The Effects of Pre-announced Consumption Tax Reforms on the Sales and Prices of Consumer Durables

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Abstract

This paper utilizes a micro data set on consumer durables to study the effect of consumption tax reforms on the time path of consumption. The dataset reports the monthly sales of individual products and their consumer prices in 22 European countries, which enacted numerous consumption tax reforms in recent years. We implement a reduced form specification for sales that allows us to test theoretical predictions of a standard model of consumer choice under different assumptions about the pass-through of taxes into prices. Our identification strategy exploits the trading of individual products in multiple countries. The results document that changes in baseline consumption tax rates are fully and quickly shifted into consumer prices and exert strong effects on the time path of consumption. We find that a one percentage point increase in consumption taxes causes an intertemporal shift in consumption by more than 2 percent. In addition, the purchases of durable goods increase temporarily by about 2 percent in the last month before a tax increase.

Key Words: Tax Reform; Fiscal Policy; Consumption Tax; Pass-Through; Tax Incidence; Durable Goods

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1 Introduction

The consumption tax is a potentially powerful instrument of fiscal policy. Economic theory suggests that, depending on the reform, an upcoming tax-rate change would incentivize consumers to bring forward or postpone purchases. Since most countries levy broad-based consumption taxes, they could exploit this intertemporal response and raise or discourage consumption through a pre-announced change of the baseline tax rate. Feldstein (2002), for example, proposes a sequence of pre-announced consumption tax increases in order to stimulate current consumption. Combined with a reduction in income taxes, this would enable fiscal policy to boost consumption without increasing budget deficits. Hall (2011) points out that pre-announced consumption tax increases could be useful to combat the decline in the US demand for consumer durables. Assuming full pass-through of tax changes into consumer prices, Correia, Farhi, Nicolini and Teles (2013) use a New Keynesian model to show that an increasing time path of consumption taxes could be an essential part of an "unconventional fiscal policy" at the zero lower bound of the nominal interest rate.

There is, however, considerable uncertainty about consumer responsiveness at the intertemporal margin (see Attanasio and Weber, 2010), all the more so since consumers might not be fully aware of the consumption tax burden (Chetty, Looney and Kroft, 2009). In addition, conventional assumptions about the pass-through 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 empirically the effects of pre-announced tax reforms on consumer behavior.

The existing literature is scarce and the available evidence offers mixed results. One common finding is a high consumer awareness of forthcoming consumption tax changes and significant shifts in sentiment toward expenditure. Crossley, Low, and Sleeman (2014) analyze the effects of a temporary VAT (value-added tax) cut implemented as a fiscal stimulus in the UK in late 2008 and find strong positive effects on consumer sentiment. Exploring the effect of a pre-announced consumption tax increase in Japan in 1997, Cashin and Unayama (2016) also document high consumer awareness of the upcoming reform. D'Acunto, Hoang, and Weber (2016) find that the 2007 consumption tax increase in Germany raised German households' inflation expectations and their willingness to purchase durable goods before the tax reform relative to other European households.

With regard to actual purchases, however, results are mixed. Crossley, Low, and Sleeman (2014) provide some evidence of a positive effect on consumer spending for retail sales in the UK relative to other European countries. But they also document that prices started to increase before the temporary tax cut was reversed. If preferences are non-separable, Ogaki and Reinhardt (1998) emphasize that to obtain unbiased estimates of the intertemporal consumption response, it is necessary to differentiate between consumer durables and non-durables. Cashin and Unayama (2016), who employ detailed household panel data that allow them to distinguish between goods according to their durability and storability, find only small intertemporal substitution effects for the 1997 tax increase in Japan.

We utilize a unique micro-level data set of major domestic appliances at the product level on a monthly basis from 2004 until 2013 in 22 European countries, which underwent numerous VAT reforms in recent years (DeMooij and Keen, 2013). A key characteristic of the dataset is the availability of information on both prices and units sold, which allows us to study separately the effects of tax reforms on consumer prices and purchases for approximately 30,000 individual products (models). We employ a consistent identification strategy for sales and price effects, which exploits the trading of individual products in multiple countries: Counterfactuals for models in a country experiencing a consumption tax reform are constructed from the contemporaneous sales and prices of the same model in other EU countries. The empirical analysis explicitly takes the timing of reforms' announcement and implementation into account. In addition, we incorporate information on the motivation behind tax reforms following the narrative approach to the analysis of fiscal policy put forward by Romer and Romer (2010).

The empirical results document that changes in baseline consumption tax rates are fully and quickly passed into consumer prices and exert strong effects on the time path of consumer spending. Sales of consumer durables are found to differ substantially before and after a tax-rate change. Specifically, in the month before implementation, sales of durables display a strong temporary change even though at this point about a third of the tax rate change has already been passed into consumer prices. The difference in sales before and after implementation is not merely offsetting the short-run demand effect for durables on the verge of a tax reform, but points to a strong intertemporal shift in total consumer demand. Quantitatively, our results show that a one percentage point increase in consumption taxes causes an intertemporal shift in consumption by more than 2 percent. In addition, purchases of durable goods increase temporarily by about 2 percent in the last month before a tax increase.

This paper contributes to different strands of the literature. The empirical analysis is concerned with the developments in sales and prices around tax reforms as in Cashin and Unayama (2016) and D'Acunto, Hoang, and Weber (2016). To derive empirical predictions, we follow Ogaki and

Reinhardt (1998) and Cashin and Unayama (2016) and use an intertemporal model of consumer choice distinguishing between durable and non-durable goods. We show that, depending on the pass-through of taxes, an appropriate specification may need higher-order leads and lags of tax-rate changes to capture pre- and post-reform effects on durables.

We generalize the analysis of pre-announced consumption tax rate changes by studying multiple tax reforms, and explore reform heterogeneity along two dimensions: in terms of the length of the implementation lag after announcement as in Mertens and Ravn (2012) and in terms of endogeneity-exogeneity as in Romer and Romer (2010). Only if we incorporate the information on when a policy reform has been announced, our results yield full shifting of consumption tax changes into consumer prices. The shift occurs within a short time period of two quarters. Following Gunter *et al.* (2017), we additionally investigate the role of the motivation behind each tax change. When focusing solely on reforms that are classified as exogenous with regard to the business cycle, we find that the price pass-through is quicker, the temporary shift in the demand for durables is more pronounced, and the long-term intertemporal shift in consumption is smaller.

In exploiting regional information within the European Union, our identification strategy is related to the analysis of local sales taxes in the US by Agarwal, Marwell and McGranahan (2016), who study the consumer response to sales-tax holidays that temporarily exempt specific items. They use household survey data as well data on credit card transactions to identify differences in the tax treatment by households' or merchants' place of residence and find a significant increase in spending that is not offset in the periods before or after the tax holiday. Baker, Johnson and Kueng (2017) use scanner data from the Nielsen Consumer Panel to study consumer responses to state and local sales tax changes at the household and store level and find that consumption spending decreases by

2% after a local sales tax increase. In contrast to state and local sales taxes, the consumption tax effects studied in this paper refer to the baseline tax-rate of a general consumption tax. As this rate is applied to most consumer goods and not on business purchases,[1] it is much closer to the type of consumption tax actually discussed in the fiscal policy literature. Since our analysis focuses on EU countries rather than local jurisdictions, cross-border shopping, addressed in the literature on local sales taxes as, for instance, in Agrawal (2015), is arguably less of an issue. We, nevertheless, perform robustness checks to confirm this.

Given its focus on consumer durables in the context of fiscal stimulus policies, this paper is also closely related to the empirical literature on the effects of temporary vehicle scrappage programs aiming to stimulate consumer spending and promote fuel efficiency. Mian and Sufi (2012) and Green, Melzer, Parker and Rojas (2016) find that the main effect of the US 2009 subsidy was to shift consumer spending within a year. This finding is confirmed by Li, Linn and Spiller (2013) who use model-level data in an empirical approach that is more similar to ours. Hoekstra, Puller and West (2017) also find strong short-term effects on car sales, but document that sales perform much worse in the post-program period than the counterfactual, pointing to long-term losses in consumer spending, which the authors attribute to fuel efficiency restrictions.

Our analysis provides new evidence on the pass-through of consumption taxes. While it is frequently assumed that pass-through is complete and immediate, some recent empirical papers explore to what extent changes in the baseline consumption tax rate are actually reflected in consumer prices. Carbonnier (2007) studies two major VAT decreases in France and finds that the pass-through is limited, especially for car sales. Carare and Danninger (2008) use monthly price data at a

[1] Ring (1999) shows that about a third of the tax base of the US states' general sales taxes consists of business purchases.

two-digit level to study how the 2007 VAT increase in Germany affected consumer prices. While their results support full shifting of the tax to the consumer, they find evidence of "inflation smoothing" in the sense that adjustments in prices start before implementation. Using a similar classification, Benedek, deMooij, Keen, Wingender (2015) provide a more comprehensive analysis based on monthly price index data for 17 European countries and 65 distinct VAT reform episodes. For changes in the standard consumption tax rate, they confirm full price pass-through, and for durables they find pre-reform price adjustments. With regard to timing, the authors' estimates show that price adjustments start 7 to 9 months before a reform and continue for 8 months after.[2]

While we focus on the baseline consumption tax rate, our analysis differs, since instead of price-index data we use primary data that allows us to focus on the pricing of individual products, and thus provide evidence on tax effects that are not confounded by shifts between products of different quality. Further, unlike previous work, which usually does not take reform-announcements into account, we show that information about the timing of the announcement and the length of the implementation lag affects the results and should not be ignored when assessing the effect of pre-announced reforms. While we find clear support for full pass-through of the consumption tax, our results also indicate that price adjustment differs depending on the economic background of the reform. If we focus on tax reforms that are not related to GDP shocks, prices for durables are found to display quicker adjustment with pre-reform effects starting only two months before implementation and full pass-through reached in the first month after implementation.

The paper proceeds as follows. The next section provides a theoretical analysis of intertemporal

[2] Benedek *et al.* (2015) also note that the pass-through of the baseline consumption tax rate differs for reduced rates, for which they find limited pass-through. The latter is confirmed by Kosonen (2015), who also documents asymmetries in the pass-through of tax increases and decreases (see also Benzarti, Carloni, Harju, Kosonen, 2017).

consumption in a model with durable and non-durable goods which is used to derive the empirical specification for sales. Section 3 describes the dataset, while Section 4 provides descriptive evidence of the effect of consumption tax increases on the sales and prices of white goods in Germany and Spain. Section 5 outlines our empirical methodology. The regression results for sales and prices are presented in Section 6, including various robustness checks. Section 7 concludes.

2 Theoretical Framework

This section develops a framework for the analysis of the consumption decisions of a representative household facing a pre-announced general consumption tax increase. We follow Ogaki and Reinhart (1998) and assume that the household derives utility from the service flow of the stock of durables, k_s, and the consumption of non-durable goods, x_s, in period s. The utility from consumption in this period has constant elasticity of substitution ϵ such that

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

Since the model focuses on pre-announced changes in the tax rate, the consumer's choice is analyzed in an intertemporal setting with certainty. The present value of the instantaneous utility in all periods is

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

where $\beta < 1$ is a discount factor reflecting the household's time preference, and σ is the intertemporal elasticity of substitution. Note that this function is additively separable over time. Let i_s designate purchases of durables in period s and δ be the rate of depreciation. The stock of consumer durables evolves according to

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

Normalizing the producer price of non-durables to unity and denoting the producer price of the durable good by q_s, consumer prices for durable and non-durable goods are $p_s = (1 + \tau_s)\, q_s$ 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 and current investment in durables

$$a_{s+1} - a_s = w_s + r a_s - (1 + \tau_s)\, x_s - p_s i_s, \tag{2}$$

with a_s being the stock of wealth at the beginning of period s, and r the interest rate.

2.1 Demand for Consumer-Durables

In each period s the household chooses consumption of non-durables, x_s, and investment in durables, i_s, to maximize its expected discounted utility subject to constraints (1) and (2). Eliminating i_s by plugging (1) into (2), the consumer's problem is equivalent to choosing x_s and k_s.

As shown in the appendix (see Appendix A.1), assuming for simplicity that $\beta\,(1 + r) = 1$, the optimum consumption structure requires that

$$k_s = b^{\epsilon} Q_s^{-\epsilon} x_s, \tag{3}$$

where Q_s is the user cost of the service flow of the durable good (Ogaki and Reinhart, 1998),

$$Q_s = \left[1 - \rho\left(1 - \delta\right)\left(\frac{p_{s+1}}{p_s}\right)\right]\frac{p_s}{1+\tau_s}, \tag{4}$$

where $\rho = \frac{1}{1+r}$. Note that the user cost in period s depends on the change in the consumer price in period $s+1$.

In order to derive implications for the demand of durable goods, we consider the optimal stock of durables in period $s+1$ relative to period s. From equation (3), the optimal path of the stock of durables is given by

$$\frac{k_{s+1}}{k_s} = \left(\frac{Q_{s+1}}{Q_s}\right)^{-\epsilon}\frac{x_{s+1}}{x_s}. \tag{5}$$

As in Cashin and Unayama (2016) (see also Appendix A.1), the optimal path of non-durables is

$$\frac{x_{s+1}}{x_s} = \left(\frac{1+\tau_{s+1}}{1+\tau_s}\right)^{-\sigma}\left(\frac{F_{s+1}}{F_s}\right)^{\frac{\sigma-\epsilon}{\epsilon-1}}, \tag{6}$$

where

$$F_s = 1 + b^{\epsilon} Q_s^{1-\epsilon} \tag{7}$$

captures the utility effect of changes in the optimum consumption structure as determined by (3). In the case of a Cobb-Douglas utility function ($\epsilon = 1$), this term does not vary with the user cost, and F_s is a constant parameter. Equations (5), (6) and (7) determine the time path of the optimal stock of durables as a function of the time paths of the user cost and the tax rate.

