Using Temporary VAT Cuts as Fiscal Stimuli – Evidence from the Netherlands∗

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Preliminary and incomplete – please do not quote

Abstract

Can a temporary VAT cut boost economic activity during a recession? We study the effects of two temporary VAT cuts on home improvement in the Netherlands during the Great Recession, using differences-in-differences. We find full pass-through of the temporary VAT cuts into lower prices, and a substantial boost in sales of home improvement services of 10–11%. In contrast to previous studies abroad, we find limited intertemporal substitution of home improvement. Hence, rather than just bringing home improvement forward in time, the reform caused an overall increase in home improvement.

JEL codes: C33, H22, H25
Keywords: Temporary VAT cut, differences-in-differences, Netherlands

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1 Introduction

During recessions, governments look for ways to boost economic activity. The Great Recession witnessed a novel type of policy to boost economic activity: a temporary cut in VAT rates [Blundell, 2009]. The idea behind this temporary VAT cut is that prices will fall temporarily, giving incentives for consumers to spend more and bring future spending forward [Crossley et al., 2014]. Indeed, this policy comes close to the recommendation of Summers [2008], who argued that an effective stimulus should be ‘timely, temporary and targeted’. If this policy is effective, temporary VAT cuts could become a widely used policy option during recessions. However, so far we know relatively little on the effects of this type of policy. Indeed, to the best of our knowledge, only 1 other study looks at the effect on prices and sales. Crossley et al. [2014] study a nationwide temporary reduction of the ‘standard’ VAT rate in the UK by 2.5 percentage points, from December 2008 up to and including December 2009. Their results suggest that initially there was full pass-through of the VAT cut into lower consumer prices, resulting in a 1% increase in retail sales mostly due to consumers bringing expenditures forward in time. This suggests that a temporary VAT cut may indeed temporarily stimulate economic activity. But does this result generalize to other countries and other reforms?

In this paper we study the effects of two temporary VAT cuts for housing repair services in the Netherlands during the Great Recession. To boost economic activity in the housing market, the Dutch government temporarily reduced the VAT rate on home improvement, from 19% to 6% from October 2010 to September 2011 and from 21% to 6% from March 2013 to June 2015 (the ‘regular’ VAT rate was increased from 19 to 21% in October 2012). Meanwhile, the VAT rate on most other goods and services was left unchanged. We exploit this natural experiment to study the effect of the temporary VAT cuts for home improvement on prices and the volume of sales.

To estimate the effects of the reforms we use differences-in-differences. The treatment group consists of firms that provide home improvement services, like plumbers and carpenters. As control groups we use painters (whose VAT rate was already 6% before the data period) and notary firms (whose VAT rate remained at the ‘regular’ rate during the data period). We test the validity of these control groups using placebo reform dummies, and indeed we cannot reject that the firms in the treated sectors share common time effects with the firms in the control sectors. We use sector-level monthly survey data on prices and firm-level quarterly administrative data on the volume of sales for the period 2007–2016. We consider
the treatment effect during the reforms, and intertemporal substitution effects in
the periods preceding and following the reforms.

Our main findings are as follows. First, we find that firms passed through the
VAT reductions into lower prices. For the first reform, where the VAT rate went
from 19 to 6% we find a drop in prices of 8 to 9%. For the second reform, where
the VAT rate went from 21 to 6%, we find a drop in prices of 13 to 14%. Second,
the drop in prices led to a substantial increase in the sales of home improvement
firms, by 10% in the first reform period and 11% in the second reform period.
Third, we find limited intertemporal substitution in response to the temporary
VAT rate reduction either before or after the reform periods. Hence, rather than
only affecting the timing of home improvement, the temporary VAT rate reductions
increased home improvement overall when considering the period before, during and
after the reforms.

