Consumption Tax Incidence: Evidence from Natural Experiment in the Czech Republic

Přenos spotřebních daní: Studie přirozeného experimentu v České republice

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Abstract

This paper estimates incidence of consumption taxation. We use data from natural experiment that took place in 2004 in the Czech Republic. Not only the value added tax (VAT) rates applicable to a range of goods and services changed but also the classification into the standard vs. reduced rate group has been modified. Most importantly, some goods and services experienced no change. This allows us to use difference-in-differences estimation to assess the extent to which taxes are shifted on consumers. Our estimates indicate that those goods and services that experienced decline of the VAT rate from 22% to 19% show no evidence of decrease in prices. We interpret this as evidence of producers and vendors taking the full advantage of the tax decline. On the other hand, goods and services belonging to the group that experienced VAT rate increase from 5% to 19% show lasting increase of prices by up to 6%. This indicates that the higher tax is at least partially shifted on consumers.

Keywords

tax incidence, value added tax, difference-in-differences estimation, natural experiment

Abstrakt

Cílem této studie je odhad dopadu spotřebních daní na základě dat z přirozeného experimentu, který proběhl v České republice v roce 2004. Tehdy došlo nejen ke změně sazeb DPH u celé řady služeb a zboží, ale také k úpravám klasifikace běžné a snížené sazby daně. Co je ale nejdůležitější, některé služby a zboží nepodléhaly žádným změnám. Díky tomu bylo možné použít metodu rozdílu v rozdílech a odhadnout tak, v jaké míře je zdanění přenášeno na spotřebitele. Naše odhady ukazují, že zboží a služby, u nichž došlo ke snížení sazby DPH z 22 % na 19 %, nevykazovaly žádné změny v koncových cenách. Tento fakt si vysvětlujeme tím, že výrobci a prodejci plně využili snížení sazby daně. Oproti tomu zboží a služby, u kterých došlo k navýšení sazby DPH z 5 % na 19 %, vykazují trvalé zvýšení cen až o 6 %. To naznačuje, že vyšší zdanění je alespoň částečně přeneseno na spotřebitele.

Klíčová slova

přenos zdanění, daň z přidané hodnoty, metoda rozdílu v rozdílech, přirozený experiment

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Introduction

Tax incidence comprises one of the core topics in public economics. Distinction between those who merely collect the taxes and send the revenue to the government and those who's income changes as a result of a tax has fascinated generations of economists. Focusing only on indirect taxation, the question becomes of how imposition or change in the relevant tax affects the price of the commodity in question.

On the theoretical level the answer is far from clear. Existing models on the topic deal either with ad valorem taxation, where the amount of the tax is expressed as a percentage of producer's price, or with specific (excise) taxation, where the amount of the tax is expressed per unit of relevant commodity. In either case, some models predict over-shifting, i.e. price of the taxed commodity rises by more than the full amount of the tax, while some models predict under-shifting, i.e. price of the taxed commodity rises by less than the full amount of the tax (see Fullerton and Metcalf, 2002, for survey of the theory). Factors that influence these results usually include assumed market structure, degree of product differentiation or elasticity of the demand and supply.

At the same time empirical literature estimating the degree to which indirect taxes are shifted on consumers is rather scant. Several studies support the idea of over-shifting. Brownlee and Perry (1967) find evidence of full-shifting following 1965 excise tax reduction in the US. Using the same natural experiment, Woodward and Siegelman (1967) analyse changes in the prices of automotive replacement parts concluding with less than full-shifting. Barzel (1976) and Johnson (1978) find evidence of over-shifting using cigarette price data in the US (Sumner and Ward, 1981, refute their results). Poterba (1996) finds over-shifting of sales taxes (American version of ad valorem tax) using clothing prices followed over the 1925-39 and 1947-77 periods in the series of US cities. Estimates in Besley and Rosen (1998) support over-shifting of sales taxes for at least half out of the 12 commodities used in the study covering 155 US cities in 1980's.

On the other hand some empirical evidence supports under-shifting. Delipalla and O'Donnell (2001) analyse European cigarette industry and conclude that both ad valorem and specific taxes tend to be under-shifted. Carbonnier (2007) reaches similar conclusion using value added tax (European version of ad valorem tax) reforms in France focusing on housing repair services and new car market.

