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The Effects of Countercyclical Fiscal Policy: Firm-Level Evidence from Temporary Consumption Tax Cuts in Turkey[1]

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Abstract

The paper investigates the effects of temporary consumption tax cuts using firm-level data. As part of its countercyclical measures implemented during the recent global economic crisis, Turkey temporarily lowered consumption taxes on selected durables. Our empirical strategy exploits variation in firm exposure to the tax cut which allows us to control for unobserved industry- and region-specific shocks to address potential endogeneity. Using data on the change of sales of firms that benefited from this measure and of those that did not over different periods, we find positive and robust effects of consumption tax cuts on the change of firm sales which is consistent with theoretical predictions.

JEL codes: H20, E62

Keywords: consumption tax cuts, countercyclical fiscal policy, firm-level data

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1 Introduction

There is a long-standing debate in economics about whether governments should engage in Keynesian-style countercyclical fiscal policy. During the recent global financial crisis, this debate has gained new momentum as many countries implemented fiscal stimulus packages. A prime reason for this was the fact that conventional monetary policy as an instrument for stabilization was no longer sufficient or feasible in an environment where interest rates had hit historically low levels in many countries. Given that the crisis was preceded by years of strong growth which increased fiscal space, even developing countries which traditionally rather pursue pro-cyclical policies were able to implement fiscal response packages. The objective of this paper is to examine the effects of temporary consumption tax cuts as an example of such stabilization policies during the recent crisis at the microeconomic level, exploiting within-industry and within-region variation in exposure to the tax cuts in combination with firm-level data from Turkey.

There is a large body of empirical macroeconomic literature that addresses the question of whether fiscal shocks, in particular a debt-financed increase of public spending or debt-financed tax cuts, can have a positive impact on output over the short run. In general, this literature mostly applies vector autoregressions (VARs) comprising quarterly series of output, fiscal variables and various other inputs to private sector production (see for example Kneller and Misch 2011, for a survey of the literature). In order to deal with the simultaneity thought to exist between output and fiscal aggregates, recent papers including Romer and Romer (2010) and Mertens and Ravn (2012) use narrative evidence such as speeches by politicians to single out those tax changes that were implemented for reasons *not* related to output or the state of the economy and only estimate the effects of those using U.S. data. While this type of identification provides credible evidence on the effects of tax shocks, it may be difficult to collect similar evidence for other countries. In addition, whether the results of studies using the narrative approach also apply to the effects of fiscal anti-crisis measures which these studies essentially omit from their analysis is unclear. From a policy perspective, another disadvantage of this literature relates to the fact that—apart from few exceptions—it estimates the effects of broader fiscal shocks, but not the specific effects that result from the change in a particular tax, for example. This makes it difficult to 'use' these macro-level results for fiscal policy design in practice.

An obvious remedy for the simultaneity bias thought to exist between fiscal aggregates and GDP and other issues relating to macroeconomic evidence is the use of microeconomic data as macroeconomic policies are not affected by the behavior of individual households or the performance of individual firms. Dating back to Shapiro and Slemrod (1995), there are several papers including for instance Shapiro and Slemrod (2003, 2009), Broda and Parker (2008) and Johnson et al. (2006) that examine the effects of mostly income tax rebates or cuts on household expenditure using mostly U.S. household data. These micro-level results point to a (modest) rise in

aggregate consumption as a result of such tax changes, albeit to varying degrees. Auerbach and Gale (2009) and Jappelli and Pistaferri (2010) survey this literature in detail. More recently, Mian and Sufi (2012) examine the effects of the US Cash for Clunkers program during the recent crisis using city-specific data on car sales.

In contrast to the existing empirical literature, this paper takes a novel approach and estimates the demand effects of one particular measure of countercyclical fiscal policy, namely a temporary consumption tax cut in Turkey, on firm sales. In the absence of detailed and higher frequency household data, we use the change in firm sales as an endogenous variable which, in aggregate, is likely to reflect changes in aggregate private demand. While such a microeconomic approach has limitations in terms of drawing conclusions for the aggregate economy which we discuss more in detail below, its strength is that it avoids the type of simultaneity problem that arises when using macro-level data. So far, there appears to be no firm-level evidence on the effects of consumption tax cuts or of other specific countercyclical fiscal measures.[2] There are however papers that use industry data to study similar questions. Aghion et al. (2009) use manufacturing industry data from OECD countries and find differences in the effects of countercyclical fiscal policy on value added and productivity growth across industries based on a Rajan and Zingales (1998) type of difference-in-difference specification.

For policy makers, temporary consumption tax cuts are appealing because they are quickly implementable and more likely to stabilize output, employment and industrial production via their effects on private consumption than other fiscal measures. The reason is that in addition to income effects, temporary consumption taxes may also induce intertemporal substitution. The latter arises if financially unconstrained economic agents change the timing of their purchasing pattern to take advantage of temporarily lower prices. This becomes possible because the expenditure on luxury/durable goods does not have to coincide with the timing of their consumption making them highly responsive to intertemporal price differences induced by temporary consumption tax cuts. In contrast, this effect is much smaller for necessity and perishable goods (Crossley et al. 2009).[3] Furthermore, income effects that are also induced by other fiscal measures are likely to be small. The reason is that if consumers are forward-looking and not financially constrained the increase in lifetime income is almost negligible as pointed out by

[2] There is also a branch of the literature that examines the effects of tax changes on firm-level investment which is reviewed in detail by Auerbach and Gale (2009) and Hassett and Hubbard (2002). However, as Auerbach and Gale (2009) argue, the results of that literature are not relevant in the context of evaluating the effects of tax changes to stimulate the private investment over the short run.

[3] There is some debate on this issue in the literature. Blundell (2009) argues that since recessions imply uncertainty about the future demand for durables, the economic value of bringing expenditure forward decreases and that of waiting increases. This rests on the assumption that poor second-hand markets effectively lead to irreversibility of consumption decisions of durable goods.

Crossley et al. (2009). This is especially the case since agents are aware of Ricardian equivalence which dictates that future tax increases will be necessary to offset the cuts. Blundell (2009) and Crossley et al. (2009) for instance provide excellent and detailed theoretical discussions on the effects of temporary consumption tax cuts.

Turkey has recently implemented a temporary consumption tax cut, namely in the value added tax (VAT) and the special consumption tax (SCT), at the peak of the financial crisis in 2009 as part of its fiscal package in response to the global economic crisis.[4] In combination with the data we use, this policy change is particularly well-suited for the purpose of our empirical research. On the one hand, the tax cuts were temporary and affected mainly *durable* and *luxury* goods (rather than necessity and non-durable goods) so that significant intertemporal substitution effects could be expected. On the other hand, the consumption tax cuts were not universal and covered some but not all durable goods. Our empirical strategy exploits the fact that our firm-level data covers the period during and after the tax change, and that there is within-industry variation in exposure to the tax cuts. 'Treated' firms are defined as those firms primarily relying on goods covered by the tax cuts, whereas 'control' firms are those firms which primarily sell goods not covered by the tax cuts represent the control group.[5]

Our empirical strategy reflects that firms may have been affected differently by the financial crisis and that the decision by the government to include or not to include specific products in the tax cut has unlikely been random. While there are important similarities between treated and untreated firms (some industries comprise firms from both groups, and many firms in the control group also sell durable goods like the treatment firms), there are nevertheless differences. For instance, untreated firms (i.e. firms that did not directly benefit from the tax cut) and treated firms (i.e. firms that were targeted by the tax cut) may have been affected differently by the financial crisis given their product mix. In turn, these unobservable factors may also have been correlated with the decision of the government with respect to which goods should have been subject to the tax cut. To account for these differences between the treatment and the control groups, we include firm fixed effects and time-specific region effects where the former capture many product-level differences. More importantly, we are also able to control for differences in exposure to shocks induced by the global financial crisis across the treatment and control groups by including time-variant industry effects and exploiting the fact that there are several industries that contain both treatment and control group firms. Note that this strategy automatically controls for the effects of other macroeconomic and industry-specific policies implemented during the crisis. The use of this type of methodology and the demonstration that

[4] In the context of this paper, SCT and VAT cuts are expected to have identical effects and are therefore not discussed separately.