2.2 Sales Effects of a Tax Rate Change

While the above theoretical model abstracts from possibly important aspects of consumer demand such as adjustment costs or limited access to the capital market, it can be used to derive some basic empirical predictions about the intertemporal effects of a tax-rate change. To explore the effect on the path of consumer durables of a change in the tax rate set to occur in period t and already anticipated in period $t-1$, we take logs and substitute eq. (6) into eq. (5). The resulting expression shows that growth in the stock of durables, $\log\left(\frac{k_t}{k_{t-1}}\right)$, is a linear function of the growth in the user cost, $\log\left(\frac{Q_t}{Q_{t-1}}\right)$, the change in the tax rate, $\log\left(\frac{1+\tau_t}{1+\tau_{t-1}}\right)$, and the growth in the intra-temporal substitution term, $\log\left(\frac{F_t}{F_{t-1}}\right)$. Recall that the latter term drops out when the intra-temporal substitution elasticity is unity ($\epsilon = 1$), or, as noted by Cashin and Unayama (2016), when the utility function is separable in the consumption of durables and non-durables ($\sigma = \epsilon$). In order to address the more general case where $\epsilon \neq 1$ and $\sigma \neq \epsilon$, $\log F_t$, $\log F_{t-1}$, Q_t and Q_{t-1} are approximated linearly around the steady-state value of $Q = [1-\rho(1-\delta)]\,q$ to obtain

$$\Delta \log F_t = (1-\epsilon)\left(\frac{b^{\epsilon}Q^{1-\epsilon}}{1+b^{\epsilon}Q^{1-\epsilon}}\right)\Delta \log Q_t.$$

Hence, the growth in the optimum stock of durables can be expressed solely in terms of the change in the tax rate and the growth rate of the user cost

$$\Delta \log k_t = -\sigma \Delta\tau_t - \varphi \Delta \log Q_t, \tag{8}$$

where $\varphi = \epsilon + (\sigma-\epsilon)\left(\frac{b^{\epsilon}Q^{1-\epsilon}}{1+b^{\epsilon}Q^{1-\epsilon}}\right) > 0$ and $\log\left(\frac{1+\tau_t}{1+\tau_{t-1}}\right)$ is approximated by $\Delta\tau_t$.

Equation (8) shows that an anticipated change in the consumption tax has basically two separate effects on the optimal stock of durable goods. Consider a tax increase in period t. As the tax rises, the stock of durables declines due to intertemporal substitution. The strength of this decline depends on the magnitude of the tax increase and on the elasticity of intertemporal substitution σ. A second effect emerges from changes in the user cost. Decomposing the change in the user cost in producer price and tax effects by means of a linear approximation yields

$$\Delta \log Q_t = -\alpha \Delta \log q_{t+1} + (1+\alpha)\, \Delta \log q_t - \alpha\left(\Delta \tau_{t+1} - \Delta \tau_t\right), \tag{9}$$

where $\alpha = \frac{\rho(1-\delta)}{1-\rho(1-\delta)} > 0$. Under the assumption of constant producer prices, *i.e.* with complete and instantaneous pass-through of the consumption tax change, the change in the consumer price is proportional to the tax rate change. In this case, the change in the user cost in period $t-1$ is $\Delta \log Q_{t-1} = -\alpha \Delta \tau_t$. Depending on the magnitude of α, this causes a temporary increase in sales in $t-1$ before the reform. In period t, when the higher tax rate is implemented, there is an opposite effect on the user cost, $\Delta \log Q_t = \alpha \Delta \tau_t$, which contributes to a decline in sales in the same period. The strength of the response hinges on various parameters, and in particular on the intra-temporal elasticity of substitution ϵ. Intuitively, the larger the degree of substitutability between durables and non-durables, the larger this transitory effect on the sales of durables.

Supposing a full and immediate pass-through of the tax rate change into consumer prices assumes away any price movements in anticipation of the consumption tax reform, and in particular, any producer price effects in the form of an early pass-through. However, high demand prior to a tax increase might induce producers to change their prices in $t-1$, triggering a change in Q_{t-2}. Similarly, a drop in demand in t might push producers to lower prices, and as a consequence, the user cost in period t might be below the long-term steady state. Hence, Q_{t+1} will differ from Q_t, and further demand effects could take place in $t+1$. With a linear approximation for $\log i_t$, the change in sales is[3]

$$\Delta \log i_t = \frac{1}{\delta}\left(\Delta \log k_t - (1-\delta)\,\Delta \log k_{t-1}\right). \tag{10}$$

Assuming a constant price elasticity of supply, the change in the producer price is proportional to the change in sales. Hence, the producer price is related to current and lagged changes in the stock of durable goods

$$\Delta \log q_t = \eta\left(\Delta \log k_t - (1-\delta)\,\Delta \log k_{t-1}\right),$$

where η is inversely related to the price elasticity of supply. Replacing the producer price in eq. (9), inserting for $\Delta \log Q_t$ in eq. (8) and introducing L and F as lag and forward operators, respectively, gives:

$$\left(1-\gamma_3\mathsf{F}-\gamma_4\mathsf{L}\right)\Delta \log k_t \quad = \quad \gamma_1\Delta\tau_{t+1}-\gamma_2\Delta\tau_t, \text{ where} \tag{11}$$

$$\gamma_1 = \frac{\varphi\alpha}{1+\varphi\eta\left(\alpha\left(1-\delta\right)+\left(1+\alpha\right)\right)}, \qquad \gamma_2 = \frac{\sigma+\varphi\alpha}{1+\varphi\eta\left(\alpha\left(1-\delta\right)+\left(1+\alpha\right)\right)},$$
$$\gamma_3 = \frac{\varphi\alpha\eta}{1+\varphi\eta\left(\alpha\left(1-\delta\right)+\left(1+\alpha\right)\right)}, \qquad \gamma_4 = \frac{\varphi\left(1+\alpha\right)\eta\left(1-\delta\right)}{1+\varphi\eta\left(\alpha\left(1-\delta\right)+\left(1+\alpha\right)\right)}.$$

Solving for $\Delta \log k_t$, the change in the optimal capital stock is determined by distributed leads and lags of the change in the tax rate. Inserting into eq. (10) yields a prediction for the change in investment

$$\Delta \log i_t = \sum_{s=1}^{p} a_s\mathsf{F}^s\Delta\tau_t + b\Delta\tau_t + \sum_{s=1}^{q} d_s\mathsf{L}^s\Delta\tau_t, \tag{12}$$

where $q \geq 1$ and $p \geq 1$ determine the number of lags and leads of the tax rate change.

[3] Note that $\log i_t$ can be approximated around k_{t-1} by $\log i_t \simeq \log\delta + \log k_{t-1} + \frac{1}{\delta}\Delta \log k_t$.

In the absence of producer price effects ($\eta = 0$), there is full and immediate pass-through of taxes into consumer prices. Hence, $\gamma_3 = \gamma_4 = 0$ and $p, q = 1$, and eq. (12) simplifies to

$$\Delta \log i_t = a_1 \mathsf{F} \Delta \tau_t + b \Delta \tau_t + d_1 \mathsf{L} \Delta \tau_t.$$

In this case, only three parameters, a_1, b and d_1, characterize the sales path and we are able to make predictions on their sign. The coefficient related to the upcoming change in the tax rate should be positive ($a_1 = \frac{\varphi\alpha}{\delta} > 0$), which suggests that the demand for durables changes in the period before the tax reform. More specifically, in case of a tax increase, sales of durables rise. A second effect on the sales path occurs in the period when the tax rate change is implemented. With a tax increase, the model predicts an unambiguous decline in sales ($b = -\frac{1}{\delta}(\sigma + \varphi\alpha) - (1-\delta)\frac{\varphi\alpha}{\delta} < 0$). This partly reflects the reversal of the temporary pre-reform increase in sales, and partly the intertemporal substitution effect that works towards a decline in general consumption. In the subsequent month, the model predicts a recovery of sales ($d_1 = \frac{1-\delta}{\delta}(\sigma + \varphi\alpha) > 0$). The sum of the three coefficients identifies the intertemporal substitution elasticity:

$$\sigma = -(a_1 + b + d_1).$$

In the presence of producer price effects, the model predicts that sales effects are not necessarily limited to the periods directly before and after a tax-reform. As a consequence, p and q would be larger than unity, and the empirical parameters $a_1, ...a_p, b, d_1, ...d_p$ would be a combination of consumer and producer responses.

3 Data Description

The dataset is provided by the market research company Gesellschaft für Konsumforschung (GfK) Retail and Technology GmbH and consists of monthly panel data at the model level on unit sales and scanner prices of durable "white goods" for all countries of the European Union (EU), except Bulgaria, Croatia, Cyprus, Ireland, Luxembourg and Malta. The white goods comprise of eight major categories: Cookers, refrigerators (coolers), dishwashers, freezers, cooktops (hobs), hoods, tumble driers and washing machines. Each individual model has a unique identification number (id), which is the same over time and across countries, in case a model is traded in more than one Member State. Table 1 summarizes the coverage of the data by country and category.

The data covers around 110,000 different models with 62.4 million units sold per year, and an average annual market size of 26.1 billion Euro. The time period generally extends from January 2004 until September 2013, although data coverage for some countries usually starts at a later date. GfK collects price and quantity data retailer by retailer. The final units sold for a model in a given country in a specific month are the sum of all sales of this model across all retailers in the country in the respective month. The corresponding price is a monthly sales-weighted average of all prices for this model across retailers. Prices are inclusive of consumption taxes and reflect all discounts, special offers and other promotions received by consumers.[4]

[4] According to GfK's panel methodology, GfK Retail and Technology generate 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 its corresponding universe. 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 model. The basic data is received retailer by retailer in a heterogenous form. Incoming data from different sources referring to the same model is translated into one single definite GfK

Table 1: 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 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 onwards.
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; BE=Belgium; CZ=the Czech Republic; DE=Germany; DK=Denmark; EE=Estonia; ES=Spain; FI=Finland; FR=France; GR=Greece; HU=Hungary; IT=Italy; LV=Latvia; LT=Lithuania; NL=the Netherlands; PL=Poland; PT=Portugal; RO=Romania; SE=Sweden; SI=Slovenia; SK=Slovakia.

Table 2 shows descriptive statistics on the average annual number of models, units sold, value of sales, the average monthly price and quantity per model and other variables. Descriptive statistics disaggregated by model category are presented in Table A-1 in the Appendix. In terms of number and value of sales, refrigerators and washing machines constitute the two biggest categories.

While the annual number of models is stable at around 110,000, the composition changes periodically, with new models entering the market and older ones exiting. The life cycle, *i.e.* the change in the number of units sold over time for models introduced in a particular year is depicted in Figure 1.[5] Clearly, sales are inversely proportional to a model's age. The sales of new models account for,

product code. Once checked, the basic data is extrapolated for each distribution channel.

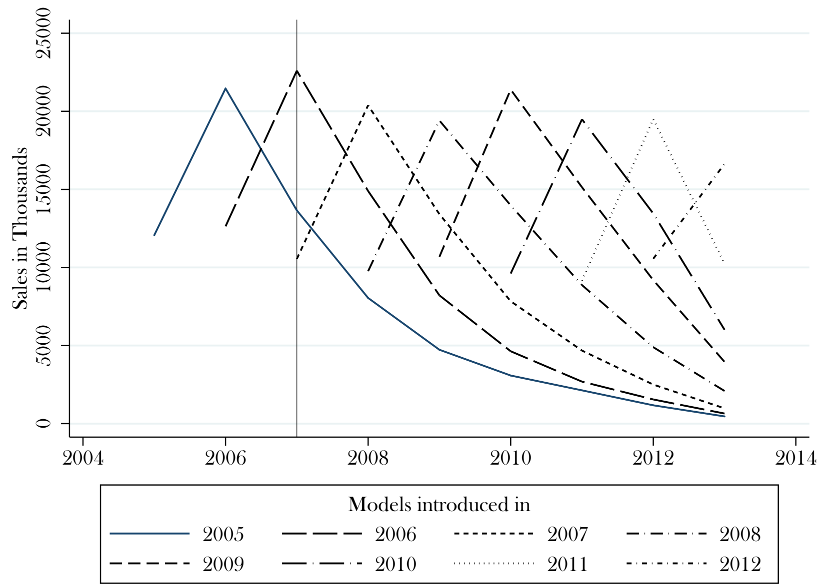
[5] Models' years of introduction are based on the assumption that the first year a product appears in the data (in any country), is the year, in which it was introduced. GfK provided us with a sample plot with exit and entry of fridges based on actual dates of introduction and exit, which was closely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

Table 2: DESCRIPTIVE STATISTICS

	Mean	Std. Dev.	Min	Max
	All Models (Full Sample)			
Average № of Models per Year	109,848	3,889	102,879	117,842
Average № Units Sold per Year (Thousands)	65,405	5,079	47,081	65,708
Average Value of Sales per Year (Millions Euro)	26,119	2,206	19,531	27,992
Average № Units Sold per Model per Month	50	184	-262	24,965
Average Price per Model (Euro)	488	398	-1,968	29,826
Average Months Model Sold	30.46	23.22	1	117
Average Market Age (Months)	26.87	22.27	1	117
Rank	892	797	1	5,364
R5 (min(rank) $\in [1, 50]$)	0.101	0.301	0	1
R10 (min(rank) $\in [1, 100]$)	0.182	0.386	0	1
	Models Sold in at Least Two Countries (Estimation Sample)			
Average № of Models per Year	29,683	6,466	10,095	36,540
Average № Units Sold per Year (Thousands)	33,185	5,906	13,829	38,692
Average Value of Sales per Year (Millions Euro)	15,187	2,558	6,743	17,389
Average № Units Sold per Model per Month	46	158	-34	19,167
Average Price per Model (Euro)	538	405	-1,628	13,284
Average Months Model Sold	27.74	21.01	1	117
Average Market Age (Months)	25.22	20.08	1	117
Rank	747	697	1	5,364
R5 (min(rank) $\in [1, 50]$)	0.110	0.313	0	1
R10 (min(rank) $\in [1, 100]$)	0.197	0.398	0	1
Standard VAT rate	.201	.023	.15	.27
Unemployment rate	8.85	4.10	3.1	27.8

Note: The market age is the number of months a model is sold in a specific country as opposed to the total number of months a model appears in the data irrespective of the location of sales (Average Months Model Sold). Min(rank) is the minimum rank achieved by a model in a given country across all years, *i.e.* it captures the highest rank of a model in its best-selling year. $R5$ is a dummy variable equal to 1 if a model reaches a rank between 1 and 50 throughout its life-cycle; $R10$ is a dummy variable equal to 1 if min(rank) is between 1 and 100. After first checks of the data, the following where eliminated: Observations without an id; observations for models for which all units/price variables are missing across all years; observations in years for a model for which all units/prices are missing. This resulted in the loss of only 15,174 observations, or 0.07% of all available data points. Across all categories, there are a very limited number of observations with negative units sold and prices. We treat these as returned items and leave the data as it is. The largest negative number for units sold is -262 in the case of cookers. Outliers in prices were also removed. In particular, we calculated percentage changes of prices of a specific model within a country and replaced prices exhibiting increases greater than 200% (929 prices) and price decreases greater than 50% (97,353 prices) with missing observations. Out of the 97,353 prices that fell by more than 50%, only 6,617 were real price changes, and the rest were driven by zero prices (percentage change of -1.) More detailed descriptive statistics disaggregated by model category are presented in Table A-1 in the Appendix.