Given the importance of the question no more than a dozen studies is rather surprising. Further discounted by the indefiniteness of their results, economists have little to offer both to public and interested policy-makers. Yet, the extent to which consumption taxes are shifted on consumers via prices is of prime concern to both monetary and fiscal policy-makers. To what extent taxes change prices is of utmost importance to all inflation targeting central banks. For the fiscal policy, to what extent taxes are shifted on consumers is central to the distributive and revenue effect of any tax change.

We contribute to the topic by analysing Czech value added tax (VAT) reform of 2004. Not only the standard rate declined from 22% to 19% but also the composition of groups of commodities to which the standard and reduced rates apply has changed. Most importantly, certain commodities experienced no change at all and serve a purpose of control group against which we can measure the effect of the reform.

The paper proceeds as follows. Next part explains in detail the nature of the Czech VAT reform and describes the data we use. Part 3 describes the methodology used to estimate the extent of tax shifting that followed the reform. Here we also check whether the data are consistent with the assumptions we need in order to proceed with the estimation. Ensuing part 4 shows the main results of the paper while part 5 concludes. In the appendix, we further check robustness of the reported results.

1 Natural Experiment Design and Data

The natural experiment we exploit for the research purposes is the Czech VAT reform of 2004 with all measures coming to force on May 1st 2004. There were two main reasons for the VAT change. The first one was the requirement to align the Czech VAT legislation with the European sixth directive which prescribes rules for the VAT legislation in the EU member states. The second reason for the reform was an attempt to bring down increasing public budget deficit.

The reform had two main component. First, the existing standard rate of 22% was reduced to 19%. We call commodities that experienced this type of change 'treated 1' or $T1$ for short. Second, many commodities to which the reduced VAT rate of 5% applied previously were relocated to the category to which the new standard rate of 19% would apply. We use 'treated 2' or $T2$ for this group. Commodities that were previously in the reduced VAT rate group and were not relocated subsequently experienced no change at all. This is our 'control' group.

To give examples of the commodities in the different groups, the control group includes most of the food, medications, personal transportation, press and books and items previously exempt. $T2$ includes veterinary services, vitamins, contraception, sport and cultural activity entrance fees, food served in restaurants and certain services. Rest comprises the $T1$ group, which includes for example electronics, housewares, cosmetics, alcohol or tobacco.

The data we use are monthly price observations of commodities included in the consumption basket used for calculation of the consumption price index (CPI) by the Czech Statistical Office. The data span the entire 2004 year and include 790 different commodities.[1] Consumption basket is chosen to be representative of household consumption composition. This fact increases relevance of our results, which are already based on large number of diverse commodities.

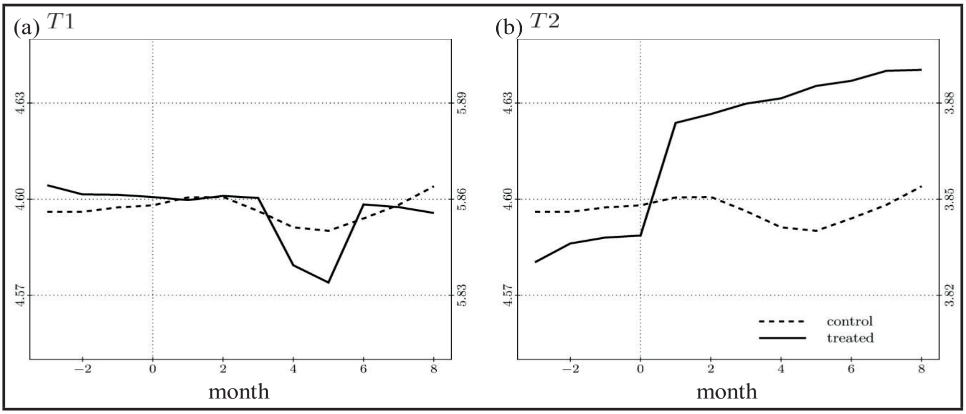
With respect to the VAT reform, we classified 322 commodities into the control group, 408 commodities into the $T1$ group and the remaining 60 into the $T2$ group. In what follows

1 *See www.czso.cz for the data collection methodology. 790 is more than 730 actually used for CPI. The discrepancy comes from the fact that as some items are being introduced and some phased out the data include more items than is needed.*

we use logarithms of the observed prices. This brings additional advantage in that our econometric estimates have simple interpretation, they represent percentage changes. Since April 2004 is the last month before the reform, we denote it as 'month 0' with the negative values denoting months before the reform decreasing to 'month -3', January 2004. 'Month 1' is the first month of the new tax regime, May 2004, and the positive values denote months after the reform going up to 'month 8', December 2004.