Our paper adds to the novel literature on the effects of temporary VAT cuts.
There are only a few papers that consider temporary VAT cuts. As noted above,
Crossley et al. [2014] study a nationwide temporary reduction of the VAT rate in the
UK by 2.5 percentage points, from December 2008 up to and including December
2009. They also find full pass-through of the VAT cut into lower consumer prices.
They further find a 1% increase in retail sales, which however results mostly from
consumers bringing expenditures forward in time. Hence, this reform seems to have
affected only the timing of expenditures. In contrast, we find limited intertemporal
substitution, higher expenditures on home improvement during the reform periods
are (almost) not compensated by lower expenditures on home improvement before
or after the reform periods. Since income effects from VAT cuts are typically found
to be small [Cashin and Unayama, 2011, Cashin, 2012], this suggests substantial
intratemporal substitution of other expenditures. PM Literature on temporary
price cuts during e.g. Holiday season like Cole [2009] and Agarwal et al.
[2017].

The outline of the paper is as follows. Section 2 outlines the reforms and con-
siders potential mechanisms via which the reform may affect prices and home im-
provement expenditures. Next, Section 3 presents the empirical methodology. In
Section 4 we describe the data sets used. Graphical evidence and estimation results
are given in Section 5. Section 6 concludes.
2 The reforms and potential mechanisms

During the Great Recession, the Dutch housing market experienced a severe slump. Sales and prices decreased by 30% and 6% respectively at the start of the recession, which dragged down construction output. In the following years, the government temporarily reduced the VAT rate twice, to give a boost to the housing sector. This is illustrated in Figure 1, and Table 1 gives the timing of events.

On the 31st of August 2010, indicated by the dotted line in Figure 1, the government announced a VAT reduction from the ‘regular’ rate of 19% to the ‘low rate’ of 6% on home renovations and improvements starting the 1st of October 2010, indicated by the start of the shaded area in Figure 1. Since there was only one month between the announcement and the start of the reform, there was little time to respond (postpone) housing renovations and improvements. Initially, the reduction was supposed to last until June 2011, indicated by the dashed-dotted line in Figure 1, but in June 2011 (the announcement indicated by a dotted line again) the government extended the reform until October 2011 (the end is again indicated by a dashed-dotted line). At that time, the recovery in construction output was considered robust enough to end the rate reduction.

In between the first and the second reform there was an intermediate period when the government increased the ‘regular’ VAT rate from 19 to 21%, in October 2012. After construction experienced a second dip in output in 2012, along with the rest of the economy, the VAT rate on housing renovations and improvements was reduced again. The second reduction was announced on the 13th of February 2013, starting only 2 weeks later on the 1st of March 2013. This time, the rate dropped from 21% to 6%, a slightly larger drop. The reform was initially announced to end in February 2014, but on the 11th of October 2013 it was extended to the 31st of December 2014, and then on the 16th of September 2014 it was extended again to the end of June 2015, the actual end date of the second reform.

The EU VAT directive determined the scope of the VAT reductions. The directive allows Member States to tax a list of labour intensive services at a reduced rate. Since 2009, home improvements are on this list with the limitation that the reduced rate only applies to labour costs. Hence, material costs remained taxed at the standard rate during the two VAT reductions. Firms could opt to use predefined labour cost components set by the Dutch Tax Authority for a non-exhaustive list of home improvements, instead of calculating the share of labour costs for each order. Furthermore, the directive limits the scope of the reduced rate to owner occupied dwellings. Hence, improvements on commercial and new built residential properties
Notes: See Table 1 for the timing of events. The dashed line is the VAT rate for housing renovations and improvements. The shaded areas indicate the reform periods, the dotted lines indicate announcements and the dashed-dotted lines indicate announced end dates of the reforms. See Table 1 for a detailed timing of the events.