Figure 1: PRODUCT LIFECYCLE BY YEAR OF INTRODUCTION



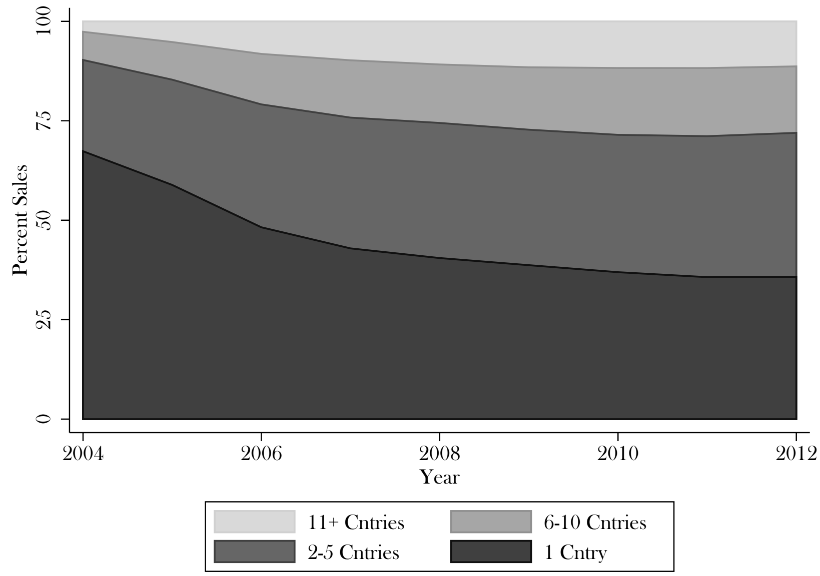
Note: The figure depicts the annual evolution of unit sales by year of introduction. It also shows the composition of sales based on models' year of introduction. For example, in 2007, models introduced in 2005 constituted 21% of total sales, models launched in 2006 – 34%, and new models (launched 2007) –16%. The remaining 29% were models first appearing in the data in 2004 and not shown in the figure.

on average, 20-25% of the total units sold in the first year they are introduced, peak in the second year, and peter out afterwards. This pattern does not vary substantially across individual product categories. About 80% of new entries drop out of the market in 5 to 6 years.

Table 2 reports two statistics on the age of models: The age of a model, with the relevant date of introduction being the first time it appears in any country, and the "market age", which reports the number of months a model has been sold in a specific market (country).[6] The table 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, but does not vary within a year. Thus, the best selling refrigerator in Germany in a given year has a rank one, and so do the best-sellers in

[6] The longest-lived categories are the ones typically sold as part of kitchens – hoods, cooktops, and cookers, and the shortest-lived are washing machines but differences are small.

Figure 2: COMPOSITION OF UNIT SALES BY NUMBER OF COUNTRIES IN WHICH MODELS ARE SOLD



Note: The figure depicts the development over time in the share of units sold (percent from total units) of models sold in one country, two to five countries, six to ten countries and in eleven or more countries.

the other categories, but the same model may not be a best-seller in France, for example. $R5$ and $R10$ are binary indicators for top-selling models. They equal unity if a model is part of the top 50 or 100 best-selling products within its respective category at least once during its life cycle.[7]

The second panel of Table 2 describes the reduced sample of models sold contemporaneously in a minimum of two countries. Even though models marketed in multiple countries constitute on average a little less than one third of all models, within a year they comprise 51% of all units sold and generate 58% of sales on average.

Compared to 2004-2006, the general trend is that models are traded in an increasing number of

[7]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. They are also 30% cheaper, on average, and sell 6 times more units per month (average price in euro 402 (s.e. 233), average sales 157 units (s.e. 356)) relative to models whose rank never exceeds 50 (average price 561 (s.e. 424) and average units 27 (s.e. 73).

countries over time. Figure 2 reports the changing composition of sales disaggregated by number of countries in which products are marketed. While models 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 models sold in two to five countries steadily taking over. This particular feature of the data means that the consumption tax rate varies not only across countries over time, but – for models sold simultaneously in multiple markets – the tax rate also varies within each cell of observations comprising the sales 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 5.

The GfK data is merged with data on the consumption taxes in the 22 countries under consideration. The baseline VAT rate is the relevant tax rate for white goods in these countries as they are not subject to reduced VAT, zero rating or exemptions.[8] While from 2004 until 2013 the VAT rates in Austria, Belgium, France, Sweden and Denmark remained unchanged, the other 17 EU countries altered the standard rate 33 times, which provides time and within country/within model variation.

The magnitude of the tax rate changes varies from ±1 pp. to ±5 pp., and their frequency varies from one to four reforms per country for the time period of interest. Close to 80% of all tax reforms considered in this paper took place between 2008 and 2013, with the vast majority being tax increases (tax decreases occurred in only 5 instances). Table 3 describes in detail the magnitude of changes in the standard VAT rate, the date of enactment, as well as the date reforms were first announced. For the announcement dates, we rely on official statements by authorities, or if such

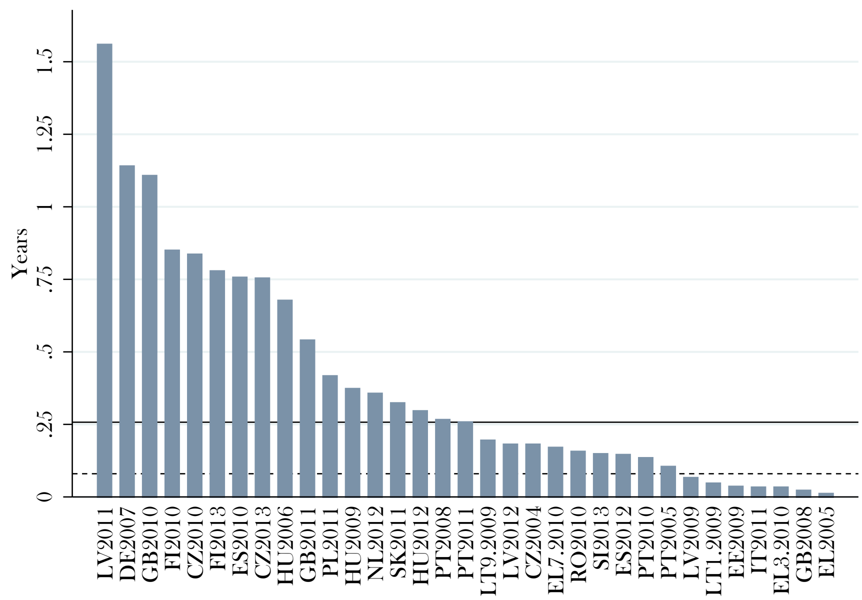
[8] There are non-VAT instruments to stimulate the consumption of energy efficient household goods, summarized in Copenhagen Economics (2008). Some policies are, for example, lump-sum rebates to consumers for the replacement of old household appliances with new ones from a higher energy efficiency class.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

Table 3: STANDARD VAT RATE CHANGES: 2004-2013

Country	Announcement Date	Implementation Date	Tax 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.

Source: Rates and implementation dates are from Ernst & Young, European Commission, and KPMG. 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).

Figure 3: TIME BETWEEN ANNOUNCEMENT AND IMPLEMENTATION



Note: 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 3. 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.

statements were not found, the first time the specific tax reform was mentioned in the media in general.

All thirty three reforms considered in this paper were pre-announced, with substantial heterogeneity in the time between announcement and implementation, *i.e.* the implementation lag. As shown in Figure 3, the implementation lag varies between three days and one and a half years. The median length of the time-interval is a little over a quarter of a year. For seven reforms, 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.[9]

The 2008 UK reform fits well within what the so-called narrative approach to analysing 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 in Greece, enacted as a consequence of fiscal crises and limited access of these governments to international credit markets (Gunter *et al.*, 2017).

[9]The 2008 United Kingdom reform is the only VAT change explicitly announced as temporary. Changes in all other countries were announced as permanent in the sense that there was no explicit commitment to a subsequent policy reversal.

We address the role of policy endogeneity by categorizing the 33 VAT reforms studied in this paper in terms of endogeneity/exogeneity and check if our results are robust to the exclusion of endogenous reforms. To this end, we rely on Gunter *et al.* (2017), who assembled a dataset 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 3 incorporates Gunter *et al.* (2017), adds information on two reforms not classified by these authors, and separates reforms into 18 endogenous and 15 exogenous tax changes.

4 The Cases of Germany and Spain

The above analysis assumes that for all pre-announced reforms, the public is well aware about the forthcoming tax increase/decrease. This section focuses in more detail on Germany and Spain to check this assumption using data on press coverage of the tax reforms. It also shows that a sales and price effects of tax-rate changes are clearly visible in the raw data.

The German VAT increase of 3pp. in 2007 is discussed in detail by D'Acunto *et al.* (2016) 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).[10] 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

[10] Based on Romer and Romer (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.

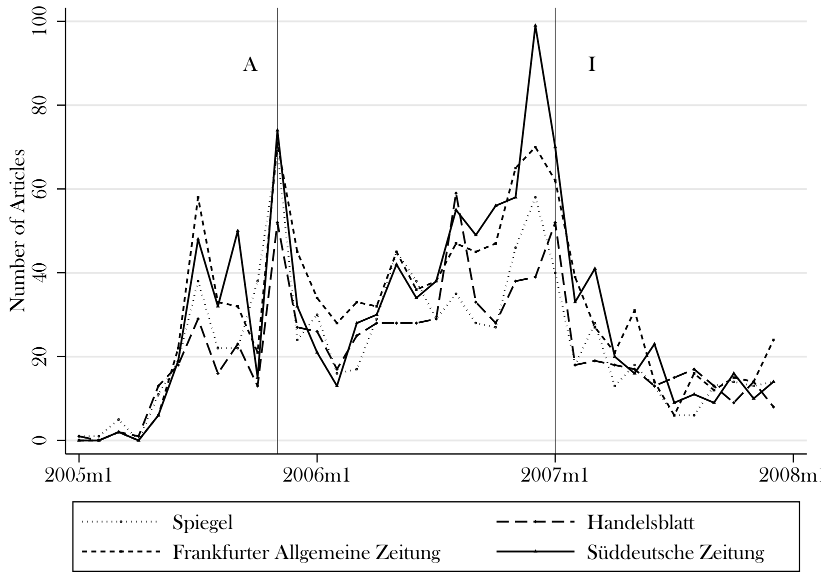
second Spanish VAT increase was only a month and a half.

Figure 4 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 at the announcement date and in the month before the implementation, even though the reform was being discussed continuously throughout 2006. Similarly to Germany, Figure 5 depicts the number of articles discussing the Spanish reforms based on three main newspapers, with the second reform receiving almost double the coverage, given its short announcement and political context.

Figure 6 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 3 percentage point VAT increase in Germany, and a substantial 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.[11] Carare and Danninger (2008), who study the same tax reform episode in Germany, but focus on all CPI items liable to VAT relative to non-VAT goods, report similar pre-reform jump in sales, but find that price increases were phased in during 2006.

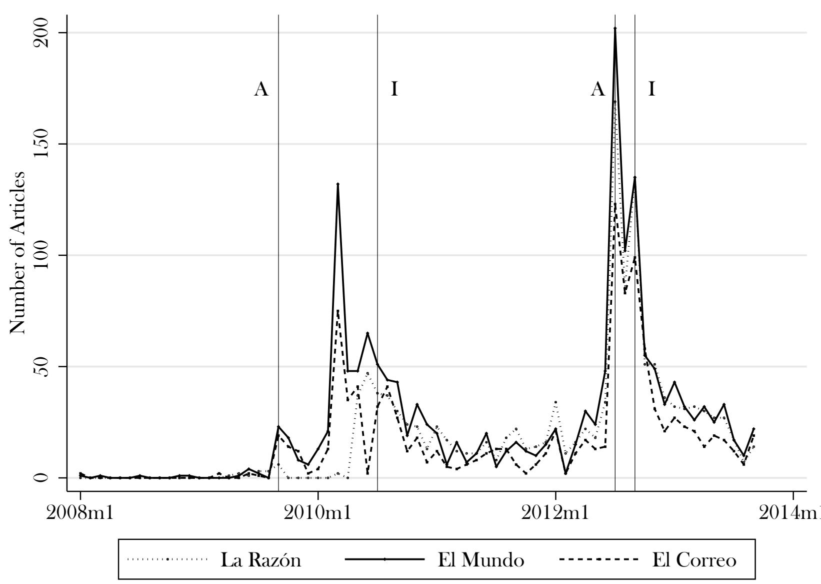
[11] There is a peak in sales also in December 2005, one month after the VAT increase was announced (see also Fig. 6). When we disaggregated the response by categories of models (Figure A-1), we found that three specific categories drive this announcement response: Cooktops, hoods, and cookers, which are usually sold as part of a kitchen unit. Since the data allows us to consider total sales disaggregated by a type of retailer (independent traders, technical superstores and chains, furniture/kitchen specialists, mass merchandisers/DIYs, and from 2006 onwards, internet sales, we were able to determine that the response at the announcement date 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 to their purchases. The dashed black line in Figure A-2 depicts the same growth rate, but cooktops, hoods and cookers are removed. 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.