[5] Taymaz (2010) proposes a difference-in-difference strategy to evaluate the impact of the SCT cuts in the automobile industry in Turkey, but does not carry out any econometric estimations with firm-level data.

firm-level data is suitable to evaluate countercyclical fiscal policy is the first contribution of our paper.

The second and closely related contribution of the paper is to provide much needed firm-level evidence on the effects of countercyclical fiscal measures from the recent crisis that predominantly uses macroeconomic data. Our results indicate that the VAT and SCT cuts indeed increased sales of and private demand for those goods benefiting from the tax cuts relative to goods not benefiting from the tax cuts. This is an essential, though admittedly not sufficient, condition for aggregate demand effects to occur, and our results do not allow estimating fiscal multipliers. While the data we use has limitations, our results nevertheless appear to be robust when we address a number of potential concerns about their reliability. Most importantly, we test the robustness of the results to the exact definition of the treatment and control groups. In some specifications we exclude firms based on their export ratio from the treatment and/or control groups. Here, the underlying rationale is that the more firms export, the less they are affected by domestic consumption taxes and the more they are affected by shocks originating from foreign countries. However, a limited number of observations imply that we are not fully able to exploit this aspect and to control for the fairly implausible possibility that shocks with different effects within regions and within industries have occurred. In addition, as a robustness check, we exclude all firms in industries that do not contain any treated firms.

Our results are consistent with related findings on the effects of VAT cuts in the existing literature. Taymaz (2010) focuses on the effects of the consumption tax cuts on the automobile industry in Turkey. Based on anecdotal evidence and econometric evidence from estimating a demand function with the number of vehicles of various types sold as a dependent variable, Taymaz (2010) finds that the SCT cuts have increased automobile sales and domestic automobile production. Studying the expected effects of the temporary VAT cut in the UK in 2009, which however somewhat differs in terms of design from the one in Turkey, Barrell and Weale (2009), Blundell (2009) and Crossley et al. (2009) all predict beneficial effects based on their theoretical analysis and anecdotal evidence.

In addition, our results are in line with anecdotal evidence on consumer price flexibility from various other studies. Changes in consumption patterns in response to consumption tax cuts only occur if producers pass on the tax cut to consumers, which in turn depends on market structure and menu costs. Taymaz (2010) compares the consumer and producer price indices for motor vehicles. He finds that the former experienced a significantly larger decrease during the period of the SCT reduction suggesting that there was significant pass-through. Blundell (2009) argues that in most sectors in developed countries, the pass-through is between 50% and 100% with the distribution tilting towards the upper limit.

The paper is organized as follows. Section 2 provides background information on the crisis and countercyclical policies implemented during the crisis in Turkey. Section 3 presents the data and

provides descriptive statistics. Section 4 presents the econometric specification and the results. Section 5 concludes.

2 Crisis and Countercyclical Cyclical Policy in Turkey

2.1 Macroeconomic Background

In Turkey, the 2008-2009 economic recession was preceded by an average and fairly constant annual GDP growth rate of 6.8% over the period 2002-2007, exceeding growth rates of many other developing economies (see Uygur 2010). Yet, Turkey experienced a significant decline in GDP during the crisis period in 2008-2009. Figure 1 compares the year-on-year growth of GDP and industrial production in Turkey with the average of the OECD countries. At the peak of the crisis, the first quarter of 2009, the y-o-y GDP decline was 13% in Turkey, while it amounted to 5.47% in the OECD as a whole. However, in subsequent quarters, the Turkish economy recovered more quickly, and growth rates of both GDP and industrial production were above the OECD average. The y-o-y percentage change of the unemployment rate followed similar patterns. As Figure 1 shows, it exceeded the OECD average during the peak of the crisis, but then employment increased earlier and more strongly. The y-o-y change of consumption shows a similar picture as well.

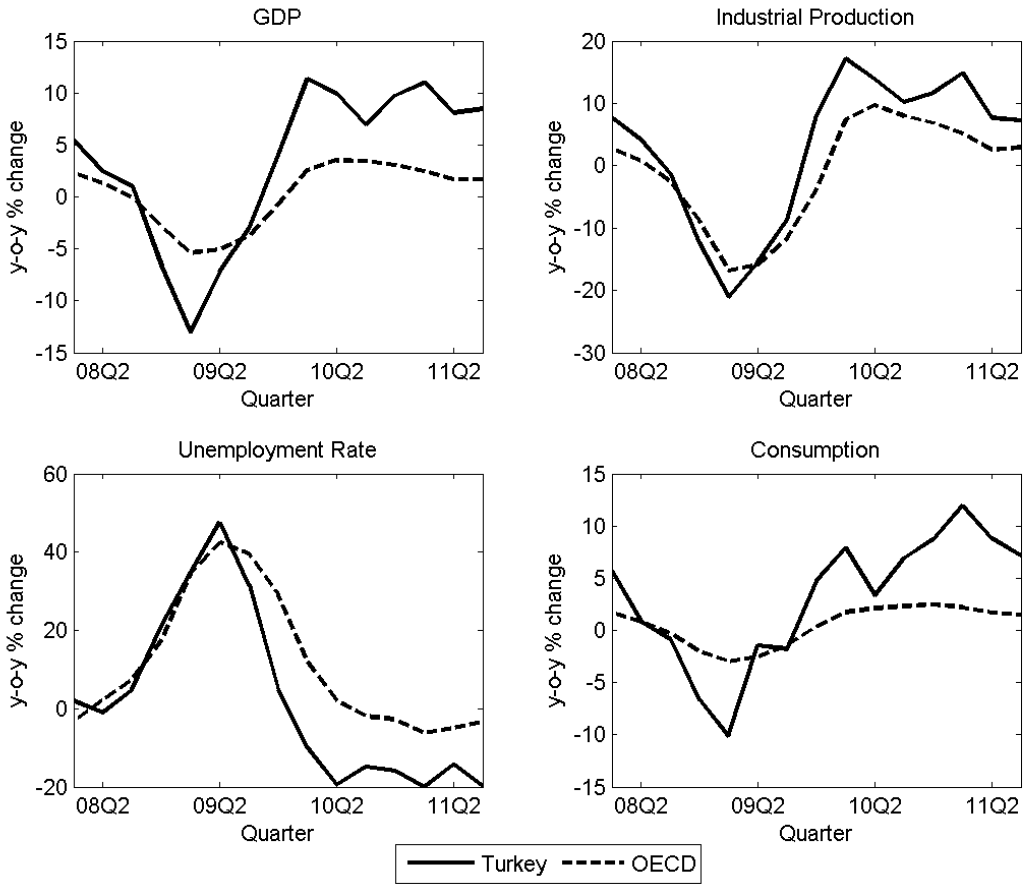
2.2 Fiscal and Monetary Response to the Crisis

Strong GDP growth coupled with debt reductions prior to the crisis (as percent of GDP, government debt declined from 61.5% in 2002 to 28.2% in 2008) and fairly low budget deficits of 0.6 to 1.8% of GDP between 2005 and 2008 provided fiscal space for relatively large fiscal response packages to counteract the crisis. According to the estimates of SPO (2009), the total costs of the direct fiscal measures taken in response to the global crisis amounted to (and were expected to amount to) 0.83%, 2.25% and 2.22% of the GDP in 2008, 2009 and 2010, respectively.[6,7]

[6] There is however some debate with respect to the magnitude of fiscal anti-crisis measures in Turkey. Although Figure 13 in Yeldan (2010), for example, suggests that the fiscal stimulus measures as percentage of GDP were highest in Turkey over 2008-2010 across a large group of countries, some of the measures, especially the ones focusing on the labor market, had already been agreed on prior to the crisis and therefore may not be considered part of the anti-crisis measures.