As the first look at the data, we calculated mean log-price for each month and each of the three groups. Figure 1 shows the results.

Figure 1: Mean log-price, control vs. treated



Note: Left axis control, right axis treated.

Close inspection of the left panel shows that, at least graphically, there is little evidence of tax shifting in the $T1$ group. Full-shifting would require sustained decrease of the solid curve by 0.03 since the prices are in logarithms. On the other hand, the right panel reveals increase of the mean log-price of $T2$ commodities by more than 0.03, i.e. more than 3% increase in prices on average. Although compelling, this is far from 14% increase required for full-shifting.

Figure 1 makes another important point. As will become clear shortly, validity of our estimates rests heavily on the assumption that the development in the control and treated group prior to the policy change is the same. In other words, for the estimates to be valid, we need to assume that the mean log-price in the control and treated group had the same trend prior to the reform. This allows us to conjecture that, absent the reform, the difference between the mean log-price in the control and treated group would remain the same into the future. Whereas it is impossible to test conjecture regarding the developments absent the reform, we can test hypothesis that difference in the mean log-price between the groups remained stable in the four months prior to the reform. Inspection of figure 1 then shows that the hypothesis is unlikely to be rejected.

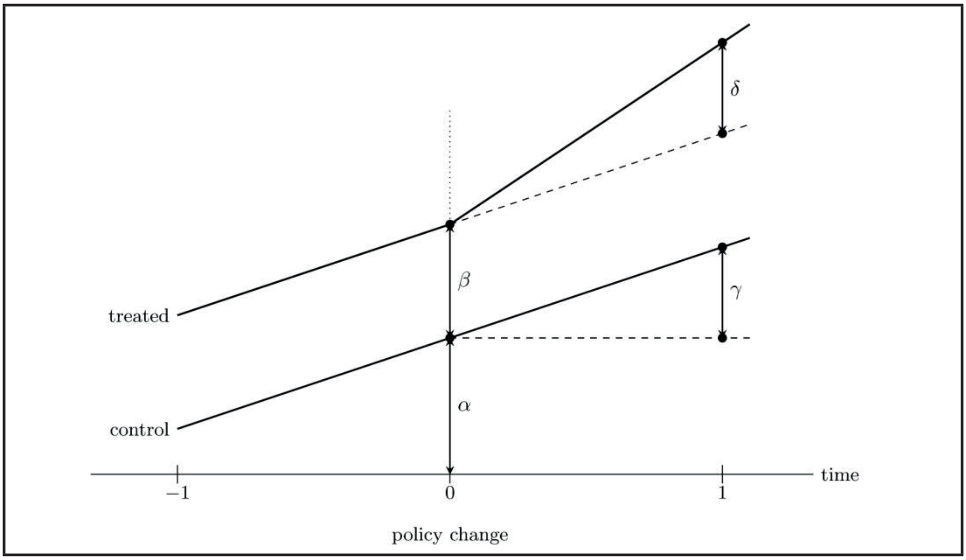
2 Methodology

This section explain difference-in-differences (DiD) estimation methodology we are about to use to estimate the extent to which VAT has been shifted following the 2004 reform.[2]

Suppose a researcher is asked to assess the effect of certain, either natural or controlled, experiment on the variable of interest. She is presented with the data about this variable. Furthermore, each observation indicates whether it has been made before or after the experiment and whether it comes from the control or treated group. In general, DiD estimation acknowledges any difference in the variable of interest between the treated and control groups and uncovers the effect of the experiment as the difference in these differences before and after the experiment, hence its name.

Figure 2 shows stylized example. Development of the variable of interest in both groups is captured by the solid lines. Straight line for the control group indicates steady trend due to the absence of any experiment related change. On the other hand change in the slope of the treated group line captures the effect of the experiment on the variable of interest.

