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Unit Sales and Price Effects of Pre-announced Consumption Tax Reforms: Micro-level Evidence from European VAT

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Abstract: This paper studies the effects of multiple pre-announced consumption tax changes on prices and unit sales of consumer durables (household appliances) utilizing micro-level product data. The identification strategy exploits the contemporaneous trading of exactly the same product in different countries within the EU Common Market. The results show that tax rate changes are fully shifted into prices, but a third of the pass-through occurs before implementation. An anticipated tax rate change causes a temporary change in unit sales shortly before implementation, which is more than offset by adjustments upon and after implementation. Quantitatively, in response to a 1 percentage point increase of the tax rate, unit sales rise by 2.5% on average in the last month before implementation. The permanent effect is a drop in unit sales by about 2% below their original level, implying relatively strong intertemporal substitution effects.

Key Words: Fiscal Policy; Consumption Tax; Price Pass-Through; Intertemporal Substitution; Durable Goods

JEL Classification: D15; H31; E62; D12; H24

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1 Introduction

The consumption tax is a potentially powerful instrument of fiscal policy. Economic theory suggests that an upcoming tax rate change would incentivize consumers to accelerate or postpone purchases. Since most countries levy broad-based consumption taxes, this response could be exploited to raise or discourage consumption through a pre-announced change of the tax rate. In particular, economists have proposed phasing in a tax increase or committing to a sequence of tax increases to stimulate consumption (*e.g.*, Shapiro, 1991; Feldstein, 2002). Combined with a reduction in income taxes, this would enable fiscal policy to boost consumption without widening budget deficits. Pre-announced consumption tax increases could specifically counter a decline in the demand for consumer durables (Hall, 2011). Correia, Farhi, Nicolini and Teles (2013) use a New Keynesian model to show that engineering an increasing path of consumption taxes could be an essential part of an unconventional fiscal policy at the zero lower bound.

There is, however, considerable uncertainty about consumer responsiveness at the intertemporal margin (Attanasio and Weber, 2010), all the more so since consumers may not be fully aware of the consumption tax burden (Chetty, Looney and Kroft, 2009). In addition, conventional assumptions about the pass-through of taxes into prices might not hold, and the consumer response might deviate from theoretical predictions due to capital market imperfections. In order to make concrete policy recommendations, it is, therefore, important to evaluate the effects of pre-announced tax rate changes on consumer behavior empirically.

Empirical research exploring the effects of pre-announced changes in consumption taxes on household spending behavior is growing. Common findings are that pre-announced consumption tax increases are salient and do exert positive effects on consumer sentiment and willingness to buy (*e.g.*, Crossley, Low, and Sleeman, 2014; Cashin and Unayama, 2016; D'Acunto, Hoang, and Weber, 2019). The effect on actual consumption, however, depends critically on the extent to which tax changes are passed on to consumers and on the speed of price adjustment. On the pass-through, the empirical literature produces mixed results: under-shifting in Carbonnier (2007), Carare and Danninger (2008) and Viren (2009), partly over-shifting in Besley and Rosen (1999), full-shifting, but early reversal in Crossley, Low, and Sleeman (2014), and full but slow pass-through, requiring more than a year in Benedek *et al.* (2015). With regard to the effects on consumption, the literature mostly finds strong temporary effects, but limited or no intertemporal substitution effects (*e.g.*, Cashin and Unayama, 2016; Baker, Johnson and Kueng, 2017; Cashin, 2018; D'Acunto, Hoang, and Weber, 2019).[1]

The paper makes several contributions to this literature. First, we study consumer responses in terms of changes in unit sales of individual products, which allows us to overcome a number of important limitations encountered in previous work. The existing literature uses expenditure data. This creates the necessity to deflate the data around the implementation of a tax rate change to infer consumption responses. This approach relies crucially on how accurately price index data reflects price movements. In addition, if consumption effects before and after a tax rate change differ across products, controlling for average price changes of a given type or basket of products is not sufficient to obtain unbiased estimates of these effects. Moreover, analysis of expenditures on groups of products is problematic due to the lack of valid counterfactuals. We utilize a unique and extensive monthly micro-level panel data set of unit sales of major domestic appliances at the product level. The data covers close to 50% of the aggregate spending on household appliances in 22 EU Member States, which, in the decade under study, underwent 33 pre-announced changes of the baseline VAT rate.

A second contribution of the paper is to provide new evidence on the contested validity of the critical and widely-used assumption that the pass-through of indirect tax changes into consumer prices is complete and instantaneous upon implementation. Since empirical research on consumption effects is typically based on reduced-form models, knowledge of the magnitude of the price pass-through is crucial to evaluating policy effectiveness. In contrast to most of the literature, we do not use price index data, but work directly with monthly scanner prices at the level of an individual product. Rather than relying on aggregate developments in product groups, our counterfactuals are also built at the product level. Further, we avoid measurement error arising from the assignment

[1]Other papers evaluate targeted subsidies to stimulate consumer spending and promote fuel efficiency (*e.g.*, Mian and Sufi, 2012, Green, Melzer, Parker and Rojas, 2016, Li, Linn and Spiller, 2013, and Hoekstra, Puller and West, 2017). However, our focus is on unconventional fiscal policy based on general consumption taxes.

of specific VAT rates to even highly disaggregated consumption categories (*e.g.,* Benedek *et al.*, 2015; Poterba, 1996).[2]

A third contribution of the paper is the analysis of multiple tax reforms. This enables us to explore differences between reforms. More specifically, we exploit reform heterogeneity along two dimensions: In terms of the length of the implementation lag, *i.e.* the timing between announcement and implementation, as in Mertens and Ravn (2012), and in terms of motivation of the tax policy changes. We show that the implementation lag matters and explore how the results are affected if the timing of tax change announcements is explicitly incorporated into the estimation. The classification of the motivation of tax changes follows the narrative approach to the analysis of fiscal policy put forward by Romer and Romer (2010). This is of particular importance when analysing EU countries, many of which increased consumption tax rates in the aftermath of the recession in 2008.

Throughout our analysis, we employ an identification strategy which exploits the trading of identical products in different countries of the EU Common Market. Counterfactuals for unit sales and prices of a product in a country experiencing a consumption tax rate change are constructed from the contemporaneous sales and prices of exactly the same product sold in other EU countries. In using regional information within the EU, our strategy is similar to the literature utilizing state and local tax variation across U.S. states for identification (*e.g.*, Agarwal, Marwell and McGranahan, 2016, and Baker, Johnson and Kueng, 2017), with important differences: By relying on counterfactuals at the level of identical products instead of aggregated consumption categories, our results are robust to composition biases that arise if consumers' shift spending to different quality goods. Since our analysis focuses on EU countries rather than local jurisdictions, also cross-border shopping as in Agrawal (2015) is less of an issue.[3]

Our results indicate complete price pass-through of consumption tax changes. With respect to the

[2]For example, the corresponding two digit COICOP category 53 for household appliances includes repair services that may be subject to reduced VAT rates.

[3]Unlike state and local sales taxes, our analysis of VAT as a broad consumption tax imposed at national level is of immediate relevance for the fiscal policy debate. Moreover, while VAT exempts business purchases, Ring (1999) shows that about a third of the tax base of the US states' general sales taxes consists of business purchases.

speed of adjustment, pass-through occurs within four months. About a third of a tax change is shifted into prices in the two months prior to implementation, and price adjustment is completed by the second month after implementation. As top-selling products are not found to exhibit different pass-through dynamics relative to other products, we argue that imperfect competition cannot account for pre-reform price pass-through, and offer alternative explanations.

With regard to unit sales, as in Cashin and Unayama (2016), our estimation approach controls for intratemporal substitution effects between durables and non-durables by explicitly estimating the temporary deviations in units sold before and after a tax rate change, and it takes account of the likely effect of consumers' adjustment costs on the time path of sales. The results indicate that in response to an exogenous 1 percentage point (pp) increase in the consumption tax rate, unit sales rise by about 2.5% in the month prior to implementation and decline sharply upon implementation. Afterwards, sales remain about 2% below their pre-reform level. The observed time path is consistent with a relatively large intertemporal elasticity of substitution.

The paper proceeds as follows. The next section discusses the theoretical predictions regarding the effects of a pre-announced tax rate change on the sales of consumer durables and gives a short overview of findings in the literature. Section 3 describes the data set. Section 4 outlines our empirical methodology. Regression results for sales and prices are presented in Section 5, including various robustness checks. Section 6 concludes.

2 Predictions for Spending Responses to Consumption Tax Changes

This section discusses the principal channels through which a pre-announced tax rate change may affect the time path of consumption, particularly of durables, as well as briefly sketches the empirical literature studying spending and price responses in this context.

Within the framework of a standard life-cycle model of consumption, a pre-announced tax rate change affects the time path of consumption through intertemporal substitution. If the tax rate change is reflected in consumer prices, consumers have an incentive to shift consumption to the low-tax period. The magnitude of this response depends on the intertemporal substitution elasticity. Note that this basic prediction of the life-cycle model holds for any type of consumption good.

Tax rate changes may also cause income effects. These effects might not be important, if an upcoming tax rate change is designed as revenue-neutral. At any rate, with pre-announced tax rate changes, in the absence of myopic behavior and credit constraints, income effects would manifest at the time of announcement, and, given a sufficiently long period until implementation, should not affect the further time path of consumption.[4]

Even with full price pass-through and no income effects, tax rate changes might exert further effects on the time path of consumption. Specifically, in the context of durables, temporary effects arise depending on the degree of substitutability between durable and non-durable consumption, the magnitude of adjustments costs, stockpiling, and the behavior of prices around a tax change. We discuss each of these elements in turn.

A first temporary effect arises in the presence of consumer durables, since their consumption is affected by the changing value of the stock of durables. Ogaki and Reinhard (1998) formalize this effect by employing the user cost of the service flow of a durable good. By rising the expected future price, a pre-announced tax increase, for example, leads to a temporary decline in the user cost of durables before implementation, which induces a temporary expansion in durable consumption. As noted by Ogaki and Reinhard (1998), with non-separable preferences, transitory effects on the consumption of durables also affect the optimal time path of non-durable consumption.

A second important factor that influences the time path of consumption are costs of adjustment. Due to such costs, consumers will be less willing to exploit temporary fluctuations in the user cost of services from consumer durables. Moreover, if expansions and contractions in the stock of durables are costly, consumption of durables will not reach its new steady-state level immediately after implementation of a tax rate change. Likewise, the temporary deviation from the steady-state value before implementation arising from the transitory change of the user cost might not

[4] D'Acunto, Hoang, and Weber (2019) provide evidence that German households did not update their perceptions of income or expectations of future income on announcement.

be confined to the last period before a tax rate change. The precise consequences on the pattern of consumption depend on the nature of the adjustment costs. With convex adjustment costs, frequently used in factor demand models (*e.g.*, Shapiro, 1986; Hamermesh and Pfann, 1996), the temporary and permanent changes in the consumption of durables would be distributed over time. Adjustment costs may also be asymmetric, for example, in the presence of information asymmetries in secondary markets for durables and the associated "lemon costs" (Bar-Ilan and Blinder, 1992).[5]

In the case of storable goods, consumers can time purchases to exploit price changes and can accumulate inventories for future consumption (Hendel and Nevo, 2004). To ensure that stockpiling responses are absent, Cashin and Unayama (2016) focus solely on non-storable non-durable goods. While pure stockpiling is especially pertinent to fast-moving non-perishable goods, there is little reason to expect that consumers stockpile furniture or major domestic appliances for later consumption.[6]

The empirical literature concerned with consumption responses to general retail taxes and VAT predominantly supports the existence of significant tax effects based on the analysis of reduced-form models.[7] Identification strategies mainly involve differences-in-differences using variation across countries or regions. Papers that explicitly differentiate between temporary and permanent changes point to limited intertemporal substitution effects (*e.g.*, Cashin and Unayama, 2016; Baker, Johnson and Kueng, 2017; Cashin, 2018). Papers that provide specific estimates for durables, typically find strong short-term effects (*e.g.*, Cashin and Unayama, 2016; Cashin, 2018; D'Acunto, Hoang, and Weber, 2019). In terms of data, all studies use expenditure data, which is only sometimes deflated by price indices.[8]

[5] If adjustment costs are non-convex due to, for instance, fixed or lumpy transaction costs, households' durable purchases are infrequent – only if the actual stock of durables deviates sufficiently from its optimal level, a purchase is made (Grossmann and Laroque, 1990, Bar-Ilan and Blinder, 1992). The implications for aggregate demand depend on the cross–sectional distribution of the vintage of the existing stock of durables (Adda and Cooper, 2000). Nevertheless, the time path of aggregate expenditures would not necessarily be different from a model with convex adjustment costs (Attanasio, 2000).

[6] There are, in addition, costs of delaying consumption, which are specific to electric durables such as foregone savings in electricity or water consumption, or the possibility of a faulty appliance.

[7] Table B.1 in the Appendix provides a brief summary of findings.

[8] Baker, Johnson and Kueng (2017) report unit sales effects separately as a robustness check for expenditure es-

The interpretation of empirical consumption effects depends critically on the extent and duration of the pass-through of tax rate changes into prices. The theoretical appendix derives the consumption path based on the assumption of fixed producer prices so that the tax change is fully reflected in the consumer price. Although complete and instantaneous pass-through is a standard assumption in models with constant returns to scale and perfect competition (Fullerton and Metcalf, 2002), the empirical literature offers little consensus on the matter.[9]

Using state-level sales tax variation in several U.S. cities and commodities, Poterba (1996) shows that sales taxes are fully reflected into consumer prices. Using similar data, Besley and Rosen (1999) find over-shifting for some commodities. For Europe, Carbonnier (2007) studies two major VAT decreases in France and finds under-shifting, especially for car sales. Based on monthly price-index data, Carare and Danninger (2008), who analyze the 2007 VAT increase in Germany, find that 73% of the tax was shifted to the consumer, with updates in prices starting before implementation. The authors suggest that pre-implementation effects may reflect imperfect competition or staggered price adjustment within the framework of Mankiw and Reis (2002). This is in line with Nakamura and Zerom (2008), who note that in the presence of menu cost, the pass-through of cost changes may be delayed. Based on aggregate price data, Viren (2009) finds significant under-shifting of VAT in Europe. Using highly disaggregated consumer price index data for a large number of European countries, Benedek *et al.* (2015) cannot reject full pass-through, but find that it takes more than a year.[10]

3 Data Description

The data set is provided by the market research company Gesellschaft für Konsumforschung (GfK) Retail and Technology GmbH and consists of monthly panel data at the product (model) level on

timates. Their analysis, however, aggregates unit sales across product categories within a household and does not control for differences in the composition of purchases.

[9] Table B.2 in the Appendix provides a brief summary of findings.

[10] Benedek *et al.* (2015) also note that the pass-through of the consumption tax differs between goods taxed at the baseline and at reduced rates. For the latter they find limited pass-through, which is also confirmed for household services by Kosonen (2015) as well as Benzarti, Carloni, Harju, Kosonen, (2017), who additionally show asymmetries with stronger pass-through for tax increases than decreases.

unit sales and scanner prices of durable "white goods" for all countries of the European Union's Common Market, except Bulgaria, Croatia, Cyprus, Ireland, Luxembourg and Malta. The white goods encompass eight major categories: Cookers, refrigerators (coolers), dishwashers, freezers, cook tops (hobs), hoods, tumble driers and washing machines. Each individual product has a unique identification number (id) and a set of physical characteristics. The identifier is the same over time and across countries in case a product is sold in more than one Member State. The time period generally extends for 117 months, from January 2004 until September 2013, although data coverage is shorter for some countries.[11]

The units sold of a product in a given country and a specific month are the sum of all sales of this product across all retailers in the country in the respective month. The corresponding price is a monthly unit-sales-weighted average of all prices for this product across retailers. Prices are inclusive of consumption taxes and any discounts received by consumers.[12]

For each year, the raw data covers around 110,000 different products with 62 million units sold, and an average annual market size of 26 billion Euro. The data set accounts for, on average, 48.7% of the annual aggregate consumption expenditure on all household appliances in the 22 countries under consideration.[13]

In terms of number of units sold and value of sales, refrigerators and washing machines constitute the two biggest categories. While the annual number of products is stable at around 110,000, the composition changes over time, with new products entering the market and older ones exiting. The life cycle, *i.e.* the change in the number of units sold over time for products introduced in the EU's Common Market in a particular year is depicted in Figure 1.[14] Clearly sales are inversely proportional to a product's age. In the first year, sales of new products account for, on average, 20-25% of the total units sold, peak in the second year, and peter out afterwards. About 80% of new

[11] In the Appendix, Table B.3 summarizes the coverage of the data by country and category and Table B.4 provides a detailed description of all available category-specific features.

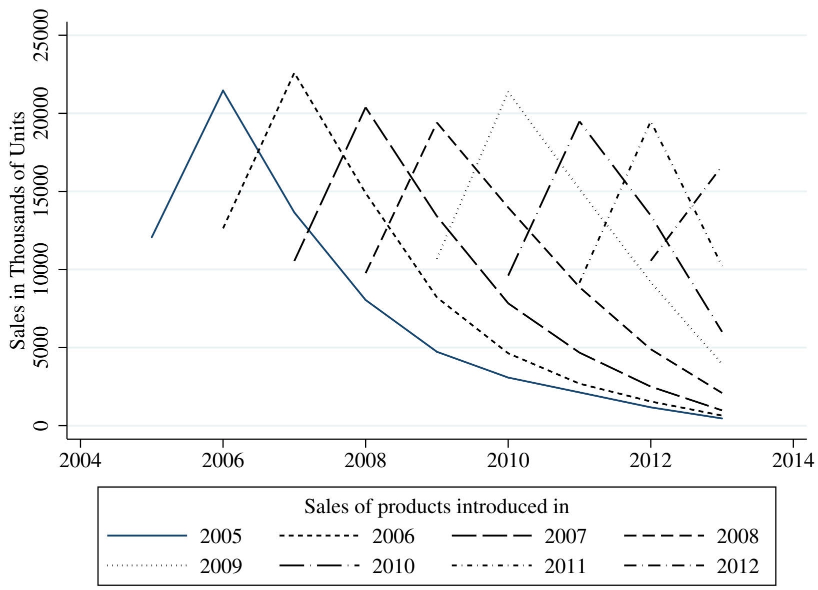
[12] Section A.1 in the Appendix provides more details on the data generation process.

[13] For annual descriptive statistics disaggregated by product category see Table B.5 in the Appendix.

[14] Products' years of introduction are based on the assumption that the first year a product appears in the data (in any country), is the year, in which it was introduced. GfK provided us with a sample plot with exit and entry of fridges based on actual dates of introduction and exit, which was closely mirrored by products' appearance in and disappearance from the data.

products drop out of the market in 5 to 6 years. This pattern does not vary much across individual product categories.

Figure 1 – Product Life-Cycle By Year Of Introduction



Notes: The figure depicts the annual evolution of unit sales by products' year of introduction based on the primary data summarized in Panel A of Table 1. The vertical axis measures the average annual number of units sold of products launched in a given year.

Panel A of Table 1 reports descriptive statistics for the primary data per model, country and month. The monthly sales of a product averaged over time, countries and models amount to 50 units and exhibit considerable dispersion ranging from 0 to 25,000 units. The average price in Euro is 527.[15]

Two statistics refer to a product's life-cycle: the "product age", reporting the number of months the sales of a product are recorded in any country of the EU's Common Market, and the "market age", which reports the number of months a product is sold in a specific country. As the mean market age is only three months less than the mean product age, the data points at a rather synchronized market introduction of products across countries. Table 1 also provides statistics on the rank of a product. All models in the data are ranked according to their sales. The rank variable is category-, country-, and year-specific. Thus, the best selling refrigerator in Germany in a given year has a

[15]Detailed analysis of the data set in terms of missing values, zero values, outliers, and a description of all transformations applied to the data for the purposes of descriptive statistics and the generation of estimation samples are provided in Section A.2 in the Appendix.