Table 1: Timing of events

<table>
<thead>
<tr>
<th>Date</th>
<th>Event</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Panel A: Temporary VAT cut 2010–2011</strong></td>
<td></td>
</tr>
<tr>
<td>1st of July 2010</td>
<td>Parliament passes a motion requesting the government to reduce the VAT rate on home improvements from 19 to 6%</td>
</tr>
<tr>
<td>31st of August 2010</td>
<td>Government announces a stimulus package targeted at the housing market, including a temporary VAT reduction for home improvements from 1st of October 2010 to the 30th of June 2011</td>
</tr>
<tr>
<td>1st of October 2010</td>
<td>Start of temporary VAT reduction for home improvement</td>
</tr>
<tr>
<td>1st of June 2011</td>
<td>Government extends the VAT reduction for home improvements started before 1st of July 2011 to 30th of September 2011</td>
</tr>
<tr>
<td>30th of September 2011</td>
<td>First temporary VAT reduction for home improvement ends</td>
</tr>
<tr>
<td>1st of October 2012</td>
<td>Increase in ‘regular’ VAT rate from 19 to 21%.</td>
</tr>
<tr>
<td><strong>Panel B: Temporary VAT cut 2013–2015</strong></td>
<td></td>
</tr>
<tr>
<td>13th of February 2013</td>
<td>Government announces another stimulus package targeted at the housing market, including a temporary VAT reduction from 21 to 6% for home improvements from 1st of March 2013 to the 28th of February 2014</td>
</tr>
<tr>
<td>1st of March 2013</td>
<td>Start of temporary VAT reduction for home improvement</td>
</tr>
<tr>
<td>11th of October 2013</td>
<td>Government extends the VAT reduction for home improvements to December 31st of 2014</td>
</tr>
<tr>
<td>16th of September 2014</td>
<td>Government extends the VAT reduction for home improvements to June 30th of 2015</td>
</tr>
<tr>
<td>30th of June 2015</td>
<td>Second temporary VAT reduction for home improvement ends</td>
</tr>
</tbody>
</table>

Figure 1: VAT rate for housing renovations and improvements: 2007Q1 to 2017Q1
did not partake in the VAT reductions.

Another relevant aspect of the rate reductions is that the completion date determined the applicable VAT rate. For home improvements that started before the end of the reduction and that were completed afterwards, builders were required to apply the regular VAT rate. So sending invoices ahead of completion to profit from the reduced VAT rate before it ended was officially not permitted. Obviously, this was difficult to enforce by the Tax Authority and anecdotal evidence from internet fora suggests that firms offered households creative invoice schemes to avoid the higher VAT rate. Invoicing ahead could explain the local (in time) peak-trough effect on sales around the end date of the second reform (see the results section).

For the reforms we can derive that the prices of services with the reduced VAT rate would have been close to 10% lower in the case of full pass-through. The proportional change in consumer prices after the reform $\Delta P$ can be written as:

$$\Delta P = \frac{P^a - P^b}{P^b} \times 100 = x\%,$$

Where $P^b$ is the pre-reform price and $P^a$ is the post-reform price. Keeping producer prices $\alpha$ constant, consumer prices can be written as $P^b = \alpha \times 1.19$ and $P^a = \alpha \times 1.06$ for the first reform, and $P^b = \alpha \times 1.21$ and $P^a = \alpha \times 1.06$ for the second reform. Hence, we obtain a proportional change in prices of –10.9% and –12.4% for the first and second reform, respectively. However, this calculation ignores that not all costs are labour costs, to which the reduced VAT rate applies, part of it is materials and other costs. In the pre-reform period, labour costs were approximately 70% of total costs. Hence, for all costs we would expect an 8% respectively 9% decrease in prices in the case of full pass-through. According to standard incidence theory, the extent of pass-through of a VAT cut into prices depends on the price elasticities of demand and supply [Harju and Kosonen, 2014]. Specifically, the pass-through is given by: $\rho = \epsilon_s / (\epsilon_s - \epsilon_d)$, where $\epsilon_s$ is the price elasticity of supply and $\epsilon_d$ is the price elasticity of demand. When the price elasticity of supply is relatively high, or the price elasticity of demand is relatively low, pass-through will be high. Specifically, in the special cases of $\epsilon_s \to \infty$ or $\epsilon_d \to 0$, there is full pass-through. The sector of housing renovation and improvement might be considered a very competitive sector, especially during an economic downturn, with a relatively high price elasticity of supply. As a result, we may expect a relatively high pass-through of the VAT cuts.