Figure 2: Difference-in-differences estimation



Uncovering the effect of the experiment means estimating δ from the available data. There are numerous ways to do so. One of them is to compute the mean of the variable of interest. For the control group before the experiment this gives α, for the control group after the experiment α + γ (effect of time), for the treated group before the experiment α + β (effect of group heterogeneity) and finally for the treated group after the experiment α + β + γ + δ (combined effect of time, group heterogeneity and of the experiment). δ is then simply calculated from the estimated means.

2 *See Angrist and Krueger (1999) for more in-depth discussion of DiD and Meyer (1995) for the discussion of its possible pitfalls.*

Rather more convenient way of estimating δ, which also readily provides standard errors of the estimates, is running the following regression

$$ln(p_i) = \alpha + \beta\ T_i + \gamma\ A_i + \delta\ (T_i \cdot A_i) + \epsilon_i \quad (1)$$

where we already use notation relevant to our data. The dependent variable, *ln* (p_i), denotes log-price of commodity i, dummy variable T_i indicates whether the observation comes from the control or treated group (unity for treated), dummy variable A_i indicates whether the observation comes from before or after the experiment (unity for after) and ϵ_i is the error term.

Notice that use of the same α, β, γ, and δ in figure 2 and equation (1) is not coincidental. For observations from the control group before the experiment, both dummy variables will always be zero and the estimate of α from the regression will be simply mean of *ln* (p_i) in this group. Similarly, estimated α+β from the regression is mean of *ln* (p_i) in the treated group before the experiment as only T_i dummy is unity. Exactly the same logic applies to both groups after the experiment. The advantage of regression based estimation is that it provides standard errors of the estimated δ, which allows for standard hypothesis testing.

We must stress that validity of DiD heavily rests on the assumption that absent the reform, the difference between the control and treated group would remain the same. With reference to figure 2, this assumption is equivalent to assuming that the solid control group line and the dashed treated group line *after* the policy change are parallel, just as the solid lines for both groups are parallel *before* the policy change.

While there is no way to test this equal trend assumption after the policy change, we can infer how likely is it to hold from the development before the policy change. In order to do so, we estimate β 's for the four months before the VAT reform an test whether they are equal. Table 1 shows the results of the test and conveys the message that the assumption we need in order to proceed with the DiD estimation is likely to hold in our data for both treated groups.

Table 1: Test of equal trends hypothesis

T1	January	February	March
February	0.00 (1)		
	0.990		
March	0.00 (1)	0.00 (1)	
	0.985	0.995	
April	0.00 (1)	0.00 (1)	0.00 (1)
	0.980	0.990	0.995
T2	January	February	March
February	0.00 (1)		
	0.979		
March	0.00 (1)	0.00 (1)	
	0.978	0.998	
April	0.00 (1)	0.00 (1)	0.00 (1)
	0.978	0.998	1.000

Note: Test of the null hypothesis of equal trends in the treated and control group before the treatment. Comparing the difference between mean log-price in the treated and control group in column vs. row months. χ^2 and (degrees of freedom) of the test in the upper part of each cell. p-value of the test in the lower part (probability that the null hypothesis is the correct one).

Our empirical strategy warrants few further comments. In general, DiD estimation does not require panel data. In other words, observations on the variable of interest before and after the experiment can come from different individuals as long as they can be unambiguously classified into control and treated groups.

When data indeed do have panel structure and include observations from before and after the experiment for each individual, as our data do, standard errors estimated by conventional methods can be invalid due to possible correlation of unobservable error for each individual. To overcome this problem, when computing standard errors we cluster on individual commodities of the consumption basket.

Lastly, up to now we have distinguished only before and after the experiment periods. Although sufficient for the DiD estimation, our data have the added advantage that for each commodity they include four monthly observations from before the reform and eight monthly observations from after the reform. This leads to the question of which observations to choose for the actual estimation. For the benchmark results we present in the next section, we use April 2004 as the base month for the period before the reform. Individual columns then correspond to different months used for the period after the reform, giving us eight estimates of the extent of tax shifting. Additional advantage of this approach is that we are able to see its development over time. In the appendix, we include similar tables with different base months for the period before the reform.