Figure 4: GERMANY: NEWSPAPER ARTICLES ADDRESSING REFORM, 2005-2007



Note: 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. Authors' calculations using the online archives of Der Spiegel, Handelsblatt, Frankfurter Allgemeine Zeitung and Süddeutsche Zeitung. 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.

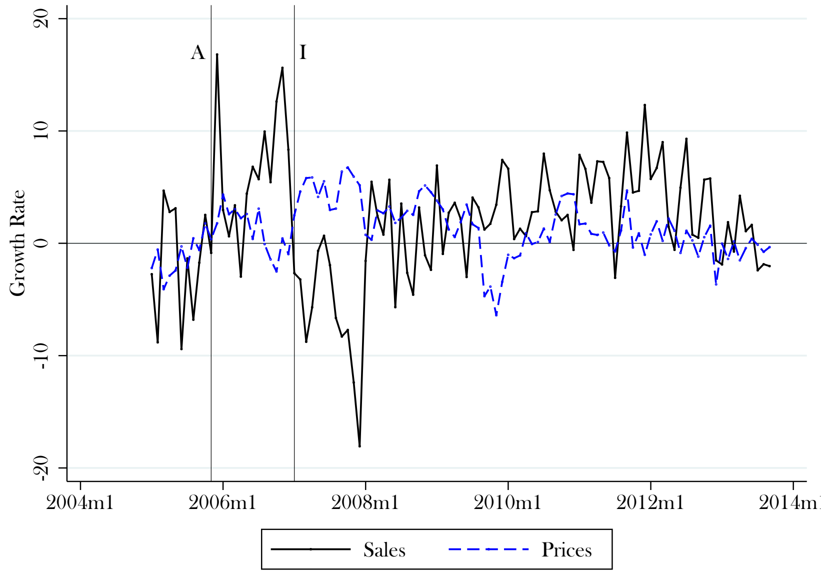
Figure 5: SPAIN: NEWSPAPER ARTICLES ADDRESSING REFORMS, 2008-2013



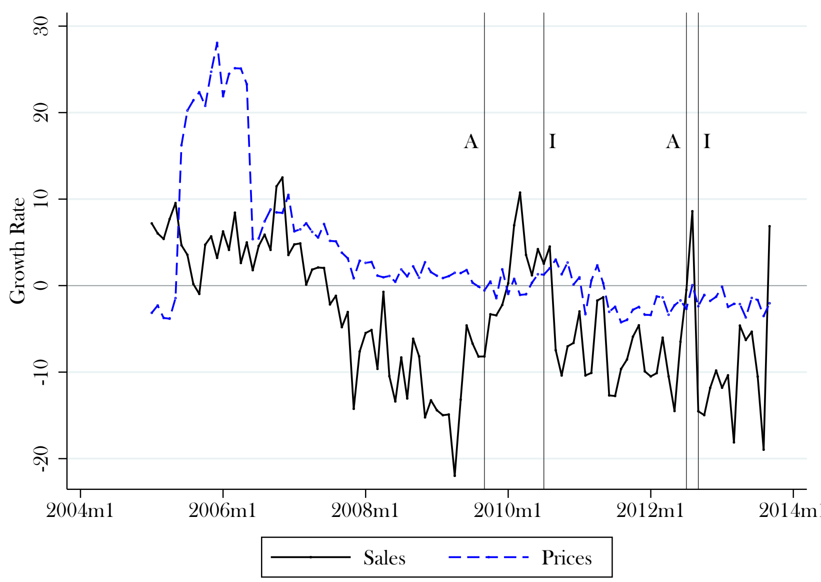
Note: The figure shows the number of articles in three major Spanish newspapers, which mention "VAT rise" either in the title, or the main text from January 2008 until September 2013. Authors' calculations using the online archives of La Razon, El Mundo, El Correo. The search keyword is "VAT rise" ("subida 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.

Figure 6: GROWTH RATE OF SALES AND PRICES

A. Germany



B. Spain



Note: The figure depicts the annual growth rate of sales and prices of white goods (WM, DW, TD, HB, HD, FRZ, RG, CO) in Germany and Spain relative to the same month of the previous year, starting from January 2008 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.

There is a clear upward trend in German sales starting from 2008. Conversely, the market for white goods in Spain shrank considerably from 2007 to 2012 as shown in Panel B. Against this negative trend, the two VAT reforms are associated with temporary pre-reform peaks in sales, without recovery after the first reform. 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.

5 Methodology

A general problem when analyzing broad consumption tax reforms is that changes in standard tax rates apply equally to most goods. Hence, there is no readily available control group of comparable goods, whose tax rate is kept constant. As a consequence, empirical research is often left with the comparison of sales and prices before and after tax-rate changes, which may be affected by many other common shocks. If, however, the sales and prices of a specific good are observed simultaneously in different regions, under certain conditions, the effects of a consumption tax reform in one region can be identified by using as counterfactual outcomes the developments in sales and prices in the other regions in which this good is available. The data for durables described in Section 3 enables us to follow this strategy, since we observe the sales and prices of identical models across EU countries. In addition, the data covers a period characterized by a relatively high frequency of standard VAT rate changes both across and within EU member states.

Under the conventional assumption of full and instantaneous pass-through of the consumption tax into consumer prices, the above theoretical analysis suggests that a tax reform affects the time path of the sales of durables in three periods. In case of a tax increase, for example, the first period affected is the month preceding the tax-rate change, since consumers take advantage of the temporary decline in the user cost of the stock of durable goods and bring purchases forward. Second, in the month of implementation, sales fall due to intertemporal substitution and the reversal of the user cost. Third, in the subsequent month, sales recover. Measuring the rate of change in sales with the log difference of units sold, $\Delta \log(UNITS)_{icd}$, of a model i in country c at date d, we formulate the following basic estimation equation:

$$\begin{aligned} \Delta \log(UNITS)_{icd} &= a_1 \mathsf{F}\Delta\tau_{cd} + b\Delta\tau_{cd} + d_1 \mathsf{L}\Delta\tau_{cd} + aX_{icd} \\ &+ \alpha_{id} + \rho_c + \gamma_{cm(d)} + u_{icd}. \end{aligned} \tag{13}$$

Note that the date d varies by month and year such that each date is associated with a specific month $m(d)$ and year $t(d)$. $\Delta\tau_{cd}$ is the current change in the tax rate relative to the previous month, $\mathsf{F}\Delta\tau_{cd}$ is a lead term, capturing the one-month-ahead change in the tax rate, and $\mathsf{L}\Delta\tau_{cd}$ is the lagged change in the tax rate. Based on the theoretical discussion in Section 2, with full and instantaneous pass-through we expect that $a_1 > 0$, $b < 0$ and and $d_1 > 0$, where the absolute value of the sum $|a_1 + b + d_1|$ is the intertemporal elasticity of substitution.

α_{id} denotes a model-date-specific fixed effect that absorbs any model specific movements in sales. Incorporating a model fixed effect is essential since each model has specific features that distinguish it from other competing products in the market. In addition, given technological progress and product innovation, the (relative) quality of a model and, hence, its attractiveness to consumers vary over time. This is reflected in the striking product-cycle patterns displayed in Figure 1. Inclusion of α_{id} ensures that identification only comes from differences in the growth rate of sales of a model across countries, while quality is kept constant.[12] Identification of the tax effect on sales thus relies on changes in the consumption tax treatment that affect only a sub-group of the observations within each model-date cell. In case of a consumption tax reform in country c, the sales of model i at date d are compared to the sales in all other countries in which model i is available. Consequently, we restrict the sample to models sold in at least two countries at the same time (see Table 2). Note that the model-date-specific fixed effects also capture differences in the size of individual cells driven by the varying number of countries, in which models are traded.

A key assumption for causal interpretation is the *Common-Trend Assumption*, which, in the current setting, requires that, conditional on all controls, had there been no reform in country r, the sales of a model would have followed the same time trend as the sales of this model in the no-reform countries $c \neq r$. As different trends might be associated with the business cycle, X_{icd} includes the monthly unemployment rate as an explanatory variable. In order to deal with differences in seasonality of sales across countries, we include a country-specific fixed effect for each month $\gamma_{cm(d)}$ together with a full set of country-specific fixed effects ρ_c. Differences in trends might also reflect heterogeneity in market entry. Therefore, we employ an indicator for the time period a model has been sold in a specific country: The market age, $m.age$, varies by country within a single model, if this model does not enter all markets at the same time. For the purpose of capturing non-linear model cycle effects, $m.age$ is also entered squared.

As shown by the theoretical analysis, if producer prices vary with the demand for durables, the anticipation of a tax reform could trigger changes in consumer prices and sales in months before $t-1$. In this case, equation (13) would fail to account for systematic effects on pre-reform outcomes,

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

and, hence, could result in a substantial bias (see Malani and Reif, 2015). Similarly, in the presence of producer price effects, the adjustment of sales after a reform might take longer than one month, which could also bias results. For instance, the post-reform recovery of sales may be underestimated.

Since the data provides separate information on prices and quantities, we explore whether and to what extent the data supports complete and immediate pass-through into prices. To this end, we follow the same estimation strategy and use differences in outcomes within a model-date group to identify tax effects. In order to test for pre-reform pass-through of tax changes into consumer prices and for lagged consumer price adjustments, we consider a specification with higher order leads and lags of the tax-rate change

$$\begin{aligned} \Delta \log(PRICE)_{icd} &= \sum_{j=1}^{P} A_j \mathsf{F}^j \Delta\tau_{cd} + B\Delta\tau_{cd} + \sum_{j=1}^{Q} D_j \mathsf{L}^j \Delta\tau_{cd} + \alpha X_{icd} \\ &+ \alpha_{id} + \rho_c + \gamma_{cm(d)} + \upsilon_{icd}. \end{aligned} \tag{14}$$

$\Delta \log(PRICE)_{icd}$ denotes the difference in the log consumer price of model i in country c in month d relative to the previous month. As before, α_{id}, ρ_c, and $\gamma_{cm(d)}$ denote model-date, country, and country-month-specific fixed effects. P determines the order of lead terms and Q the order of lagged terms of $\Delta\tau_{cd}$. Estimation of eq. (14) enables us to test whether there is full pass-through of consumption taxes into consumer prices and how fast the pass-trough takes place. 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, as pointed out by Benedek *et al.* (2015), can be interpreted as a pass-through elasticity. 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. Instantaneous and complete pass-through is included as a special case, which requires that $P, Q = 0$, and $B = 1$. The presence and magnitude of any pre-reform or post-reform pass-through can be assessed using the statistical significance and the quantitative estimates of the slope parameters of the lead and lag terms.

The optimal width of a window around a reform, or, equivalently, the values of P and Q, could be selected via statistical testing that checks whether a gradual extension of the specification with wider windows of tax effects provides a better fit.[13] 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 take account of the information set of agents, at least in a stylized way, we utilize the announcement dates reported in Table 3 and replace j-period ahead lead terms, $\mathsf{F}^j \Delta\tau_{cd}$, with their expected values $\mathsf{E}_{d-j}\left[\mathsf{F}^j \Delta\tau_{cd}\right]$ 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 reform is announced n months in advance, we set $\mathsf{E}_{d-j}\left[\mathsf{F}^j \Delta\tau_{cd}\right] = 0,\ \forall j > n$, whereas $\mathsf{E}_{d-j}\left[\mathsf{F}^j \Delta\tau_{cd}\right] = \mathsf{F}^j \Delta\tau_{cd},\ \forall j \leq n$. While this ensures that the specification fully reflects publicly available information about upcoming reforms, the estimation of higher-order-lead terms rests on a declining number of identifying reforms (and countries) due to the varying length of the implementation lags as shown in Fig. 3. As a consequence, estimates of the pre-reform response are likely biased due to composition effects. Table A-3 lists the different number of reforms and countries identifying the coefficients of up to 14 leads. Clearly, even a pre-reform window as short as three months involves a changing set of reforms across the three leads. In order to mitigate possible biases from composition effects, the empirical analysis employs a parsimonious specification with a

[13] An alternative statistical procedure starts out with a long window and tests whether the window size can be reduced. This is, for example, the strategy employed by Benedek *et al.* (2015) who consider a two-year time span cantered around the month of implementation.

limited number of lead terms. Figure 3 shows that 55% of all reforms were announced at most a quarter of a year before their entry into force. Therefore, we restrict P to three months, or less.

Seven out of the 33 VAT reforms analyzed in this paper were announced less than a month before the tax rate change became effective (Figure 3). In these cases, one can neither distinguish the income effect associated with the announcement from the effect on the consumption path, nor clearly separate the effects of government policy from those of the economic shocks that may have triggered the government intervention in the first place. For this reason, we also provide results of specifications excluding these reforms.