[7] Öniş and Güven (2010) claim that Turkey was one of the only two OECD countries without a clear fiscal stimulus package until March 2009. Turkish policy-makers including Prime Minister Erdoğan seem to have sincerely believed until October 2008 that the global crisis would “pass tangent to Turkey”.

Figure 1: Macroeconomic indicators of Turkey and the OECD in comparison



In addition, there were strong anti-crisis measures taken by the Turkish Central Bank (CBRT). After the beginning of the recession, the overnight lending rate of CBRT reached a peak of 20.25% in June 2008 and declined thereafter gradually to 8.75% in November 2010. As the first wave of the consumption tax measures (see next sub-section) were announced in mid-March 2009, the rate was 13%, and it had gradually declined to 9.75% by the time the second wave of the measures were reaching an end in September 2009.

The natural question that arises in this context is whether and to what extent the anti-crisis measures contributed to the superior economic performance of Turkey following the peak of the crisis. Alp and Elekdağ (2011), who focus on the role played by monetary policy in Turkey during the global financial crisis, argue that the recession would have been much more severe without the interest rate cuts of CBRT. With respect to the fiscal measures, the effects are likely to vary. SPO (2009) classifies the direct fiscal measures into two groups: revenue and expenditure measures. The expenditure measures cover the extraordinary government consumption and investments, social security contributions as well as transfers to households and business.

Revenue measures include changes of income taxation and consumption taxes. In this paper, we focus on the consumption tax cuts. However, in our empirical specification, by controlling for unobserved effects, we nevertheless take into account effects induced by monetary policy and the remaining fiscal measures which we discuss below.

2.3 VAT and SCT Cuts

In Turkey, there are two main indirect taxes imposed on sales, VAT and SCT. Both taxes are imposed on the same value. VAT is applied to all goods and services sold with three general rates of 1%, 8% or 18% depending on the type of good. SCT is imposed only on certain groups of goods such as oil and its products as well as natural gas, transportation vehicles, tobacco products and beverages, home appliances and luxury goods. The VAT and SCT cuts which we focus on in this paper and which were an important element of the fiscal response to the crisis amounted to fiscal costs of approximately 0.27% in terms of GDP which is significant, in particular since the measures were not universal but applied only to specific products within few sectors of the Turkish economy.[8] Note that these numbers reflect the fiscal costs of the undertaken measures, while their impact on GDP may have been larger due to multiplier effects.

The Turkish government temporarily lowered VAT and SCT rates for some product groups including predominantly final consumer goods but also to a lesser extent capital goods during the period between March and September 2009.[9] These tax cuts were implemented in two phases: at first, in March, the government announced to lower tax rates up to mid/end June, but shortly prior to the expiration of the tax cuts, the government extended the tax cuts until the end of September. Table 1 provides the details. The first three government decrees numbered 14802, 14812 and 14881 referred to the period March/April-June, the last decree, numbered 15081, to the period June/July-September 2009. All products that were covered in the second phase of the tax cut were also included in one of the previous decrees corresponding to the first period of the measures. However, not all products that were mentioned in the first-period decrees were also subject to reductions in the second period. The decrees numbered 14802, 14812, 14881 and 15081 were announced and approved by the government only shortly before they entered into force, namely on March 13, March 25, April 4 and June 12, respectively. In other words, the period between announcement and implementation of these measures appears to be rather short.

[8] See Box 3.1 in SPO (2009).

[9] While the burden of VAT is mostly borne by the final consumers because firms are able to offset VAT paid on supplies against VAT charged on sales, there are exceptions as some expenses such as certain capital goods and equipment are not fully deductible under the VAT law. This essentially imposes a tax burden on firms as well.

Table 1: Consumption tax reductions during the crisis

Types of products covered	Decree		Period of tax cut	
	2nd quarter 2009	3rd quarter 2009	2nd quarter 2009	3rd quarter 2009
SCT reduction in white goods and electronic household goods as well as car industry	2009/14802	2009/15081a	17.03-15.06	16.06-30.09
VAT reduction in furniture, information, communication, industrial and office equipment	2009/14812	2009/15081b	30.03-30.06	01.07-30.09
The list of products covered by 2009/14812 further extended	2009/14881	2009/15081b	15.04-30.06	01.07-30.09

The cabinet decrees reduced VAT and/or SCT for different product groups at different rates. The decree 14802 covers, among others, an SCT reduction from 6.7% to 0% for various white and electronic goods and different levels of SCT reductions on different types of cars. The reduction in less-than-1600 cc passenger cars is, for example, from 37% to 18%, whereas it is from 1% to 0% for buses. Thus, when the VAT is also taken into account, the total consumption tax declines from 55% to 37% for small passenger cars and from 19% to 18% for buses. The decrees 14812 and 14881 cover VAT reductions in new offices, furniture, and some information, communication and bureau equipment. One part of the decree 15081, that contains the measures for the second period, is called 15081a in Table 1 and refers to a sub-group of goods in the decree 14802 implying that not all consumption tax cuts were extended to the third quarter. The remaining part of the same decree referred to as 15081b is related to the decrees 14812 and 14881.[10]

3 Data

Our firm level data comes primarily from the Financial Crisis Surveys (FCS) provided by the World Bank, a specialized firm-level dataset to study firm behavior and performance during the global economic crisis.[11] Firms were surveyed during three periods which are referred to as

[10] Scrapping subsidies were another countercyclical fiscal measure targeted at the automobile industry which was also a prime beneficiary of the consumption tax cuts. However, as Taymaz (2010) convincingly argues, this measure was not effective, given that it did not impose any conditionality such as buying a new vehicle and that its size was relatively small.

[11] This dataset is available at www.enterprisesurveys.org.

waves: during June and July 2009 (wave 1), during February and March 2010 (wave 2), and during June and July 2010 (wave 3). Depending on the sample chosen (see Table 2), the resulting panel is unbalanced with up to 532 different firms surveyed and 882 observations; the average number of observations per firm is around 1.66. All firms surveyed as part of the FCS are also included in the 2008 round of the Enterprise Survey (ES) of Turkey also provided by the World Bank and carried out at the onset of the crisis and some are also included in the 2005 ES round. [12] This allows us to use additional variables on firm characteristics not included in the FCS that can be considered as quasi time-invariant over the short run.

3.1 Assignment of Treatment and Control Groups

We assign all firms in the dataset to either the group of treated firms (i.e., firms most affected by the tax cut) and the group of control firms (i.e., firms less or not at all affected by the tax cut). Given the number of treated firms, we are not able to differentiate the magnitudes of the tax rate reductions within the group of treated firms. However, while the rate cuts significantly differ by product in absolute terms (i.e. percentage points), in relative terms (i.e. in percent of the base rate), their magnitudes differ much less and seem fairly large in most cases. To classify each firm, we use firm-level information on the industry included in the FCS and on the main product which is the product that represented the largest share of firm sales in the 2008 ES. We then match this information with detailed information on which product types were covered in the government decrees implementing the tax cuts. Table 10 in the appendix summarizes the results of this procedure. Most goods covered by the tax cut and included in the table are final consumer goods. In five cases, based on the product description, the industry seems to be consistently misclassified in all waves and years due to key-punch errors which we correct. In only one case, we relied on product information from the 2005 round of the ES which did not conflict with information from 2008 ES but is slightly more detailed (in all other cases, this was either not feasible or not necessary).

We recognize that this way of assigning firms to control and treatment groups may be subject to various concerns which we address in several ways. First, and most obviously, even if the main product is not subject to tax cuts, the firm may still have benefited from tax cuts if secondary products that it sells but which we do not observe are subject to tax cuts. However, (unobserved) secondary products are likely to include close substitutes as in many cases firms specialize in certain areas, and the tax cuts are designed such that they typically include most substitutes. Although we cannot completely rule out that secondary goods of control firms may have been affected by tax cuts, we argue that control firms are affected relatively less by the consumption tax cut compared to treated firms.

[12] This dataset is available at www.enterprisesurveys.org.