Table 1 – Descriptive Statistics

	Mean	Std. Dev.	Min	Max	N
	A. Full data set				
№ Units sold	50.35	185	0	24,965	12,296,125
Price (Euro)	527	388	0.004	29,826	10,887,367
Product age (months)	30.46	23.22	1	117	20,651,469
Market age (months)	26.87	22.27	1	117	20,651,469
Rank	892	798	1	5,364	20,651,469
	B. Estimation sample (identical products sold in two or more countries)				
№ Units Sold	59.96	181	0*	19,062	4,126,760
$dlog(UNITS)$	-0.016	0.892	-22.3	22.2	4,126,760
Price (Euro)	559	367	0.300	11,392	4,032,501
$dlog(PRICE)$	-0.003	0.092	-0.693	1.10	4,032,501
Market Age (Months)	25.65	17.01	2	117	4,129,009
Rank	450	486	1	5,364	4,129,009
R50	0.180	0.384	0	1	4,129,009
R100	0.307	0.461	0	1	4,129,009
Standard VAT rate	0.205	0.023	0.15	0.27	4,129,009
Unemployment rate	8.54	4.08	3.1	27.8	4,129,009
	C. Estimation sample (products with identical characteristics sold in two or more countries)				
№ Units Sold	67.06	213	0*	24,965	7,784,367
$dlog(UNITS)$	-0.022	0.917	-22.7	22.5	7,784,367
Price (Euro)	539	361	0	23,230	7,496,238
$dlog(PRICE)$	-0.003	0.094	-0.693	1.10	7,596,937
Market age (months)	25.82	18.0	2	117	7,784,367
Rank	553	577	1	5,364	7,784,367
R50	0.159	0.366	0	1	7,784,367
R100	0.274	0.446	0	1	7,784,367
Standard VAT rate	0.201	0.023	0.15	0.27	7,784,367
Unemployment rate	8.78	4.14	3.1	27.8	7,784,367

Notes: The table shows summary statistics per model per country per month averaged across time, countries, and models. Panel A summarizes the primary data set. Data in Panel B is restricted to products sold in at least two countries at the same time. Data in Panel C is restricted to groups of products with an identical set of characteristics traded in at least two countries. Product (market) age captures the number of months a product is sold (in a specific country). A best-selling model in any country, year, and category has a rank 1. $R50/R100 = 1$ if a model reaches a rank ∈[1,50]/[1,100] at least once. The exact value of the entries marked with asterisk is 1.00E^{-08}. For detailed description of the data generation process and all data transformations applied to Panels A, B, and C, refer to Section A.2 in the Appendix.

rank one. $R50$ ($R100$) are binary indicators for top-selling models. They equal unity if a model is part of the top 50(100) best-selling products within its respective category at least once during its life-cycle.

Panels B and C present descriptive statistics of two restricted samples used in the empirical analysis in Section 5. Panel B removes all products sold in a single country in a given year, and from the remaining products, keeps only those sold contemporaneously in several countries. Compared to Panel A, the restriction leads to the loss of more than half of all observations for units sold and prices, but within a year the products in this sample comprise 51% of all units sold and generate 58% of the value of sales on average (see Table B.5 and Section A.2).

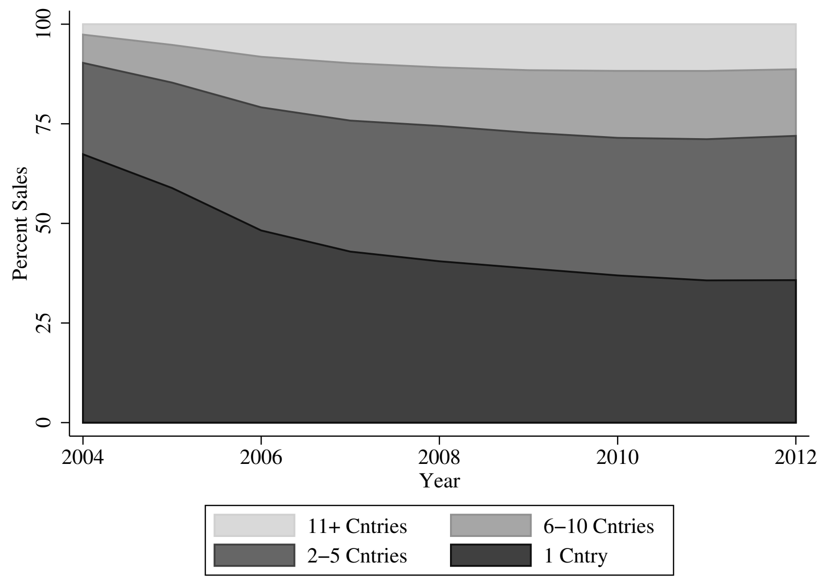
The number of products sold in multiple countries increases over time. Figure 2 reports the changing composition of sales disaggregated by number of countries in which products are sold. While products sold in a single country generated 67% of the total number of units sold in 2004, their share dropped to 35% in 2012, with sales of products sold in two or more countries steadily taking over.

The observation of sales and prices of individual products in multiple countries means that the consumption tax rate varies not only across countries over time, but – for models sold simultaneously in multiple countries – also within each cell of observations comprising the sales and prices of an individual model in a specific time period. It is this characteristic of the data that we exploit in our main identification strategy as explained in Section 4.

Unlike Panel B, which looks at *identical products* sold in multiple countries, Panel C of Table 1 focuses on groups of products with an *identical set of physical characteristics* sold in multiple countries. Products with missing characteristics are removed (Section A.2). This sample reincorporates models sold only in one country and is used for a robustness check in the subsequent empirical analysis.

We supplement the GfK data with data on the consumption taxes in the 22 countries under consideration. While VAT rates differ, the administration and legislation on VAT is harmonized via the European VAT Directive, which guarantees that the VAT treatment of household appliances

Figure 2 – Composition of Unit Sales by Number of Countries in which Products are Sold



Notes: The figure depicts the development over time in the share of units sold (percent from total units) of products sold in one country, two to five countries, six to ten countries and in eleven or more countries. The figure uses the primary data summarized in Panel A of Table 1.

is identical in all Member States. The baseline VAT rate is the relevant tax rate for white goods as they are not subject to reduced VAT, zero rating or exemptions.[16] While from 2004 until 2013 the VAT rates in Austria, Belgium, France, Sweden and Denmark remained unchanged, the other countries in the data altered the standard rate 33 times, leading to considerable time and within-country/within-product variation.

The magnitude of the tax rate changes varies from ±1 pp. to ±5 pp., and their frequency varies from one to four per country in the time period under investigation. Close to 80% of all tax changes took place after 2008, the vast majority being tax increases (decreases occurred in only 5 instances). Table 2 describes in detail the magnitude of changes in the VAT rate, the date of implementation, as well as the date reforms were first announced. For the announcement dates, we rely on official

[16]There are non-VAT instruments to stimulate the consumption of energy efficient household goods, summarized in Copenhagen Economics (2008). Some policies are, for example, lump-sum rebates to consumers for the replacement of old household appliances with new ones from a higher energy efficiency class. These programs, however, typically focus on a small subset of products in a very narrow time frame, and thus are unlikely to confound the empirical effects of VAT hikes.

Table 2 – Standard Vat Rate Changes: 2004-2013

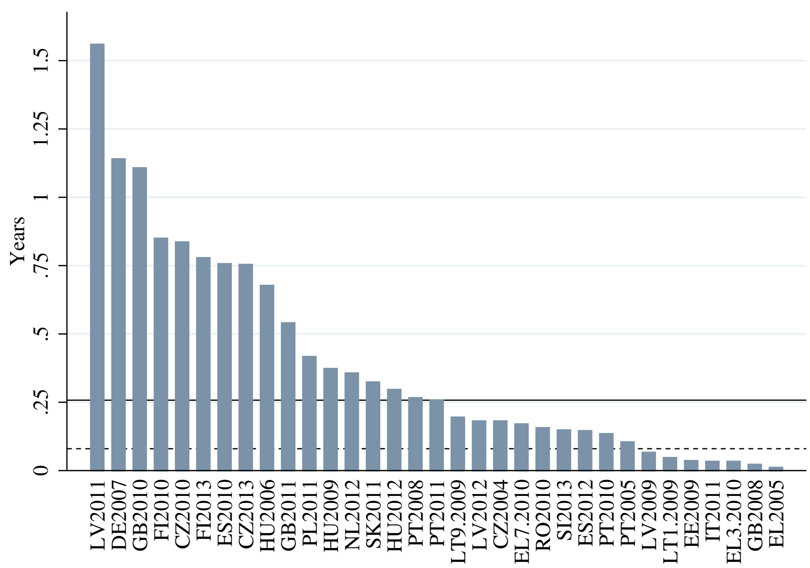
Country	Announcement Date	Implementation Date	Change	Rationale	Classification
Austria	–	–	–	–	–
Belgium	–	–	–	–	–
Czech Republic	26.02.2004	01.05.2004	-0.03	Offsetting, within VAT	Endog.
	03.03.2009	01.01.2010	0.01	GDP-driven, pro-cyclical	Endog.
	02.04.2012	01.01.2013	0.01	Deficit-driven	Exog.
Denmark	–	–	–	–	–
Estonia	18.06.2009	01.07.2009	0.02	Pro-cyclical	Endog.
Finland	26.08.2009	01.07.2010	0.01	GDP-driven, pro-cyclical	Endog.
	24.03.2012	01.01.2013	0.01	Deficit-driven	Exog.
France	–	–	–	–	–
Germany	12.11.2005	01.01.2007	0.03	Debt-driven	Exog.
Greece	29.03.2005	01.04.2005	0.01	Debt-driven	Exog.
	04.03.2010	15.03.2010	0.02	GDP-driven, pro-cyclical	Endog.
	01.05.2010	01.07.2010	0.02	GDP-driven, pro-cyclical	Endog.
Hungary	30.04.2005	01.01.2006	-0.05	GDP-driven, pro-cyclical	Endog.
	16.02.2009	01.07.2009	0.05	GDP-driven, pro-cyclical	Endog.
	16.09.2011	01.01.2012	0.02	Debt-driven	Exog.
Italy	06.09.2011	17.09.2011	0.01	Debt-driven	Exog.
Latvia	09.12.2008	01.01.2009	0.03	GDP-driven, pro-cyclical	Endog.
	12.06.2009	01.01.2011	0.01	Deficit-driven	Exog.
	27.04.2012	01.07.2012	-0.01	Long-run growth	Exog.
Lithuania	16.12.2008	01.01.2009	0.01	GDP-driven, pro-cyclical	Endog.
	23.06.2009	01.09.2009	0.02	GDP-driven, pro-cyclical	Endog.
Netherlands	25.05.2012	01.10.2012	0.02	Debt-driven	Exog.
Poland	03.08.2010	01.01.2011	0.01	Debt-driven	Exog.
Portugal	25.05.2005	01.07.2005	0.02	Debt-driven	Exog.
	26.03.2008	01.07.2008	-0.01	GDP-driven, counter-cyclical	Endog.
	14.05.2010	01.07.2010	0.01	GDP-driven, pro-cyclical	Endog.
	29.09.2010	01.01.2011	0.02	Debt-driven	Exog.
Romania	06.05.2010	01.07.2010	0.05	GDP-driven, pro-cyclical	Endog.
Slovakia	06.09.2010	01.01.2011	0.01	Deficit-driven	Exog.
Slovenia	09.05.2013	01.07.2013	0.02	Long-run growth	Exog.
Spain	29.09.2009	01.07.2010	0.02	GDP-driven, pro-cyclical	Endog.
	11.07.2012	01.09.2012	0.03	GDP-driven, pro-cyclical	Endog.
Sweden	–	–	–	–	–
United Kingdom	24.11.2008	01.12.2008	-0.025	GDP-driven, counter-cyclical	Endog.
	24.11.2008	01.01.2010	0.025	GDP-driven, pro-cyclical	Endog.
	22.06.2010	04.01.2011	0.025	Debt-driven	Exog.

Notes: The announcement dates are either specific dates on which the authorities officially announced the future change in the standard VAT rate, or the earliest date a change in VAT was mentioned generally in the media. With the exception of Estonia and Slovenia, the classification and motivation of reforms are taken from Gunter *et al.* (2017). Source: Rates and implementation dates are from Ernst & Young, European Commission, and KPMG.

statements by authorities, or, if such statements were not found, on media reports. Appendix D provides information on the salience and graphical evidence on the effects of consumption tax increases in the raw data for Germany and Spain.

Among the thirty three reforms considered in this paper, there is substantial heterogeneity in the time between announcement and implementation, *i.e.* the implementation lag. As shown in Figure 3, the implementation lag ranges from one and a half years to three days. The median length of the time-interval is a little over a quarter of a year. In seven cases, announcements occurred less than a month before their implementation. Such short anticipation horizons are typically observed in countries facing economic and fiscal difficulties such as the Baltic states in 2009 or Greece in 2010. Similarly, the temporary VAT cut in the UK in December 2008, intended as a fiscal stimulus to boost sales, became effective one week after its announcement.[17]

Figure 3 – Time between Announcement and Implementation



Notes: The graph shows the length of the period between announcement and implementation measured in days and scaled by the total number of days in a year for the 33 VAT reforms summarized in Table 2. The solid horizontal line depicts the median time between announcement and implementation, which is a little over a quarter of a year. All reforms below the dashed line were announced less than a month before their enactment. Authors' calculations (see note to Table 2)

[17]The 2008 United Kingdom reform is the only explicitly temporary tax change. In all other countries, tax changes where enacted as permanent.

The 2008 UK reform fits well within what the so-called narrative approach to analyzing fiscal policy would classify as an endogenous tax change. Given its motivation to stimulate consumer spending in the aftermath of the financial crisis, it is a tax reform undertaken "*to offset developments that would cause output growth to differ from normal*" (*cf.* Romer and Romer, 2010, p.769). Relying on endogenous tax reforms when studying how sales and prices of durables react to tax changes could be misleading, since it might be difficult to disentangle the effect of these developments from that of government actions taken in response. A similar issue arises with respect to the above mentioned pro-cyclical fiscal policy measures observed in the Baltic countries and Greece, enacted as a consequence of a fiscal crisis and a limited access of these governments to international credit markets (Gunter, Riera-Crichton, Végh and Vuletin, 2017).

We address policy endogeneity by categorizing the 33 VAT changes in terms of endogeneity/exogeneity, and checking whether results remain robust to the exclusion of endogenous reforms. To this end, we rely on Gunter *et al.* (2017), who assembled a data set of 96 tax reforms of baseline consumption taxes worldwide in the period 1970-2014 and classified them based on the narrative approach of Romer and Romer (2010). Table 2 adds information on two reforms not classified by Gunter *et al.* (2017), and identifies 18 endogenous and 15 exogenous tax changes.

4 Methodology

As noted above, from a theoretical perspective, a pre-announced change in the consumption tax rate incentivizes intertemporal substitution, as consumers increase consumption in the time period in which the tax rate and, hence, consumer prices are low. In addition to this permanent effect, in a life-cycle model of consumption with durables goods, a tax rate change will also induce temporary effects, particularly in the periods immediately before and after implementation (see the theoretical appendix). In the following analysis, we develop an empirical specification that takes account of both temporary and permanent effects.

Measuring the rate of change in sales with the log difference of units sold, $\Delta\log(UNITS)_{icd}$, of a product i in country c at date d, we formulate the following estimation equation:

$$\begin{aligned} \Delta \log(UNITS)_{icd} &= \sum_{j=1}^{p} a_j \mathsf{L}^{-j} \Delta\tau_{cd} + b\Delta\tau_{cd} + \sum_{j=1}^{q} d_j \mathsf{L}^{j} \Delta\tau_{cd} + aX_{icd} \\ &+ \alpha_{id} + \rho_c + \gamma_{cm(d)} + u_{icd}. \end{aligned} \tag{4.1}$$

The date d varies by month $m(d)$ and year $t(d)$. $\Delta\tau_{cd}$ is the current change in the tax rate relative to the previous month, $\mathsf{L}^{-j}\Delta\tau_{cd}$ is a lead term, capturing the j-month-ahead change in the tax rate, and $\mathsf{L}^{j}\Delta\tau_{cd}$ is the change in the tax rate lagged by j-months, where p and q indicate the numbers of leads and lags.

α_{id} denotes a product-date-specific fixed effect that absorbs any product specific movements in sales. Incorporating a product fixed effect is essential since each product has specific features that distinguish it from other products on the market. Given technological progress and product innovation, the (relative) quality of a product and, hence, its attractiveness to consumers vary over time. This is reflected in the striking product-cycle patterns displayed in Figure 1. Inclusion of product-date fixed effects α_{id} ensures that identification comes only from differences in the growth rate of sales of the same product across countries.[18] Consequently, this specification focuses on products sold in at least two countries at the same time.

Identification of the tax effect on unit sales relies on changes in the consumption tax treatment that affect only a sub-group of the observations within each product-date cell. In case of a tax rate change in country c on date d, for a given product i sold in countries $k = \{1, 2, ..., c\}$, unit sales, $UNITS_{icd}$, are compared to the sales in all other countries, in which the identical product i is available, $UNITS_{ik\setminus\{c\}d}$. This counterfactual requires the Common-Trend assumption to hold, implying that, conditional on all controls, had there been no tax rate change in country c, the sales of the product would have followed the same time trend as the sales of this product in the other countries $k \setminus \{c\}$. Since all sales occur within the EU's Common Market, which prevents internal borders or regulatory obstacles, and ensures that products are subject to identical legislation, our

[18] In an analysis of subsidy effects on car sales at product level, Li, Linn and Spiller (2013) follow a similar approach and employ product-year-specific fixed effects.

data is in accordance with this assumption.

Differential trends might nevertheless arise due to cross-country differences in seasonal patterns, the timing of holidays, climate, or the business cycle. As product portfolios vary across countries, divergent cross-country product life-cycle trends cannot be precluded. Therefore, we control for seasonal patterns by country and introduce further control variables. More specifically, X_{icd} includes the monthly unemployment rate in country c, as well as an indicator for the time period a product has been sold in a specific country, and its square term as explanatory variables. The "market age", $M.age$, varies by country within a product-date cell, if a product does not enter all countries at the same time, while the square term should capture any non-linear product cycle effects. To deal with differences in seasonality of sales across countries, we include country-month specific fixed effects, $\gamma_{cm(d)}$, together with a set of country-specific fixed effects, ρ_c.

The empirical specification for sales captures the time trend around a tax rate change with a set of current, forward and lagged month dummies scaled by the respective tax rate change. As in Cashin and Unayama (2016), the empirical identification of the elasticity of intertemporal substitution, σ, requires the separation of temporary effects in the months around implementation from the permanent effect. As noted above, and shown in the theoretical appendix, the sum $\sum_{j=1}^{p} a_j + b + \sum_{j=1}^{q} d_j$ captures the effect of the permanent change in the tax rate on the unit sales path, which reveals the elasticity of intertemporal substitution when p, q are sufficiently large.[19] Due to the transitory effects on the user cost, strong short-term effects might be present, in particular if the elasticity of intratemporal substitution is large (Cashin, 2018).

If adjustment costs are unimportant and price-pass through is quick, it might suffice to set $p, q = 1$.

[19] Cashin and Unayama (2016) do not scale the month dummies with the tax rate change, as they consider only a single tax rate change. Apart from that, they use an equivalent specification. It includes first differences of the monthly effects such that the coefficient for the tax rate change immediately reveals the permanent effect. In our context, this implies estimating

$$\begin{aligned}\Delta \log(UNITS)_{icd} &= \sum_{j=1}^{p} \alpha_j \left[\mathsf{L}^{-j}\Delta\tau_{cd} - \mathsf{L}^{-j+1}\Delta\tau_{cd}\right] + \beta\Delta\tau_{cd} + \sum_{j=1}^{q} \delta_j \left[\mathsf{L}^{j-1}\Delta\tau_{cd} - \mathsf{L}^{j}\Delta\tau_{cd}\right] + aX_{icd} \\ &+ \alpha_{id} + \rho_c + \gamma_{cm(d)} + u_{icd}.\end{aligned}$$

The term β in this equation is equivalent to $\sum_{j=1}^{p} a_j + b + \sum_{j=1}^{q} d_j$ in equation (4.1).

In this case, $a_1 > 0$, $b < 0$ and $d_1 > 0$. In the presence of large adjustment costs and/or a price-pass through that takes longer, too narrow intervals before and after tax rate changes would exclude systematic effects and lead to biased estimates (see Malani and Reif, 2015). Hence, the empirical analysis allows $p, q \geq 1$, and tests for tax effects using wider pre- and post-implementation windows. In a statistical sense, the optimal width of a window around a tax rate change, or, equivalently, the values of p and q, could be selected by specification testing for better fit via gradual extension of the window. With the introduction of higher-order leads, however, this procedure would employ information about an upcoming tax reform, regardless of whether it has already been announced or not.