In light of the theoretical analysis, if price pass-through is not instantaneous, we need to extend the basic specification for sales in (13) with further leads and lags of the tax rate change. A generalized reduced-form specification that takes account of the consumer response to producer-price effects is

$$\begin{aligned} \Delta \log(UNITS)_{icd} &= \sum_{j=1}^{p} a_j \mathsf{F}^j \Delta\tau_{cd} + b\Delta\tau_{cd} + \sum_{j=1}^{q} d_j \mathsf{L}^j \Delta\tau_{cd} + aX_{icd} \\ &+ \alpha_{id} + \rho_c + \gamma_{cm(d)} + u_{icd}, \end{aligned} \tag{15}$$

where p and q indicate the number of leads and lags of the tax rate change. The basic specification above is a special case with $p, q = 1$. With $p, q > 1$, the impact of a tax reform would be captured by wider pre- and post-reform windows. In the empirical section below, we generally consider the same pre-reform window range for unit sales as for prices, since, similarly to the price equation, the choice of specification for sales needs to balance between testing for early pre-reform adjustments and biases due to composition effects.

Producer price effects may not only alter the time path of sales: If a single producer serves just two markets, and one market is hit by a consumption-tax-induced demand shock, the country without a tax reform might not serve as a valid control group. The reason is that, given a strong demand shock, the effect on the producer price may trigger opposing demand effects in the other country. More specifically, in the month before the reform, the producer price might increase and hence contribute to a decline in demand in the other country. Similarly, if demand drops in the first period after a consumption tax increase, the producer price might fall and lead to an increase in demand in the no-reform country. These cross-country effects would cause an upward bias in the empirical estimates. An alternative reason for cross-country effects of tax reforms is cross-border shopping. The existence of cross-country effects would violate what is referred to as the *Stable Unit Treatment Value Assumption* in the econometric literature (*e.g.*, Lechner, 2011). To see whether this is a relevant concern, we test sales and price regressions in subsamples with products sold in more than 2, 3, 4 *etc.* markets. In the presence of cross-country effects, the empirical estimates should get smaller in specifications using products sold in an increasing number of countries.

Restricting the sample to products sold in at least two countries for the purposes of identification can lead to estimates that are specific to this group, if single-country models are different from multi-country models. One possibility is that models sold in several countries are likely manufactured by multinationals, of higher quality and more expensive than single-county models, which are either domestically produced, or imported from outside the EU. The incentives to buy before and after consumption tax reforms could, therefore, vary between these two types of models.

As a robustness check, therefore, we employ an alternative approach to identifying the tax effects in equation (13) that enables us to re-incorporate single-country models into the estimation, by using larger cells comprising not just identical models, but a group of models with the same characteristics.[14] Given the limited number of available characteristics, there will be product heterogeneity left in the individual model-group cells. Since this heterogeneity is reflected in different market performance, the estimates are less reliable than a specification that uses cells containing only identical models.

6 Results

6.1 Basic Results

Results from the basic specification of tax effects on sales of durables following eq. (13) are presented in Table 4. The estimation sample includes data for 22 EU countries.[15] We explore the effects of 33 consumption tax reforms that altered the baseline consumption tax rate. As summarized in Table 2, the sample employs data for approximately 72,000 unique models sold in at least two countries, resulting in about 1,330,000 model-date pairs, and a little over 4 million model-date-country observations.

The first column of Table 4 reports estimates from a specification using only model-date and country-specific fixed effects. The second column adds a full set of country-month dummies, which account for country-specific seasonality in sales such as variability in the timing of discount offers, holidays and others. Column (3) additionally controls for the market age, $m.age$, as well as $m.age^2$. These explanatory variables exert significant non-linear effects, with stronger growth in the first months after a model's introduction and slower growth as models get older. As a robustness check,

[14] Apart from the model identifier, the data provides a list of main characteristics for each model. Table A-2 in the Appendix provides a detailed description of all available category-specific features.

[15] 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.

column (4) additionally contains 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 errors for all tax terms.

The point estimates in column (3) indicate that a tax increase by one percentage point causes sales to rise by 2.4% in the last month with a low tax rate. Once the higher tax rate is implemented, sales drop by about 4.4% relative to the month before the reform. These effects are consistent with the theoretical predictions derived under the assumption of full and instantaneous pass-through of taxes into constant producer prices. However, the coefficient of the lagged change in the tax rate is at odds with the theoretical prediction, since it indicates that sales do not recover after the first month. Instead, with a tax-rate increase by 1 percentage point, sales decline further by 1.7% in the month after the tax change. With this caveat in mind, note that the sum of the coefficients for lead, lagged, and contemporaneous tax change effects is close to 4 in the basic specifications. This points to a rather large elasticity of intertemporal substitution in comparison to the existing estimates in the literature (see Cashin and Unayama, 2016).

6.2 Price Effects

The basic estimation for sales presented above rests on the conventional assumption of full and instantaneous pass-through of taxes in consumer prices. To test this assumption and to come up with a more accurate empirical representation of the price response, this subsection studies the dynamics of price adjustment to VAT rate changes.

Table 4: BASIC ESTIMATES

	(1)	(2)	(3)	(4)
$\mathsf{F}\Delta\tau_d$	2.615***	2.444***	2.426***	2.421***
	(0.189)	(0.198)	(0.198)	(0.209)
$\Delta\tau_d$	-3.817***	-4.338***	-4.350***	-4.412***
	(0.206)	(0.210)	(0.210)	(0.221)
$\mathsf{L}\Delta\tau_d$	-2.146***	-1.700***	-1.717***	-1.754***
	(0.199)	(0.208)	(0.208)	(0.219)
unempl	0.001***	0.001***	0.001***	-0.001
	(0.000)	(0.000)	(0.000)	(0.001)
mage			-0.485***	-0.532***
			(0.016)	(0.017)
*mage*2			0.420***	0.468***
			(0.020)	(0.020)
Cons.	-0.030***	-0.236***	-0.159***	-0.128***
	(0.004)	(0.013)	(0.014)	(0.025)
Cumulative Effect				
Total ($a_1 + b + d_1$)	-3.349***	-3.594***	-3.640***	-3.744***
	(0.347)	(0.358)	(0.358)	(0.402)
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
Model-date groups	1,331,154	1,331,154	1,331,154	1,331,154
Models	72,056	72,056	72,056	72,056

Notes: Regressions in columns (1)-(4) are based on data for 22 EU countries and 33 VAT reforms. Data for Slovenia and Slovakia is restricted to after Jan. 1st, 2007, and Jan. 1st, 2009, respectively. Data for Estonia is considered up to Dec. 31st, 2010. The data is restricted to goods sold contemporaneously in at least 2 countries. The dependent variable is the change in the logarithm of sales $\Delta\log(UNITS)$. The lead term, $\mathsf{F}^1\Delta\tau_d$, captures all reforms in the month before their implementation. All specifications include a full set of model-date-specific (id) and country-specific fixed effects. All categories of white goods, namely WM, TD, DW, HB, HD, CO, FRZ, RG, are covered. The monthly change in the standard VAT rate is denoted as $\Delta\tau_d$. *unempl* is the monthly unemployment rate. $m.age$ is the number of months a model appears in the data in a specific country. Standard errors are robust in all specifications and clustered by model-date cells. Standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: PRICE EFFECTS

Reforms	All			All		$n \geq 1$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\mathsf{F}^3\Delta\tau_d$			0.154^{***}				
			(0.021)				
$\mathsf{F}^2\Delta\tau_d$			-0.104^{***}				
			(0.022)				
$\mathsf{F}\Delta\tau_d$		0.116^{***}	0.116^{***}				
		(0.020)	(0.020)				
$\mathsf{E}\left[\mathsf{F}^3\Delta\tau_d\right]$					0.220^{***}		0.250^{***}
					(0.025)		(0.025)
$\mathsf{E}\left[\mathsf{F}^2\Delta\tau_d\right]$					0.045^{**}		0.034
					(0.022)		(0.022)
$\mathsf{E}\left[\mathsf{F}\Delta\tau_d\right]$				0.126^{***}	0.126^{***}	0.141^{***}	0.142^{***}
				(0.021)	(0.021)	(0.022)	(0.022)
$\Delta\tau_d$	0.219^{***}	0.219^{***}	0.217^{***}	0.219^{***}	0.218^{***}	0.164^{***}	0.163^{***}
	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.023)	(0.023)
$\mathsf{L}\Delta\tau_d$		0.388^{***}	0.387^{***}	0.389^{***}	0.389^{***}	0.432^{***}	0.431^{***}
		(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)
$\mathsf{L}^2\Delta\tau_d$			-0.126^{***}		-0.128^{***}		-0.125^{***}
			(0.022)		(0.022)		(0.022)
$\mathsf{L}^3\Delta\tau_d$			0.086^{***}		0.086^{***}		0.104^{***}
			(0.023)		(0.023)		(0.024)
	Cumulative effects:						
Total	0.219^{***}	0.723^{***}	0.730^{***}	0.734^{***}	0.956^{***}	0.737^{***}	0.999^{***}
	(0.022)	(0.037)	(0.057)	(0.038)	(0.059)	(0.039)	(0.061)
Pre-reform		0.116^{***}	0.166^{***}	0.126^{***}	0.391^{***}	0.141^{***}	0.426^{***}
		(0.020)	(0.037)	(0.021)	(0.039)	(0.022)	(0.040)
Post-reform		0.608^{***}	0.564	0.608^{***}	0.565^{***}	0.596^{***}	0.573^{***}
		(0.031)	(0.045)	(0.031)	(0.045)	(0.032)	(0.046)
Pass-through $F(1)$		54.8^{***}	22.2^{***}	50.2^{***}	0.55	45.8^{***}	0.00
N	4,032,508	4,032,508	4,032,508	4,032,508	4,032,508	3,916,700	3,916,700
Model-date groups	1,302,880	1,302,880	1,302,880	1,302,880	1,302,880	1,275,887	1,275,887
Models	71,223	71,223	71,223	71,223	71,223	70,663	70,663

Notes: Regressions are based on data for 22 EU countries and up to 33 VAT reforms. The dependent variable is the change in the logarithm of the actual consumer price $\Delta\log(PRICE)$. Data for Slovenia and Slovakia is restricted to after Jan. 1st, 2007, and Jan. 1st, 2009, respectively. Data for Estonia is considered up to Dec. 31st, 2010. 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. Note that $\mathsf{E}_{d-j}\left[\mathsf{F}^j\Delta\tau_d\right] = \mathsf{F}^n\Delta\tau_d$ for all reforms that were announced $n > j$ periods ahead, and $\mathsf{E}_{d-j}\left[\mathsf{F}^j\Delta\tau_d\right] = 0$ for reforms announced $n \leq j$. All specifications include a full set of model-date, country, and country-month fixed effects. All categories of white goods, namely WM, TD, DW, HB, HD, CO, FRZ, RG, are covered. The monthly change in the standard VAT rate is denoted as $\Delta\tau_d$. The monthly unemployment rate, $unempl$, and the number of months a model appears in the data in a specific country, $m.age$, as well as $m.age^2$ are controlled for not reported. Standard errors are robust in all specifications and clustered by model-date cells. Standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5 reports results of a regression of the monthly (log) change in consumer prices on tax-rate changes following eq. (14) 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 4. Column (1) reports a contemporaneous tax effect of a 0.22% rise in prices per 1 percentage point tax increase, clearly rejecting the null hypothesis of full and instantaneous pass-through.

Column (2) includes the tax-rate changes in the preceding as well as in the following month, with both coefficients being significantly positive. The magnitude and statistical significance of the leading and lagged terms indicate that the pass-through for major domestic appliances starts before a reform becomes effective and continues for sometime after implementation. The cumulative effect, as reported in the lower portion of the table, suggests that within these three months, about three quarters of the tax-rate change is shifted onto the consumer. According to the corresponding F-statistic, full pass-through can still be rejected. Widening the window to three months 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.

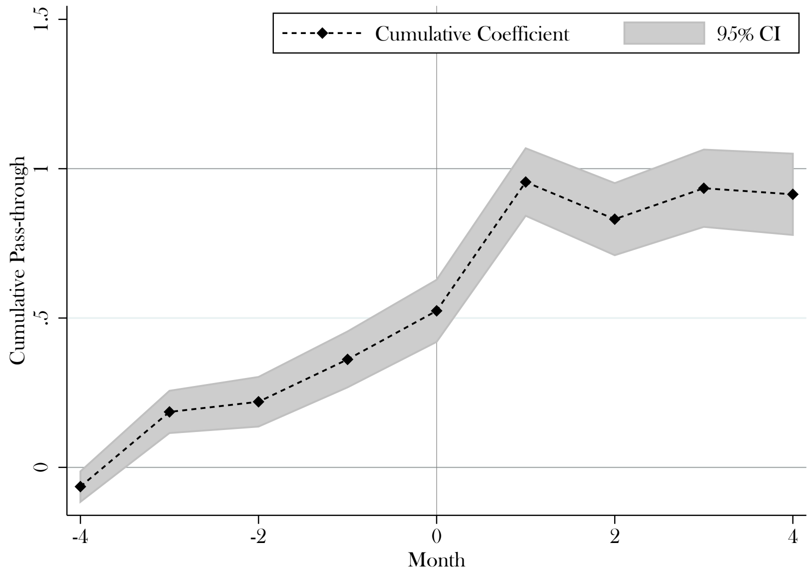
The specifications in columns (2) and (3) employ forward terms of tax-rate changes ignoring differences across reforms stemming from the timing between announcement and implementation. As discussed above, in some cases, this means that the estimation uses information on tax policy that, in fact, was not available to consumers and producers. 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 around implementation, the estimated magnitude of the total pass-through, 73%, is not statistically different from the case with no announcements. However, the ex-ante price adjustment rises to 39% once a longer window is employed, with all leading terms now exhibiting larger and consistently positive coefficients as compared to specification (3). As the post-reform pass-through implied by this specification is 57%, the cumulative price effect is not significantly different from unity with an F-statistic of 0.55. 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 pre-reform pass-through estimate in column (5) relative to column (3) clearly highlights the importance of the announcement information: Despite a sufficiently long window, the specification in (3) would point to a much smaller pre-reform response of prices.