Second, given that the product-level information comes from 2008, it is possible that firms discontinued a particular product line, or that changes in demand led to changes in the product mix so that firms started a new product line possibly changing their main product and thereby the classification in treatment / control groups. In turn, demand changes may have been the result of tax changes. For instance, firms that did not previously produce or sell a particular product may have deliberately changed their strategy and increased the share of goods subject to tax cuts in their overall sales through increased production for instance. To take these problems into account, we exclude observations where industry information in the FCS is contradictory and does not coincide with the 2008 ES information, although in some specifications we slightly relax this restriction to increase the number of observations. While inconsistencies in the industry classification may simply indicate mistakes in the coding of the industry, they may also possibly indicate changes in the product mix.

Furthermore, given that the decrees which led to the tax cuts were announced only a few days prior to implementation and that the duration of the tax cuts was only relatively short, it is unlikely that, with adjustment costs, firms deliberately changed their product mix in response to the tax cuts themselves. Adjustment costs make it also unlikely that firms which were subject to tax cuts according to their main product in 2008 changed their product mix to the extent that their classification as either firms of the treatment or control group changes. Even if they discontinued the main product of 2008, it still seems likely that the remaining products are substitutes or otherwise related and are also affected by the tax cuts.

Third, sales of firms of which products are not covered by the tax cuts may still increase if their products are complements with the products covered by the tax cut. This is typically the case for intermediate inputs. For instance, sales for parts used in the production of automobiles may increase as well if taxes for automobiles are lowered, thereby triggering an increase in demand and production.[13] As a robustness check, we therefore also include those firms in the treatment group that produce goods which we regard as complementary with those goods subject to tax cuts. In particular, we include firms in the treatment group that produce parts exclusively used by car producers.

Fourth, sales of firms that are exporting a large share of their output are affected less by the tax cuts as VAT and SCT are only levied on domestic sales, but not on exports. As a robustness check, we exclude firms that sell more than 30% of their output abroad, and any exporters, according to the 2008 round of the ES either only from the treatment group or from the entire sample. Given that demand in export markets contracted as a result of the crisis, it is unlikely

[13] Taymaz (2010) reports that the SCT reductions during the crisis in Turkey did not only lead to a reduction in the inventories, but production increased in the motor vehicles industry as well, thereby increasing demand for intermediate inputs.

that firms were able to increase their exports share to compensate for a decline in demand in the domestic market. In turn, this implies that the export share recorded in 2008 is likely to have remained constant or may have declined, rather than increased, during the financial crisis.

Fifth, while the decrees to implement the tax cuts contain detailed product descriptions, the product descriptions in our firm-level dataset are sometimes imprecise in the sense that there may still be uncertainty as to whether a particular product is covered by the decrees or not. In particular, the terms used for the same product in the decrees and the firm dataset may be different. In a robustness check, we therefore include all those firms in the treatment group for which this type of uncertainty arises, but where we assume that the product was covered by the tax cut (see Table 10 for details).

In essence, depending on which of these factors we take into account, we construct seven different samples with different definitions of the treatment group that only partially overlap in terms of the firms they cover (see Table 2). Focusing on the *first panel* of the table, sample 1 includes only those observations for which industry classifications do not change compared to 2008. In all of the remaining samples—samples 2 to 7—we allow for limited sectoral changes; in particular, we also include observations for which the change in industry is from textiles to garments, from wholesale to retail and from fabricated metal products to other manufacturing. In these cases, the assumption is that this type of sectoral changes is likely not to reflect a change of the product mix of the firm, but rather simply a reclassification of the products that the firm sells. In samples 5 and 7, we exclude all firms that exported in 2008 based on the assumption that they may be subject to other types of shocks or possibly even to stimulus programs in other countries. In samples 6 and 7, we exclude industries that do not contain any treatment group firms.

Focusing on the *second panel*, Table 2 also shows that the treatment groups differ across the samples by whether we include firms that produce intermediate inputs for the production of goods covered by the tax cuts and firms for which we assume that their main product was subject to the tax cut without being absolutely sure about the quality of that assumption as the firm-level product description is not always fully clear. The treatment groups also differ by whether exporters and firms that export more than 30% of their output are included in the sample. Accordingly, we combine these characteristics of the treatment group in several ways in the samples.

Finally, in the *bottom panel,* Table 2 contains the number of total observations, the number of observations in the treatment group, and the number of observations subject to the treatment effect for each subsample. In sample 1, the number of observations in the treatment group is relatively small, whereas in sample 2 and 3, it is much larger. However, this increase comes at a cost, namely by including observations which are more likely to be subject to some of the issues described above. In addition, we recognize that variation in the treatment of firms remains

somewhat limited in the sense that the treatment group is relatively small across all samples which represents a weakness of the data we use. The size of the treatment group implies that our results are inevitably sensitive to how we classify firms; this is another reason for using several different samples.

Table 3 provides information on the composition of the treatment group firms in terms of their industry. It shows that all industries which contain treatment group firms also contain control group firms. This helps to identify unobserved time-specific industry effects which we control for in the regressions. This is especially important as the products covered by the tax cuts are likely to not have been randomly selected. In particular, the government may have primarily targeted products in those industries with significant projected/observed falls in demand. These unobserved factors may then be correlated with the treatment effects and the dependent variable. We therefore control for various unobserved effects. However, while our approach in principle allows controlling for product-specific time-varying effects as we explain later, a limited number of observations per product type implies that we are not able to do this with the dataset we are using. Data limitations also prevent us from using a more refined list of industries. Nevertheless, we include firm fixed effects which together with the industry-time effects we use are likely to cover most if not all aspects of the unobserved product-specific effects that are relevant in our context.

3.2 Dependent Variable

As the dependent variable in our baseline specification, we use the change of sales compared to the same month in the previous year, which we label as SALES_CHANGE. The corresponding question asked in the interview of the first round of the FCS (wave 1) was "If you compare this establishment's sales for the last completed month in 2009 with the same month in 2008, how did they change?" For the subsequent rounds carried out in 2010, the question was identical but referred to 2009. We refer to the same month in the previous year as the 'reference' period, and 'last month' as the 'comparator' period; for wave 1 SALES_CHANGE is the difference in sales between May / June 2009 (comparator period) and May / June 2008 (base period), for wave 2 SALES_CHANGE is the difference between January / February 2010 (comparator period) and January / February 2009 (base period) and for wave 3 SALES_CHANGE denotes the difference between May / June 2010 (comparator period) and May / June 2009 (base period). Table 4 summarizes this information.

Table 2: Definition of subsamples and number of observations

Characteristics of each sample		Sample 1	Sample 2	Sample 3	Sample 4	Sample 5	Sample 6	Sample 7
Observations included in the sample	Observations with change of industry compared to 2008	no	no	no	no	no	no	no
	Change of industry compared to 2008: • textile → garments • wholesale → retail • fabricated metal products → other manufacturing	no	yes	yes	yes	yes	yes	yes
	Exporters	yes	yes	yes	yes	no	yes	no
	Industries that do not contain treatment firms	yes	yes	yes	yes	yes	no	no
Firms included in the treatment group	Intermediate goods used for the production of goods subject to tax cuts and goods probably (but not certainly) subject to tax cuts	no	yes	yes	yes	yes	yes	Yes
	Exporting firms that export more than 30% of their output	yes	yes	no	no	no	yes	No
	Exporting firms that export less than 30% of their output	yes	yes	yes	no	no	yes	No
Number of observations	Total number of observations	748	880	880	880	694	224	200
	No. of obs. in treatment group	21	39	33	22	33	39	33
	No. of obs. subject to treatment effect	8	17	13	7	13	17	13

The timing of the interviews together with the framing of the question in the survey is crucial for our estimation strategy. Supposing that the tax cuts have measurable effects, we would expect them to affect the change in sales if they were effective in the comparator period but not in the base period and vice versa. Holding all other factors constant, they raise the levels of sales in the comparator period leaving sales in the base period unaffected and thereby increasing the change in sales. If both base and comparator periods are affected by the tax cut or unaffected by the tax cut, we do not expect to observe any effects of the tax cut on the change in sales.

