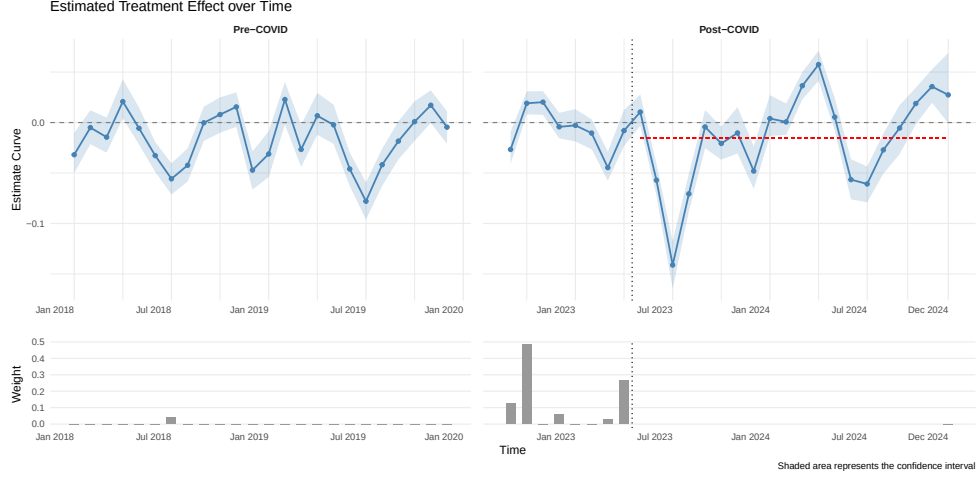


Figure A.4: Estimation of the monthly treatment effect (blue marks) using the SDiD approach with corresponding bootstrapped 95% confidence interval (blue shaded area) is displayed in the upper panel. The black dotted line marks the introduction of the *Deutschlandticket* in May 2023. The red dashed line represents the average treatment effect after treatment was introduced. The lower panel shows the weighting of time periods ($\hat{\lambda}_t^{sdiD}$). Results correspond to Table A.1, Column (2), *SDiD* with covariates using all countries and road types. Note: Confidence intervals are computed using a **block bootstrap** stratified by treatment status with 200 replications

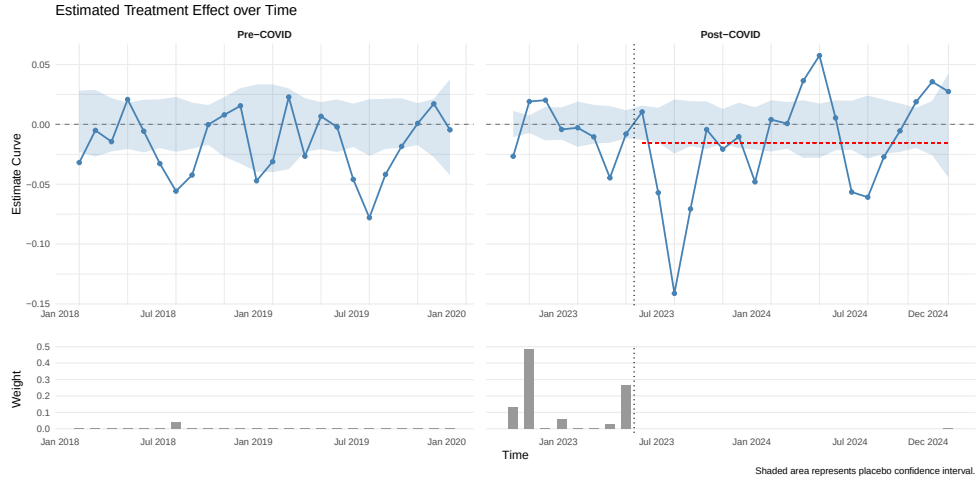


Figure A.5: Estimation of the monthly treatment effect (blue marks) using the SDiD approach with corresponding 95% placebo-based confidence interval (blue shaded area) is displayed in the upper panel. The black dotted line marks the introduction of the *Deutschlandticket* in May 2023. The red dashed line represents the average treatment effect after treatment was introduced. The lower panel shows the weighting of time periods ($\hat{\lambda}_t^{sdiD}$). Results correspond to Table A.1, Column (2), *SDiD* with covariates using all countries and road types. Note: **Placebo-based intervals** are computed using a 50/50 split of control units with 200 replications.