Given the pass-through into lower prices, there are a number of different channels through which the targeted temporary VAT reductions could have affected the sales in housing renovations and improvements [see also Crossley et al., 2009, 2014].
First, when the VAT reduction is passed on to lower prices this will increase the real incomes of the consumers of these services, causing them to spend more on these services, resulting in more sales. However, since the effect on lifetime income is relatively small, and this is only one of many potential spending categories, this income effect is likely to be small. Second, as the price of housing renovations and improvements falls, consumers will shift part of their expenditures from other goods and services to housing renovations and improvements, leading to potentially substantial (intratemporal) substitution effects. Third, as the price of housing renovations and improvements drops during the reform periods relative to the periods before and after the reform, consumers will postpone or bring forward housing renovations and improvements to periods when the price is low, causing potential substantial intertemporal substitution effects. Indeed, in theory we might expect the intertemporal substitution effect to be relatively large for housing renovation and improvements, as the exact timing of the renovations and improvements may have a limited effect on the utility they derive from their housing.\footnote{Pm Compare with the analysis of durable vs. non-durable goods.}

\section{Empirical methodology}

We use differences-in-differences (DID) to estimate the effects of the reform on prices and sales.\footnote{For a general introduction to the differences-in-differences methodology see e.g. Angrist and Pischke [2008].} In the DID approach we estimate the impact of the policy reform by taking a double difference between the treatment group and control group in the outcome variable. First, we take the difference in the outcome variable between the treatment group and the control group during the reform period. Second, we subtract the difference in the outcome variable between the treatment group and the control group before the reform period. In this way we control for the time-invariant difference between treatment and control group and for common time effects in the outcome variable. Two key assumptions in the DID analysis are common time effects between the treatment and control group in the absence of the reform and the absence of anticipation effects. We will test these assumptions by pre-reform placebo treatment dummies.\footnote{Furthermore, given the short time period between the announcement of the reform and the start of the reform, anticipation effects are likely to be limited to a very short time span.} Furthermore, another key assumption is that the control group is unaffected by the reform. We will test this assumption by estimating the treatment effect using different control groups.

Our treatment group consists of firms that are classified as plumbers, carpenters,
roofers, repointers, tilers and terrace layers. For prices we use painters as the main control group, for which we have monthly observations (for both the treatment and control group) and we do not reject that they share common time effects with the treatment group. For sales we use notary firms as the main control group, for which we have quarterly observations (for both the treatment and control group) and we do not reject that they share common time effects with the treatment group as well.

In the regressions for the effect on prices, we use the log of the monthly price index (normalized to 1 in the first month of observations). By taking logs we implicitly assume common time effects in percentage terms. The treatment effects can then readily be interpreted as effects in percentages (when multiplied by 100). The estimation equation for the monthly indices is:

$$\log P_{g,t} = \alpha_t + \alpha_g + \beta_{\text{Reform}1} DID_{g,1} + \beta_{\text{Inbetween}} DID_{g,1} + \beta_{\text{Reform}2} DID_{g,2} + \epsilon, \quad (2)$$

where \( g \) indicates the group (treatment or control) and \( t \) time. \( \alpha_t \) is a full set of month-year fixed effects and \( \alpha_g \) is a group dummy which is 1 for the treatment group and 0 for the control group. \( DID_{g,1}, DID_{g,1} \) and \( DID_{g,2} \) are the treatment dummies, which are 1 for the first targeted reduction of the VAT rate from October 2010 up to and including September 2011, for the period in between the first and second targeted reduction of the VAT rate from October 2011 up to and including February 2013, and for the second targeted reduction of the VAT rate from March 2013 onwards. \( \beta_{\text{Reform}1}, \beta_{\text{Inbetween}} \) and \( \beta_{\text{Reform}2} \) are the corresponding parameters, which capture the average impact of the reforms and the period in between, all compared to the period before the first reform. In an extension we include a placebo treatment dummy for the year before the reform (October 2009 up to and including September 2010). The error term \( \epsilon \) accounts for heteroscedasticity and group-specific first-order autocorrelation.