With respect to wave 1, the tax cut was in effect in the comparator period but not in the base period suggesting that we would observe a positive effect of tax cuts on the change in sales of those firms selling relevant goods if tax cuts indeed boosted firm sales. With respect to wave 2, the tax cut was in effect neither in the comparator period nor in the base period suggesting that we do not observe any direct treatment effects. However, it is possible that if intertemporal substitution effects were present, demand for products under the tax cut were reduced in the period following the tax cut, i.e., in the months following the expiry of the tax cuts in September 2009. As a result, it may be possible to observe either nil or indirect negative treatment effects in wave 2, i.e. the firms in the treatment group may have experienced a greater fall in sales in comparison to the control group holding other factors constant. Finally, with respect to wave 3, the tax cut was in effect in the base period but not in the comparator period which would imply negative treatment effects on the change of sales if tax cuts indeed helped stabilize private demand. Table 4 summarizes this information.

Table 3: Share of treatment group firms (sample 2)

Industry	% treatment firms (all firms)	% treatment firms (all treatment firms)	% all firms
Other manufacturing	30.91%	60.71%	6.24%
Food	0.00%	0.00%	23.70%
Textiles	0.00%	0.00%	40.02%
Chemicals	4.30%	14.29%	10.54%
Plastics & rubber	3.85%	3.57%	2.95%
Non-metallic mineral products	0.00%	0.00%	8.28%
Basic metals	0.00%	0.00%	0.79%
Fabricated metal products	0.00%	0.00%	0.68%
Machinery and equipment	20.00%	10.71%	1.70%
Electronics (31 & 32)	0.00%	0.00%	0.79%
Construction Section F	0.00%	0.00%	0.91%
Services of motor vehicles	0.00%	0.00%	0.23%
Wholesale	0.00%	0.00%	0.79%
Retail	17.65%	10.71%	1.93%
Transport Section I: (60-64)	0.00%	0.00%	0.45%
Total	3.17%	100.00%	100.00%

Given that the objective of the government was to boost firm sales in wave 1, we focus on the effects of the treatment in wave 1. In other words, we evaluate whether the treatment (i.e., the inclusion of the main product of a firm in the tax cuts) had positive effects on the change of firm sales in wave 1, although as a robustness check, we also evaluate the effects in wave 2 and wave 3. Table 5 presents the mean change in sales both in the overall sample and the treatment

group for all samples. It suggests that in wave 1 the decline in sales was larger among the treatment group firms in most but not all samples and that the consumption tax cuts at best lowered the decline of firm sales.

Table 4: Timing of tax cuts and survey periods

Wave	Wave 1	Wave2	Wave 3
Survey period	June & July 2009	February & March 2010	June & July 2010
Base period of change in sales	May & June 2008	Jan. & Feb. 2009	May & June 2009
Comparator period of change in sales	May & June 2009	Jan. & Feb. 2010	May & June 2010
Tax cut effective in base period	no	No	Yes
Tax cut effective in comparator period	yes	No	No
Predicted impact of tax cut on change in sales if effective	positive	nil or negative	Negative

Table 5: Average change in sales by wave and by sample for all and treatment group firms (in %)

	Wave 1	**Wave 2**	**Wave 3**
Sample 1 all	-17.61	-5.81	-1.47
Sample 1 treatment	-21.88	-11.00	3.00
Sample 2 all	-17.46	-7.09	-2.97
Sample 2 treatment	-23.24	-13.36	4.75
Sample 3 all	-17.46	-7.09	-2.97
Sample 3 treatment	-16.54	-15.92	2.57
Sample 4 all	-17.46	-7.09	-2.97
Sample 4 treatment	-33.71	-23.14	-5.33

Sample 5 all	-16.46	-7.20	-0.91
Sample 5 treatment	-17.92	-18.82	2.57
Sample 6 all	-15.17	-6.37	13.56
Sample 6 treatment	-24.69	-18.82	4.75
Sample 7 all	-11.82	-6.43	15.21
Sample 7 treatment	-17.92	-18.82	2.57

3.3 Independent Variables

The choice of the exogenous variables used in our specifications is driven by data availability. Our dataset contains only few relevant control variables including standard ones such as the number of employees in the previous calendar year (labor), the share of exports prior to the crisis (which is taken from the 2008 ES), and last year's sales. From the latter two variables, we construct two dummy variables: 'non_exporter', which indicates whether a firm exports more than 30% of its output, and 'large', a dummy for large firms which assumes 1 if last year's sales exceed the 75th percentile of all firms and years. In addition, we include capacity utilization in the previous month as a control variable.

There are also other potential control variables in our dataset including whether the firm received state aid. However, a large number of missing observations implies that including these variables in our specification would significantly reduce the number of observations. Along the same lines, our 'large' dummy is missing for many observations so that we choose to exclude it in most specifications. Finally, capacity utilization may be subject to reverse causality and driven by the level of sales; we therefore exclude this variable in several specifications. Table 6 provides descriptive statistics of all variables used.

Table 6: Descriptive statistics[14]

Variable	min	p25	p50	mean	p75	Max	sd
sales_change	-100	-30	-5	-10.35	5	100	34.14
labor	1	15	40	145.71	115	3590	353.66
capacity	0	30	60	55.17	80	100	29.49
large	0	0	0	0.25	0	1	0.43
non_exporter	0	1	1	0.79	1	1	0.41

[14] min: minimum value, p25: 25th percentile, p50: median, p75: 75th percentile, max: maximum value, sd: standard deviation; based on sample 2.

4 Empirical Specification and Results

4.1 Baseline Results

We start out by estimating a straightforward specification which corresponds to specification 1 in Table 7 and is based on sample 1 as defined in Table 2:

SALES_CHANGE = const. + treat.group + wave1 + wave2 + treat.group x wave1 + controls + error

Treat.group is a dummy for the treatment group, wave1 and wave2 are time dummies for the respective waves and the interaction term treat.group x wave1 measures the treatment effect. We also include four control variables including the number of employees last year (labor), capacity utilization (capacity), a dummy indicating whether the firm is large reflecting whether the last year's sales exceeded the 75th percentile (large) and a dummy which indicates whether the firm exports more than 30% of its output (non_exporter). For all specifications, we report clustered standard errors at the region-industry level because our sample includes only 17 different industries. Nevertheless, the treatment effect remains robust if we cluster at the firm level or at the industry level in all but one specification (the treatment effect becomes insignificant only in specification 2 in Table 7 with clustering at the industry level).

The signs of the coefficients are generally plausible and mostly remain robust across all specifications (except for the 'large' dummy). In specification 1, the coefficient of the size of the firm measured by the number of employees is positive but not significant; the positive sign may suggest that larger firms are more diversified so that their sales declined less during the crisis. The coefficient of capacity utilization is positive and significant implying that firms with higher levels of capacity utilization showed a higher efficiency in production and experienced thereby a less steep decline of sales or conversely a stronger increase. Yet, as we already acknowledged above, reverse causality may play a role here. Note however that our results in terms of the sign and significance remain robust if we exclude capacity utilization. The coefficient of the 'large' dummy measuring size with respect to firm sales is significant in specification 1, but the sign is not robust across specifications. Finally, firms that relied less heavily on foreign markets at the onset of the crisis as measured by the time-invariant non_exporter variable taken from the 2008 ES also experienced a smaller decline of sales or conversely stronger sales increases, but the coefficient is not significant. Given that the origin of the crisis was abroad, this may be somewhat intuitive.

We also include industry effects, region effects and time effects. The interaction term of the dummy indicating whether the firm is part of the treatment group with the time dummy for wave 1 is the treatment effect and therefore the variable of interest. In specification 1, the coefficient is positive in line with our theoretical expectations but not significant. This would indicate that the tax cut had no effects on firm sales, or alternatively, that the effects of the tax cuts are

poorly identified, especially given that there may be additional unobserved effects that we do not control for in specification 1.