Columns (6) and (7) drop observations associated with reforms pre-announced by less than a month, since the pre-reform adjustment may capture the effect of the announcement itself, as well as the effect of the economic circumstances that have motivated the government to act quickly.[16] The pre-reform pass-through effects are found to be qualitatively and quantitatively similar to those reported in columns (3) and (4). The estimates in column (7) indicate that a tax rate increase by one 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. Again, the cumulative price effect in this specification is not significantly different from unity, which is consistent with full but not instantaneous price pass-through.

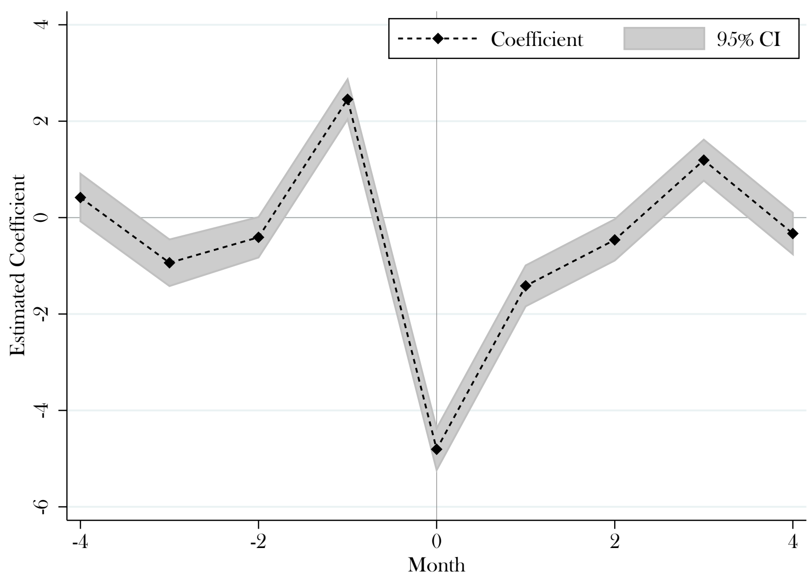
A closer inspection of column (7) of Table 5 as well as of the cumulative graphical representation of the coefficients of a regression including four leads and lags of the tax rate change in panel A of Figure 7, reveals that price pass-through starts a quarter prior to a reform and is already completed

[16] Observations are dropped six months before and six months after implementation for models in the relevant countries and years, without removing the model from the data in non-reform years, or its sales in other countries.

Figure 7: DYNAMICS OF SALES AND PRICES AROUND VAT REFORMS
A. Cumulative Pass-through



B. Estimated Leading and Lagged Coefficients for Sales



Note: Panel A depicts the cumulative sum of the estimated coefficients in a price regression including four leads and lags of the percentage change of the VAT rate, while Panel B depicts the estimated coefficients from an identical regression for sales (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.

by the second month after implementation. All in all, it takes five months for the tax-rate change to be fully reflected in consumer prices.

The pass-through estimates are 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 (7) of Table 5, Table A-4 in the Appendix gradually restricts 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 model-date cell. The null hypothesis of full cumulative pass-through in the long-run cannot be rejected, even when the sample is down to 6,600 products traded in at least eight countries.

6.3 Sales Effects

Given the findings in the previous section, the data supports full price pass-through in accordance with the conventional view in the literature. However, the price pass-through is not instantaneous, and, in particular, prices start to rise before a tax reform. This implies that the pre-reform response of sales might not be confined to the last period before the reform. In addition, the recovery of demand might not take place immediately after implementation. To test for implications of non-instantaneous pass-through, Table 6 extends the basic reduced-form specification in (13) with additional leads and lags of the tax rate change.

All specifications include a full set of model-date, country- and country-month fixed effects and the same controls as in Column (3) of Table 4. For convenience, Column (1) repeats the results of this specification. Column (2) uses a wider window of three months before and after the tax rate change, with the results pointing at a drop in purchases a quarter prior to a reform, which conforms with a pre-reform pass-through of taxes. With regard to lagged terms, demand continues to decline in the second month after the tax rate change, but the third month shows a recovery.

The specifications in Columns (3) and (4) employ the expected rather than the actual tax-rate change to capture any pre-reform effects. While the post-reform response is quite similar, the coefficients for the pre-reform effects are slightly smaller. As noted above, however, the observations encompass reforms for which the time between announcement and implementation is less than a month. In these cases, the expected tax-rate change also captures an announcement effect and estimates may also be confounded by specific economic events that caused quick government action. For this reason, Columns (5)-(6) report estimates obtained after dropping observations before and after these reforms. The results point to a stronger loss in sales at the month of implementation, but a weaker decline in the following month. Moreover, in the month before the tax-rate change, we find a slightly stronger positive effect.

To 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. Visually, the estimated coefficients from a regression adding a fourth lead and lag are displayed in Panel B of Figure 7.

The identification of tax effects in our analysis relies on differences in the consumption tax treatment of a model across countries. As noted in Section 5, this strategy might be invalidated by cross-country effects on producer prices. To check for the presence of such effects, we test our specifications in subsamples of products sold in more than just two markets. Table 7 shows estimation results obtained 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 model-date cells. Column (1) removes products traded in

Table 6: SALES EFFECTS

Reforms	All		All		n $\geq$ 1	
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathsf{F}^3\Delta\tau_d$		-0.850***				
		(0.206)				
$\mathsf{F}^2\Delta\tau_d$		-0.218				
		(0.202)				
$\mathsf{F}\Delta\tau_d$	2.426***	2.397***				
	(0.198)	(0.198)				
$\mathsf{E}\left[\mathsf{F}^3\Delta\tau_d\right]$				-0.833***		-0.937***
				(0.242)		(0.246)
$\mathsf{E}\left[\mathsf{F}^2\Delta\tau_d\right]$				-0.331		-0.416*
				(0.209)		(0.214)
$\mathsf{E}\left[\mathsf{F}\Delta\tau_d\right]$			2.349***	2.320***	2.464***	2.455***
			(0.202)	(0.202)	(0.212)	(0.212)
$\Delta\tau_d$	-4.350***	-4.357***	-4.351***	-4.358***	-4.797***	-4.806***
	(0.210)	(0.210)	(0.210)	(0.210)	(0.221)	(0.221)
$\mathsf{L}\Delta\tau_d$	-1.717***	-1.689***	-1.716***	-1.702***	-1.432***	-1.417***
	(0.208)	(0.208)	(0.208)	(0.208)	(0.216)	(0.216)
$\mathsf{L}^2\Delta\tau_d$		-0.456**		-0.453**		-0.452**
		(0.208)		(0.208)		(0.218)
$\mathsf{L}^3\Delta\tau_d$		1.197***		1.198***		1.193***
		(0.210)		(0.210)		(0.217)
			Cumulative Effects			
Total	-3.640***	-3.976***	-3.717***	-4.159***	-3.765***	-4.379***
	(0.358)	(0.546)	(0.360)	(0.567)	(0.378)	(0.591)
Pre-reform	2.426***	1.329***	2.349***	1.156***	2.464***	1.102***
	(0.198)	(0.351)	(0.202)	(0.381)	(0.212)	(0.391)
Post-reform	-6.066***	-5.304***	-6.067***	-5.315***	-6.229***	-5.481***
	(0.296)	(0.421)	(0.296)	(0.421)	(0.310)	(0.441)
N	4,126,760	4,126,760	4,126,760	4,126,760	4,006,044	4,006,044
Model-date groups	1,331,154	1,331,154	1,331,154	1,331,154	1,302,736	1,302,736
Models	72,056	72,056	72,056	72,056	71,492	71,492

Notes: Regressions are based on data for 22 EU countries and up to 33 VAT reforms. The dependent variable is the change in the logarithm of sales, $\Delta\log(UNITS)$. Data for Slovenia and Slovakia is restricted to after Jan. 1st, 2007, and Jan. 1st, 2009, respectively. Data for Estonia is considered up to Dec. 31st, 2010. 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. Note that $\mathsf{E}_{d-j}\left[\mathsf{F}^j\Delta\tau_d\right] = \mathsf{F}^n\Delta\tau_d$ for all reforms that were announced $n > j$ periods ahead, and $\mathsf{E}_{d-j}\left[\mathsf{F}^j\Delta\tau_d\right] = 0$ for reforms announced $n \leq j$. All specifications include a full set of model-date-, country-, and country-month-specific fixed effects. All categories of white goods, namely WM, TD, DW, HB, HD, CO, FRZ, RG, are covered. The monthly change in the standard VAT rate is denoted as $\Delta\tau_d$. The monthly unemployment rate, *unempl*, and the number of months a model appears in the data in a specific country, $m.age$, as well as $m.age^2$ are controlled for but not reported. Standard errors are robust in all specifications and clustered by model-date cells. Standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

only two countries, Column (3) further drops products sold in 3 countries, and so on until Column (6), which looks at products sold in 8 or more countries at the same time. Although the estimated total effects are slightly higher, mainly due to larger post-reform decline in sales, in general, the estimates are similar to the benchmark results presented in Column (6) of Table 6, which leads us to conclude that it is unlikely that cross-country effects are driving the results.

Table A-5 in the Appendix performs another robustness check on whether the estimates using models traded in at least two countries can be extrapolated to the full sample. It employs an alternative estimation approach, which, as explained in Section 5, groups observations not based on the model identifier, but based on model characteristics. In this manner, single-county models can enter the estimation as they are grouped together with other models having an identical set of characteristics.[17] Despite heterogeneity within model-group-date cells as demonstrated by larger standard errors, the estimates in Table A-5 remain very close to those reported in Table 6.

Across all specifications, the estimates point to pre-reform effects not only in sales, but also in prices. The pre-reform producer price effects might be explained by various kinds of adjustment costs. If price adjustment is costly, and therefore, infrequent, firms may start altering prices as early as the VAT change announcement (Carare and Danninger, 2008). Adjustment cost may also occur with changes in production capacity. Rather than fully adjusting production capacity in response to temporary demand changes around a tax-reform, producers with adjustment cost[T] facing an upcoming tax-rate increase, for instance, would rather deplete inventories. [18] As a consequence, even if the pre-reform change in demand is expected, producer prices might vary with the short-term

[17]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.

[18] A similar point is made by Copeland and Kahn, (2013) who study a temporary subsidy for car sales provided by the 2009 US vehicle scrappage program and find that inventories buffered the movement in sales.

Table 7: SALES EFFECTS: INCREASING NUMBER OF CONTROL COUNTRIES IN THE MODEL-DATE CELLS

	(1) $c \geq 3$	(2) $c \geq 4$	(3) $c \geq 5$	(4) $c \geq 6$	(5) $c \geq 7$	(6) $c \geq 8$
$\mathsf{E}\left[\mathsf{F}^3\Delta\tau_d\right]$	-0.922***	-1.112***	-1.145***	-1.106***	-1.233***	-0.957**
	(0.265)	(0.286)	(0.318)	(0.352)	(0.385)	(0.433)
$\mathsf{E}\left[\mathsf{F}^2\Delta\tau_d\right]$	-0.689***	-0.644***	-0.768***	-0.775***	-1.008***	-1.034***
	(0.227)	(0.243)	(0.263)	(0.287)	(0.317)	(0.354)
$\mathsf{E}\left[\mathsf{F}\Delta\tau_d\right]$	2.794***	2.924***	2.967***	3.081***	3.382***	3.508***
	(0.228)	(0.247)	(0.269)	(0.294)	(0.320)	(0.357)
$\Delta\tau_d$	-4.635***	-4.799***	-4.789***	-4.723***	-4.674***	-4.394***
	(0.232)	(0.246)	(0.262)	(0.282)	(0.309)	(0.342)
$\mathsf{L}\Delta\tau_d$	-1.655***	-1.924***	-2.143***	-2.306***	-2.287***	-2.216***
	(0.232)	(0.249)	(0.271)	(0.296)	(0.324)	(0.359)
$\mathsf{L}^2\Delta\tau_d$	-0.419*	-0.365	-0.284	-0.169	-0.383	-0.193
	(0.229)	(0.242)	(0.259)	(0.279)	(0.308)	(0.339)
$\mathsf{L}^3\Delta\tau_d$	1.172***	0.989***	0.850***	0.917***	0.842**	0.712*
	(0.234)	(0.252)	(0.274)	(0.303)	(0.334)	(0.372)
			Cumulative Effects			
Total	-4.353***	-4.931***	-5.311***	-5.080***	-5.362***	-4.573***
	(0.631)	(0.676)	(0.734)	(0.803)	(0.882)	(0.983)
Pre-reform	1.183***	1.168***	1.054**	1.200**	1.141*	1.518**
	(0.419)	(0.452)	(0.495)	(0.543)	(0.596)	(0.667)
Post-reform	-5.536***	-6.095***	-6.366***	-6.281***	-6.503***	-6.091***
	(0.468)	(0.500)	(0.538)	(0.586)	(0.644)	(0.713)
N	3,255,452	2,611,985	2,115,467	1,700,080	1,359,930	1,074,686
Model-date groups	927,440	656,984	475,835	344,538	250,059	180,918
Models	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 sales, $\Delta \log(UNITS)$. 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. All specifications include a full set of model-date-, country- and country-month-specific fixed effects. All categories of white goods, namely WM, TD, DW, HB, HD, CO, FRZ, RG, are covered. The monthly change in the standard VAT rate is denoted as $\Delta\tau_d$. The monthly unemployment rate, *unempl*, and the number of months a model appears in the data in a specific country, $m.age$, as well as $m.age^2$ are controlled for but not reported. Standard errors are robust in all specifications and clustered by model-date cells. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

fluctuation in demand.

An alternative explanation rests on imperfect 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 explains pre-reform price pass-through, we should expect that its extent varies depending on models' market power. Market leaders may exhibit divergent pass-through and sales patterns around a consumption tax reform as compared to goods that sell fewer units. As discussed in Section 3, we create binary indicators for market power using the within year, within category, and within country ranking of models on the basis of their volume of sales. In particular, the dummy variables $R5(R10)$ equal one for all models that reach ranks between one and fifty (one and hundred) in at least one year throughout their life-cycle.