In the regression for the effect in sales, we use the log of sales. For sales we have quarterly firm-level panel data. The estimation equation for sales is:

$$\log S_{i,g,t} = \gamma_t + \gamma_{g,q} + \gamma_i + \delta_{\text{Reform}1} DID_{g,1} + \delta_{\text{Inbetween}} DID_{g,1} + \delta_{\text{Reform}2} DID_{g,2} + \delta_{\text{After}} DID_{g,A} + \epsilon, \quad (3)$$

where \( i \) indicates firm \( i \), \( \gamma_t \) are year fixed effects, \( \gamma_q \) are treatment and control group specific quarterly effects, \( \gamma_i \) is a firm fixed effect. For prices we do not have observations after December 2014, when the price series was discontinued by Statistics Netherlands. Using the Stata package xtpcse. Since firms never change between treatment and control group, the group fixed effect is ab-
$DID_{g,2}$ are the treatment dummies for the first reform period, the intermediate period and the second reform period, and $\delta_{Reform1}$, $\delta_{Inbetween}$ and $\delta_{Reform2}$ are the corresponding parameters, respectively. For sales we also have observations after the second reform, for the period July 2015 until the end of observations December 2016. Hence, we can also study how sales are affected after the second reform ends. $\epsilon$ are robust standard errors.

4 Data

For our price estimates, we use monthly survey data on the consumer price index from Statistics Netherlands. Prices series are available for services of two of our seven treatment groups: plumbers and carpenters. However, these two branches make up about 60% of total revenue in our treatment group, offering a good approximation of the price effects for the whole treatment group. As our main control group, we use the price index of services by painters. The services by painters are taxed at the reduced rate since 2001 and hence they were not directly affected by the rate reductions which are the focus of this paper. The price data are available for the period 1996-2014. Unfortunately, Statistics Netherlands stopped producing the prices series in 2015, six months before the second rate reduction ended. We therefore can only observe post rate reduction price developments for the first reduction period. In the empirical analysis we use data for the period 2007–2014, or 96 months for the treatment and control group, for a total of 192 observations.

For our sales estimates, we use administrative data on VAT returns from the Dutch Tax Administration. In the VAT returns, firms have to state the amount of VAT they are due for the reduced rate and the standard rate separately. Using the applicable VAT rates, we can derive revenues for both VAT categories on a quarterly basis. Some firms are allowed to file returns on a three-month basis that does not overlap the calendar quarters. In our sample of construction and notary firms, however, the amount of deviating filers is small (2.4%), causing our revenue data to overlap very closely with the timespan of the tariff reductions.

We use sector codes assigned by the Chamber of Commerce to select firms for our treatment and control groups. The assignment of these sector codes is based on the business activities firms report when registering at the Chamber.\footnote{Firm activities may change after registration, but we do not expect this to affect our results, as changes in activity are likely to take place within the construction sector itself.} We select seven construction branches whose activities clearly fell under the activities defined by the temporary rate reduction to be in our treatment group. These branches are:

sorbéd by the firm fixed effect.
plumbers, carpenters, masons, roofers, tilers, mortar masons and terrazzo workers. As our control group for the analysis of the sales effects we use notary firms, which share common time effects to our treatment group in the period up to the reforms.

5 Results

First consider the results for prices. Figure 2 shows the monthly price index for the treatment groups, carpenters and plumbers, and the control group, painters, from January 2007 until December 2014. We see that in the years before the reform the price indices move in tandem for the treatment and control groups. Then when the reform starts, the price indices of the treatment groups drop, in particular for plumbers. When the first reform was first supposed to end, in July 2011, we see prices rise again, although not back to the level of the control group of painters. It seems that firms were able to raise prices even though the VAT rate reduction was extended until the end of September 2011. In the intermediate period from October 2011 to March 2013, the price indices of the treatment groups appear to converge somewhat to the control group. Also note the sudden increase in October 2012, when the regular VAT rate was increased from 19% to 21%. Then the price indices drop again at the start of the second reform in March 2013. The drop in the VAT rate is more substantial, dropping from 21% rather than 19% to 6% in the second reform, and the price indices appear to drop even more. Again, we see a rise in the price indices of the treatment group when the second reform was first supposed to end, in February 2014. The price indices of the treatment groups remain well below the control group until the end of the data period, December 2014.