To provide empirical estimates that reflect the information set of agents, at least in a stylized way, we utilize the announcement dates reported in Table 2 and replace j-period ahead lead terms, $\mathsf{L}^{-j}\Delta\tau_{cd}$, with their expected values $\mathsf{E}_{d-j}\left[\mathsf{L}^{-j}\Delta\tau_{cd}\right]$, thus taking account of the precise point in time when information about an upcoming VAT change becomes available in a given country. In particular, if a tax rate change is announced n months in advance, we set $\mathsf{E}_{d-j}\left[\mathsf{L}^{-j}\Delta\tau_{cd}\right] = 0$, $\forall j > n$, and $\mathsf{E}_{d-j}\left[\mathsf{L}^{-j}\Delta\tau_{cd}\right] = \mathsf{L}^{-j}\Delta\tau_{cd}$, $\forall j \leq n$.

Further, the estimation of higher-order-lead terms rests on a declining number of identifying reforms (and countries) due to the varying length of implementation lags.[20] As a consequence, estimates of pre-implementation responses at varying time horizons may suffer from composition effects. To address this concern, the empirical analysis employs a parsimonious specification with a limited number of lead terms. Guided by Figure 3, which shows that the median implementation lag is about a quarter, we focus on leads in the interval $j \in [1, 3]$. Seventeen reforms were announced three or less than three months before enactment. For seven of them, announcement and implementation occurred within the same month. In these cases, it would be difficult to separate the effects of government policy from those of the macroeconomic shocks that may have triggered the government intervention in the first place. Likewise, we cannot rule out the presence of income effects. We check whether the temporary deviations in sales before implementation are biased downwards by excluding tax changes with implementation lags shorter than 30 or even 90 days as

[20] See Table B.7 in the Appendix.

in Mertens and Ravn (2012).

As variation in tax rate changes is at a country level, standard errors in eq. (4.1) should preferably be clustered by country (Bertrand, Duflo, and Mullainathan, 2004). With 22 countries in the data, the asymptotic assumptions required for a consistent estimate of a heteroskedastic- and cluster-robust variance-covariance matrix are not satisfied (Cameron and Miller, 2015). We opt instead to adjust standard errors for 165 product category-country clusters.[21] This approach is valid provided that strong within-group correlation is mostly confined to products that belong to the same product category in the same country.[22]

In the subsequent analysis, we perform two main robustness checks. First, we address the possibility that counterfactual unit sales in countries $l \in k \setminus \{c\}$ could be affected by a tax rate change in c. The estimation could, therefore, be vulnerable to a violation of the Stable Unit Treatment Value assumption (*e.g.*, Lechner, 2011). Given the size of the EU Common Market, tax rate changes in a single EU country are unlikely to influence the total market. However, if a product is sold only in a few countries, and one of them is hit by a consumption-tax induced demand shock, the others might not serve as a valid control group, since their prices could be susceptible to the shock. An alternative explanation for cross-country effects of tax rate changes is cross-border shopping. To see whether these are relevant concerns, we test sales and price regressions in sub-samples with products sold in more than 2, 3, 4 *etc.* countries. The larger the number of countries in k, the smaller the likelihood of cross-country shock spill-overs, since the share of the total market that is affected by a tax rate change declines.

The second robustness check pertains to sample selection. Focusing on products sold in two or more countries may cause selectivity bias, if products sold in a single country differ systematically from those sold in multiple countries. The latter might be of higher quality and more expensive, compared to single-country products, which are probably domestically produced. If single-country

[21] As a robustness check, we estimated (4.1) clustering over countries, but bootstrapped standard errors following Cameron, Gelbach and Miller (2008) by implementing the wild bootstrap post-estimation procedure developed in Roodman *et. al.* (2018). The estimated p-values in this exercise are similar to those in the benchmark regressions and are reported in Table B.8 in the Appendix.

[22] Exploring brand composition by product category revealed that leading brands are different for different types of products, implying limited within brand correlation across product categories.

products are specifically designed for a country, they may face less competition from other products. The incentives to buy before and after consumption tax changes could, therefore, vary between these two types of products. As a robustness test, we employ an alternative approach to identifying the tax effects in equation (4.1). Specifically, we re-incorporate single-country products into the estimation sample by using larger cells comprising not just identical products, but a group of products with a set of identical characteristics. A drawback of this procedure is that, due to the limited number of available characteristics, there will be heterogeneity left in the individual group-date cells, which may result in less precisely estimated effects.

Given that the theoretical predictions depend crucially on price responses, we explore whether and to what extent the data supports complete and immediate pass-through of taxes into prices. To this end, we follow an equivalent estimation strategy to eq. (4.1) and use differences in outcomes within a product-date cell to identify tax effects within the following specification:

$$\begin{aligned} \Delta \log(PRICE)_{icd} &= \sum_{j=1}^{P} A_j \mathsf{L}^{-j} \Delta\tau_{cd} + B\Delta\tau_{cd} + \sum_{j=1}^{Q} D_j \mathsf{L}^{j} \Delta\tau_{cd} + \alpha X_{icd} \\ &+ \alpha_{id} + \rho_c + \gamma_{cm(d)} + \nu_{icd}. \end{aligned} \quad (4.2)$$

$\Delta \log(PRICE)_{icd}$ denotes the difference in the log consumer price of product i in country c in month d relative to the previous month. As before, α_{id}, ρ_c, and $\gamma_{cm(d)}$ denote product-date-, country-, and country-month-specific fixed effects. P determines the order of lead terms and Q the order of lagged terms of $\Delta\tau_{cd}$.

The empirical specification is flexible with regard to the window range. The theoretical prior, on which much of the literature on consumption effects is based, is that the price pass-through is instantaneous and complete. In equation (4.2) this would be equivalent to finding $B = 1$ and small and statistically insignificant coefficients A_j or D_j on the pre- and post- implementation terms, so that a simpler specification without leads and lags would suffice. With non-instantaneous pass-through, pre- and/or post-reform effects would be significant. Even in this case, eq. (4.2) allows us to test whether there is full pass-through of consumption taxes into consumer prices. The sum $\sum_{j=1}^{P} A_j + B + \sum_{j=1}^{Q} D_j$ gives the long-term effect of the change in the VAT rate on prices, which can be interpreted as a pass-through elasticity (Benedek *et al.*, 2015). In the current framework, an elasticity of unity would indicate complete pass-through. Under-shifting (over-shifting) occurs when the elasticity is smaller (greater) than one.

Instead of using prices in levels, the dependent variable in (4.2) is in log changes, which removes the pre-tax price differential for individual products between countries, but does not remove any time variation in this differential.[23] If the mark-up charged by a producer in a country is sensitive to tax rate changes, the empirical specification (4.2) will reject full price pass-through and indicate over- or under-shifting.

5 Results

5.1 Basic Results for Unit Sales

The empirical analysis starts with studying the tax effects on unit sales of durables following eq. (4.1). The estimation sample includes data for 22 EU countries.[24] We explore the effects of 33 consumption tax reforms that altered the baseline consumption tax rate. As summarized in Table 1, the sample employs data for approximately 72,000 unique products sold in at least two countries, resulting in about 1,330,000 product-date pairs, and over 4 million product-country-date observations.

The estimation results in Table 3 are based on pre- and post implementation periods restricted to one month each, so that $p, q = 1$. The first column reports estimates from a specification using only product-date and country-specific fixed effects. The second column adds a full set of country-month dummies, which account for country-specific seasonality in unit sales due to differences in the timing of holidays, sales promotions and other factors. In this specification, the

[23]Figure C.2 plots the distribution of all observed bilateral price differentials net of VAT within each product-date cell. Even though the distribution is centered around zero, indicating that a large number of products are sold at identical prices in different countries, deviations by 0.25 log points or more are not uncommon.

[24]To avoid structural breaks stemming from the transition of Slovenia, Slovakia, and Estonia from national currencies to Euro, data for these countries is restricted to after Jan. 1st, 2007, after Jan. 1st, 2009, and before Dec. 31st, 2010, respectively.

TABLE 3 – BASIC ESTIMATES OF UNIT SALES EFFECTS

	(1)	(2)	(3)	(4)
$\mathsf{L}^{-1}\Delta\tau_d$	2.615	2.444	2.426	2.421
	(0.366)	(0.314)	(0.315)	(0.340)
$\Delta\tau_d$	-3.817	-4.338	-4.350	-4.412
	(0.648)	(0.415)	(0.415)	(0.436)
$\mathsf{L}\Delta\tau_d$	-2.146	-1.700	-1.717	-1.754
	(0.433)	(0.289)	(0.291)	(0.313)
$Unempl$	0.001	0.001	0.001	-0.001
	(0.000)	(0.000)	(0.000)	(0.002)
$M.age$			-0.485	-0.532
			(0.038)	(0.038)
$M.age2$			0.420	0.468
			(0.040)	(0.041)
$Constant$	-0.023	-0.024	0.058	0.085
	(0.002)	(0.002)	(0.006)	(0.015)
Cumulative Effects				
Total $\sum_{j=-1}^{j=1} \mathsf{L}^j \Delta\tau_d$	-3.349	-3.594	-3.640	-3.744
	(0.544)	(0.453)	(0.454)	(0.571)
Month-country effects	No	Yes	Yes	Yes
Year-country effects	No	No	No	Yes
N	4,126,760	4,126,760	4,126,760	4,126,760
Product-date effects	1,331,154	1,331,154	1,331,154	1,331,154
Products	72,056	72,056	72,056	72,056

Notes: Regressions in columns (1)-(4) are based on data for 22 EU countries. The data is restricted to goods sold contemporaneously in at least 2 countries. The dependent variable is the change in the logarithm of unit sales $\Delta\log(UNITS)$. The monthly change in the standard VAT rate is denoted by $\Delta\tau_d$. The lead term, $\mathsf{L}^1\Delta\tau_d$, captures all reforms in the month before their implementation. The lag term, $\mathsf{L}\Delta\tau_d$, refers to the month after implementation. $Unempl$ is the monthly unemployment rate. $M.age$ is the number of months a product appears in the data in a specific country, scaled by 1/100. All specifications include a set of product-date specific (id) and country-specific fixed effects. Standard errors in parentheses are robust in all specifications and clustered by product category-country.

contemporaneous tax effect is larger and the lagged response smaller. Column (3) additionally controls for the market age, $M.age$, as well as $M.age^2$, whose estimated coefficients imply that the growth rate in unit sales declines non-linearly with a product's country-specific age.[25] As a robustness check, column (4) incorporates country-specific year effects, which might be important in the presence of annual budgeting of households, or due to annual economic shocks from fiscal policy. Compared to the results in column (3), augmenting the specification with country-year dummies yields similar results – the differences in the estimated slope parameters for the tax effects are below the standard error for all tax terms.

The point estimates in column (3) indicate that a tax increase by 1 percentage point causes unit sales to rise by 2.4% in the last month with a low tax rate. Once the higher tax rate is implemented, unit sales drop by about 4.4% relative to the month before the reform. The lagged tax change shows that units sales continue to decline by 1.7% in the month following implementation. The sum of the coefficients on leading, lagged, and contemporaneous tax change effects is about 3.7 in the basic specifications, which points to a rather strong permanent effect. Before we test the robustness of these findings, we turn to studying price adjustment. If we find that price pass-through is slow, for instance, consumers might still postpone purchases, as prices continue to increase after implementation. This would suggest increasing the order of lags.

5.2 Price Effects

Table 4 reports results of a regression of the monthly (log) change in consumer prices on tax rate changes following eq. (4.2) with varying lengths of the pre- and post-reform windows. All specifications include an identical set of fixed effects and control variables as in column (3) of Table 3. Column (1) reports a contemporaneous price increase of 0.22% if the tax rate increases by 1 percentage point, clearly rejecting the null hypothesis of full pass-through at the point of implementation. Column (2) includes the tax rate changes in the preceding as well as in the following month, with both coefficients being significantly positive. The cumulative effect, as reported in

[25] $M.age$ and $M.age^2$ are scaled by $1/100$ and $1/100^2$ in the estimations.

TABLE 4 – PRICE EFFECTS

Reforms	All			All		$n \geq 1$		n>3
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathsf{L}^{-3}\Delta\tau_d$			0.154					
			(0.033)					
$\mathsf{L}^{-2}\Delta\tau_d$			-0.104					
			(0.064)					
$\mathsf{L}^{-1}\Delta\tau_d$		0.116	0.116					
		(0.031)	(0.031)					
$\mathsf{E}\left[\mathsf{L}^{-3}\Delta\tau_d\right]$					0.220		0.250	0.229
					(0.037)		(0.037)	(0.043)
$\mathsf{E}\left[\mathsf{L}^{-2}\Delta\tau_d\right]$					0.045		0.034	0.116
					(0.027)		(0.028)	(0.033)
$\mathsf{E}\left[\mathsf{L}^{-1}\Delta\tau_d\right]$				0.126	0.126	0.141	0.142	0.158
				(0.031)	(0.032)	(0.032)	(0.032)	(0.035)
$\Delta\tau_d$	0.219	0.219	0.217	0.219	0.218	0.164	0.163	0.100
	(0.047)	(0.047)	(0.046)	(0.047)	(0.047)	(0.038)	(0.037)	(0.043)
$\mathsf{L}\Delta\tau_d$		0.388	0.387	0.389	0.389	0.432	0.431	0.507
		(0.037)	(0.037)	(0.037)	(0.037)	(0.034)	(0.034)	(0.035)
$\mathsf{L}^2\Delta\tau_d$			-0.126		-0.128		-0.125	-0.250
			(0.046)		(0.045)		(0.051)	(0.043)
$\mathsf{L}^3\Delta\tau_d$			0.086		0.086		0.104	0.160
			(0.031)		(0.031)		(0.032)	(0.041)
				Cumulative Effects				
Total	0.219	0.723	0.730	0.734	0.956	0.737	0.999	1.021
	(0.047)	(0.050)	(0.069)	(0.050)	(0.070)	(0.047)	(0.070)	(.086)
Pre-reform		0.116	0.166	0.126	0.391	0.141	0.426	0.504
		(0.031)	(0.087)	(0.031)	(0.055)	(0.032)	0.056)	(0.061)
Post-reform		0.608	0.564	0.608	0.565	0.596	0.573	0.517
		(0.043)	(0.062)	(0.043)	(0.062)	(0.038)	0.052)	(0.056)
Pass-through F(1)		30.00	15.34	28.46	0.39	30.82	0.00	0.06
N	4,032,508	4,032,508	4,032,508	4,032,508	4,032,508	3,916,700	3,916,700	3,747,035
Product-date effects	1,302,880	1,302,880	1,302,880	1,302,880	1,302,880	1,275,887	1,275,887	1,227,989
Products	71,223	71,223	71,223	71,223	71,223	70,663	70,663	71,167

Notes: Regressions are based on data for 22 EU countries. The dependent variable is the change in the logarithm of the actual consumer price $\Delta\log(PRICE)$. The data is restricted to goods sold contemporaneously in at least 2 countries. Estimates in columns (6) and (7) are based on a reduced sample, in which observations in countries with reforms announced less than a month before implementation, are removed around the respective reform date. The monthly change in the standard VAT rate is denoted by $\Delta\tau_d$. Note that $\mathsf{E}\left[\mathsf{L}^{-j}\Delta\tau_d\right] = \mathsf{L}^{-j}\Delta\tau_d$ for all reforms that were announced $n > j$ periods ahead, and $\mathsf{E}\left[\mathsf{L}^{-j}\Delta\tau_d\right] = 0$ for reforms announced $n \leq j$. All specifications include a full set of product-date, country, and country-month fixed effects. The monthly unemployment rate, $Unempl$, and the number of months a product appears in the data in a specific country, $M.age$, as well as $M.age^2$ are controlled for but not reported. Standard errors in parentheses are robust in all specifications and clustered by product category-country.

the lower portion of the table, suggests that within these three months, about three quarters of the tax rate change is shifted to the consumer. According to the corresponding F-statistic, full pass-through can still be rejected at conventional levels of significance. Widening the window to three months before and after implementation yields an almost identical estimate of the total pass-through, although the specification clearly points to a price response as early as a quarter of a year before the policy adoption.

The specifications in columns (2) and (3) employ forward terms of tax rate changes but do not account for the different implementation lags of reforms. As discussed above, in several cases, this means that the estimation uses information on tax policy that, in fact, was not available to consumers. To remedy this, the specifications in columns (4) and (5) employ expected values of upcoming tax rate changes. These variables take account of the actual information set by restricting leading terms to zero in the months when an upcoming tax reform has not yet been announced. For the short window of one month before and after implementation (column (4)), the estimated magnitude of the total pass-through, 73%, is not statistically different from the case with no announcements (column (2)). However, the ex-ante price adjustment rises to 39% once a longer window is employed, with all leading terms in specification (5) exhibiting larger and consistently positive coefficients in contrast to specification (3). As the post-reform pass-through implied by specification (5) is 57%, the cumulative price effect is not significantly different from unity at conventional levels of significance. This suggests that full price pass-through occurs within a seven-month period – three months before and three months after the tax rate change. The substantially higher estimate of the pre-reform pass-through in column (5) in comparison to (3) clearly highlights the importance of the announcement information: Despite a sufficiently long window, the specification in (3) would point to an incomplete pass-through of taxes.

Columns (6) and (7) exclude observations associated with reforms pre-announced by less than a month, since the pre-implementation adjustment may capture income or announcement effects.[26] The pre-reform pass-through effects are found to be qualitatively and quantitatively similar to those

[26] Observations are excluded six months before and six months after implementation for products in the relevant countries and years, without removing the product from the data in non-reform years, or its sales in other countries. See Section A.2 for description of the exact procedure.

reported in columns (4) and (5). The estimates in column (7) indicate that a tax rate increase by 1 percentage point causes consumer prices to rise by 0.43% before the reform and by 0.57% after the reform, one third of which is a contemporaneous effect. Column (8) employs a more stringent restriction, requiring implementation lags longer than 3 months as in Mertens and Ravn (2012). Again, the cumulative price pass-through in this specification is equal to unity, which suggests that income effects are not important for the empirical adjustment path of prices.

The estimates in columns (7) and (8) of Table 4 indicate that price pass-through starts a quarter prior to a tax rate change and is completed by the third month after implementation. Note that windows larger than seven months centered around a tax rate change did not yield statistically significant coefficients past the third leads and lags. Hence, there is no indication of over-shifting or price reversals.

The pass-through estimates are also robust to a more demanding identification strategy achieved through sample reduction. Excluding reforms announced less than a month before their entry into force and using expected values of tax rate changes as in columns (6) to (8) of Table 4, we gradually restrict the sample to products traded in more and more countries simultaneously. This ensures that there are multiple observations from countries without a reform within each product-date cell. The null hypothesis of full pass-through cannot be rejected at usual levels of significance, even when the sample is down to 6,690 products traded in at least eight countries.[27]

5.3 Generalized Unit Sales Effects

Based on the findings in the previous section, the data supports full price pass-through in accordance with the conventional view in the literature. However, the pass-through is not instantaneous, as prices start to rise before the tax rate changes, and continue to adjust a quarter after implementation. This implies that the pre-implementation response of unit sales might not be confined to the last period before a reform, and demand recovery might be delayed. To account for implications of non-instantaneous pass-through, but also for further temporary effects arising due to adjustment

[27] The results are reported in Table B.9 in the Appendix.

costs, Table 5 applies a specification with additional leads and lags of the tax rate change. All specifications include a full set of product-date, country and country-month fixed effects and the same controls as in column (3) of Table 3. For convenience, column (1) repeats the results of this specification. Column (2) uses a wider window of three months before and after a tax rate change, with the results pointing at a drop in purchases in the third and second month prior to implementation, which reduces the total pre-implementation response compared to column (1). With regard to lagged terms, unit sales continue to decline in the second month after a tax rate change, and recover in the third month. The specifications in columns (3) and (4) employ the expected rather than the actual tax rate change for the estimation of pre-reform effects. Unlike for prices, announcement information seems to matter less for unit sales as demonstrated by the largely similar results in columns (2) and (4).

Columns (5)-(6) exclude tax changes announced and implemented in the same month, resulting in somewhat stronger effects in the month of implementation. Column (7) focuses on an even smaller number of reforms, for which the implementation lag exceeds three months. Results are qualitatively similar to column (6), which suggests that, as with prices, income and other announcement effects do not influence the estimates of the pre-implementation responses. In another check for announcement effects, we added a variable using an indicator of the tax rate change at the time of the announcement, but did not detect any significant response.