3 Results

We are now in position to present our main results. Table 2 depicts the results for the first treated group $T1$ and table 3 for the second treated group $T2$. Each column in both tables estimates model from (1) where A_i becomes At_i for $t \in \{1, \dots, 8\}$ and denotes different months after the reform used in the estimation. For example, the fifth column of table 2 estimates the degree of tax shifting for the commodities from the first treated group $T1$. In doing so the regression includes log-price observations from April 2004, our base month for the whole table representing the period before the reform, and from September 2004, fifth month after the reform.

The estimates have straightforward interpretation explained in detail in the previous section. Since we have converted all the data into logarithms, the estimated coefficients have interpretation of percentage changes. Estimate of δ, which we call *tax effect*, of, say, 0.03 means that prices of relevant commodities increased by 3% as the result of the reform.

Inspection of table 2 reveals that for the commodities experiencing VAT rate decrease from 22% to 19%, the resulting change in prices is rather marginal. Largest decrease in table 2 can be found in the fifth column. Yet it still reaches only -1.9% and is not statistically significant. Inspection of other columns reveals similar picture. All the estimated tax effects are insignificant. We interpret this result as evidence of producers and vendors taking the full advantage of the VAT rate decrease.

On the other hand table 3 reveals completely different set of results for the commodities experiencing VAT rate increase from 5% to 19%. The estimated tax effect ranges from 3.3% in the first month after the reform to 5.5% in the fifth month after the reform. Furthermore, all the tax effect estimates are statistically significant indicating that the effect lasts well after the reform. Taking the 5.5% increase at its face value means that approximately 40% of the tax increase has been shifted on consumers (5.5 percentage points out of 14 percentage points).

Our results thus point to asymmetry in tax shifting since increase in the VAT rate has been reflected in prices while decrease in the VAT rate left prices unchanged. While certainly influenced by the extent of the change, we suspect this to be manifestation of a more general pattern.

Table 2: Estimates of tax incidence with April 2004 as a base month, first treated group $T1$

	Dependent variable: log of price							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
tax effect	-0.003	-0.002	0.001	-0.014	-0.019	0.002	-0.003	-0.011
	(0.003)	(0.005)	(0.008)	(0.020)	(0.021)	(0.010)	(0.008)	(0.008)
$T1$	1.263***	1.263***	1.263***	1.263***	1.263***	1.263***	1.263***	1.263***
	(0.162)	(0.162)	(0.162)	(0.162)	(0.162)	(0.162)	(0.162)	(0.162)
$A1$	0.002							
	(0.002)							
$A2$		0.003						
		(0.004)						
$A3$			-0.002					
			(0.007)					
$A4$				-0.007				
				(0.009)				
$A5$					-0.008			
					(0.010)			
$A6$						-0.004		
						(0.009)		
$A7$							0.000	
							(0.007)	
$A8$								0.006
								(0.007)
constant	4.598***	4.598***	4.598***	4.598***	4.598***	4.598***	4.598***	4.598***
	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)
N	1460	1460	1460	1460	1460	1460	1460	1460
R^2	0.07	0.07	0.07	0.07	0.07	0.07	0.07	0.07

Note: $T1$ is dummy variable for treated group. At is dummy for t-th month into the treatment. Tax effect in t-th column is interaction term between $T1$ and At. Full tax shifting would require tax effect estimates of -3% or -0.03. Robust clustered standard errors (on individual commodities) in parentheses. ***, **, * denotes significance on 1%, 5% and 10% respectively.

Table 3: Estimates of tax incidence with April 2004 as a base month, second treated group $T2$