Table 2 gives regression results for the differences-in-differences equation (2), where we control for a full set of year-month time fixed effects to capture common shifts in the price index over time. Column (1) gives the results for carpenters. In the first period we find a drop in the price index of –7.6%, consistent with full pass-through (see Section 2). In the intermediate period, the price index bounces back partly, up to –4.9%. Hence, carpenters continue to offer lower prices even after the reform is over. This may be due to the build up of additional capacity in the reform period, which puts downward pressure on prices. After the second reform, prices drop by –13.9%, relative to the pre first-reform period, which suggests more than full pass-through in prices of the VAT reduction (which suggests a drop of about –9%). Column (2) gives the placebo treatment effect in the year before the reform.

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8When carpenters were able to increase the labour cost share on the final bill during the reform periods, we might expect a higher drop in prices than –9%.
Notes: Survey results data are taken from Statline of Statistics Netherlands. The monthly price indices for the treatment groups were discontinued by Statistics Netherlands after December 2014.

Table 2: Regression results prices

<table>
<thead>
<tr>
<th></th>
<th>Services carpenters</th>
<th>Services plumbers</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>W/o placebo</td>
<td>With placebo</td>
</tr>
<tr>
<td>Placebo</td>
<td>0.0056</td>
<td>-0.0030</td>
</tr>
<tr>
<td></td>
<td>(0.0058)</td>
<td>(0.0059)</td>
</tr>
<tr>
<td>First reduction</td>
<td>-0.0754***</td>
<td>-0.0725***</td>
</tr>
<tr>
<td></td>
<td>(0.0057)</td>
<td>(0.0064)</td>
</tr>
<tr>
<td>Intermediate period</td>
<td>-0.0489***</td>
<td>-0.0469***</td>
</tr>
<tr>
<td></td>
<td>(0.0059)</td>
<td>(0.0062)</td>
</tr>
<tr>
<td>Second reduction</td>
<td>-0.1386***</td>
<td>-0.1365***</td>
</tr>
<tr>
<td></td>
<td>(0.0059)</td>
<td>(0.0061)</td>
</tr>
<tr>
<td>Observations</td>
<td>192</td>
<td>192</td>
</tr>
</tbody>
</table>

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses. The standard errors account for heteroscedasticity and group-specific first-order autocorrelation. In columns (1) and (2) carpenters are the treatment group while painters are the control group. In columns (3) and (4) plumbers are the treatment group while painters are the control group. All regressions include a group dummy for the treatment group and a full set of year–month dummies. The placebo dummy is 1 for the treatment group from October 2009 up to and including September 2010, the dummy for the first reduction is 1 for the treatment group from October 2010 up to and including September 2011, the dummy for the intermediate period is 1 for the treatment group from October 2011 up to and including February 2013 and the dummy for the second reduction is 1 for the treatment group from March up to and including December 2014. Results are obtained using monthly data from Statistics Netherlands from January 2007 to December 2014 (the price series are discontinued by Statistics Netherlands from 2015 onwards).
This coefficient is statistically insignificant, small and hardly affects the treatment coefficients in the subsequent periods. This lends support to our assumption of common time effects.

When we consider the treatment group of plumbers, in Column (3) and (4), we find very similar results. The first reform caused a drop in the price index of –8.7%, the price index rebounded partly in the intermediate period, to –3.8%, and then dropped again substantially during the second reform, by –12.9%. The placebo treatment dummy for the year before the reform is again small, statistically insignificant and hardly affects the treatment effects in the subsequent periods.