In the remaining specifications in Table 7, we control for additional unobserved effects to examine whether identification is indeed an important issue. In specification (2), we add firm fixed effects and drop the region and industry effects given that firms in the sample do not move between regions and do not switch industries. In this specification, we omit the treatment group dummy because with firm fixed effects the coefficient is no longer identified. The coefficient of the treatment effect increases in size and becomes significant suggesting that temporary cuts of indirect taxes on durable goods indeed boosted sales of firms benefiting from this measure.

In all remaining specifications of Table 7, i.e. specifications 3 to 5, in addition to firm fixed effects, we include time-varying industry effects and time-varying region effects to account for unobserved region or industry-specific shocks which are likely to have occurred during the crisis. Both types of effects allow us to control for the possibility that the treatment effect is indeed correlated with this type of unobserved factors which may have affected firm sales. These effects also control for other countercyclical measures taken during the crisis. For instance, monetary policy may have played an important role in stabilizing the economy. It seems plausible that the effects of monetary policy on firm sales are either identical across all firms, or that they differ by industry so that the unobserved industry-time effects that we include pick up the effects of monetary policy on the change in firm sales. Along the same lines, it seems unlikely that the effects of other fiscal measures that may have targeted private consumption differed across firms within one industry; in this case, industry-time effects pick again up the effects of such fiscal measures too.

In specification 3, the coefficient of the treatment group increases in size and remains significant. In specification 4, we test the robustness of the results by dropping all but one control variable which also increases the number of observations. While specifications 1 to 4 are all based on sample 1, in specification 5, we use sample 2. As explained above, sample 2 also includes firms that changed the industry but where we assume that these changes are likely due to reclassifications rather than due to changes in the product mix. In addition, sample 2 includes firms in the treatment group whose product classification is not fully clear and which sell intermediate products that are likely exclusively used as inputs to the goods subject to the tax cuts. In specification (5), the coefficient of interest is roughly 39, which implies that being subject to the treatment (i.e. the tax cut) increases the change of firm sales by almost 39 percentage points in wave 1. While the magnitude of the coefficients may appear to be large, it seems plausible that in the absence of significant financial constraints, intertemporal substitution is highly responsive to price differences, and in relative terms, the consumption tax cuts were very large. Indeed, Blundell (2009) and Crossley et al. (2009) both conjecture that the intertemporal elasticity of substitution is close to unity which supports our findings.

While we have convincingly established that temporary consumption tax cuts – when they are in effect – increase private demand for goods benefiting from this measure, there may be the policy concern that private demand falls once they expired which we refer to as 'second-round effects'. Intuitively, this may be the case because private agents simply shift purchases to the period when the tax cut is in effect leaving the overall volume of purchases over the medium term unchanged. This, in turn, would undermine the value of implementing such a measure to stimulate the economy. Based on the descriptive statistics presented in Table 5, evidence on this issue is mixed. For example, Table 5 indicates that on average and in most samples, treatment group firms experienced an increase of sales in wave 3 whereas in some samples, overall mean sales continued to decline. This would suggest that at least in wave 3, second-round effects were superseded by other factors so that the resulting 'net' effect was positive. We explore this issue in more detail in the next sub-section.

4.2 Robustness Checks

In Table 8, we test the robustness of the results across different samples that differ both in overall size and in the exact definition of the treatment group, and in particular explore the role of the export status of firms in detail. In principle, exporters benefit to a lesser extent from the tax cuts as they do not sell their entire output domestically. In specification 1 of Table 8, we exclude firms from the treatment group that export more than 30% of their output, and in specification 2, we exclude all exporters from the treatment group. The rationale is that firms exporting a large percentage of their output are less affected by changes of domestic consumption taxes. We thereby essentially exploit firm-level differences in the effects of the tax cuts within the treatment group by distinguishing treatment group firms that export more than 30% of their output and those that do not. In principle, this identification strategy would allow us to control for unobserved time-varying product-specific effects. However, per product type, our dataset mostly only contains one firm rather than several firms including exporters and non-exporters which implies that we are not able to exploit this. We are confident however that the firm fixed and industry-time effects capture most aspects of this type of unobserved effects. For instance, as product characteristics remain constant over time and to the extent that the product mix did not change, firm fixed effects capture all differences between product types.

Table 7: Results – baseline

VARIABLES	(1) sample 1	(2) sample 1	(3) sample 1	(4) sample 1	(5) sample 2
labor (last year)	0.00188	0.0185	0.0231	0.0281	0.0182
	(0.00232)	(0.0169)	(0.0176)	(0.0185)	(0.0153)
capacity utilization	0.348***	0.279***	0.294***		
	(0.0739)	(0.0927)	(0.0926)		
Large	7.939*	-6.370	-5.436		
	(4.119)	(6.928)	(8.195)		
Wave 1 x treat. group	10.54	19.44*	42.13***	24.75*	39.25***
	(13.30)	(9.936)	(10.17)	(13.74)	(7.345)
non_exporter	3.261				
	(3.210)				
treat. group	6.499				
	(7.376)				
Constant	-47.37***	-31.92***	-36.11***	-23.09***	-23.95***
	(6.763)	(7.134)	(5.947)	(3.116)	(2.209)
Observations	717	717	717	748	880
R-squared		0.093	0.241	0.191	0.200
Number of idstd	452	452	452	468	532
time effects	yes	yes	no	no	no
industry effects	yes	no	no	no	no
region effects	yes	no	no	no	no
firm effects	no	yes	yes	yes	yes
ind.-time effects	No	no	yes	yes	yes
reg.-time effects	No	no	yes	yes	yes

Industry-region clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
sales_change dependent variable

The treatment effect in specification 1 is similar to specification 5 in Table 7 and significant. This is hardly surprising since samples 2 and 3 do not differ much. Interestingly, the treatment effect in specification 2 in Table 8 where we exclude all exporters is still positive and significant, but smaller in magnitude compared to specification 1. While this might suggest that some of the treated firms that export benefited from shocks from abroad like for instance countercyclical fiscal measures in other countries, specification 3 in which we exclude all exporters from the treatment and controls groups does not confirm this conclusion as the treatment effect again increases. Here, the rationale for excluding all exporters is that exporters are subject to different types of shocks that are not necessarily captured by industry-time effects. In specifications 4 and 5, we exclude industries from the sample that do not contain treatment group firms. The rationale is that firms in industries that are completely 'untreated' may not be comparable to

treated firms. In specification 5, we also exclude all exporters. In both cases, the coefficient is again positive and significant, although if we include exporters in both the treatment and control group (specification 4), the point estimate is slightly smaller.

Table 8: Results - robustness

VARIABLES	(1) sample 3	(2) sample 4	(3) sample 5	(4) sample 6	(5) sample 7
labor (last year)	0.0181	0.0181	0.0280	0.0885***	0.0903**
	(0.0152)	(0.0153)	(0.0190)	(0.0298)	(0.0328)
wave 1 x treat. group	36.10***	30.23***	38.05***	32.98***	34.78***
	(9.361)	(10.03)	(8.750)	(8.818)	(10.62)
Constant	-23.03***	-22.04***	-25.94***	-34.28***	-34.06***
	(2.270)	(2.184)	(3.315)	(3.740)	(4.203)
Observations	880	880	694	224	200
R-squared	0.199	0.197	0.249	0.522	0.522
Number of idstd	532	532	422	158	141
time effects	no	no	no	no	no
industry effects	no	no	no	no	no
region effects	no	no	no	no	no
firm effects	yes	yes	yes	yes	yes
ind.-time effects	yes	yes	yes	yes	yes
reg.-time effects	yes	yes	yes	yes	yes

Industry-region clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
sales_change dependent variable

Given that our data covers three periods, as a last step, we check whether our results are robust to including an additional treatment interaction term in Table 9 based on samples 2 and 4 as examples. In Table 4, second last line, we conjecture that the tax cuts imply potentially negative effects on the change of sales in wave 2 and in wave 3. The fact that the coefficient estimate of the treatment group in period 1 is positive automatically implies that the joint effects of the tax cut in in wave 2 and 3 are negative given that the treatment group dummy interacted with wave 2 and with wave 3 represent the 'omitted category'. In specifications (1) and (3) of Table 9, we include an additional interaction term between wave 2 time dummy and the treatment group dummy so that the wave 3 interaction term is the omitted category, whereas in specifications (2) and (4), we include an additional interaction term between the wave 3 time dummy and the treatment group dummy implying that the wave 2 interaction term is the omitted variable. In all cases, the wave 2 and wave 3 interaction terms are not significant suggesting that the tax reduction had at most negligible second-round effects in wave 2 and wave 3. By contrast, the treatment effect in wave 1 remains significant and robust.