The identification of tax effects in our analysis relies on the Stable Unit Treatment Value assumption, which might be invalidated by cross-country effects of tax rate changes. To check for the presence of such effects, we conducted robustness tests by gradually restricting the sample to products sold in an increasing number of countries, which ensures that identification of the effect of tax rate changes on sales comes from a larger number of control countries within product-date cells. The estimates are similar to the benchmark results presented in columns (6) and (7) of Table 5.[28]

Another robustness check of the specification employs an alternative estimation strategy that, as

[28]Table B.10 in the Appendix shows the results.

Table 5 – Generalized Estimates Of Unit Sales Effects

Reforms	All		All		n ≥ 1		n>3
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\mathsf{L}^{-3}\Delta\tau_d$		-0.850					
		(0.296)					
$\mathsf{L}^{-2}\Delta\tau_d$		-0.218					
		(0.306)					
$\mathsf{L}^{-1}\Delta\tau_d$	2.426	2.397					
	(0.315)	(0.313)					
$\mathsf{E}\left[\mathsf{L}^{-3}\Delta\tau_d\right]$				-0.833		-0.937	-1.345
				(0.362)		(0.371)	(0.365)
$\mathsf{E}\left[\mathsf{L}^{-2}\Delta\tau_d\right]$				-0.331		-0.416	-0.030
				(0.316)		(0.320)	(0.428)
$\mathsf{E}\left[\mathsf{L}^{-1}\Delta\tau_d\right]$			2.349	2.320	2.464	2.455	2.241
			(0.324)	(0.324)	(0.327)	(0.326)	(0.383)
$\Delta\tau_d$	-4.350	-4.357	-4.351	-4.358	-4.797	-4.806	-4.387
	(0.415)	(0.414)	(0.415)	(0.414)	(0.440)	(0.439)	(0.477)
$\mathsf{L}\Delta\tau_d$	-1.717	-1.689	-1.716	-1.702	-1.432	-1.417	-1.891
	(0.291)	(0.290)	(0.291)	(0.292)	(0.305)	(0.304)	(0.325)
$\mathsf{L}^2\Delta\tau_d$		-0.456		-0.453		-0.452	0.186
		(0.289)		(0.289)		(0.309)	(0.361)
$\mathsf{L}^3\Delta\tau_d$		1.197		1.198		1.193	0.702
		(0.306)		(0.306)		(0.290)	(0.302)
			Cumulative Effects				
Total	-3.640	-3.976	-3.717	-4.159	-3.765	-4.379	-4.525
	(0.454)	(0.553)	(0.453)	(0.592)	(0.506)	(0.612)	(0.724)
Pre-reform	2.426	1.329	2.349	1.156	2.464	1.102	0.865
	(0.315)	(0.454)	(0.324)	(0.512)	(0.327)	(0.504)	(0.558)
Post-reform	-6.066	-5.304	-6.067	-5.315	-6.229	-5.481	-5.390
	(0.417)	(0.531)	(0.417)	(0.530)	(0.450)	(0.582)	(0.660)
N	4,126,760	4,126,760	4,126,760	4,126,760	4,006,044	4,006,044	3,834,262
Product-date effects	1,331,154	1,331,154	1,331,154	1,331,154	1,302,736	1,302,736	1,254,537
Products	72,056	72,056	72,056	72,056	71,492	71,492	72,003

Notes: Regressions are based on data for 22 EU countries. The dependent variable is the change in the logarithm of unit sales, $\Delta \log(UNITS)$. The data is, restricted to goods sold contemporaneously in at least 2 countries. Estimates in columns (5) and (6) are based on a reduced sample, in which observations in countries with reforms announced less than a month before implementation, are removed around the respective reform date. The monthly change in the standard VAT rate is denoted by $\Delta\tau_d$. Note that $\mathsf{E}\left[\mathsf{L}^{-j}\Delta\tau_d\right] = \mathsf{L}^{-j}\Delta\tau_d$ for all reforms that were announced $n > j$ periods ahead, and $\mathsf{E}\left[\mathsf{L}^{-j}\Delta\tau_d\right] = 0$ for reforms announced $n \leq j$. All specifications include a full set of product-date, country, and country-month-specific fixed effects. The monthly unemployment rate, $Unempl$, and the number of months a products appears in the data in a specific country, $M.age$, as well as $M.age^2$ are controlled for but not reported. Standard errors in parentheses are robust in all specifications and clustered by product category-country.

explained in Section 4, groups observations not based on a product identifier, but based on product characteristics. In this approach, single-country products are included in the estimation sample because they are grouped together with other products having an identical set of characteristics.[29] Despite heterogeneity within group-date cells, as indicated by larger standard errors, the estimates remain very close to those in Table 5, demonstrating that there is no sample selectivity regarding products sold in multiple countries.[30]

Given the significant pre-reform response of unit sales, one might wonder whether the pre-reform price effects estimated in Section 5.2 reflect imperfection competition. Producers that have some market power may adjust their prices to take advantage of expected shifts in consumer demand (Carare and Danninger, 2008). If imperfect competition does explain pre-reform price adjustments, best-selling products might exhibit different pass-through and sales patterns around a tax rate change compared to products that sell fewer units. To test for such differences, we create binary indicators for market power using the within-year, within-category, and within-country ranking of products on the basis of their volume of sales. The dummy variables $R50$($R100$) equal one for all products that reach ranks between one and fifty (one and hundred) in at least one year throughout their life-cycle.[31] Once announcement information is taken into account, the results point to small differences between best-selling and other products. While no significant effects are detected for the sales response, the pre-reform price pass-through of top-selling products is found to be larger. This effect, however, is small relative to the standard error.[32]

As these results do not support effects of imperfect competition, the question arises as to what else may explain pre-reform price pass-through. The first potential explanation relates to staggered price setting (*e.g.*, Carare and Danninger, 2008). Expecting a tax rate change, some retailers may

[29]This procedure results in 686 unique characteristic sets (*e.g.*, 5 kg, 1200 spin speed front-loading washing machines *etc.*) and approximately 50,000 characteristic-set-date fixed effects.

[30]For the results see Table B.11.

[31]Together, the top 50 products in each of the eight categories of white goods in each country account for 53% of the total number of units sold, on average. On average, they are 30% cheaper and sell 6 times more units per month (average price in euro 402 (s.e. 233), average sales of 157 units (s.e. 356)) relative to products whose rank never exceeds 50 (average price 561 (s.e. 424) and average sales of 27 (s.e. 73).

[32]Table B.12 in the Appendix reports results of specifications extending equations (4.1) and (4.2) by adding the $R50$ or $R100$ dummies and their interactions with all leads, lags as well as the contemporaneous tax variable.

start adjusting prices before implementation. Other firms do this adjustment later. A second explanation is the presence of adjustment costs at the level of the retailer that are reflected in the retail price. A third explanation deals with sales, *i.e.* special offers and price discounts, which are an important driver of price movements (*e.g.*, Nakumara and Steinsson, 2008). As our price data includes all discounts, the average price charged in a country is sensitive to the frequency of discounts and sales offers. If this frequency is low (high) before a tax rate increase (decrease) and high (low) after a tax rate increase (decrease), the average price path for a model can display the pre- and post-implementation pattern indicated in our data. This interpretation is supported by Anderson *et al.* (2017), who find that discounts decline in the weeks before a wholesale price increase is set to take place, and increase afterwards so as to “mask" the price increase. A still different explanation is based on the observation that the actual price also reflects the search effort of consumers (Coibion *et al.*, 2015). If consumers put varying effort in searching for the lowest price before and after a tax rate change, pre- and post implementation effects may arise. Since our data does not allow us to distinguish between special offers and regular prices, or prices of different retailers, we cannot assess the validity of these explanations in the current setting.

Similar to Table 3, Table 5 yields large intertemporal substitution effects ranging from -3.6 to -4.5, predominantly stemming from modest non-monotonic pre-implementation increases in unit sales, followed by a precipitous decline upon implementation that continues for a couple of months. Results in these tables, however, reflect unit sales responses to tax rate changes with various motivations. Given that these are major tax policy events, some of the tax changes are related to the state of the economy. The following section explores the implications of this point further using Romer and Romer (2010)'s reform classification.

5.4 Exogenous Tax Rate Changes

Table 6 reports results only for exogenous tax rate changes as listed in Table 2. All observations for the sales and prices of products in countries with endogenous tax reforms are removed from the estimation six months before and six months after implementation, as well as in the month of the reform. Given a median implementation lag of three months, this ensures that the immediate, and any pre- and post-reform effects are removed from the estimation sample.[33]

The first three columns of Table 6 show results for prices, which are qualitatively similar to the results presented in Section 5.2. When the timing of announcements is taken into account, full price pass-through cannot be rejected at conventional levels of significance, and about a third of the price change takes place before a reform's implementation. Note, however, that with exogenous tax changes price adjustment occurs within a shorter time period: The price change starts two instead of three months in advance, and is completed in the month after implementation. Across specifications, we find that announcement dates and implementation lags matter less. The total pass-through estimated in column (1) is only slightly below that in columns (2) and (3) in contrast to the considerable differences between results with and without announcements documented in Table 4. Since many of the endogenous reforms were undertaken during recessions, the faster pass-through of exogenous tax rate changes, suggests that price adjustments may start earlier and last longer in recessions. A potential explanation may be the cyclicality of special offers and discounts, which are included in the price data (scanner prices). The recent literature indicates, however, that the frequency of such offers does not decline in downturns (Coibion *et al.*, 2015, and Anderson *et al.*, 2017).

Columns (5) to (8) report the unit sales effects for exogenous reforms. Similar to prices, we find that sales responses take place in a narrower time interval. In particular, pre-implementation effects are concentrated in the last month before enactment and, cumulatively, are much larger than in the equivalent specifications in Table 5. Interestingly, the negative statistically significant unit sales effects estimated for the second and third leads in Table 5 are no longer found with exogenous reforms. To the extent that endogenous reforms took place in recessions, the change in these coefficients supports the presence of confounding effects on unit sales for these reforms. Taken together, the cumulative results for exogenous reforms point to a stronger temporary shift in consumer demand before implementation, but substantially smaller intertemporal substitution

[33] For example, a product sold in Spain in July 2010 when a tax increase was implemented will have missing values for its Spanish sales and prices from January to December 2010.

Table 6 – Exogenous Tax Rate Changes

Dependent variable	$\Delta\log(PRICE)$				$\Delta\log(UNITS)$			
Reforms	All		n ≥ 1	n>3	All		n ≥ 1	n>3
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathsf{L}^{-3}\Delta\tau_d$	-0.011				-0.207			
	(0.051)				(0.518)			
$\mathsf{L}^{-2}\Delta\tau_d$	0.234				0.786			
	(0.061)				(0.668)			
$\mathsf{L}^{-1}\Delta\tau_d$	0.014				2.480			
	(0.043)				(0.606)			
$\mathsf{E}\left[\mathsf{L}^{-3}\Delta\tau_d\right]$		0.002	0.001	-0.009		-0.235	-0.252	-0.219
		(0.051)	(0.052)	(0.053)		(0.536)	(0.541)	(0.541)
$\mathsf{E}\left[\mathsf{L}^{-2}\Delta\tau_d\right]$		0.230	0.230	0.273		0.364	0.343	0.395
		(0.063)	(0.063)	(0.067)		(0.698)	(0.698)	(0.748)
$\mathsf{E}\left[\mathsf{L}^{-1}\Delta\tau_d\right]$		0.041	0.045	0.065		2.485	2.469	2.244
		(0.043)	(0.044)	(0.047)		(0.621)	(0.618)	(0.647)
$\Delta\tau_d$	0.170	0.170	0.166	0.130	-4.563	-4.563	-4.806	-4.684
	(0.083)	(0.083)	(0.085)	(0.092)	(0.710)	(0.710)	(0.713)	(0.778)
$\mathsf{L}\Delta\tau_d$	0.362	0.362	0.359	0.379	-1.491	-1.488	-1.079	-1.352
	(0.066)	(0.066)	(0.068)	(0.075)	(0.541)	(0.541)	(0.525)	(0.567)
$\mathsf{L}^2\Delta\tau_d$	-0.017	-0.017	-0.013	-0.008	-0.153	-0.149	-0.256	0.912
	(0.057)	(0.057)	(0.059)	(0.068)	(0.717)	(0.717)	(0.749)	(0.745)
$\mathsf{L}^3\Delta\tau_d$	0.073	0.073	0.078	0.109	1.222	1.222	1.211	0.543
	(0.056)	(0.056)	(0.059)	(0.064)	(0.485)	(0.485)	(0.467)	(0.455)
	Cumulative Effects							
Total	0.824	0.861	0.867	0.938	-1.927	-2.364	-2.369	-2.162
	(0.153)	(0.156)	(0.162)	(0.182)	(0.945)	(0.934)	(0.957)	(1.036)
Pre-reform	0.237	0.273	0.277	0.328	3.059	2.613	2.560	2.420
	(0.092)	(0.094)	(0.094)	(0.100)	(0.759)	(0.772)	(0.783)	(0.836)
Post-reform	0.587	0.588	0.590	0.610	-4.986	-4.977	-4.929	-4.581
	(0.109)	(0.110)	(0.115)	(0.128)	(0.753)	(0.753)	(0.776)	(0.859)
Pass-through $\mathsf{F}(1)$	1.32	0.80	0.68	0.12				
N	3,633,795	3,633,795	3,589,523	3,557,461	3,724,133	3,724,133	3,676,201	3,643,046
Product-date effects	1,200,757	1,200,757	1,189,120	1,181,310	1,228,615	1,228,615	1,215,792	1,207,765
Products	69,614	69,614	69,277	68,956	70,455	70,455	70,118	69,790

Notes: Regression results are based on data for 22 EU countries. The dependent variable in columns (1) to (4) is the change in the logarithm of price, $\Delta\log(PRICE)$, and in columns (5) to (9) it is the change in the logarithm of unit sales, $\Delta\log(UNITS)$. Observations up to two quarters before and after reforms classified as endogenous (see Table 2) are removed from the estimation. Estimates in columns (3) and (7) are based on a reduced sample, in which observations in countries with reforms announced less than a month before implementation, are removed around the respective reform date. The monthly change in the standard VAT rate is denoted by $\Delta\tau_d$. Note that $\mathsf{E}\left[\mathsf{L}^{-j}\Delta\tau_d\right] = \mathsf{L}^{-j}\Delta\tau_d$ for all reforms that were announced $n > j$ periods ahead, and $\mathsf{E}\left[\mathsf{L}^{-j}\Delta\tau_d\right] = 0$ for reforms announced $n \leq j$. All specifications include a full set of product-date, country and country-month specific fixed effects. The monthly unemployment rate, $Unempl$, and the number of months a product appears in the data in a specific country, $M.age$, as well as $M.age^2$ are controlled for but not reported. Standard errors in parentheses are robust in all specifications and clustered by product category-country.

effects relative to the results in Table 5.

The point estimates from the specification in column (6) show that a tax reform, which exogenously raises the tax rate by 1 percentage point triggers a temporary increase in unit sales by about 2.5 percent in the month preceding the reform. After implementation, sales drop by about 5 percent. The point estimate for the permanent effect of -2.4% is still at the upper bound of the range of comparable estimates in the existing literature and is robust to the exclusion of short-implementation-lag reforms in columns (7) and (8).

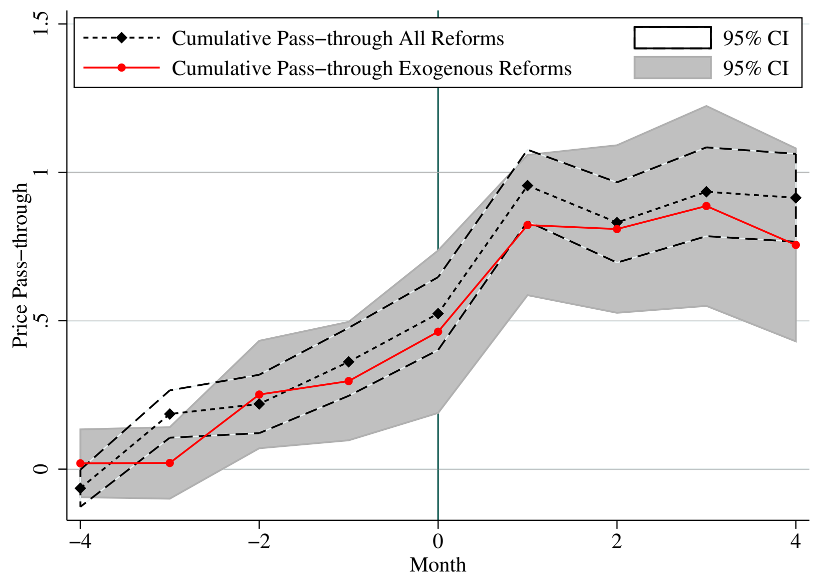
Figure 4 summarizes visually the results from sections 5.2-5.4 for a tax increase of 1 pp. Part A of the figure juxtaposes the cumulative price pass-through estimated for exogenous tax changes with that for all tax changes. Part B performs a similar comparison for the cumulative response in unit sales. The confidence bounds for estimates based on exogenous tax changes, especially for prices, are larger probably due to the loss of half of the identifying variation in tax rates (Table 2). Part A illustrates the pre-reform increase in prices and the completion of pass-through after implementation. The pass-through starts a month later for exogenous reforms. Part B clearly depicts the temporary increase in unit sales prior to implementation and the strong drop upon implementation as well as the lack of recovery.

To check for eventual recovery, we run regressions that extend the post-implementation period for unit sales to twelve months. For both the full set of tax changes and for exogenous reforms, we find similar patterns indicating no recovery after the implementation that would point to a smaller intertemporal shift (Figure C.1 in the Appendix).

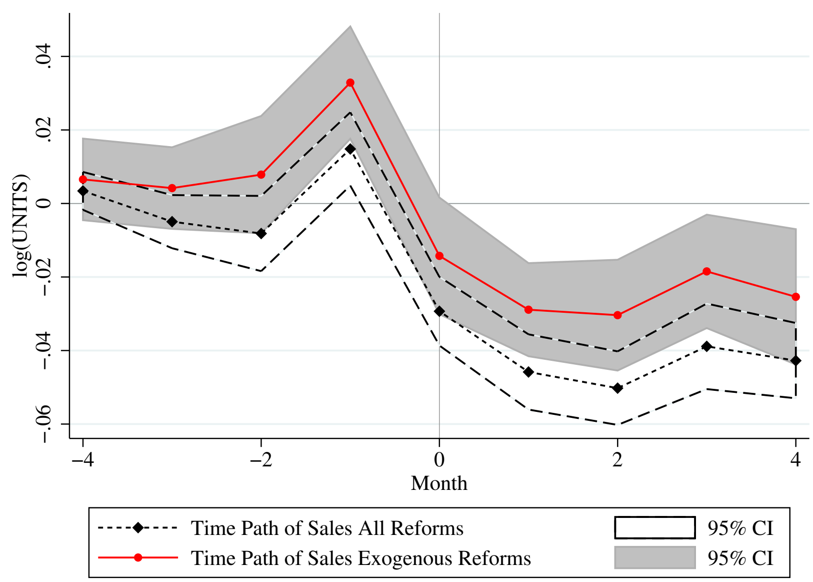
An important observation regarding Figure 4 is that estimates based on expenditure data that do not take into consideration pre- and post-reform movements in prices risk overestimating the ex-ante and underestimating the ex-post adjustment in expenditure, which would bias the estimated elasticity of intertemporal substitution towards zero. Even deflating with a narrowly-defined price index may not resolve this issue. When we use spending on durables rather than unit sales, and deflate with the appropriate COICOP 5.3 harmonized consumer price index for household appliances provided by Eurostat, we obtain a quantitative estimate of the intertemporal substitution effect,

Figure 4 – Predicted Time Paths for Unit Sales And Prices

A. Cumulative Price Pass-through



B. Unit Sales



Notes: Panel A depicts the cumulative sum of the estimated coefficients in a price regression extending the specification in Column (7) of Table 4 by including a fourth lead and lag of the percentage change of the VAT rate. Panel B depicts the cumulative sum of the estimated coefficients from a corresponding extension of the regression for changes of unit sales in Column (6) of Table 5 (both not reported). The month of the reform is denoted by zero. In both panels the shaded area captures the 95% confidence interval based on the robust estimate of the covariance matrix.

which is smaller by about 2/3 of the standard error.[34]

5.5 Temporary Effects by Product Category and Brand Quality

Although the intertemporal substitution effect of pre-announced tax rate changes applies equally to all consumer goods, including different types of durables, temporary responses could vary with the degree of intratemporal substitution, the depreciation rate and the adjustment cost. In particular, household appliances with a higher elasticity of intratemporal substitution might show a stronger expansion before a tax rate increase, as well as stronger drop upon implementation.