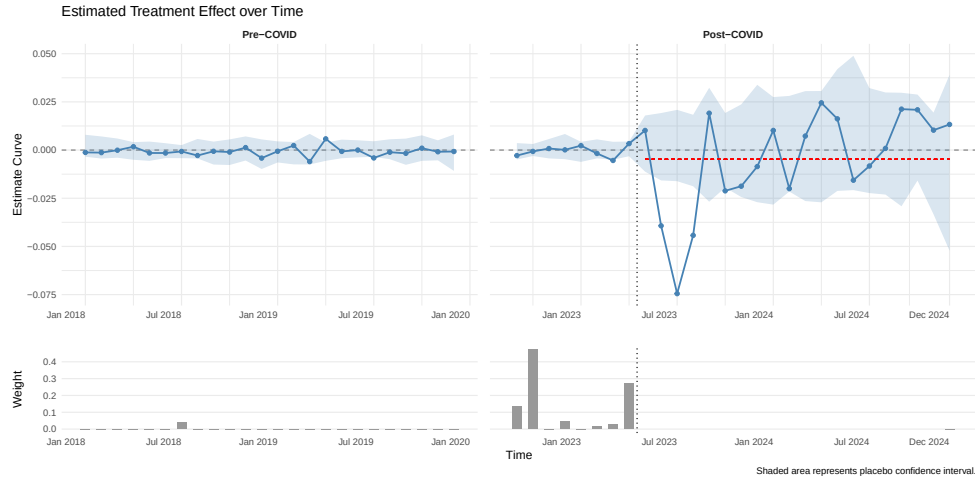


Figure A.6: Estimation of the monthly treatment effect (blue marks) using the *SDID-SCM* approach with corresponding 95% placebo-based confidence interval (blue shaded area) is displayed in the upper panel. The black dotted line marks the introduction of the *Deutschlandticket* in May 2023. The red dashed line represents the average treatment effect after treatment was introduced. The lower panel shows the weighting of time periods ($\hat{\lambda}_t^{sdid}$). Results correspond to Table 4, Column (2), *SDID-SCM* with covariates using all countries and road types. Note: **Placebo-based intervals** are computed using a 50/50 split of control units with 200 replications.

5 Conclusion

This dissertation analyses how government interventions, policy shocks, and structural market shifts shape pricing behavior and economic outcomes in the health, retail, and transport sectors. Across five manuscripts, the research evaluates the effects of market regulations, vertical restraints, cost shocks, and broad subsidy programs. To achieve this, it utilizes advanced econometric methods drawn from empirical industrial organization, such as difference-in-differences, staggered difference-in-differences, synthetic difference-in-differences, high-dimensional error correction models, propensity score matching, and double machine learning.

The first two manuscripts analyze the German brick-and-mortar pharmacy market, focusing on the impact of price competition, digitization, and regulation. The first manuscript evaluates the 2020 partial ban on prescription (Rx) drug rebates, revealing that while the policy successfully induced a shift in consumer behavior toward local pharmacies, the financial benefits were highly asymmetric. Ultimately, the intervention disproportionately benefited high-revenue pharmacies and proved insufficient to halt the long-term decline in the number of brick-and-mortar locations. The second manuscript investigates cost pass-through in the liberalized over-the-counter segment and for non-pharmaceuticals, documenting a rockets and feathers pricing asymmetry and systematic long-run cost overshifting. It highlights how rural pharmacies leverage spatial market power to offset lower demand and argues that pharmacies cross-subsidize their operations via the strictly regulated Rx segment, a mechanism now threatened by the digital disruption of electronic prescriptions.

The third manuscript examines the retail fuel market by evaluating the pass-through of temporary fuel tax reductions in France, Italy, and Germany following the 2022 energy crisis. The findings demonstrate a complete pass-through of tax cuts to consumers, albeit at varying speeds across countries, with retail margins normalizing quickly after a brief initial spike. While effective at immediately

lowering consumer prices, the study raises critical questions regarding the fiscal efficiency, distributional fairness, and climate-economic suitability of such broad interventions.

The fourth manuscript shifts the focus to digital retail, investigating the pricing implications of the agency model versus the wholesale model for e-books on Amazon in United Kingdom. Challenging traditional antitrust perspectives on vertical restraints, the results show that delegating pricing authority to publishers under the agency model can actually decrease retail prices by approximately 20 % for niche (long-tail) titles. This suggests that this vertical restraint, similar to a Fixed Book Price system, can support market variety and make long-tail content more affordable.

The final manuscript assesses the environmental and traffic impacts of the Deutschlandticket, a nationwide public transport fare reduction in Germany. The empirical analysis reveals that this demand-side subsidy failed to induce a lasting modal shift away from private car usage, yielding only a transient reduction in extra-urban traffic during the initial summer months. These findings suggest that standalone price subsidies are highly insufficient to overcome the non-monetary costs of public transport, pointing instead to the necessity of structural, supply-side infrastructure investments.

Overall, this dissertation provides comprehensive insights into the efficacy, limitations, and unintended consequences of market interventions across diverse economic sectors. Furthermore, the dissertation demonstrates that consumer price sensitivity, spatial market power, and structural market frictions dictate the success or failure of regulatory policies. Policymakers and industry participants can draw on these findings to design more targeted interventions, recognizing, for example, that asymmetric price regulations or broad demand-side subsidies often fall short without addressing underlying supply-side conditions. Future research could extend these empirical frameworks to monitor the ongoing digital transformation of these markets, evaluating how emerging technologies and continuous regulatory shifts affect consumer welfare and firm survival.

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Affidavit

I hereby declare that I completed the papers submitted and listed hereafter independently and with only those forms of support mentioned in the relevant paper or in the following supplementary list. When working with the authors listed, I contributed no less than a proportionate share of the work. In the analyses that I have conducted and to which I refer in the papers, I have followed the principles of good academic practice, as stated in the statutes of the Justus Liebig University Giessen for ensuring good scientific practice.

Gießen, March 17, 2026

Maximilian Maurice Gail