Table 9: Effects of the tax cut in wave 2 and 3

VARIABLES	(1) sample 2	(2) sample 2	(3) sample 4	(4) sample 4
labor (last year)	0.0182	0.0182	0.0181	0.0181
	(0.0153)	(0.0153)	(0.0153)	(0.0153)
wave 1 x treat. group	40.33***	37.98**	33.07**	28.05**
	(5.510)	(14.31)	(12.91)	(12.10)
wave 2 x treat. group	2.354		5.016	
	(14.88)		(14.75)	
wave 3 x treat. group		-2.354		-5.016
		(14.88)		(14.75)
Constant	-24.03***	-23.77***	-22.07***	-21.86***
	(2.122)	(2.432)	(2.139)	(2.362)
Observations	880	880	880	880
R-squared	0.200	0.200	0.197	0.197
Number of idstd	532	532	532	532
time effects	no	no	no	no
industry effects	no	no	no	no
region effects	no	no	no	no
firm effects	yes	yes	yes	yes
ind.-time effects	yes	yes	yes	yes
reg.-time effects	yes	yes	yes	yes

Industry-region clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
sales_change dependent variable

5 Conclusions

In this paper, we have estimated the effects of temporary consumption tax cuts on firm sales. Our first contribution is what we consider as a methodological one: we have argued that our empirical strategy which exploits within-industry and within-region variation of the tax cuts in combination with firm-level data is a suitable and feasible way to evaluate the effectiveness of a specific fiscal measure implemented during the recent crisis. Such an evaluation would be difficult or even impossible using macro-level data. We are able to control for a variety of unobserved factors at the firm, industry and region level, in particular for industry-time and region-time effects. This is critical in times of the recent economic crisis where different industries and regions in Turkey were likely to be subject to a range of different shocks and policies affecting firm sales and the decision of the government about which products to cover by the tax cuts.

The second contribution is to shed more light on the long-standing debate about the effectiveness of countercyclical fiscal policy in the sense that we provide evidence that the tax cuts in Turkey appeared to have boosted sales of those firms benefiting from the tax cuts. The coefficients of interest are remarkably robust, and qualitatively, the effects that we find are consistent with the effects of temporary consumption tax cuts reported in the existing literature and with anecdotal evidence on consumer price flexibility. So far, the macroeconomic literature on the effects of fiscal stimuli has usually considered the effects of aggregate fiscal shocks such as general tax cuts or general public spending increases but not of specific fiscal measures such as temporary VAT cuts. In addition, this literature has predominantly been limited to advanced economies, and it is questionable if those results apply to developing countries as well. We have used data from Turkey, an important emerging market economy, where the recovery from the crisis was fairly quick. Our results indicate that the contribution of a specific aspect of the fiscal response package may have played a role in this context.

We recognize that the data we use has limitations which we addressed to the extent possible in a number of robustness checks. On the one hand, given a small treatment group, variation in the treatment effect is limited in our data. We have addressed this problem by expanding the sample and thereby the number of observations included in the treatment group at the expense of unambiguity in the classification of firms. On the other hand, while the identification strategy we propose, namely to exploit variation in the effects of the tax cuts within the treatment group using the export ratio of the firms, would in principle allow controlling for unobserved time-varying product-specific effects, our dataset does not include several firms producing the same product and therefore does not allow using this empirical strategy. We have argued that the other types of unobserved factors we controlled for should capture most of these effects.

Working with micro-level data as we do inevitably has limitations for the discussion of macroeconomic issues which we recognize. Most importantly, we consider our results as providing evidence on an essential pre-condition for aggregate effects to occur, which is that the sales of firms benefiting from the tax cuts increase relative to those not benefiting from the tax cut. However, we cannot rule out the possibility that aggregate demand remains unchanged or even falls as a result of the tax cut which would require substitution of consumption of goods not targeted by the tax cuts by consumption of goods benefiting from the tax cuts. While this may occur up to a point, we believe that the effects we find are not fully driven by this type of substitution: given that targeted and untargeted goods often share only few – if any – similarities, the elasticity of substitution between targeted and non-targeted goods is likely to be fairly low.

Further, it is conceivable that firms have satisfied increased demand induced by the tax cuts by either selling off inventory stocks or by increasingly relying on import, thereby attenuating the stabilizing effects on domestic employment and production. While we do not provide evidence on the magnitude of fiscal spillovers to other countries, given that many firms in our sample are

manufacturers and maintain production facilities in Turkey, our evidence suggests that there have been positive effects on domestic production. In addition, the findings with respect to the automobile industry in Turkey by Taymaz (2010) suggest that domestic production did increase and that inventory stocks were insufficient to meet additional demand induced by the consumption tax cuts. From this perspective, our evidence is consistent with the notion that temporary VAT cuts are a suitable countercyclical measure.

Even if temporary consumption tax cuts stabilize output through temporarily increasing private demand, from a policy perspective there are still various concerns which would caution against adopting such a countercyclical fiscal measure. On the one hand, there is likely to be a reversal of the demand effects once the original consumption tax rates are restored as consumption has simply been brought forward. With respect to the Cash for Clunkers program implemented during the crisis in the U.S. which consisted of temporary subsidies to trade in old cars and to buy new ones, Mian and Sufi (2012) find that once these measures expired, there was reversal in car purchases. However, as Blundell (2009) implicitly argues in the context of temporary consumption tax cuts, this is only a concern if the economy continues to be in recession once the tax cuts expire. Turkey experienced significantly stronger growth compared to the U.S., and these effects appear not to be important based on our evidence. On the other hand, temporary consumption tax cuts may conflict with other objectives of policy makers in developing countries including the protection of the most vulnerable groups in times of recession that by definition consume less than other parts of the population. Other measures that simultaneously protect the poor, such as targeted transfers, may be more suitable if this is an important concern for policy makers. As Heady (2011) points out, a VAT cut is inconsistent with the desire to promote long-run growth: from a long-run growth perspective, increases of productive spending financed by indirect taxes or income tax cuts financed by increases of indirect taxes are growth-enhancing as shown by various studies.

6 Appendix – Product Classification

Table 10 contains a list of all products included in sample 2 for the industries which cover treatment group firms to illustrate how products were assigned to treatment and control groups. The first column includes the industry, the second column contains the description of the main product taken directly from the 2008 round of the ES as recorded by the interviewers (which sometimes contains orthographical errors or is ambiguous), and the third column contains the number of observations for that product (which typically come all from one identical firm). If the product benefited from the tax cuts, the fourth column specifies the name of the decree. The remaining columns indicate whether we consider the product as an input to a final good that was subject to the tax cut and whether there remains some uncertainty about whether the product has indeed been covered by the tax cuts.