Based on this separation of products, Table A-6 in the Appendix reports results of specifications extending equations (13) and (14) by adding the $R5$ or $R10$ dummies and their interactions with all leads, lags as well as the contemporaneous term. Once announcement information is taken into account, the results point to small differences between best-selling and other models. While, no significant effects are detected for the sales response, the pre-reform price pass-through of top-selling models is found to be larger. This effect, however, is only weakly significant.

6.4 Exogenous Reforms

Table 8 reports results only for tax reforms classified as exogenous (see Table 3). Given a median implementation lag of three months, all observations for the sales and prices of models 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. This ensures that the immediate, and

Table 8: EXOGENOUS TAX REFORMS

Dependent variable	$\Delta\log(PRICE)$			$\Delta\log(UNITS)$		
Reforms	All		n $\geq$ 1	All		n $\geq$ 1
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathsf{F}^3\Delta\tau_d$	-0.011			-0.207		
	(0.038)			(0.368)		
$\mathsf{F}^2\Delta\tau_d$	0.234***			0.786*		
	(0.045)			(0.418)		
$\mathsf{F}\Delta\tau_d$	0.014			2.480***		
	(0.038)			(0.357)		
$\mathsf{E}\left[\mathsf{F}^3\Delta\tau_d\right]$		0.002	0.001		-0.235	-0.252
		(0.039)	(0.039)		(0.377)	(0.377)
$\mathsf{E}\left[\mathsf{F}^2\Delta\tau_d\right]$		0.230***	0.230***		0.364	0.343
		(0.046)	(0.046)		(0.425)	(0.426)
$\mathsf{E}\left[\mathsf{F}\Delta\tau_d\right]$		0.041	0.045		2.485***	2.469***
		(0.038)	(0.038)		(0.359)	(0.358)
$\Delta\tau_d$	0.170***	0.170***	0.166***	-4.563***	-4.563***	-4.806***
	(0.044)	(0.044)	(0.045)	(0.419)	(0.419)	(0.428)
$\mathsf{L}\Delta\tau_d$	0.362***	0.362***	0.359***	-1.491***	-1.488***	-1.079***
	(0.036)	(0.036)	(0.036)	(0.354)	(0.355)	(0.356)
$\mathsf{L}^2\Delta\tau_d$	-0.017	-0.017	-0.013	-0.153	-0.149	-0.256
	(0.041)	(0.041)	(0.042)	(0.409)	(0.409)	(0.420)
$\mathsf{L}^3\Delta\tau_d$	0.073*	0.073*	0.078**	1.221***	1.222***	1.211***
	(0.038)	(0.038)	(0.039)	(0.362)	(0.362)	(0.363)
Cumulative Effects						
Total	0.824***	0.861***	0.867***	-1.927*	-2.364**	-2.369**
	(0.106)	(0.107)	(0.108)	(1.016)	(1.023)	(1.033)
Pre-reform	0.237***	0.273***	0.277***	3.059***	2.613***	2.560***
	(0.070)	(0.071)	(0.072)	(0.662)	(0.705)	(0.673)
Post-reform	0.587***	0.588***	0.590***	-4.986***	-4.977***	-4.929***
	(0.080)	(0.080)	(0.082)	(0.774)	(0.774)	(0.786)
Pass-through F(1)	2.77*	1.70	1.51			
N	3,633,795	3,633,795	3,589,523	3,724,133	3,724,133	3,676,201
Model-date groups	1,200,757	1,200,757	1,189,120	1,228,615	1,228,615	1,215,792
Models	69,614	69,614	69,277	70,455	70,455	70,118

Notes: Regression results in columns (1) to (6) are based on data for 22 EU countries. The dependent variable in columns (1) to (3) is the change in the logarithm of price, $\Delta\log(PRICE)$, and in columns (4) to (6) it is the change in the logarithm of sales, $\Delta\log(UNITS)$. Observations up to two quarters before and after reforms classified as endogenous(see Table 3) are removed from the estimation. Estimates in columns (3) 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. All specifications include a full set of model-date-, country- and country-month-specific fixed effects. All categories of white goods, namely WM, TD, DW, HB, HD, CO, FRZ, RG, are covered. The monthly change in the standard VAT rate is denoted as $\Delta\tau_d$. The monthly unemployment rate, *unempl*, and the number of months a model appears in the data in a specific country, $m.age$, as well as $m.age^2$ are controlled for but not reported. Standard errors are robust in all specifications and clustered by model-date cells. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

any pre- and post-reform effects are removed from the estimation sample.[19]

The first three columns of Table 8 show results for prices, which are qualitatively similar to the results presented in section 6.2. When the timing of announcements is taken into account, full price pass-through cannot be rejected, and about a third of the price change takes place before the implementation. However, price adjustment occurs within a shorter time period: The price change starts two instead of three months before the reform, 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 Column (2) as opposed to the considerably different results with and without announcements in Table 5.

Columns (4) to (6) report the corresponding sales effects. Similarly to prices, we find that the sales responses take place in a narrower time interval. Pre-reform effects are concentrated in the last-period before the reform and, cumulatively, are sizeably larger than in the equivalent specifications in Table 6. Conversely, the cumulative post-reform effect is found to be slightly smaller. The recovery in sales is of similar magnitude and shows up three month after the reform, as before. Taken together, the results for exogenous reforms point to a stronger temporary shift in consumer demand and a smaller long-term effect. The point estimates suggest that a tax reform, which exogenously raises the tax rate by one percentage point triggers a temporary growth of sales by two and a half percent in the month preceding the reform. The long-term effect on sales is a drop by about 2.4% percent.

[19] For example, a model 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.

7 Conclusions

Interest in a fiscal policy using consumption taxes to stimulate consumer demand has recently increased. Theory suggests that an upcoming tax-rate change would induce consumers to bring forward or post-pone spending. In addition, the demand for consumer durables is expected to display a further temporary shock immediately before implementation. The empirical analysis provided in this paper supports these views and shows that the changes of baseline consumption tax rates in the EU countries in recent years have in fact exerted strong effects on the time path of consumer spending.

We utilize a unique micro data set for consumer durables that allows us to distinguish between the monthly purchases of individual products and their consumer prices in 22 European countries over the last decade, which has seen numerous changes in consumer tax rates. We implement a reduced-form specification for sales that tests theoretical predictions by a standard model of consumer choice under different assumptions about the consumer price effects of taxes. To identify tax effects empirically, we explore how prices and sales of individual products differ in countries where the baseline consumption tax-rate is changed relative to countries where taxes remain unchanged. Separate analysis of the number of units sold of individual products and their prices allows us to provide evidence on tax effects that are not confounded by shifts between products of different quality.

While the price data clearly supports the full pass-through of consumption taxes as typically assumed in the theoretical literature, the results indicate that about a third of the pass-through takes place pre-reform, more specifically in the last quarter before implementation. Price pass-through is completed in the first quarter after implementation. Given the finding of full price pass-through, it is difficult to explain the pre-reform adjustment in prices with imperfect competition. Robustness checks also did not detect major differences between the group of top-sellers and other models. Hence, the pre-reform price effects are most likely a consequence of adjustment costs faced by producers. The empirical results for sales confirm the theoretical predictions under full price pass-through. Purchases differ substantially before and after the tax-rate change and this difference is not just offsetting the short-run demand effect for durables on the verge of the tax reform but points to a strong intertemporal shift in total consumer demand. The basic results suggest that an increase of an ad-valorem consumption tax rate by one percentage point causes an intertemporal shift of consumption by up to 4 percent. In addition, purchases in durable goods are found to increase temporarily by about 2 percent in the last month before the tax increase.

In the standard models of consumer choice with forward-looking consumers who have full access to the capital market, these results imply a sizeable intertemporal elasticity of consumption. Yet our analysis also shows that the consumer response differs between reforms even if differences in the implementation lag after announcement are taken into account. More specifically, following the narrative approach to the analysis of fiscal policy and distinguishing reforms by their motivation, we find that exogenous consumption tax reforms, which are unrelated to GDP shocks, exert smaller effects on consumer spending than results from specifications including all reforms. Reforms implemented by countries facing limited access to capital markets or forced to reduce cyclical deficits in order to meet the EU's fiscal policy requirements, elicit stronger consumptions responses. This seems to be in line with evidence of stronger effects of fiscal policy in a recessionary economic environment (*e.g.*, Auerbach and Gorodnichenko, 2012). However, in the presence of GDP shocks, reform effects may be confounded by economic developments. Focusing on exogenous reforms, we find that the price-pass through is quicker, the temporary shift in the demand for durables is more pronounced, and the long-term intertemporal shift in consumption is smaller. Based on these results, a one percentage point increase in consumption taxes causes a long-term shift in consumption of more than 2 percent. This strong intertemporal shift supports the basic potential of using pre-announced tax increases as an instrument to stimulate current consumption. The sharp drop in sales after tax increases come into force, however, highlights the risks of possible adverse effects on future consumer spending.

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A.1 Household Optimization

The Lagrangian for the intertemporal optimization problem is

$$\mathcal{L} \;=\; \sum_{s=t}^{\infty}\Big\{\beta^{s-t}\frac{\sigma}{\sigma-1}u_s^{1-\frac{1}{\sigma}}+\lambda_{s+1}\beta^{s-t}\left[(1+r)\,a_s+w_s-(1+\tau_s)\,x_s-p_s\,(k_s-(1-\delta)\,k_{s-1})-a_{s+1}\right]\Big\},$$

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

The first order condition for the consumption of non-durables is

$$u_s^{-\frac{1}{\sigma}}\frac{\partial u_s}{\partial x_s}-\lambda_{s+1}\,(1+\tau_s)\stackrel{!}{=}0, \tag{A.16}$$

where λ_{s+1} represents the marginal utility of income in period s. The condition for the optimal stock of consumer durables is

$$u_s^{-\frac{1}{\sigma}}\frac{\partial u_s}{\partial k_s}-\lambda_{s+1}p_s+(1-\delta)\,\beta p_{s+1}\lambda_{s+2}\stackrel{!}{=}0. \tag{A.17}$$

Assuming that $\beta\,(1+r)=1$, the Euler equation for wealth simplifies to $\frac{\lambda_{s+2}}{\lambda_{s+1}}=1$. Given a CES intra-temporal utility function, $\frac{\partial u_s}{\partial k_s}=bu_s^{\frac{1}{\epsilon}}k_s^{\frac{-1}{\epsilon}}$ and $\frac{\partial u_s}{\partial x_s}=u_s^{\frac{1}{\epsilon}}x_s^{\frac{-1}{\epsilon}}$. Replacing λ_{s+1} (and λ_{s+2}) in (A.17) by using eq. (A.16), and rearranging terms yields eq. (3) with user cost as defined in eq. (4).

Eq. (6) is obtained by replacing λ_{s+2} and λ_{s+1} in the Euler equation $\frac{\lambda_{s+2}}{\lambda_{s+1}} = 1$ using eq. (A.16), which yields

$$\frac{u_{s+1}^{\sigma-\epsilon} x_{s+1}^{-\sigma}}{u_{s+1}^{\sigma-\epsilon} x_s^{-\sigma}} = \left(\frac{1+\tau_{s+1}}{1+\tau_s}\right)^{\sigma\epsilon}$$

Noting that $u_s = x_s \left[1 + b\left(\frac{k_s}{x_s}\right)^{\frac{\epsilon-1}{\epsilon}}\right]^{\frac{\epsilon}{\epsilon-1}}$, using eq. (3) and rearranging terms, yields eq. (6).

Table A-1: FULL SAMPLE: DESCRIPTIVE STATISTICS BY MODEL CATEGORY

	Mean	Std. Dev.	Min	Max
	Average № Models per Year			
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
Cooktops	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
	Average № of Units Sold per Year (Thousands)			
Cookers	8,623	729	6,252	9,206
Fridges	14,067	1,100	10,707	15,018
Dishwashers	6,783	686	5,401	7,431
Freezers	3,836	380	2,631	4,112
Cooktops	5,919	463	4,691	6,342
Hoods	4,948	433	3,714	5,370
Tumble dryers	3,522	414	2,268	3,942
Washing machines	14,728	1,205	11,415	15,654
	Average Value of Sales per Year (Millions Euro)			
Cookers	3,935	389	2,756	4,356
Fridges	6,350	542	4,786	6,887
Dishwashers	3,431	303	2,618	3,659
Freezers	1,354	118	980	1,445
Cooktops	2,192	191	1,729	2,352
Hoods	1,260	110	986	1,356
Tumble dryers	1,430	151	1,035	1,601
Washing machines	6,184	500	4,643	6,577
	Average Market Age [Average Months Model Sold]			
Cookers	27.6 [30.8]	22.6 [23.4]	1	117
Fridges	25.3 [28.9]	20.8 [21.8]	1	117
Dishwashers	24.3 [27.7]	19.9 [20.7]	1	117
Freezers	25.7 [28.6]	21.1 [22.0]	1	117
Cooktops	29.8 [34.5]	24.5 [25.5]	1	117
Hoods	32.5 [36.9]	26.7 [27.6]	1	117
Tumble dryers	26.7 [29.5]	21.4 [22.0]	1	117
Washing machines	24.1 [27.1]	19.5 [20.3]	1	117

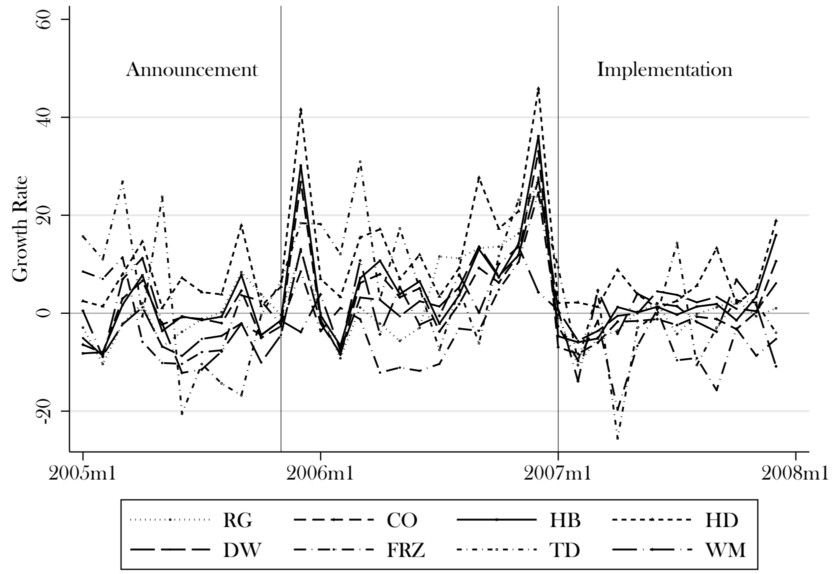
Note: The market age is the number of months a model is sold in a specific country as opposed to the total number of months a model appears in the data irrespective of the location of sales (Average Months Model Sold). Descriptive statistics for the full sample as well as for the reduced sample of models traded in at least two countries, are presented in Table 2.