In fact, available data sources point at relevant differences between the product categories used in our analysis. Surveys indicate that the majority of European consumers regard refrigerators and washing machines as necessary or absolutely necessary for a decent standard of living.[35] Provided that these appliances mainly substitute household production rather than consumption of non-durables, they should exhibit smaller temporary effects around tax changes than less essential durables such as tumble driers or dishwashers. Given their high ownership rates, weaker effects might also be attributed to a smaller likelihood of first-time buyers.[36]

To explore differences between product categories empirically, we interacted the leading, lagged and contemporaneous tax rate change terms in eq. (4.1) with dummies for the product categories. Focusing on exogenous reforms only, the results reported in Panel A of Table 7 are obtained from a specification that keeps the permanent effect constant across product categories, as suggested by the theory. Specification tests (see the F-statistics reported in the table) indicate that this restriction cannot be rejected at conventional levels of significance. This supports the view that the permanent

[34]This may be partly caused by the much weaker price pass-through indicated by the price index data. In fact, when we run specification (7) in Table 4 with changes in logs of the price index on reforms in our data set, we obtain a total price pass-through of only 42%.

[35]European Commission, Special Eurobarometer 279/ Wave 67.1. 58% (48%) of the surveyed population in 27 EU countries views refrigerators (washing machines) an "absolute necessity", and 95% (89%) as a necessity. In 2018, the Indian government reduced the GST rate on refrigerators and washing machines with the justification that these durables have become household daily essentials.

[36]In 2013, the German household survey (EVS) reported ownership rates of 46.5% for tumble driers, 68.7% for dishwashers, 94.4% for washing machines.

effects reflect a common intertemporal substitution effect. The same qualitative pattern is found for all product categories. Consistent with our expectations, pre-implementation effects are strongest for tumble driers, dishwashers and freezers, whereas washing machines, cookers and refrigerators display weaker pre-reform effects. Note that these regressions yield relatively smaller permanent effects than the benchmark estimates.

Section 2 noted the role of depreciation rates and the adjustment cost in the context of consumer durables. If reliable brands retain a higher percentage of their value in secondary markets due to lower uncertainty regarding their quality, then the adjustment costs associated with a decrease in the stock of durables would be smaller for such products compared to unreliable brands (Hendel and Lizzeri, 1999). Alternatively, these goods might have lower rates of physical depreciation. As a consequence, temporary effects of tax rate changes should be stronger for top-level brands. To test this hypothesis, we use a subset of the data on refrigerators, freezers and washing machines, which contains brand name information.[37] Using average brand prices, we classify brands into three quality/reliability groups by price intervals.[38]

Panel B of Table 7 reports the results of this exercise. We apply the same estimation approach as in the case of product categories. The estimated effects for the three brand-quality groups indicate that the higher the brand quality, the larger the sales' spikes and troughs around a reform. The quantitative difference is considerable, with an expansion of unit sales of high-level brands by 5.2% before a 1 percentage point tax increase is implemented, compared to 4.5% and no response for medium- , and low-level brands, respectively. The varying strength of temporary effects is in line with the view that higher quality brands are characterized by a smaller adjustment cost or a smaller depreciation rate. While the permanent effects are substantially smaller than our benchmark estimates, they are estimated with considerable imprecision.

[37] The descriptive statistics for the subset are similar to those for the total sample, *cf.* Table B.6 in the Appendix.

[38] The exact procedure and brand composition of the three groups are described in the note to Table B.6. The Table shows that, relative to top-level brands, low-level brands are, on average, over 400 Euro cheaper, and sell 24 units more per model per month, while mid-level brands are 280 cheaper, but their sales are comparable to high-end brands.

Table 7 – Pre-Implementation Response Differences

	Exogenous (1)	Exogenous & n≥ 1 (2)	Exogenous & n>3 (3)
	Panel A: Product Categories		
$\sum_{j=1}^{3} \mathsf{E}\left[\mathsf{L}^{-j}\Delta\tau_d\right]^{\text{RG}}$	3.057	2.925	2.847
	(1.042)	(1.089)	(1.200)
$\sum_{j=1}^{3} \mathsf{E}\left[\mathsf{L}^{-j}\Delta\tau_d\right]^{\text{WM}}$	1.701	1.733	1.603
	(1.193)	(1.229)	(1.353)
$\sum_{j=1}^{3} \mathsf{E}\left[\mathsf{L}^{-j}\Delta\tau_d\right]^{\text{CO}}$	2.699	2.611	2.761
	(1.522)	(1.558)	(1.661)
$\sum_{j=1}^{3} \mathsf{E}\left[\mathsf{L}^{-j}\Delta\tau_d\right]^{\text{FRZ}}$	6.111	6.490	5.922
	(1.455)	(1.444)	(1.313)
$\sum_{j=1}^{3} \mathsf{E}\left[\mathsf{L}^{-j}\Delta\tau_d\right]^{\text{DW}}$	4.561	4.396	4.530
	(1.229)	(1.264)	(1.457)
$\sum_{j=1}^{3} \mathsf{E}\left[\mathsf{L}^{-j}\Delta\tau_d\right]^{\text{TD}}$	7.904	7.453	6.018
	(3.821)	(3.941)	(4.739)
Permanent effect	-1.639	-1.784	-1.643
	(0.953)	(0.971)	(1.044)
F-test: Different permanent effects	1.45	1.50	0.99
P-value	0.21	0.19	0.43
N	3,046,467	3,008,886	2,981,514
Product-date effects	996,031	986,525	980,035
Products	57,807	57,587	57,352
	Panel B: Brand Quality Groups		
$\sum_{j=1}^{3} \mathsf{E}\left[\mathsf{L}^{-j}\Delta\tau_d\right]^{\text{Top}}$	5.362	5.192	5.113
	(1.169)	(1.159)	(1.194)
$\sum_{j=1}^{3} \mathsf{E}\left[\mathsf{L}^{-j}\Delta\tau_d\right]^{\text{Mid}}$	4.549	4.573	4.130
	(1.803)	(1.847)	(1.989)
$\sum_{j=1}^{3} \mathsf{E}\left[\mathsf{L}^{-j}\Delta\tau_d\right]^{\text{Low}}$	-0.152	0.260	0.217
	(1.756)	(1.775)	(1.886)
Permanent effect	-0.674	-0.821	-0.898
	(1.280)	(1.269)	(1.303)
F-test: Different permanent effects	0.73	0.44	0.04
P-value	0.49	0.64	0.96
N	1,356,247	1,342,146	1,329,674
Product-date effects	370,814	368,771	367,483
Products	16,463	16,429	16,388

Notes: Regression results are based on data for 22 EU countries. The dependent variable in columns (1) to (3) is the change in the logarithm of unit sales, $\Delta\log(UNITS)$. Panel A reports results from regressions where tax effects are interacted with the specific product category, while the sum of all tax effects is restricted to be common across groups. Panel B reports results from regressions where tax effects are interacted with the specific brand quality group, while the sum of all tax effects is restricted to be common across groups. Both specifications also allow seasonal patterns to differ between product categories/brand quality groups. The F-statistics refer to tests of the equality of permanent effects based on regressions allowing also the permanent effect to differ between product categories/brand quality groups.

6 Conclusions

The effectiveness of a pre-announced consumption tax change intended as a measure of unconventional fiscal policy depends crucially on two mutually interconnected responses, namely consumption shifting at the intertemporal margin and the magnitude of the tax change pass-through into prices. This paper sheds light on both responses in the context of major domestic appliances sold in the EU's Common Market. Based on an identification strategy that exploits the contemporaneous sales of identical products in multiple countries, we conduct a nuanced analysis of multiple consumption tax rate changes, taking into account announcement dates and implementation lags, and differentiating between exogenous and endogenous tax changes.

The results show that tax changes are fully reflected in prices, so that a basic pre-condition for an effective fiscal policy is met. The pass-through, however, is not confined to the point of implementation. Instead, prices adjust predominantly a few months before and after the implementation of the new tax rate. We argue that the non-instantaneous timing of the pass-through cannot be attributed to imperfect competition and leave more concrete evidence on specific explanations to future research. The results for unit sales indicate that, in response to a 1 pp. increase in the consumption tax rate, unit sales rise temporarily by 2.5% in the month before implementation, drop sharply on implementation, and stay permanently 2% below their original level. The estimated time path points to a large intertemporal substitution effect, indicating an elasticity of intertemporal substitution of about 2.

Our analysis highlights important caveats to estimating consumption responses to tax rate changes. First, the above estimates are based on unit sales. When we intentionally transform our data into expenditure data, which we then deflate by a harmonized price index for household appliances, the estimated intertemporal substitution effect is found to be considerably lower. Similarly, when using changes in the price index as a dependent variable, price pass-through is far from complete. The separation of prices from quantities, or prices from expenditure is, therefore, an important step towards a more robust evaluation of these crucial fiscal policy responses.

Second, distinguishing between endogenous and exogenous tax changes is shown to be essential to isolating the effect of an upcoming tax rate change on consumption from the confounding influence of macroeconomic developments. As endogenous tax changes take place predominantly during recessions, consumption responses would likely reflect the depressed state of the economy. The latter is confirmed in specifications using only exogenous reforms, which find stronger pre-implementation effects.

Our results lead to a positive assessment of the effectiveness of pre-announced tax rate changes as a tool of fiscal policy. While the temporary stimulus on sales of durables before implementation is modest and concentrated in a single month, our estimates point to a larger elasticity of intertemporal substitution than found in previous studies. Based on our results, therefore, pre-announced consumption tax changes can exert stimulating effects on total consumption. However, policymakers should be aware of the sharp and lasting drop in consumption after the tax change, leading us to conclude that a sound fiscal policy using consumption taxes needs to be based on a careful intertemporal policy design.

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Appendix

Unit Sales and Price Effects of Pre-announced Consumption Tax Reforms: Micro-level Evidence from European VAT

September 1, 2019

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Contents

A Data Analysis

A.1 Data Production

Gesellschaft für Konsumforschung (GfK) Retail and Technology GmbH generates the data in the following way: First, distribution channels are defined, which are relevant for a respective product group. Examples of distribution channels are hypermarkets, technical superstores, department stores, *etc.* An address database is established for all outlets in a given country belonging to a certain distribution channel with the goal of determining the universe of retailers. This is achieved through census data and special questionnaires to dealers/retailers. Once the universe is known in its structure, the sample is drawn through disproportional quota sampling, taking into account three key factors – region, distribution channel, and turnover class. The aim is to make sure that the data provides an equally good representation of developments for each product. GfK collects price and quantity data retailer by retailer. Incoming data from different sources referring to the same product is translated into one single definite GfK product code. Once checked, the basic data is extrapolated for each distribution channel. GfK's data collection, sampling and extrapolation methodology are described in detail in Fischer (2012), who uses similar data for washing machines from 1995-2005, at a four-monthly or bi-monthly frequency, to study price convergence in the countries of the European Monetary Union (EMU).

A.2 Data Transformation

Transformations applied to all estimation samples:

The complete untransformed data contains a total of 20,666,643 observations, some of which are removed. In particular, observations without an identifier (id) are dropped (10,242 obs.), observations for products for which all units/price variables are missing across all years, and observations within a product for which all units and prices in a given year are reported as zero (4,932 obs). A small number of units sold (13,512 obs.) and prices (1,336 obs.) have negative values, which are replaced with missing observations. The negative values likely arise due to returned items. Out of 20,666,643 observations for units sold, 8,341,832 are missing values, and 1,370,799 are zeros. For prices, 8,901,213 data points are missing and 861,537 are zeros. Usually zero/missing units sold are coupled with a zero/missing price.

Monthly percentage changes in prices calculated within product-country groups are restricted to no more than 200% increases and no less that 50% decreases by replacing prices with missing observations when the percentage change exceeds the specified range. This affects 272,175 observations (decreases), of which the vast majority, 255,084, are due to a percentage change exactly equal to -100%, which occurs when a positive price is followed by a price of zero. 17,091 changes are due to prices falling by more than 50% from one month to the next, while 3,808 prices are replaced with missing values because the increase is larger than 200%. This restriction applies to all descriptive statistics presented in Panels B and C of Table 1. All results are robust to an alternative transformation, which drops zero prices without imposing any other restriction on the percentage change. In this case, the mean of $\Delta \log(PRICE)$ is -0.005 (0.142) with a min. -11.15 and a max of 33.57. Further, results remain robust if zero prices are left in the data as they are. Both sets of results are available upon request.

Due to membership into the EMU, in all estimation samples, data for Slovakia is dropped before January 1st, 2009 (175,848 obs), for Slovenia – before January 1st, 2007 (65,520 obs.), and for Estonia all observations after December 2010 are excluded (94,641 obs.). Panel A of Table 1 reports descriptive statistics based on all available data for Slovenia, Slovakia, and Estonia.

For the purpose of providing descriptive statistics, prices in Table 1 are shown in Euro, calculated using monthly exchange rates sourced by Eurostat, but all log-changes used in the estimation and summarized in Table 1 are based on prices in national currencies.

Outliers in $\Delta \log(UNITS)$ are present as clearly shown by the min-max range of this variable in Panels B and C in Table 1. Such outliers arise as a result of two characteristics of the data. First, 543,832 units sold lie in an interval (0,1), with some values as small as 0.0000001, which typically occurs in the last year a model is in the panel. The log-transformation of such small values results in substantial log-changes in units. Our results are robust to the replacement of all such values with zero (results available upon request). In this case, the mean of $\Delta \log(UNITS)$ becomes -0.016 (0.878) with a minimum of -7.87 and a maximum of 8.89. The maximum value of 8.89 is for a product entering the German market with units sold of 1 in its first month and 7,276 in the second month. The minimum value is generated by a product that exits the market with sales of 1 unit in its last month, but 2,626 units in the preceding month. Apart from the (0,1) values, therefore, outliers in $\Delta \log(UNITS)$ arise naturally from the fluctuations in sales at the beginning and the end of products' life-cycles.

Transformations applied to estimation sample of Panel B of Table 1

In this estimation sample the data is restricted to models traded in at least two countries at the same time. This results in the loss of 9,644,145 observations. Refer to Table B.5 for some summary statistics of the full and the reduced sample. The restriction removes two thirds of all models in the data, but the remaining 29,683 products on average account for 53% of all units sold and generate 58% of the sales value within a year. Panel B of Table 1 provides summary statistics only for the observations that are actually used in the estimations in Tables 3 and 4. The remaining variables in Panel B are summarized based on the union of sales and price estimation samples.

Transformations applied to estimation sample of Panel C of Table 1

The estimates in Table B.11 are based on the estimation sample described in Panel C of Table 1. This is the sample that incorporates models traded in only one country in the estimation by collecting, within a product category, all models with an identical set of characteristics into one group (Table B.4). For example, all built-in, 2-door, freezer-top refrigerators with a no-frost system belong into one group. A number of models have a single or multiple unknown/non-available characteristics, which necessitated dropping these models from the data. In total, 39,481 models (2,207,532 obs.) were removed. 92% of the lost observations stem from two product categories – hoods and cooktops, which have numerous models with missing information on the shape of chimney and heating type characteristics (see Table B.4). We further had to ensure that models in the resulting products groups-date cells are traded in at least two countries, which resulted in the loss of 26,217 additional observations. Panel C of Table 1 provides summary statistics only for the observations that are actually used in the estimation in Table B.11.

Endogenous reforms and reforms announced less than a month before implementation

Seven reforms were announced less than one month before their implementation (see Table 2 and Figure 3). To identify observations affected by these reforms, we generated a variable $early$, which has a value of unity for all observations in countries undergoing such reforms six months before and six months after the respective implementation dates. All specifications excluding relevant models' observations around the seven reforms are estimated on the condition that $early = 0$. Endogenous reforms are identified in a similar fashion. We generated a variable $endog$, which is set to unity six months before and six months after the implementation dates of all endogenous reforms listed in Table 2. Specifications using exogenous reforms are run subject to $endog = 0$.

TABLE B.1 – LITERATURE ON SPENDING RESPONSES TO CONSUMPTION TAX RATE CHANGES

Paper	Policy Varation	Data	Identification	Finding
Crossley *et al.*(2014)	UK: 2008 VAT decrease by 2.5pp. temporary	Aggregate expenditure and retail sales per GDP	Diff-in-diff relative to 1) non-VAT goods 2) other OECD countries	Retail sales increase by 1%
Agarwal *et al.* (2016)	US: 2003 Nine sales tax holidays	Household daily transactions (Consumer Expenditure Survey) and credit card data	Diff-in-diff relative to household consumption on same date in states without sales tax holidays	Increased spending on apparel (41% and 56%) during holidays, without offsetting declines before and after
Cashin & Unayama (2016)	Japan: 1997 VAT increase	Household micro-level data on spending on durables, storables, non-storable non-durables; deflated; household characteristics (JFIES Survey)	Time-series	Intertemporal consumption shift by 0.21% before tax increase by 1pp., strong transitory effects on durables
Baker *et al.* (2017)	US: 2008-2014 ZIP code level sales tax rates	Nielsen Consumer Panel: household- and store-specific purchases	Diff-in-diff relative to households in states without sales tax change	1pp. increase exerts transitory effect by 1.19% and intertemporal consumption shift of 0.3%
Cashin (2018)	Japan: 1997 VAT increase	JFIES Survey as in Cashin & Unayama (2016); deflated	Structural model estimation	Intertemporal consumption shift by 0.13% before increase by 1pp., transitory effects.
D'Acunto *et al.* (2019)	Germany: 2007 VAT increase by 3pp.	Micro-level household data on inflation expectations, willingness to pay for consumption goods and household-specific characteristics (GfK MAXX Survey)	Diff-in-diff relative to households in other EU countries (not Germany)	10.3% higher durable consumption before increase

Notes: The table draws from a specific selection of papers that deal with VAT or retail sales taxes. We exclude studies that consider effects of targeted subsidies that aim to stimulate consumer spending and promote fuel efficiency (*e.g.*, Mian and Sufi, 2012, Green, Melzer, Parker and Rojas, 2016, Li, Linn and Spiller, 2013, and Hoekstra, Puller and West, 2017). Moreover, the table does not aim to provide a general overview of findings in the listed papers, but focuses on selected empirical findings.

TABLE B.2 – LITERATURE ON CONSUMPTION TAX PASS-THROUGH INTO PRICES

Paper	Policy Variation	Data	Identification	Findings
Poterba (1996)	US: 1925-1939 21 state sales tax changes; US: 1947-1977: 33 state and local sales tax changes	City-specific CPI index for clothing and personal care items	Diff-in-diff relative to national price changes for clothing and personal care	1925-1939: Incomplete forward shifting 1947-1977: Full-shifting
Besley & Rosen (1999)	US: 1982-1990 State and local taxes	City-specific CPI data disaggregated in 12 commodities	Intertemporal deviations from city-specific means	Over-shifting for 50% of commodities
Carbonnier (2007)	France: 1987 VAT decrease for cars; France: 1999 VAT decrease for household repair services	CPI disaggregated according to COICOP group	Double diff-in-diff relative to overall and energy-/rent-price indices	Under-shifting
Carare & Danninger (2008)	Germany: 2007 VAT increase	Harmonized CPI; disaggregated	Diff-in-diff relative to non-VAT-liable CPI items	Under-shifting; 24% pre-implementation pass-through
Viren (2009)	15 EU countries: 1970-2004 VAT increases	Harmonized CPI	Panel Regression with fix.eff.	Under-shifting
Crossley *et al.*(2014)	UK: 2008 temporary VAT decrease	Harmonized CPI	Diff-in-diff relative to 1) non-VAT goods in the UK 2) prices in other OECD countries	Full-shifting, but early reversal
Benedek *et al.* (2015)	17 EU countries: 1999-2013; 65 changes incl. reduced rates	Harmonized CPI; disaggregated into 67 COICOP groups	Diff-in-diff relative to identical consumption categories in countries without tax changes	Full-shifting; 35% pre-reform pass-through for durables.