Table 10: Product classification (sample 2)

Industry	Product	No. of obs.	Decree	Intermediate good	Classification unclear
Other manufacturing	agricultural materials	1	no	0	0
Other manufacturing	fork, knife, spoon	1	no	0	0
Other manufacturing	tube manufacturing	1	no	0	0
Other manufacturing	counter used in metal factories manufacturing and roler groups	1	2009/14812	0	1
Other manufacturing	kitchen sink	2	no	0	0
Other manufacturing	etichet	2	no	0	0
Other manufacturing	perfume	1	no	0	0
Other manufacturing	catalogue - brochure printing	3	no	0	0
Other manufacturing	animal food	1	no	0	0
Other manufacturing	air conditioner	1	2009/14802	1	0
Other manufacturing	bolt and loaf manufacturing	1	no	0	0
Other manufacturing	food	1	no	0	0
Other manufacturing	iron	3	no	0	0
Other manufacturing	products made from metal sheets	1	no	0	0
Other manufacturing	resonance battens	1	no	0	0
Other manufacturing	silage machine	1	no	0	0
Other manufacturing	creamer cabinet	1	no	0	0

Other manufacturing	heating stove	1	2009/14802	0	1
Other manufacturing	rear-view mirror	3	2009/14802	1	0
Other manufacturing	automotive	2	2009/14802	1	0
Other manufacturing	conductor	1	no	0	0
Other manufacturing	plate manufacturing 100 %	1	no	0	0
Other manufacturing	craft package paper	1	no	0	0
Other manufacturing	motor-vehicle chassis	1	2009/14802	1	0
Other manufacturing	natural gas pipes	1	no	0	0
Other manufacturing	automotive bodies	1	2009/14802	1	0
Other manufacturing	machinery	2	2009/14812	0	1
Other manufacturing	fabric press	1	no	0	0
Other manufacturing	asphalt materials	1	no	0	0
Other manufacturing	copper souvenirs	1	no	0	0
Other manufacturing	wax	2	no	0	0
Other manufacturing	refrigerator	4	2009/14802	0	0
Other manufacturing	aluminum kitchen furniture	1	2009/14812	0	0
Other manufacturing	teakettle production and sales	1	2009/14802	0	0
Other manufacturing	refrigerator	1	2009/14802	0	0
Other manufacturing	white goods	3	2009/14802	0	0

Other manufacturing	freeze machine	1	2009/14802	0	0
Chemicals	color	3	no	0	0
Chemicals	nutrition place	1	no	0	0
Chemicals	chemical article	1	no	0	0
Chemicals	paint manufacturing	2	no	0	0
Chemicals	dish washer detergent	1	no	0	0
Chemicals	anti-blint for paints	2	no	0	0
Chemicals	cosmetic	4	no	0	0
Chemicals	textile	1	no	0	0
Chemicals	transfer ticket	1	no	0	0
Chemicals	ink	2	no	0	0
Chemicals	dye	4	no	0	0
Chemicals	cleaning products	1	no	0	0
Chemicals	colors and hobby colors	1	no	0	0
Chemicals	leader chemicals trade manu-facturing	1	no	0	0
Chemicals	plastic raw material manufac-turing	1	no	0	0
Chemicals	agricultural cure formulation	2	no	0	0
Chemicals	Sponge	1	no	0	0
Chemicals	pigment color	3	no	0	0

Chemicals	ceramic color	2	no	0	0
Chemicals	stain type smc-cmc	3	no	0	0
Chemicals	sponge glue	2	no	0	0
Chemicals	shoe glue	2	no	0	0
Chemicals	lube oil ; low level of anti-freeze	1	no	0	0
Chemicals	deodorant	1	no	0	0
Chemicals	plastic raw material	1	no	0	0
Chemicals	cleaning chemical care (henkel)	3	no	0	0
Chemicals	liquid dish detergent	1	no	0	0
Chemicals	cleaning products and washba-sin smells	2	no	0	0
Chemicals	polyester	1	no	0	0
Chemicals	shirts	1	no	0	0
Chemicals	motor oil	2	no	0	0
Chemicals	chlor production facilities man-ufacturing chemical production	1	no	0	0
Chemicals	powder paint	1	no	0	0
Chemicals	grain	1	no	0	0
Chemicals	cloth coloring	1	no	0	0
Chemicals	detergent	1	no	0	0
Chemicals	parofin emulsion (textile chem-icals)	1	no	0	0

Chemicals	electrostatic powder paint	1	no	0	0
Chemicals	cologne	2	no	0	0
Chemicals	floor surface disinfectant materials	1	no	0	0
Chemicals	nail polish	2	no	0	0
Chemicals	medical lamp	2	no	0	0
Chemicals	copper sulfate	3	no	0	0
Chemicals	package	1	no	0	0
Chemicals	paint	6	no	0	0
Chemicals	thermo plastic road line techniques	1	no	0	0
Chemicals	interior paints	1	no	0	0
Chemicals	construction	1	no	0	0
Chemicals	fdg	2	no	0	0
Chemicals	construction chemicals / isolation equipment	1	no	0	0
Chemicals	pool chemicals	2	no	0	0
Chemicals	Catalyst	1	no	0	0
Chemicals	natural paradox (natural products)	1	no	0	0
Chemicals	paratolian sulfuric acid	1	no	0	0
Chemicals	lightening equipment	1	2009/14812	0	0
Chemicals	stabilizator manufacturing	3	2009/14881	0	0

Plastics & rubber	window	1	no	0	0
Plastics & rubber	pipe manufacturing	1	no	0	0
Plastics & rubber	boots	2	no	0	0
Plastics & rubber	plastic package	1	no	0	0
Plastics & rubber	plastic	1	no	0	0
Plastics & rubber	slippers	3	no	0	0
Plastics & rubber	regulated hinge	1	2009/14802	1	1
Plastics & rubber	rubber	1	no	0	0
Plastics & rubber	technical flour	2	no	0	0
Plastics & rubber	plastic bags	2	no	0	0
Plastics & rubber	presentation stand	2	no	0	0
Plastics & rubber	fiberglass	1	no	0	0
Plastics & rubber	grain	1	no	0	0
Plastics & rubber	granule	2	no	0	0
Plastics & rubber	pvc	2	no	0	0
Plastics & rubber	car rental	2	no	0	0
Plastics & rubber	20dom connection bit	1	2009/14881	0	0
Fabricated metal products	souvenir	2	no	0	0
Fabricated metal products	refrigerator parts	1	2009/14802	1	0

Fabricated metal products	products made from metal sheets	2	no	0	0
Fabricated metal products	fork, knife, spoon	1	no	0	0
Machinery and equipment	mining materials	1	no	0	0
Machinery and equipment	raw material	1	no	0	0
Machinery and equipment	agriculture machine manufacturing	1	no	0	0
Machinery and equipment	agricultural drugs	1	no	0	0
Machinery and equipment	cotton romark manufacturing	2	no	0	0
Machinery and equipment	compressor manufacturing	2	no	0	0
Machinery and equipment	transformer materials	2	no	0	0
Machinery and equipment	replacement parts for equipment	1	no	0	0
Machinery and equipment	gear manufacturing	1	2009/14802	1	0
Machinery and equipment	portable steel shelves	2	2009/14812	0	0
Machinery and equipment	refrigerator	1	2009/14802	0	0
Electronics (31 & 32)	cimnigragh equipment production	2	no	0	0
Electronics (31 & 32)	remote controls	1	2009/14802	1	1
Electronics (31 & 32)	aluminum teflon	1	no	0	0
Electronics (31 & 32)	electricity producer	1	no	0	0
Electronics (31 & 32)	transformer	2	no	0	0
Wholesale	outwear	1	no	0	0

Wholesale	book sales	1	no	0	0
Wholesale	lab materials	1	no	0	0
Wholesale	electrical materials	1	no	0	0
Wholesale	shock absorber	1	2009/14802	1	0
Wholesale	car replacement goods whole-sale	2	no	0	0
Retail	coleston bag	1	no	0	0
Retail	medicine	1	no	0	0
Retail	leather haute couture	1	no	0	0
Retail	retail sales	2	no	0	0
Retail	packaking materials	1	no	0	0
Retail	software	1	no	0	0
Retail	iron sales	2	no	0	0
Retail	lace	1	no	0	0
Retail	food shopping	2	no	0	0
Retail	contour card	1	no	0	0
Retail	paper	1	no	0	0
Retail	white goods	1	2009/14802	0	0
Retail	white goods retail sales	1	2009/14802	0	0
Retail	cell phone	1	2009/14881	0	0

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