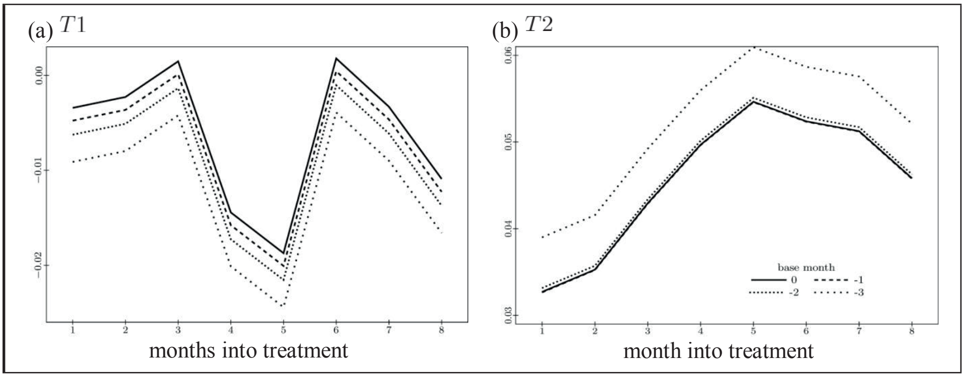
	Dependent variable: log of price							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
tax effect	0.033***	0.035***	0.043***	0.050***	0.055***	0.052***	0.051***	0.046***
	(0.004)	(0.006)	(0.009)	(0.010)	(0.011)	(0.010)	(0.009)	(0.009)
$T2$	-0.759***	-0.759***	-0.759***	-0.759***	-0.759***	-0.759***	-0.759***	-0.759***
	(0.160)	(0.160)	(0.160)	(0.160)	(0.160)	(0.160)	(0.160)	(0.160)
$A1$	0.002							
	(0.002)							
$A2$		0.003						
		(0.004)						
$A3$			-0.002					
			(0.007)					
$A4$				-0.007				
				(0.009)				
$A5$					-0.008			
					(0.010)			
$A6$						-0.004		
						(0.009)		
$A7$							0.000	
							(0.007)	
$A8$								0.006
								(0.007)
constant	4.598***	4.598***	4.598***	4.598***	4.598***	4.598***	4.598***	4.598***
	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)
N	764	764	764	764	764	764	764	764
R^2	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03

*Note: $T2$ is dummy variable for treated group. At is dummy for t-th month into the treatment. Tax effect in t-th column is interaction term between $T2$ and At. Full tax shifting would require tax effect estimates of +14% or +0.14. Robust clustered standard errors (on individual commodities) in parentheses. ***, **, * denotes significance on 1%, 5% and 10% respectively.*

We further note that the extent of tax shifting indicated by our results, even for the $T2$ group, belongs to the lower range, compared to the empirical results briefly surveyed in the introduction. Hence, our results are more in line with the studies supporting under-shifting, Delipalla and O'Donnell (2001) and Carbonnier (2007). Coincidentally, the very same studies deal with the VAT rather than the sales or specific taxes, which are focus of the studies supporting over-shifting.

As already hinted, we re-ran all the estimations using different base months to check robustness of our findings. Although detailed tables are included in the appendix, we summarize the results using figure 3. It shows the estimated tax effects. Different lines represent different base months used and the horizontal axis denotes the month after the reform used in the estimation. With reference to figure 3, we note the robustness of our findings.

Figure 3: Estimates of tax incidence



Note: Full tax shift is -3%. *Note: Full tax shift is +14%.*

Conclusions

This paper tries to assess the extent of tax shifting of the VAT. For this purpose we use natural experiment, Czech 2004 VAT reform. Using monthly data on prices of almost eight hundred commodities included in the CPI basket we use difference-in-differences estimation.

Two main conclusions emerge. First, for the commodities that experienced decrease in the applicable VAT rate from 22% to 19%, there is no evidence of reform effect on prices. Second, for the commodities for which the VAT rate increased from 5% to 19%, there is evidence of less than full tax shifting. Increase in the VAT rate by 14 percentage points translates into at most 6% increase in the prices of the affected commodities. The estimates are statistically and, we believe, economically significant.

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Appendix

This appendix checks robustness of the results from the main part of the paper. Tables 4, 5 and 6 differ from Table 2 only in using different base month for the estimation. Similar difference links Tables 7, 8, 9 and Table 3. The tax effect estimates are summarized in Figure 3.