Notes: *Notes*: The table shows a selection of papers that deal with VAT or retail sales taxes. We exclude studies that consider the pass-through of reduced VAT rates in the context of household services (*e.g.*, Kosonen, 2015, Benzarti, Carloni, Harju, Kosonen, 2017). The table does not aim to survey the listed papers, but focuses on selected empirical findings.

Table B.3 – Data Coverage

Country	Coverage
AT, BE, CZ, DE, ES, FR, IT, NL, PL, PT, SE, UK	Jan. 2004 - Sept. 2013 for all categories of white goods.
DK	Jan. 2004 - Sept. 2013 WM, TD, CO, RG; Jan. 2007 - Sept. 2013 FRZ; Jan. 2008 - Sept. 2013 HB; HD are not covered.
EE, LV, LT	Jan. 2006 - Sept. 2013 for WM, CO, RG; Jan. 2008 - Sept. 2013 for HB, DW; HD,TD, FRZ are not covered.
GR	Jan. 2005 - Sept. 2013 for all product categories except TD, which is covered from Jan. 2007 - Sept. 2013.
FI	Jan. 2005 - Sept. 2013 for all product categories, except HD, which is not covered.
HU	Jan. 2004 - Sept. 2013 for all product categories except HD, which is covered from Oct. 2006 - Sept. 2013.
RO	Jan. 2009 - Sept. 2013 for all product categories except HD, which is covered from Jan. 2012 - Sept. 2013.
SI	Jan. 2005 - Sept. 2013 for all product categories except HD, which is covered from Jan. 2009 - Sept. 2013.
SK	Jan. 2006 - Sept. 2013 for all product categories.

Notes: CO: Cooker; DW: Dishwasher; FRZ: Freezer; HB: Hob/Cooktop; HD: Hood; RG: Refrigerator; TD: Tumble dryer; WM: Washing machine. AT: Austria (5.52); BE: Belgium (5.40); CZ: the Czech Republic (4.56); DE: Germany (10.01); DK: Denmark (2.88); EE: Estonia (1.27); ES: Spain (7.62); FI: Finland (2.67); FR: France (9.47); GR: Greece (2.99); HU: Hungary (3.24); IT: Italy (8.25); LV: Latvia (0.96); LT: Lithuania (1.73); NL: the Netherlands (5.48); PL: Poland (4.87); PT: Portugal (5.02); RO: Romania (1.10); SE: Sweden (3.84); SI: Slovenia (1.90); SK: Slovakia (2.80); UK: United Kingdom (8.43). Numbers in parentheses after country labels are the number of observations associated with the respective country as a percent from total observations in the data set.

TABLE B.4 – PRODUCT CHARACTERISTICS

Product Category	Characteristics
Cookers	Construction (built-in, under-, freestanding); type (cooker, oven); fuel (electric, gas, mixed).
Coolers/Refrigerators	No-frost system (yes/no); construction (built-in, under-, freestanding); type (1 door (dr) 81-90 cm, 1 dr.>90 cm, 1 dr. up to 80 cm, 2 drs. freezer bottom, 2 drs. freezer top, 3+ drs., side-by-side); brand*.
Dishwashers	Construction (built-in, under, freestanding); size (compact, full size, slimline, table top); integration (fully, partly, no).
Freezers	Construction (built-in, under, freestanding); type (upright, chest, box); height in cm (42-213 cm); brand*.
Hobs/Cooktops	Fuel (electric, gas, mixed); surface (ceramic/glass, sealed, gas on glass, mixed sealed+ceramic); heating type (halogen, induction, radiant).
Hoods	Hood type (canopy/cartridge, ceiling, chimney, integrated, standard, table/hob extra, telescopic); chimney (corner, island, wall, no chimney/deco); shape chimney (box, decorative, head-free, pyramid/trapeze, not applicable).
Tumble Driers	Type (condenser, ventilation); control type (electronic, timer); loading capacity in kg (1-10 kg).
Washing Machines	Type (front- or top-loading, wash-dry, other); spin speed (400-3100); loading capacity in kg (1-17 kg); brand*.

Notes: *Brand information is available for a subset of products in three categories, namely for 48% of refrigerators, 46% of freezers, and 44% of washing machines in the estimation sample of Panel B of Table 1. The characteristic sets used in the group-date fixed effects in Table B.11 are all possible combinations of the characteristics above per product category. In total, in the estimation sample of Panel C of Table 1, there are 686 groups of products with an identical set of characteristics. Refer to Section A.2 for details on how the groups are constructed.

Table B.5 – Full Sample: Descriptive Statistics By Product Category

	Mean	Std. Dev.	Min	Max
	Average № Products per Year			
Total, of which:	109,848	3,890	102,879	117,844
Cookers	21,582	503	20,477	22,134
Fridges	24,102	1,359	22,402	26,712
Dishwashers	11,185	1,318	8,745	13,305
Freezers	6,265	416	5,722	7,117
Cook tops	14,006	783	12,572	14,875
Hoods	14,918	1,733	10,810	17,148
Tumble dryers	3,195	196	2,966	3,531
Washing machines	14,877	708	13,855	16,019
Sold in at least 2 countries	29,683	6,466	10,095	36,540
	Average № of Units Sold per Year (Thousands)			
Total, of which:	62,408	5,079	47,083	65,712
Cookers	8,623	729	6,252	9,207
Fridges	14,069	1,101	10,708	15,020
Dishwashers	6,784	686	5,401	7,432
Freezers	3,836	381	2,631	4,113
Cook tops	5,920	464	4,691	6,342
Hoods	4,949	433	3,714	5,371
Tumble dryers	3,523	415	2,268	3,942
Washing machines	14,729	1,205	11,416	15,655
Sold in at least 2 countries	33,159	5,906	13,829	38,692
	Average Value of Sales per Year (Millions Euro)			
Total, of which:	25,987	2,193	19,447	27,883
Cookers	3,908	386	2,740	4,334
Fridges	6,313	538	4,765	6,859
Dishwashers	3,413	302	2,604	3,638
Freezers	1,349	118	976	1,440
Cook tops	2,178	189	1,720	2,337
Hoods	1,245	108	974	1,337
Tumble dryers	1,427	151	1,032	1,598
Washing machines	6,171	498	4,635	6,565
Sold in at least 2 countries	15,187	2,558	6,743	17,389
	Product Age			
Full sample:	30.5	23.2	1	117
Cookers	30.8	23.4	1	117
Fridges	28.9	21.8	1	117
Dishwashers	27.7	20.7	1	117
Freezers	28.6	22.0	1	117
Cook tops	34.5	25.5	1	117
Hoods	36.9	27.6	1	117
Tumble dryers	29.5	22.0	1	117
Washing machines	27.1	20.3	1	117
Sold in at least 2 countries	31.2	21.8	1	117

Notes: The descriptive statistics are based on the primary data in Panel A of Table 1. Product age shows the average number of months from the earliest date a product enters the market in any country and the latest date it exits the market in any country in the data.

Table B.6 – Descriptive Statistics By Brand Quality

	Mean	Std. Dev.	Min	Max	
	Sub-sample with Brand Information				
№ Units Sold	67.10	194.68	0	19,062	1,481,867
Price (Euro)	572.74	392.36	0	11,392	1,458,501
Market Age (months)	25.38	16.43	2	117	1,481,867
Rank	546	567	1	5,364	1,481,867
	Top-level Brands				
№ Units Sold	60.53	177.56	0	8,815	685,218
Price (Euro)	754.38	468.91	0	11,392	672,332
Market Age (months)	25.51	16.53	2	117	685,218
Rank	620	600	1	5,364	685,218
	Medium-level Brands				
№ Units Sold	65.10	190.65	0	19,062	475,306
Price (Euro)	471.56	238.80	0	4,355	468,638
Market Age (months)	24.44	15.61	2	117	475,306
Rank	509	542	1	5,364	475,306
	Low-level Brands				
№ Units Sold	84.08	231.11	0	7,927	321,343
Price (Euro)	337.49	130.68	0	3,999	317,531
Market Age (months)	26.46	17.28	2	117	321,343
Rank	445	506	1	5,064	321,343

Notes: The table refers to the sub-sample of refrigerators, freezers and washing machines with brand information. Assignment into reliability/quality groups is based on mean brand prices, so that across the full product range of a brand over time, the mean price of top level brands lies within an interval $[500, +\infty)$, and for medium-level brands–in the interval $(500, 390]$. Given this selection, the list of top brands includes 32 brands. 24 brands are classified as medium-level. The list of lower-level brands is composed of 76 brands.

TABLE B.7 – NUMBER OF IDENTIFYING REFORMS BY ORDER OF LEADS

Lead	№ Identifying countries	№ Identifying reforms
$\Delta\tau_d$	17	33
$\mathsf{E}\left[\mathsf{L}^{-1}\Delta\tau_d\right]$	16	29
$\mathsf{E}\left[\mathsf{L}^{-2}\Delta\tau_d\right]$	15	26
$\mathsf{E}\left[\mathsf{L}^{-3}\Delta\tau_d\right]$	12	20
$\mathsf{E}\left[\mathsf{L}^{-4}\Delta\tau_d\right]$	11	17
$\mathsf{E}\left[\mathsf{L}^{-5}\Delta\tau_d\right]$	9	12
$\mathsf{E}\left[\mathsf{L}^{-6}\Delta\tau_d\right]$	7	10
$\mathsf{E}\left[\mathsf{L}^{-7}\Delta\tau_d\right]$	6	8
$\mathsf{E}\left[\mathsf{L}^{-8}\Delta\tau_d\right]$	6	8
$\mathsf{E}\left[\mathsf{L}^{-9}\Delta\tau_d\right]$	6	8
$\mathsf{E}\left[\mathsf{L}^{-10}\Delta\tau_d\right]$	5	6
$\mathsf{E}\left[\mathsf{L}^{-11}\Delta\tau_d\right]$	3	3
$\mathsf{E}\left[\mathsf{L}^{-12}\Delta\tau_d\right]$	2	2
$\mathsf{E}\left[\mathsf{L}^{-13}\Delta\tau_d\right]$	2	2
$\mathsf{E}\left[\mathsf{L}^{-14}\Delta\tau_d\right]$	2	2

Notes: The table shows the varying number of VAT reforms and countries captured by higher-order leads of the change in the tax rate, $\Delta\tau_d$. Due to data limitations for Latvia such as market size and narrower time and category coverage, we take the earliest announcement in the data to be that of the German VAT increase in 2007, which was announced 14 months prior to implementation. For this reason, no more than 14 leads are considered.

Table B.8 – Basic Estimates Of Unit Sales Effects: Alternative S.E. Clustering

	(1)	(2)	(3)	(4)
$\mathsf{F}\Delta\tau_d$	2.615	2.444	2.426	2.421
Cluster Product-date s.e.	(1.136) [0.021]	(0.718) [0.001]	(0.730) [0.001]	(0.725) [0.001]
Cluster Country s.e.	(0.608) [0.000]	(0.446) [0.000]	(0.453) [0.000]	(0.516) [0.000]
Cluster Country Wild Bootstrap	- [0.007]	- [0.004]	- [0.007]	- [0.011]
Cluster Product categ.-Country s.e.	(0.366) [0.000]	(0.314) [0.000]	(0.315) [0.000]	(0.340) [0.000]
$\Delta\tau_d$	-3.817	-4.338	-4.350	-4.412
Cluster Product-date s.e.	(1.699) [0.025]	(0.844) [0.000]	(0.839) [0.000]	(0.830) [0.000]
Cluster Country s.e.	(1.377) [0.011]	(0.711) [0.000]	(0.707) [0.000]	(0.697) [0.000]
Cluster Country Wild Bootstrap	- [0.058]	- [0.001]	- [0.001]	- [0.001]
Cluster Product categ.-Country s.e.	(0.648) [0.000]	(0.415) [0.000]	(0.415) [0.000]	(0.436) [0.000]
$\mathsf{L}\Delta\tau_d$	-2.146	-1.700	-1.717	-1.754
Cluster Product-date s.e.	(0.910) [0.018]	(0.498) [0.001]	(0.498) [0.001]	(0.522) [0.001]
Cluster Country s.e.	(0.836) [0.018]	(0.423) [0.001]	(0.436) [0.001]	(0.471) [0.001]
Cluster Country Wild Bootstrap	- [0.084]	- [0.007]	- [0.012]	- [0.011]
Cluster Product categ.-Country s.e.	(0.433) [0.000]	(0.289) [0.000]	(0.291) [0.000]	(0.313) [0.000]
Cumulative Effect	-3.349	-3.594	-3.640	-3.744
Cluster Product-date s.e.	(2.257) [0.138]	(1.211) [0.003]	(1.215) [0.003]	(1.275) [0.003]
Cluster Country s.e.	(0.825) [0.001]	(0.417) [0.000]	(0.425) [0.000]	(0.587) [0.000]
Cluster Country Wild Bootstrap	- [0.003]	- [0.003]	- [0.000]	- [0.000]
Cluster Product categ.-Country s.e.	(0.544) [0.000]	(0.453) [0.000]	(0.454) [0.000]	(0.571) [0.000]
Month-country effects	No	Yes	Yes	Yes
Year-country effects	No	No	No	Yes
N	4,126,760	4,126,760	4,126,760	4,126,760
Product-date effects	1,331,154	1,331,154	1,331,154	1,331,154
Products	72,056	72,056	72,056	72,056

Notes: The table repeats the basic estimation of unit sales effects in Table 3, but clusters standard errors at product-date, α_{id}, or country level. Standard errors are in parentheses, and p-values in squared brackets. We report two sets of p-values when clustering over country: From a standard fixed-effects estimation with 22 country clusters, and from the wild bootstrap post-estimation procedure developed in Roodman *et.al.* (2018) using 999 bootstrap replications. For convenience, the table also shows standard errors at our default level of clustering over 165 product category-country clusters. Note that, since not all 8 product categories are covered in all 22 countries (Table B.3), the number of product category-country groups is smaller than 176.

Table B.9 – Price Effects: Increasing Number of Countries in Product-Date Cells

	(1) $k_i \geq 3$	(2) $k_i \geq 4$	(3) $k_i \geq 5$	(4) $k_i \geq 6$	(5) $k_i \geq 7$	(6) $k_i \geq 8$
$\mathsf{E}\left[\mathsf{L}^{-3}\Delta\tau_d\right]$	0.241	0.234	0.240	0.234	0.237	0.250
	(0.039)	(0.044)	(0.048)	(0.050)	(0.060)	(0.067)
$\mathsf{E}\left[\mathsf{L}^{-2}\Delta\tau_d\right]$	0.046	0.045	0.046	0.059	0.069	0.080
	(0.029)	(0.031)	(0.033)	(0.036)	(0.041)	(0.047)
$\mathsf{E}\left[\mathsf{L}^{-1}\Delta\tau_d\right]$	0.130	0.113	0.111	0.085	0.082	0.089
	(0.034)	(0.036)	(0.040)	(0.043)	(0.047)	(0.053)
$\Delta\tau_d$	0.165	0.184	0.197	0.222	0.263	0.260
	(0.038)	(0.039)	(0.041)	(0.041)	(0.042)	(0.047)
$\mathsf{L}^1\Delta\tau_d$	0.438	0.443	0.445	0.421	0.412	0.390
	(0.034)	(0.034)	(0.037)	(0.039)	(0.043)	(0.043)
$\mathsf{L}^2\Delta\tau_d$	-0.120	-0.111	-0.088	-0.079	-0.050	-0.039
	(0.054)	(0.057)	(0.060)	(0.058)	(0.062)	(0.065)
$\mathsf{L}^3\Delta\tau_d$	0.100	0.115	0.106	0.104	0.083	0.089
	(0.033)	(0.034)	(0.037)	(0.039)	(0.045)	(0.043)
	Cumulative Effects					
Total pass-through	1.000	1.022	1.057	1.045	1.096	1.119
	(0.072)	(0.073)	(0.080)	(0.076)	(0.086)	(0.100)
Pre-reform	0.416	0.392	0.398	0.378	0.387	0.420
	(0.058)	(0.059)	(0.065)	(0.066)	(0.071)	(0.079)
Post-reform	0.584	0.631	0.660	0.667	0.708	0.700
	(0.054)	(0.057)	(0.061)	(0.059)	(0.068)	(0.077)
N	3,190,634	2,562,872	2,077,872	1,671,169	1,337,784	1,057,569
Product-date effects	912,854	648,451	470,798	341,567	248,367	179,899
Products	42,066	26,809	18,366	12,943	9,274	6,690

Notes: Regression results in columns (1) to (6) are based on data for 22 EU countries. The dependent variable is the change in the logarithm of price, $\Delta\log(PRICE)$. Reforms' announcement information is fully incorporated. Observations in countries with reforms announced less than a month before implementation are removed around the respective reform date. The sample is gradually restricted to products sold contemporaneously in at least 3 up to at least 8 countries, where k_i is number of countries in which model i is sold. The monthly change in the standard VAT rate is denoted by $\Delta\tau_d$. Note that $\mathsf{E}\left[\mathsf{L}^{-j}\Delta\tau_d\right] = \mathsf{L}^{-j}\Delta\tau_d$ for all reforms that were announced $n > j$ periods ahead, and $\mathsf{E}\left[\mathsf{L}^{-j}\Delta\tau_d\right] = 0$ for reforms announced $n \leq j$. All specifications include a full set of product-date (id), country and country-month specific fixed effects. The monthly unemployment rate, $Unempl$, and the number of months a product appears in the data in a specific country, $M.age$, as well as $M.age^2$ are controlled for but not reported. Standard errors in parentheses are robust in all specifications and clustered by product category-country.

Table B.10 – Unit Sales Effects: Increasing Number of Countries in Product-Date Cells

	(1) $k_i \geq 3$	(2) $k_i \geq 4$	(3) $k_i \geq 5$	(4) $k_i \geq 6$	(5) $k_i \geq 7$	(6) $k_i \geq 8$
$\mathsf{E}\left[\mathsf{L}^{-3}\Delta\tau_d\right]$	-0.922	-1.112	-1.145	-1.106	-1.233	-0.957
	(0.279)	(0.303)	(0.331)	(0.364)	(0.406)	(0.456)
$\mathsf{E}\left[\mathsf{L}^{-2}\Delta\tau_d\right]$	-0.689	-0.644	-0.768	-0.775	-1.008	-1.034
	(0.244)	(0.263)	(0.286)	(0.313)	(0.347)	(0.388)
$\mathsf{E}\left[\mathsf{L}^{-1}\Delta\tau_d\right]$	2.794	2.924	2.967	3.081	3.382	3.508
	(0.240)	(0.259)	(0.281)	(0.308)	(0.341)	(0.381)
$\Delta\tau_d$	-4.635	-4.799	-4.789	-4.723	-4.674	-4.394
	(0.238)	(0.255)	(0.275)	(0.299)	(0.330)	(0.367)
$\mathsf{L}^1\Delta\tau_d$	-1.655	-1.924	-2.143	-2.306	-2.287	-2.216
	(0.239)	(0.257)	(0.278)	(0.303)	(0.335)	(0.372)
$\mathsf{L}^2\Delta\tau_d$	-0.419	-0.365	-0.284	-0.169	-0.383	-0.193
	(0.240)	(0.258)	(0.278)	(0.303)	(0.334)	(0.371)
$\mathsf{L}^3\Delta\tau_d$	1.172	0.989	0.850	0.917	0.842	0.712
	(0.241)	(0.259)	(0.280)	(0.306)	(0.338)	(0.376)
	Cumulative Effects					
Total	-4.353	-4.931	-5.311	-5.080	-5.362	-4.573
	(0.659)	(0.709)	(0.769)	(0.842)	(0.933)	(1.041)
Pre-reform	1.183	1.168	1.054	1.200	1.141	1.518
	(0.445)	(0.480)	(0.522)	(0.573)	(0.637)	(0.713)
Post-reform	-5.536	-6.095	-6.366	-6.281	-6.503	-6.091
	(0.484)	(0.520)	(0.561)	(0.612)	(0.676)	(0.751)
N	3,255,452	2,611,985	2,115,467	1,700,080	1,359,930	1,074,686
Product-date effects	927,440	656,984	475,835	344,538	250,059	180,918
Products	42,298	26,897	18,400	12,963	9,281	6,693

Notes: Regression results in columns (1) to (6) are based on data for 22 EU countries. The dependent variable is the change in the logarithm of unit sales, $\Delta\log(UNITS)$. Reforms' announcement information is fully incorporated. Observations in countries with reforms announced less than a month before implementation are removed around the respective reform date. The sample is gradually restricted to products sold contemporaneously in at least 3 up to at least 8 countries, where k_i is number of countries in which model i is sold. The monthly change in the standard VAT rate is denoted by $\Delta\tau_d$. Note that $\mathsf{E}\left[\mathsf{L}^{-j}\Delta\tau_d\right] = \mathsf{L}^{-j}\Delta\tau_d$ for all reforms that were announced $n > j$ periods ahead, and $\mathsf{E}\left[\mathsf{L}^{-j}\Delta\tau_d\right] = 0$ for reforms announced $n \leq j$. All specifications include a full set of product-date, country and country-month specific fixed effects. The monthly unemployment rate, $Unempl$, and the number of months a products appears in the data in a specific country, $M.age$, as well as $M.age^2$ are controlled for but not reported. Standard errors in parentheses are robust in all specifications and clustered by product category-country.