Table A-2: MODEL 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).
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).
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 (condensor, 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).

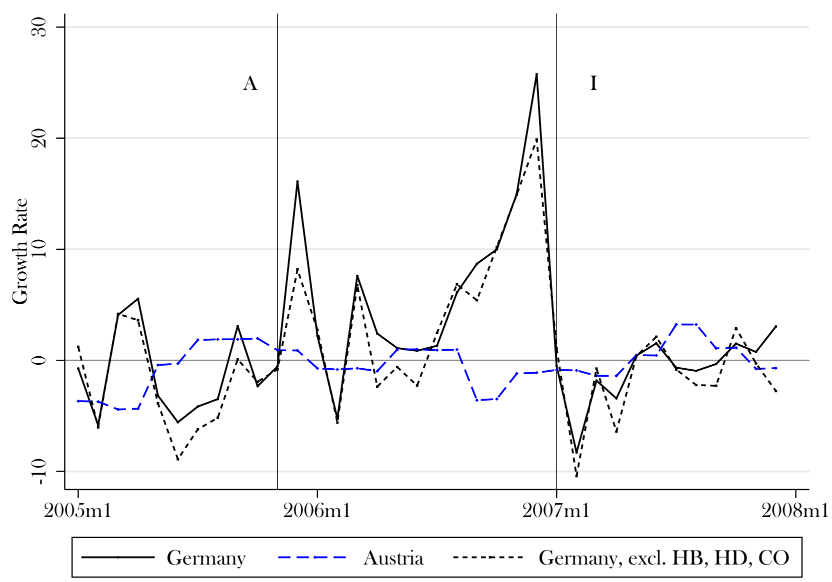
Notes: The characteristic sets used in the characteristic set-date fixed effects in Table A-5 are all possible combinations of the characteristics above per product category. In total, there are 686 groups of models with an identical set of characteristics.

Figure A-1: GERMANY: GROWTH RATE OF WHITE GOODS' SALES PER PRODUCT GROUP, 2005-2007



Note: The figure depicts the growth rate of the total number of units sold of white goods 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 Figure 4 in the main text. Germany increased the standard VAT rate from 16 to 19% on 1.1.2007, with the tax increase officially announced in November 2005.

Figure A-2: GERMANY: GROWTH RATE OF SALES



Note: The figure depicts the growth rate of the total number of units sold of white goods (WM, DW, TD, HB, HD, FRZ, RG, CO). 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 for 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 dashed line is the same growth rate in Austria. Germany increased the standard VAT rate from 16 to 19% on 1.1.2007, with the tax increase officially announced in November 2005. No VAT rate change occurred in Austria. The black dashed line depicts the growth rate excluding HB, HD, and CO.

Table A-3: COMPOSITION OF IDENTIFYING REFORMS (COUNTRIES) PER LEAD

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

The table shows the various composition of VAT reforms and countries involved in the estimation of each lead. 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 A-4: ROBUSTNESS PRICES: INCREASING NUMBER OF CONTROL COUNTRIES IN MODEL-DATE CELLS

	(1) $c \geq 3$	(2) $c \geq 4$	(3) $c \geq 5$	(4) $c \geq 6$	(5) $c \geq 7$	(6) $c \geq 8$
$\mathsf{E}\left[\mathsf{F}^3 \Delta\tau_d\right]$	0.241***	0.234***	0.240***	0.234***	0.237***	0.250***
	(0.027)	(0.029)	(0.032)	(0.036)	(0.040)	(0.045)
$\mathsf{E}\left[\mathsf{F}^2 \Delta\tau_d\right]$	0.046*	0.045*	0.046*	0.059*	0.069**	0.080**
	(0.024)	(0.025)	(0.028)	(0.030)	(0.034)	(0.038)
$\mathsf{E}\left[\mathsf{F}^1 \Delta\tau_d\right]$	0.130***	0.113***	0.111***	0.085***	0.082**	0.089**
	(0.023)	(0.025)	(0.028)	(0.031)	(0.034)	(0.038)
$\Delta\tau_d$	0.165***	0.184***	0.197***	0.222***	0.263***	0.260***
	(0.024)	(0.025)	(0.027)	(0.030)	(0.033)	(0.036)
$\mathsf{L}^1 \Delta\tau_d$	0.438***	0.443***	0.445***	0.421***	0.412***	0.390***
	(0.024)	(0.026)	(0.028)	(0.031)	(0.034)	(0.037)
$\mathsf{L}^2 \Delta\tau_d$	-0.120***	-0.111***	-0.088***	-0.079***	-0.050	-0.039
	(0.024)	(0.025)	(0.027)	(0.029)	(0.033)	(0.036)
$\mathsf{L}^3 \Delta\tau_d$	0.100***	0.115***	0.106***	0.104***	0.083**	0.089**
	(0.025)	(0.027)	(0.030)	(0.033)	(0.036)	(0.040)
Total pass-through $(\sum A_j + B + \sum D_j)$	1.000***	1.022***	1.057***	1.045***	1.096***	1.119***
	(0.065)	(0.070)	(0.076)	(0.084)	(0.093)	(0.104)
Pre-reform $(\sum A_j)$	0.416***	0.392***	0.398***	0.378***	0.387***	0.420***
	(0.043)	(0.047)	(0.051)	(0.057)	(0.063)	(0.071)
Post-reform $(B + \sum D_j)$	0.584***	0.631***	0.660	0.667***	0.708***	0.700***
	(0.049)	(0.052)	(0.056)	(0.062)	(0.068)	(0.075)
N	3,190,634	2,562,872	2,077,872	1,671,169	1,337,784	1,057,569
Model-date groups	912,854	648,451	470,798	341,567	248,367	179,899
Models	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. All specifications include a full set of model-date- (id), country- and country-month-specific fixed effects. All categories of white goods, namely WM, TD, DW, HB, HD, CO, FRZ, RG, are covered. The monthly change in the standard VAT rate is denoted as $\Delta\tau_d$. The monthly unemployment rate, *unempl*, and the number of months a model appears in the data in a specific country, $m.age$, as well as $m.age^2$ are controlled for but not reported. Standard errors are robust in all specifications and clustered by model-date cells. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-5: SALES EFFECTS: INCLUDING SINGLE COUNTRY MODELS

Reforms	All		All		$n \geq 1$	
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathsf{F}^3\Delta\tau_d$		-0.357				
		(0.228)				
$\mathsf{F}^2\Delta\tau_d$		-0.346*				
		(0.197)				
$\mathsf{F}^1\Delta\tau_d$	1.897***	1.874***				
	(0.231)	(0.230)				
$\mathsf{E}\left[\mathsf{F}^3\Delta\tau_d\right]$				-0.311		-0.402
				(0.255)		(0.260)
$\mathsf{E}\left[\mathsf{F}^2\Delta\tau_d\right]$				-0.536***		-0.641***
				(0.206)		(0.208)
$\mathsf{E}\left[\mathsf{F}^1\Delta\tau_d\right]$			2.014***	1.987***	2.050***	2.043***
			(0.242)	(0.241)	(0.247)	(0.247)
$\Delta\tau_d$	-3.426***	-3.433***	-3.428***	-3.436***	-3.941***	-3.957***
	(0.306)	(0.307)	(0.306)	(0.306)	(0.322)	(0.322)
$\mathsf{L}^1\Delta\tau_d$	-1.775***	-1.759***	-1.773***	-1.764***	-1.379***	-1.372***
	(0.283)	(0.283)	(0.283)	(0.283)	(0.278)	(0.278)
$\mathsf{L}^2\Delta\tau_d$		-0.774***		-0.770***		-0.995***
		(0.241)		(0.241)		(0.239)
$\mathsf{L}^3\Delta\tau_d$		1.116***		1.115***		1.324***
		(0.190)		(0.190)		(0.195)
			Cumulative Effects			
Total	-3.304***	-3.678***	-3.187***	-3.715***	-3.270***	-3.999***
	(0.468)	(0.627)	(0.474)	(0.647)	(0.493)	(0.666)
Pre-reform	1.897***	1.172***	2.014***	1.140***	2.050***	1.000***
	(0.231)	(0.371)	(0.242)	(0.405)	(0.247)	(0.409)
Post-reform	-5.201***	-4.849***	-5.201***	-4.855***	-5.320***	-5.000***
	(0.412)	(0.506)	(0.411)	(0.506)	(0.426)	(0.527)
N	7,789,960	7,789,960	7,789,960	7,789,960	7,584,953	7,584,953
Model-group-date effect	49,990	49,990	49,990	49,990	49,665	49,665
Models	279,152	279,152	279,152	279,152	279,120	279,120

Notes: Regressions are based on data for 22 EU countries and up to 33 VAT reforms. The dependent variable is the change in the logarithm of sales, $\Delta\log(UNITS)$. Data for Slovenia and Slovakia is restricted to after Jan. 1st, 2007, and Jan. 1st, 2009, respectively. Data for Estonia is considered up to Dec. 31st, 2010. Note that $\mathsf{E}_{d-j}\left[\mathsf{F}^j\Delta\tau_d\right] = \mathsf{F}^n\Delta\tau_d$ for all reforms that were announced $n > j$ periods ahead, and $\mathsf{E}_{d-j}\left[\mathsf{F}^j\Delta\tau_d\right] = 0$ for reforms announced $n \leq j$. 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. All specifications include a full set of country-, country-month-specific and model-group-date specific fixed effects where the groups are based on all possible combinations of the characteristics per product category as shown in Table A-2. Model-group-date cells, which contain a single model are dropped from the estimation. All categories of white goods, namely WM, TD, DW, HB, HD, CO, FRZ, RG, are covered. The monthly change in the standard VAT rate is denoted as $\Delta\tau_d$. The monthly unemployment rate, *unempl*, and the number of months a model appears in the data in a specific country, $m.age$, as well as $m.age^2$ are controlled for but not reported. Standard errors are robust in all specifications and clustered by characteristic set-date cells. Standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-6: ROBUSTNESS CHECK: DIFFERENTIAL EFFECTS FOR TOP-SELLING MODELS

Forward terms	$\mathsf{F}^i\Delta\tau_d$	$\mathsf{E}\left[\mathsf{F}^i\Delta\tau_d\right]$	
Reforms	All	All	$n \geq 1$
	(1)	(2)	(3)
		Cumulative price effects	
$R5$			
Cumulative	0.592***	0.349***	0.217*
	(0.120)	(0.121)	(0.125)
Pre-reform	0.375***	0.132*	0.130
	(0.077)	(0.078)	(0.080)
Post-reform	0.217**	0.217**	0.086
	(0.093)	(0.093)	(0.095)
$R10$			
Cumulative	0.611***	0.342***	0.215*
	(0.102)	(0.110)	(0.114)
Pre-reform	0.412***	0.143**	0.123*
	(0.059)	(0.072)	(0.074)
Post-reform	0.199**	0.199**	0.092
	(0.083)	(0.083)	(0.086)
N	4,033,450	4,033,450	3,917,656
Model-date groups	1,303,336	1,303,336	1,276,343
Models	71,237	71,237	70,677
		Cumulative sales effects	
$R5$			
Cumulative	-1.06	-0.835	-0.083
	(1.17)	(1.18)	(1.23)
Pre-reform	-0.306	-0.081	-0.012
	(0.780)	(0.798)	(0.821)
Post-reform	-0.753	-0.754	-0.070
	(0.870)	(0.870)	(0.908)
$R10$			
Cumulative	-0.679	-0.559	-0.558
	(0.977)	(1.07)	(1.11)
Pre-reform	-0.461	-0.337	-0.521
	(0.576)	(0.717)	(0.739)
Post-reform	-0.218	-0.222	-0.037
	(0.788)	(0.788)	(0.823)
N	4,127,049	4,127,049	4,006,331
Model-date groups	1,331,295	1,331,295	1,302,878
Models	72,057	72,057	71,493

Notes: The table shows regressions for sales, $\Delta\log(UNITS)_{icd} = \sum_{j=1}^{3} a_j \mathsf{F}^j \log\Delta\tau_{cd} + b\log\Delta\tau_{cd} + \sum_{j=1}^{3} d_j \mathsf{L}^j \Delta\tau_{cd} + R5 * (\sum_{j=1}^{3} a_j \mathsf{F}^j \log\Delta\tau_{cd} + b\log\Delta\tau_{cd} + \sum_{j=1}^{3} d_j \mathsf{L}^j \Delta\tau_{cd}) + cR5 + aX_{icd} + \alpha_{id} + \rho_c + \gamma_{cm(d)} + u_{icd}$, and an identical regression for prices, where $R5$ ($R10$) are dummy variables equal to one if a model 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.