Table 4: Estimates of tax incidence with March 2004 as a base month, first treated group $T1$

	Dependent variable: log of price							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
tax effect	-0.005	-0.004	0.000	-0.016	-0.020	0.000	-0.005	-0.012
	(0.004)	(0.005)	(0.009)	(0.021)	(0.021)	(0.010)	(0.008)	(0.008)
$T1$	1.264***	1.264***	1.264***	1.264***	1.264***	1.264***	1.264***	1.264**
	(0.162)	(0.162)	(0.162)	(0.162)	(0.162)	(0.162)	(0.162)	(0.162)
$A1$	0.003							
	(0.002)							
$A2$		0.003						
		(0.004)						
$A3$			-0.001					
			(0.008)					
$A4$				-0.006				
				(0.009)				
$A5$					-0.007			
					(0.010)			
$A6$						-0.003		
						(0.009)		
$A7$							0.001	
							(0.007)	
$A8$								0.007
								(0.006)
constant	4.597***	4.597***	4.597***	4.597***	4.597***	4.597***	4.597***	4.597**
	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)
N	1460	1460	1460	1460	1460	1460	1460	1460
R^2	0.07	0.07	0.07	0.07	0.07	0.07	0.07	0.07

*Note: $T1$ is dummy variable for treated group. At is dummy for t-th month into the treatment. Tax effect in t-th column is interaction term between $T1$ and At. Full tax shifting would require tax effect estimates of -3% or -0.03. Robust clustered standard errors (on individual commodities) in parentheses. ***, **, * denotes significance on 1%, 5% and 10% respectively.*

Table 5: Estimates of tax incidence with February 2004 as a base month, first treated group $T1$

	Dependent variable: log of price							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
tax effect	-0.006	-0.005	-0.001	-0.017	-0.022	-0.001	-0.006	-0.014*
	(0.004)	(0.006)	(0.009)	(0.021)	(0.021)	(0.010)	(0.009)	(0.008)
$T1$	1.265***	1.265***	1.265***	1.265***	1.265***	1.265***	1.265***	1.265**
	(0.162)	(0.162)	(0.162)	(0.162)	(0.162)	(0.162)	(0.162)	(0.162)
$A1$	0.004							
	(0.003)							
$A2$		0.005						
		(0.004)						
$A3$			0.000					
			(0.008)					
$A4$				-0.005				
				(0.009)				
$A5$					-0.006			
					(0.010)			
$A6$						-0.002		
						(0.009)		
$A7$							0.002	
							(0.008)	
$A8$								0.008
								(0.007)
constant	4.596***	4.596***	4.596***	4.596***	4.596***	4.596***	4.596***	4.596**
	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)
N	1460	1460	1460	1460	1460	1460	1460	1460

*Note: $T1$ is dummy variable for treated group. At is dummy for t-th month into the treatment. Tax effect in t-th column is interaction term between $T1$ and At. Full tax shifting would require tax effect estimates of -3% or -0.03. Robust clustered standard errors (on individual commodities) in parentheses. ***, **, * denotes significance on 1%, 5% and 10% respectively.*

Table 6: Estimates of tax incidence with January 2004 as a base month, first treated group $T1$

	Dependent variable: log of price							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
tax effect	-0.009**	-0.008	-0.004	-0.020	-0.024	-0.004	-0.009	-0.017**
	(0.005)	(0.006)	(0.009)	(0.023)	(0.023)	(0.010)	(0.009)	(0.008)
$T1$	1.268***	1.268***	1.268***	1.268***	1.268***	1.268***	1.268***	1.268**
	(0.162)	(0.162)	(0.162)	(0.162)	(0.162)	(0.162)	(0.162)	(0.162)
$A1$	0.004							
	(0.004)							
$A2$		0.005						
		(0.005)						
$A3$			0.000					
			(0.009)					
$A4$				-0.005				
				(0.010)				
$A5$					-0.006			
					(0.011)			
$A6$						-0.002		
						(0.010)		
$A7$							0.002	
							(0.008)	
$A8$								0.008
								(0.007)
constant	4.596***	4.596***	4.596***	4.596***	4.596***	4.596***	4.596***	4.596**
	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)
N	1460	1460	1460	1460	1460	1460	1460	1460

*Note: $T1$ is dummy variable for treated group. At is dummy for t-th month into the treatment. Tax effect in t-th column is interaction term between $T1$ and At. Full tax shifting would require tax effect estimates of -3% or -0.03. Robust clustered standard errors (on individual commodities) in parentheses. ***, **, * denotes significance on 1%, 5% and 10% respectively.*

Table 7: Estimates of tax incidence with March 2004 as a base month, second treated group $T2$