Table B.11 – Unit Sales Effects: Including Single-Country Products

Reforms	All		All		n≥ 1	
	(1)	(2)	(3)	(4)	(5)	(6)
$L^{-3}\Delta\tau_d$		-0.357				
		(0.286)				
$L^{-2}\Delta\tau_d$		-0.346				
		(0.271)				
$L^{-1}\Delta\tau_d$	1.897	1.874				
	(0.267)	(0.267)				
$E\left[L^{-3}\Delta\tau_d\right]$				-0.311		-0.402
				(0.309)		(0.315)
$E\left[L^{-2}\Delta\tau_d\right]$				-0.536		-0.641
				(0.278)		(0.274)
$E\left[L^{-1}\Delta\tau_d\right]$			2.014	1.987	2.050	2.043
			(0.291)	(0.290)	(0.300)	(0.299)
$\Delta\tau_d$	-3.426	-3.433	-3.428	-3.436	-3.941	-3.957
	(0.529)	(0.530)	(0.529)	(0.530)	(0.538)	(0.539)
$L^{1}\Delta\tau_d$	-1.775	-1.759	-1.773	-1.764	-1.379	-1.372
	(0.295)	(0.293)	(0.295)	(0.295)	(0.298)	(0.298)
$L^{2}\Delta\tau_d$		-0.774		-0.770		-0.995
		(0.249)		(0.249)		(0.258)
$L^{3}\Delta\tau_d$		1.116		1.115		1.324
		(0.232)		(0.231)		(0.225)
			Cumulative Effects			
Total	-3.304	-3.678	-3.187	-3.715	-3.270	-3.999
	(0.316)	(0.455)	(0.300)	(0.478)	(0.319)	(0.409)
Pre-reform	1.897	1.172	2.014	1.140	2.050	1.000
	(0.267)	(0.433)	(0.291)	(0.455)	(0.300)	(0.440)
Post-reform	-5.201	-4.849	-5.201	-4.855	-5.320	-5.000
	(0.429)	(0.450)	(0.428)	(0.450)	(0.449)	(0.456)
N	7,784,370	7,784,370	7,784,370	7,784,370	7,579,291	7,579,291
Group-date effects	44,457	44,457	44,457	44,457	44,062	44,062
Products	236,743	236,743	236,743	236,743	234,265	234,265

Notes: Regressions are based on data for 22 EU countries. The dependent variable is the change in the logarithm of unit sales, $\Delta\log(UNITS)$. Estimates in columns (5) to (6) are based on a reduced sample, in which observations in countries with reforms announced less than a month before implementation, are removed around the respective reform date. The monthly change in the standard VAT rate is denoted by $\Delta\tau_d$. Note that $E\left[L^{-j}\Delta\tau_d\right] = L^{-j}\Delta\tau_d$ for all reforms that were announced $n > j$ periods ahead, and $E\left[L^{-j}\Delta\tau_d\right] = 0$ for reforms announced $n \leq j$. All specifications include a full set of country-, country-month specific and group-date-specific fixed effects, where the groups are based on all possible combinations of the characteristics per product category as shown in Table B.4. For more details on the formation of the groups, refer to Section A.2 in the Appendix. Group-date cells, which contain a single country, are dropped from the estimation. The monthly unemployment rate, $Unempl$, and the number of months a product appears in the data in a specific country, $M.age$, as well as $M.age^2$ are controlled for but not reported. Standard errors in parentheses are robust in all specifications and clustered by group-date cells.

TABLE B.12 – DIFFERENTIAL UNIT SALES AND PRICE EFFECTS FOR TOP-SELLING PRODUCTS

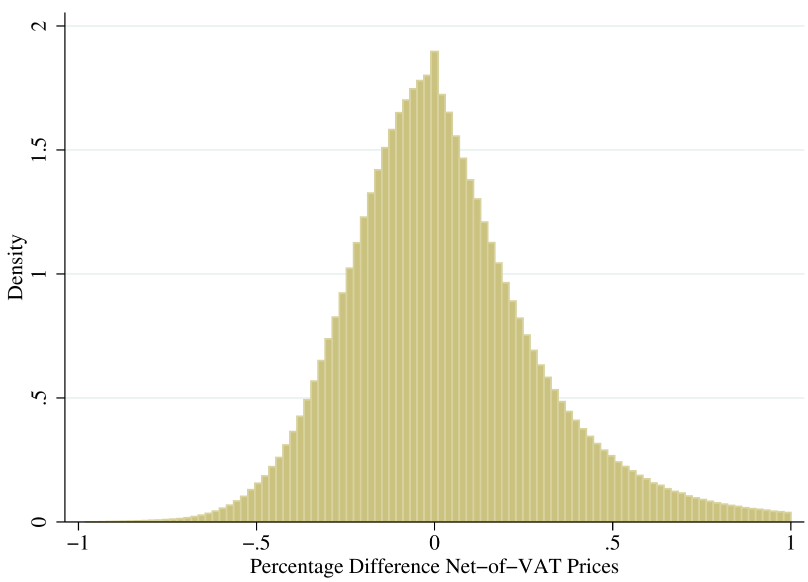
Forward terms	$\mathsf{L}^{-i}\Delta\tau_d$	$\mathsf{E}\left[\mathsf{L}^{-i}\Delta\tau_d\right]$		
Reforms	All	All	n≥ 1	n>3
	(1)	(2)	(3)	(4)
		Price effects $R50$		
Total	0.592	0.349	0.217	0.230
	(0.160)	(0.119)	(0.109)	(0.128)
Pre-reform	0.375	0.132	0.130	0.144
	(0.098)	(0.072)	(0.074)	(0.091)
Post-reform	0.217	0.217	0.086	0.086
	(0.101)	(0.101)	(0.088)	(0.109)
		Price effects $R100$		
Total	0.611	0.342	0.215	0.279
	(0.164)	(0.116)	(0.112)	(0.129)
Pre-reform	0.412	0.143	0.123	0.144
	(0.108)	(0.072)	(0.071)	(0.077)
Post-reform	0.199	0.199	0.092	0.135
	(0.085)	(0.085)	(0.077)	(0.088)
N	4,032,497	4,032,497	3,916,710	3,747,026
Product-date effects	1,302,880	1,302,880	1,275,887	1,227,984
Products	71,223	71,223	70,663	69,586
		Sales effects $R50$		
Total	-1.059	-0.835	-0.083	-0.879
	(1.341)	(1.387)	(1.337)	(1.726)
Pre-reform	-0.306	-0.081	-0.012	-0.657
	(0.949)	(0.974)	(0.981)	(1.087)
Post-reform	-0.753	-0.754	-0.070	-0.222
	(0.853)	(0.853)	(0.840)	(1.011)
		Sales effects $R100$		
Total	-0.679	-0.559	-0.558	-1.482
	(1.062)	(0.985)	(0.971)	(1.220)
Pre-reform	-0.461	-0.337	-0.521	-0.891
	(0.818)	(0.785)	(0.794)	(0.886)
Post-reform	-0.218	-0.222	-0.037	-0.592
	(0.682)	(0.682)	(0.699)	(0.756)
N	4,126,763	4,126,763	4,006,045	3,834,262
Product-date effects	1,331,154	1,331,154	1,302,736	1,254,536
Products	72,056	72,056	71,492	70,413

Notes: The table shows regressions for unit sales and prices following eq. (4.1) and eq.(4.2), with a full set of interaction terms for $\Delta\tau_d$ with indicators $R50$ ($R100$). The latter denote dummy variables equal to one if a product reaches a top 50 (top 100) rank within its respective category at some point in its life-cycle. The table reports the cumulative sum of pre-reform and post-reform coefficients as well as the total effect only for the interaction terms. In other words, it focuses solely on the differential effect for top-sellers and other goods. The monthly change in the standard VAT rate is denoted by $\Delta\tau_d$. Note that $\mathsf{E}\left[\mathsf{L}^{-j}\Delta\tau_d\right] = \mathsf{L}^{-j}\Delta\tau_d$ for all reforms that were announced $n > j$ periods ahead, and $\mathsf{E}\left[\mathsf{L}^{-j}\Delta\tau_d\right] = 0$ for reforms announced $n \leq j$. All specifications include a full set of product-date, country and country-month specific fixed effects. The monthly unemployment rate, $Unempl$, and the number of months a products appears in the data in a specific country, $M.age$, as well as $M.age^2$ are controlled for but not reported. Standard errors in parentheses are robust in all specifications and clustered by product category-country.

Figure C.1 – Unit Sales Response: 12 Months after Implementation

Notes: The figure depicts the time path of unit sales 12 months after a VAT tax rate change and is, in all other respects, identical to Figure 4.

Figure C.2 – Distribution of Price-Level Differences



Notes: The histogram plots all price differentials in log points generated within product-date cells. For a product sold in k countries in a given month-year d with k non-missing price observations, the total number of possible relative price combinations are $k!/2!(k-2)!$. Note that since prices are inclusive of VAT, we first remove the VAT component, and translate all prices into Euro before calculating relative prices. The histogram excludes log point deviations in relative prices greater (smaller) or equal to 1 (-1), which constitute 1.3% of all observations.

D The Cases of Germany and Spain

The above analysis assumes that consumers are well aware of a forthcoming tax increase/decrease. This part of the appendix focuses in more detail on Germany and Spain to check this assumption using data on the press coverage of tax reforms. It also explores whether sales and price effects of tax rate changes are visible in the raw data.

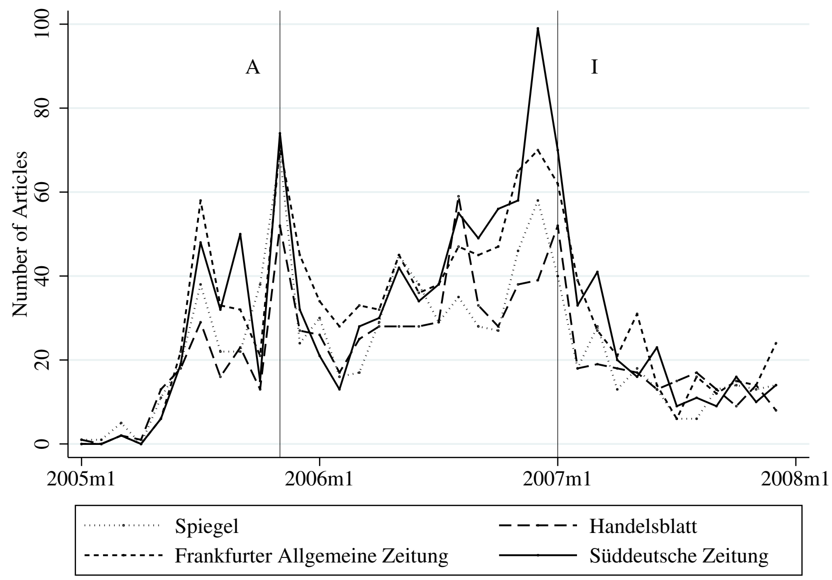
The German VAT increase of 3pp. in 2007 is discussed in detail by D'Acunto *et al.* (2019) and Carare and Danninger (2008). As a reform not tackling current or projected economic conditions, it meets the exogeneity criteria of Romer and Romer (2010).[39] In contrast, the VAT increases in Spain in 2010 (by 2pp.) and 2012 (by 3pp.) took place in a more difficult macroeconomic environment and were clearly motivated by fiscal predicaments in the aftermath of the 2008 financial crisis. Consequently, Gunter *et al.* (2017) classify both Spanish reforms as endogenous given their GDP-driven and pro-cyclical nature. The German reform and the first Spanish reform were announced well in advance – 14 months and 10 months, respectively, whereas the implementation lag for the second Spanish VAT increase was only a month and a half.

Figure D.1 graphs the number of articles in the German media discussing the VAT increase, based on four major non-tabloid newspapers in the country. The announcement and implementation dates for the tax reform are marked with reference lines. Two clear spikes in the number of articles are observed, one at the announcement date and one in the month before the implementation, even though the reform was being discussed continuously throughout 2006. Similarly to Germany, Figure D.2 depicts the number of articles discussing the Spanish reforms based on three main newspapers, with the second reform receiving almost double the coverage, which is not surprising given its short announcement and political context.

Figure D.3 shows annual growth rates of sales and prices in Germany and Spain relative to the same month of the previous year. Panel A depicts a strong growth in sales, especially in the last two to three months before the implementation of the VAT increase in Germany, and a substantial

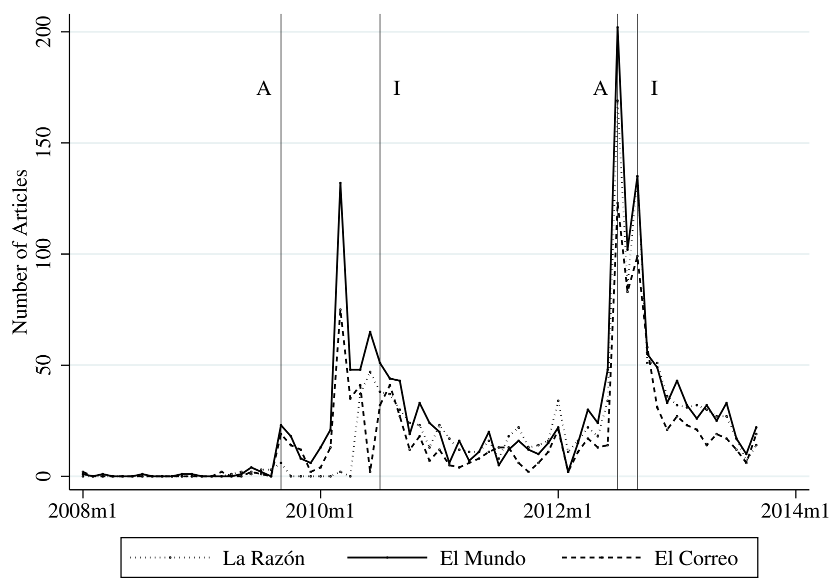
[39] Based on Romer and Romer's (2010) classification, tax changes serving long-run objectives, or those addressing past economic conditions such as tax increases dealing with an inherited budget deficit, are treated as exogenous.

Figure D.1 – Germany: Newspaper Articles Addressing Reform, 2005-2007



Notes: The figure depicts the number of articles in four major German newspapers, which mention "VAT rise" either in the title, or the main text from January 2005 until December 2007. The search keyword is "VAT rise" ("Mehrwertsteuererhöhung"). Germany increased the standard VAT rate from 16 to 19% on 1.1.2007, with the tax increase officially announced in November 2005. Authors' calculations using the online archives of Der Spiegel, Handelsblatt, Frankfurter Allgemeine Zeitung and Süddeutsche Zeitung.

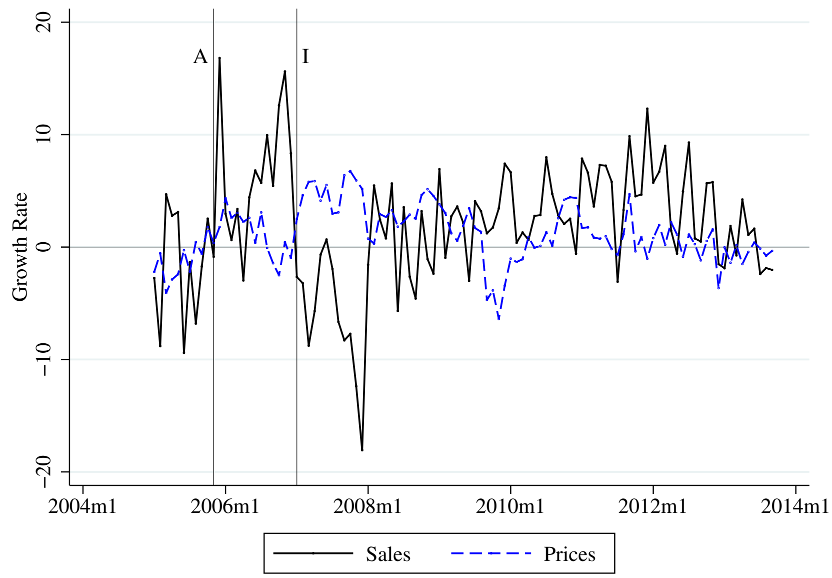
Figure D.2 – Spain: Newspaper Articles Addressing Reforms, 2008-2013



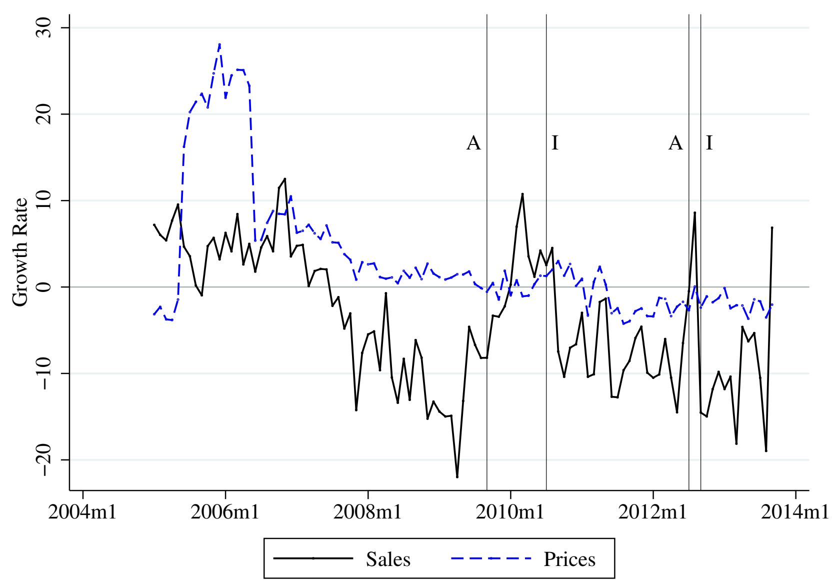
Notes: The figure shows the number of articles in three major Spanish newspapers, which mention "VAT rise" either in the title, or the main text from January 2008 until September 2013. The search keyword is "VAT rise" ("subida de IVA"). Spain increased the standard VAT rate twice in the depicted period: from 16 to 18% on 1.7.2010, with the tax increase officially announced in September 2009, and from 18 to 21% on 1.9.2012, announced on 11.7.2012. Authors' calculations using the online archives of La Razon, El Mundo, and El Correo.

Figure D.3 – Growth Rate Of Unit Sales And Prices

A. Germany



B. Spain



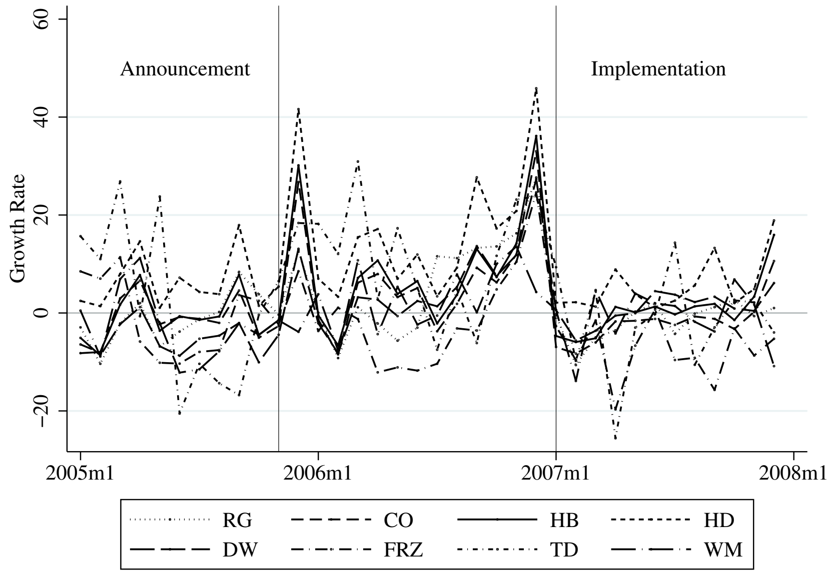
Notes: The figure depicts the annual growth rate of sales and prices in Germany and Spain relative to the same month of the previous year, starting from January 2004 and ending in September 2013. Germany increased the standard VAT rate from 16 to 19% on 1.1.2007, with the tax increase officially announced in November 2005. Spain increased the standard VAT rate twice in the depicted period: from 16 to 18% on 1.7.2010, with the tax increase officially announced in September 2009, and from 18 to 21% on 1.9.2012, announced on 11.7.2012.

drop afterwards. The period after implementation is characterized by substantially higher prices. This pattern is consistent with the theoretical predictions for sales and with full and instantaneous price pass-through.

The growth rate of unit sales jumps also in December 2005, one month after announcement. Disaggregating by categories of products (see Fig. D.4), we found that this response is driven by cooktops, hoods, and cookers, which are often sold as part of a kitchen unit. Closer inspection revealed that this effect is entirely driven by sales of Kitchen and Furniture specialising stores. A possible explanation is that those durables may have substantial delivery lags, which would induce consumers to buy early in order to ensure that the lower VAT rate applies. The dashed black line in Figure D.5 depicts the growth rate without cooktops, hoods and cookers. The announcement response then falls by half. Finally, the figure also shows growth rate of sales in neighbouring Austria, a closely integrated market to the German economy. Austria did not change its standard VAT rate and the sales growth rate does not deviate much around zero.

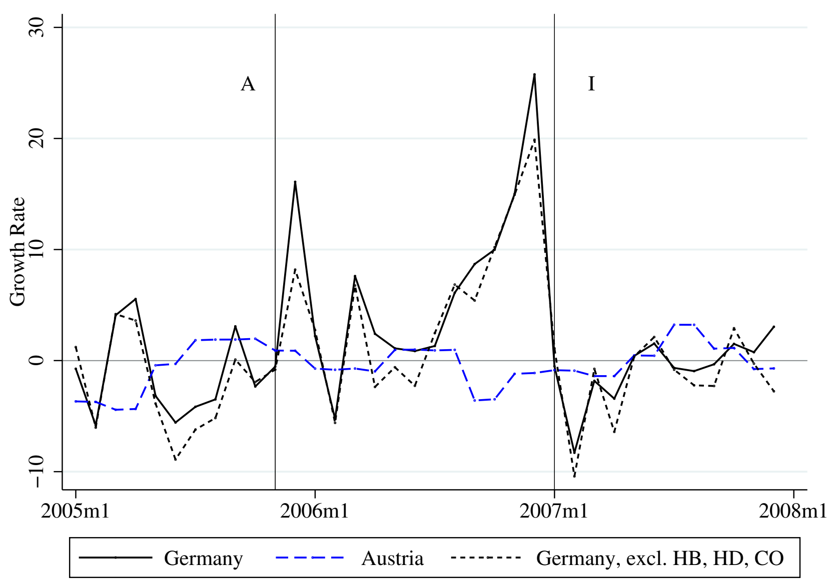
As shown in Panel B of Figure D.3, the market for white goods in Spain shrank considerably from 2007 to 2012. Against this negative trend, the two VAT reforms are associated with temporary pre-reform peaks in sales. In contrast to the German case, after the first reform, sales seem not to recover. With regard to price effects, a price increase is visible after the first reform, but a year after the reform prices are falling again. The second VAT increase is also not clearly reverting the negative price trend.

Figure D.4 – Germany: Growth Rate Of White Goods' Unit Sales By Product Category, 2005-2007



Note: The figure depicts the growth rate of the number of units sold in month m in years 2005, 2006, and 2007 relative to the average sales in 2004 and 2008 for the same month m for eight categories of durable goods: refrigerators (RG), cookers (CO), hobs/cooktops (HB), hoods (HD), dishwashers (DW), freezers (FRZ), tumble driers (TD) and washing machines (WM). The aggregate growth rate is depicted in two different ways in Figures D.3 and D.5. Germany increased the standard VAT rate from 16 to 19% on 1.1.2007, with the tax increase officially announced in November 2005.

Figure D.5 – Germany: Growth Rate Of Unit Sales



Notes: The figure depicts the growth rate of the total number of units sold in Germany. The upper panel shows the growth rate in month m in years 2005, 2006, and 2007 relative to the average sales in 2004 and 2008 in the same month m. For example, sales in Dec. 2005 were 16% higher relative to the average sales in Dec. 2004 and Dec. 2008. The black dashed line depicts the same growth rate excluding HB, HD, and CO. The dashed line is the growth rate of units sold in Austria, where no VAT rate change occurred.

E Theoretical Appendix

E.1 Demand for Consumer Durables with a Pre-announced Tax Rate Change

This appendix provides a brief analysis of the demand for durable goods by a household facing a pre-announced change in a general consumption tax. The following section characterizes the houshold's optimization problem. Subsequently, section E.3 derives Euler equations, *i.e.* the optimal time path of consumption of durable and non-durable goods. Section E.4 discusses predictions for the effects of a tax rate change.

E.2 Household Optimization Problem

The household derives utility from the consumption of durable and non-durable goods. The intra-period utility function is

$$u_s = \left[(1-b)^{\frac{1}{\epsilon}} x_s^{\frac{\epsilon-1}{\epsilon}} + b^{\frac{1}{\epsilon}} k_s^{\frac{\epsilon-1}{\epsilon}}\right]^{\frac{\epsilon}{\epsilon-1}},$$

where x_s is current consumption of non-durable goods and k_s indicates the consumption of services from the stock of consumer durables in the same period. ϵ denotes the elasticity of substitution.

Since the analysis deals with pre-announced changes in the tax rate, the consumer's choice is analyzed in a setting of certainty. The present value of the instantaneous utility in all periods is

$$\sum_{s=1}^{\infty} \beta^{s-t} \frac{\sigma}{\sigma-1} u_s^{1-\frac{1}{\sigma}},$$

where $\beta < 1$ is a discount factor reflecting the household's time preference, and σ is the intertemporal elasticity of substitution. In the specific case of $\sigma = \epsilon$, the utility function becomes additively separable in durable and non-durable goods consumption.

The stock of consumer durables evolves according to

$$k_s - k_{s-1} = i_s - \delta k_{s-1}.$$

where δ is the rate of depreciation. Writing $d = 1 - \delta$, we can solve for gross investment

$$i_s = k_s - k_{s-1}d. \tag{E.1}$$

Following standard practice, we assume a convex adjustment cost, formally

$$\frac{c}{2}\left(k_s - k_{s-1}\right)^2.$$

For simplicity, the adjustment cost is determined by net investment. Hence, it is zero if the stock of durables is constant.[40] Normalizing the pre-tax price of non-durables to unity and setting the pre-tax, or producer price of the durable good to q_s, consumer prices for durable and non-durable goods are

$$p_s = (1+\tau_s)\, q_s \text{ and } (1+\tau_s),$$

respectively.

The evolution of (financial) wealth is determined by total income, which consists of labor income w_s, and interest income, net of current purchases of non-durable consumption goods, current investment in durable goods and adjustment costs:

$$a_{s+1} - a_s = w_s + ra_s - (1+\tau_s)\, x_s - (1+\tau_s)\, q_s\left(k_s - k_{s-1}d\right) - \frac{c}{2}\left(k_s - k_{s-1}\right)^2, \tag{E.2}$$

where a_s is the stock of wealth at the beginning of period s, and r is the interest rate.

Eliminating i_s by plugging (E.1) into (E.2), for each period $s \in [1, 2, \ldots]$, the household chooses consumption of non-durables x_s and of durables k_s to maximize expected discounted utility subject to constraint (E.2).[41]

[40] The results below can be generalized to hold also if the adjustment cost is related to gross investment $(k_s - dk_{s-1})$ as in Shapiro (1986).

[41] The Lagrangian for the intertemporal optimization problem is

$$\mathcal{L} = \sum_{s=1}^{\infty}\left\{\beta^{s-1}\frac{\sigma}{\sigma-1}u_s^{\frac{\sigma-1}{\sigma}} + \lambda_{s+1}\beta^{s-1}\left[(1+r)\, a_s + w_s - (1+\tau_s)\, x_s - (1+\tau_s)\, q_s\left(k_s - k_{s-1}d\right) - \frac{c}{2}\left(k_s - k_{s-1}\right)^2 - a_{s+1}\right]\right\},$$

E.3 Euler Equations for Consumption

In period t, the optimal consumption structure obeys

$$\frac{k_t}{x_t} = \frac{b}{1-b} \left(Q_t + C_t\right)^{-\epsilon} . \tag{E.3}$$

Q_t denotes the user cost of the service flow of the durable good (Ogaki and Reinhard, 1998). C_t denotes the marginal adjustment cost. If $\epsilon > 0$, equation (E.3) states that a reduction in the user cost and a decline in the adjustment cost are associated with a substitution of non-durable with durable goods. The user cost is defined as

$$Q_t = \left[1 - \rho d \left(\frac{p_{t+1}}{p_t}\right)\right] q_t,$$

where $\rho = \frac{1}{1+r}$. Note that the user cost depends on the change in the consumer price in the next period $\frac{p_{t+1}}{p_t} = \frac{1+\tau_{t+1}}{1+\tau_t} \frac{q_{t+1}}{q_t}$. The user cost declines in period t if the consumer price increases in $t+1$. Assuming that the producer prices is fixed, $q_{t+1} = q_t$, and the user cost changes only with the tax rate. Note that the effect of the tax change on the user cost is larger if the depreciation rate is small.

The marginal adjustment cost is

$$C_t = \frac{c}{1+\tau_t} \left[\left(k_t - k_{t-1}\right) - \beta \left(k_{t+1} - k_t\right)\right] .$$

In order to derive implications for the demand for durable goods, we first consider the time path of consumption of non-durables.

With the simplifying assumption that $\beta (1+r) = 1$, the Euler equation for consumption of non-durables in period $t+1$ is

$$x_{t+1} = x_t \left(\frac{1+\tau_{t+1}}{1+\tau_t}\right)^{-\sigma} \left(\frac{1 + \frac{b}{1-b} \left(Q_{t+1} + C_{t+1}\right)^{1-\epsilon}}{1 + \frac{b}{1-b} \left(Q_t + C_t\right)^{1-\epsilon}}\right)^{\frac{\sigma-\epsilon}{\epsilon-1}} . \tag{E.4}$$

where λ_{s+1} is the Lagrange multiplier in current value terms.

Inserting from equation (E.3), we can use (E.4) to derive the corresponding Euler equation for the capital stock

$$k_{t+1} \;=\; k_t \left(\frac{1+\tau_{t+1}}{1+\tau_t}\right)^{-\sigma} \left(\frac{1+\frac{b}{1-b}\left(Q_{t+1}+C_{t+1}\right)^{1-\epsilon}}{1+\frac{b}{1-b}\left(Q_t+C_t\right)^{1-\epsilon}}\right)^{\frac{\sigma-\epsilon}{\epsilon-1}} \left(\frac{Q_{t+1}+C_{t+1}}{Q_t+C_t}\right)^{-\epsilon}. \tag{E.5}$$

Equations (E.4) and (E.5) provide the optimal pattern of consumption of non-durable and durable goods. In the following section we discuss the empirical implications of a pre-announced change in the tax rate.

E.4 Effects of a Tax Rate Change

Equations (E.4) and (E.5) indicate that there are direct and indirect effects of the tax rate on the time path of consumption of non-durable and durable goods.

Turning first to non-durables, equation (E.4) suggests that there are two direct effects of taxes on the optimal path of consumption. First, there is a direct effect associated with intertemporal substitution. If the tax rate changes, say it increases in period $t+1$, the first term in parentheses shows that the consumption of non-durables after the tax rate increase is small relative to consumption before the increase. The strength of this effect is determined by the elasticity of intertemporal substitution.

A second direct effect is associated with the user cost of durables. With a tax increase in period $t+1$ relative to period t, the user cost of durables declines temporarily $Q_t < Q_{t+1}$. If the two types of consumption goods are substitutes, *i.e.* $\epsilon > 0$, this provides an incentive to substitute the consumption of non-durable goods with durable goods. As noted by Cashin and Unayama (2016), the implications for the time path of consumption of non-durables depend on whether the elasticity of intratemporal substitution is large or small relative to the elasticity of intertemporal substitution. With a small ϵ, such that $\epsilon < \sigma$ and $\epsilon < 1$, the last term in parentheses in equation (E.4) further contributes to a high level of consumption before and a low level after the tax rate increase. If the elasticity of intratemporal substitution is relatively large, $\epsilon > \sigma$ and $\epsilon < 1$, the intratemporal substitution of non-durable with durable goods works against a high level of consumption in period t and a low level in $t+1$. In the case of separable utility $\sigma = \epsilon$, the time path of consumption of non-durables would only be affected by intertemporal substitution effects.

Besides direct effects, the pattern of consumption of non-durables around a tax rate change would also depend on indirect effects. With given producer prices, these are caused by changes in the marginal adjustment cost, which is a function of the consumption of durables.

Equation (E.5) shows that the two determinants of the time path of non-durable consumption also affect the time path of the consumption of durables. In fact, the first term in parentheses is identical to equation (E.4) indicating that both types of consumption are subject to the same permanent intertemporal substitution effect.

While the temporary decline in the user cost, caused by an increase in the tax rate, also affects both types of consumption goods, the effect on durables differs from the effect on non-durables due to the last term in parentheses in equation (E.5). Interestingly, the changes in the user cost matter for the time path of durables, even if the utility function is separable in consumption of durable and non-durable goods $\sigma = \epsilon$. With full price pass-through, the predictions are straightforward. If the tax rate increases in period $t+1$, the user cost of durables declines temporarily in period t and reverts to its steady state level in period $t+1$, so that, $Q_t < Q_{t+1}$. This contributes to a high level of the consumption of durables in period t relative to period $t+1$.[42] As above, indirect effects for durables are caused by the marginal adjustment cost.

Although the actual time paths of consumption depend on the specific parameter values, the difference equations (E.4) and (E.5) suggest that we can distinguish temporary and permanent effects of tax rate changes. The temporary effects are associated with changes in the user cost and the marginal adjustment cost and are shaped by preference parameters. However, the permanent effects are determined solely by intertemporal substitution. This property of the optimal time path of consumption has been exploited by Cashin and Unayama (2016) to identify the elasticity of

[42]Note that with $\epsilon, \sigma > 0$, the partial derivatives of k_{t+1} are unambiguous: $\frac{\partial k_{t+1}}{\partial Q_{t+1}} < 0, \frac{\partial k_{t+1}}{\partial Q_t} > 0$, regardless of whether $\epsilon > \sigma$ or $\epsilon < \sigma$.

intertemporal substitution using non-storable non-durables.

To show this property, we consider a tax rate increase by $\Delta\tau$ announced by the government in period 0 to take place in period $t+1$. In the periods before $t+1$, the tax rate is equal to τ, and in all periods after the implementation, the tax rate is $\tau + \Delta\tau$. In this setting, given full pass-through, we can separate two time periods in which the user cost is constant: The period after implementation, $j = t+1, t+2, \ldots$, and the period before implementation except period t, $j = 1, 2 \ldots, t-1$. In both periods, the precise pattern of consumption depends on initial values and on the marginal adjustment cost.

Given stability of the Euler equations, if the time spans are sufficiently long, in each period, the levels of consumption will approach stationary levels. In the period after implementation, provided that the tax policy is unchanged, there is a time period $t+p$ with $p > 1$ such that $k_{t+p} - k_{t+p-1} \approx 0$. But also after the announcement and prior to the implementation, when adjustment to the initial policy innovation has already taken place, a stationary state is reached by $t-q$ with $q > 1$ such that $k_{t-q} - k_{t-q-1} \approx 0$. This requires that either adjustment costs are small, or that the implementation lag with length $1, \ldots t+1$ is large. Hence, for a given adjustment cost function, the implementation lag has to be sufficiently long.

These observations enable us to predict the difference in consumption levels before and after the tax increase. From equation (E.4), forward and backward substitution provides

$$x_{t+p} = x_{t-q} \left(\frac{1+\tau_{t+p}}{1+\tau_{t-q}} \right)^{-\sigma} \left(\frac{1 + \frac{b}{1-b} \left(Q_{t+p} + C_{t+p} \right)^{1-\epsilon}}{1 + \frac{b}{1-b} \left(Q_{t-q} + C_{t-q} \right)^{1-\epsilon}} \right)^{\frac{\sigma-\epsilon}{\epsilon-1}}. \tag{E.4}$$

With full price pass-through, the user cost in $t+p$ and $t-q$ is equal to its steady-state level, $Q_{t+p} = Q_{t-q} = Q$. Moreover, if p is sufficiently large, changes in the optimal stock of durables around $t+p$ are small ($k_{t+p} - k_{t+p-1} \approx 0$, $k_{t+p+1} - k_{t+p} \approx 0$). Hence, the marginal adjustment cost C_{t+p} is approximately zero. Similarly, if q is large, changes in the optimal stock of durables around period $t-q$ are small ($k_{t-q} - k_{t-q-1} \approx 0$, $k_{t-q+1} - k_{t-q} \approx 0$) and the marginal adjustment cost C_{t-q} is approximately zero. Consequently, $\frac{Q_{t+q}+C_{t+q}}{Q_{t-p}+C_{t-p}} \approx 1$. Hence,

$$\frac{x_{t+p}}{x_{t-q}} \approx \left(\frac{1+\tau+\Delta\tau}{1+\tau}\right)^{-\sigma}. \tag{E.5}$$

By applying the same reasoning to the Euler equation for the consumption of durables, it is straightforward to show that

$$\frac{k_{t+p}}{k_{t-q}} = \left(\frac{1+\tau+\Delta\tau}{1+\tau}\right)^{-\sigma}. \tag{E.6}$$

This indicates that the relative difference in the levels of consumption of non-durables as well as of durables in periods p and q is determined by σ and the tax rate change.

While the permanent effects of a tax rate change on consumption levels are the same for both types of consumer goods, in contrast to non-durables, with durable goods it is important to distinguish between household consumption and investment. Also the empirical analysis in this paper is concerned with household unit purchases rather than consumption. In terms of the theoretical discussion, this suggests deriving empirical predictions on the investment in durables rather than on the stock of durables. Based on the definition of investment, the log of investment in period s can be approximated by

$$\log i_s = \log\delta + \log k_{s-1} + \frac{1}{\delta} d\log k_s.$$

First differencing yields an expression for changes in investment

$$d\log i_s = \frac{1}{\delta}\left[d\log k_s - d\log k_{s-1}\right] + d\log k_{s-1}.$$

Summing all investment changes around a tax rate change in a time interval from $t-q$ to $t+p$ we get:

$$\sum_{s=t-q}^{t+p} d\log i_s = \frac{1}{\delta}\sum_{s=t-q}^{t+p}\left[d\log k_s - d\log k_{s-1}\right] + \sum_{s=t-q}^{t+p} d\log k_{s-1}.$$

If the stock of durables is approximately constant at the beginning and end of the time interval, $d\log k_{t+p} \approx \log k_{t-q-1} \approx 0$, and $\sum_{s=t-q}^{t+p} d\log k_s \approx \sum_{s=t-q}^{t+p} d\log k_{s-1}$. Noting that the sum of net-investment in all periods corresponds to the total change in the stock of durables,we obtain

$$\sum_{s=t+p}^{t-q} d \log i_s \approx \log \frac{k_{t+p}}{k_{t-p}}.$$

This indicates that the sum of changes in investment is approximately equal to the total change in the stock of durables. Recall from equation (E.6) that the total change in the stock of durables is determined by the tax rate change and the elasticity of intertemporal substitution

$$-\sigma = \frac{\sum_{s=t-q}^{t+p} d \log i_s}{\Delta\tau}.$$

Thus, we can infer the elasticity of intertemporal substitution by summing the investment changes and using the information about the magnitude of the tax rate change.

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