	Dependent variable: log of price							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
tax effect	0.033***	0.035***	0.043***	0.050***	0.055***	0.052***	0.051***	0.046**
	(0.005)	(0.006)	(0.009)	(0.011)	(0.011)	(0.010)	(0.009)	(0.009)
$T2$	-0.759***	-0.759***	-0.759***	-0.759***	-0.759***	-0.759***	-0.759***	-0.759**
	(0.160)	(0.160)	(0.160)	(0.160)	(0.160)	(0.160)	(0.160)	(0.160)
$A1$	0.003							
	(0.002)							
$A2$		0.003						
		(0.004)						
$A3$			-0.001					
			(0.008)					
$A4$				-0.006				
				(0.009)				
$A5$					-0.007			
					(0.010)			
$A6$						-0.003		
						(0.009)		
$A7$							0.001	
							(0.007)	
$A8$								0.007
								(0.006)
constant	4.597***	4.597***	4.597***	4.597***	4.597***	4.597***	4.597***	4.597**
	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)
N	764	764	764	764	764	764	764	764

*Note: $T2$ is dummy variable for treated group. At is dummy for t-th month into the treatment. Tax effect in t-th column is interaction term between $T2$ and At. Full tax shifting would require tax effect estimates of +14% or +0.14. Robust clustered standard errors (on individual commodities) in parentheses. ***, **, * denotes significance on 1%, 5% and 10% respectively.*

Table 8: Estimates of tax incidence with February 2004 as a base month, second treated group $T2$

	Dependent variable: log of price							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
tax effect	0.033***	0.036***	0.043***	0.050***	0.055***	0.053***	0.052***	0.046**
	(0.005)	(0.007)	(0.009)	(0.011)	(0.012)	(0.011)	(0.010)	(0.009)
$T2$	-0.760***	-0.760***	-0.760***	-0.760***	-0.760***	-0.760***	-0.760***	-0.760**
	(0.160)	(0.160)	(0.160)	(0.160)	(0.160)	(0.160)	(0.160)	(0.160)
$A1$	0.004							
	(0.003)							
$A2$		0.005						
		(0.004)						
$A3$			0.000					
			(0.008)					
$A4$				-0.005				
				(0.009)				
$A5$					-0.006			
					(0.010)			
$A6$						-0.002		
						(0.009)		
$A7$							0.002	
							(0.008)	
$A8$								0.008
								(0.007)
constant	4.596***	4.596***	4.596***	4.596***	4.596***	4.596***	4.596***	4.596**
	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)
N	764	764	764	764	764	764	764	764

*Note: $T2$ is dummy variable for treated group. At is dummy for t-th month into the treatment. Tax effect in t-th column is interaction term between $T2$ and At. Full tax shifting would require tax effect estimates of +14% or +0.14. Robust clustered standard errors (on individual commodities) in parentheses. ***, **, * denotes significance on 1%, 5% and 10% respectively.*

Table 9: Estimates of tax incidence with January 2004 as a base month, second treated group $T2$

	Dependent variable: log of price							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
tax effect	0.039***	0.042***	0.049***	0.056***	0.061***	0.059***	0.058***	0.052**
	(0.006)	(0.007)	(0.010)	(0.012)	(0.012)	(0.011)	(0.010)	(0.009)
$T2$	-0.766***	-0.766***	-0.766***	-0.766***	-0.766***	-0.766***	-0.766***	-0.766**
	(0.160)	(0.160)	(0.160)	(0.160)	(0.160)	(0.160)	(0.160)	(0.160)
$A1$	0.004							
	(0.004)							
$A2$		0.005						
		(0.005)						
$A3$			0.000					
			(0.009)					
$A4$				-0.005				
				(0.010)				
$A5$					-0.006			
					(0.011)			
$A6$						-0.002		
						(0.010)		
$A7$							0.002	
							(0.008)	
$A8$								0.008
								(0.007)
constant	4.596***	4.596***	4.596***	4.596***	4.596***	4.596***	4.596***	4.596**
	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)
N	764	764	764	764	764	764	764	764

*Note: $T2$ is dummy variable for treated group. At is dummy for t-th month into the treatment. Tax effect in t-th column is interaction term between $T2$ and At. Full tax shifting would require tax effect estimates of +14% or +0.14. Robust clustered standard errors (on individual commodities) in parentheses. ***, **, * denotes significance on 1%, 5% and 10% respectively.*