Next, we consider whether these lower prices resulted in higher sales. Figure 3 shows quarterly sales (normalized to 100 in the first quarter of 2007) for the treatment groups, plumbers, carpenters, masons, roofers, tilers, mortar masons and terrazzo workers, and the control group of notary firms, from the first quarter of 2007 until the fourth quarter of 2016. After some divergence in 2007, the treatment firms and the control firms move in tandem up to the first reform starting in October 2010. We see that sales rise strongly in the treatment group, especially until mid 2011, whereas there is at best modest growth in the control group. After the peak mid 2011, the sales of treatment groups again follows the control group, with little evidence of a drop off in sales just after the first reform, suggesting limited intertemporal substitution. Then during the intermediate period we observe that just before and just after October 2012, when the regular VAT rate was increased from 19% to 21%, there is a peak-trough in sales. This is probably mostly due to ‘manipulation’ of dates on bills sent to customers. In the second reform period we again see sales in the treatment group rising faster than the control group for the most part. Towards the end of the reform period we again see a peak-trough pattern for sales, probably due to manipulation of dates on bills for customers. Hence, we see some evidence of intertemporal shifts, but the effect seems very local (in time) and hence is probably only happening on paper. After the second reform, the treatment and control group both trend upwards.

Table 3 gives the regression results, where we account for firm fixed-effects, and seasonal patterns which we allow to be different for the treatment and control group. In Column (1) we find that sales increase by 10.1% during the first reform period. In the intermediate period they return to just 1.5% more than before the first reform period. In the second reform period sales rise again, by 11.0%. After the second reform period, sales drop somewhat relative to the control group, by 9.

As noted in the data section, we have quarterly sales data, from the Dutch Tax Authority, for a longer period than the price data, from Statistics Netherlands.
Figure 3: Sales of treatment groups and control group over time

Notes: Administrative firm-level data from the Dutch Tax Authority. The quarterly sales are available from the first quarter of 2007 up to and including the fourth quarter of 2016. The treatment group consists of plumbers, carpenters, masons, roofers, tilers, mortar masons and terrazzo workers. The control group consists of notary firms.

Table 3: Regression results sales

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>W/o placebo</td>
<td>With placebo</td>
</tr>
<tr>
<td>Placebo</td>
<td>0.0275*</td>
<td>(0.0154)</td>
</tr>
<tr>
<td>First reduction</td>
<td>0.1012***</td>
<td>0.1255***</td>
</tr>
<tr>
<td></td>
<td>(0.0147)</td>
<td>(0.0155)</td>
</tr>
<tr>
<td>Intermediate period</td>
<td>0.0152***</td>
<td>0.0720***</td>
</tr>
<tr>
<td></td>
<td>(0.0142)</td>
<td>(0.0138)</td>
</tr>
<tr>
<td>Second reduction</td>
<td>0.1095***</td>
<td>0.1344***</td>
</tr>
<tr>
<td></td>
<td>(0.0116)</td>
<td>(0.0125)</td>
</tr>
<tr>
<td>After second reduction</td>
<td>–0.0312</td>
<td>–0.0070</td>
</tr>
<tr>
<td></td>
<td>(0.0153)</td>
<td>(0.0159)</td>
</tr>
</tbody>
</table>

Notes: *** p < 0.01, ** p < 0.05, * p < 0.1. Robust standard errors in parentheses. The treatment group consists of plumbers, carpenters, masons, roofers, tilers, mortar masons and terrazzo workers, while notary firms are the control group. All regressions include firm fixed effects, year dummies, quarter dummies and treatment and control group specific quarter dummies. The placebo dummy is 1 for the treatment group from October 2009 up to and including September 2010, for the first reduction from 2010Q4 up to and 2011Q3, for the intermediate period from 2011Q4 up to and including 2013Q1, for the second reduction from 2013Q2 up to and including 2015Q2, and for after the second reform until the end of the data 2016Q4. Results are obtained using quarterly data from the Dutch Tax Authority for 2007Q1 up to and including 2016Q4.
Notes: Administrative firm-level data from the Dutch Tax Authority. The quarterly sales are available from the first quarter of 2007 up to and including the fourth quarter of 2016. The treatment group consists of lumbers, carpenters, masons, roofers, tilers, mortar masons and terrazzo workers. The control group consists of notary firms. We show the estimated treatment effect with individual quarter treatment dummies, including dummies for the four quarters before the first reform, the quarters in the intermediate period and the four quarters after the second reform period.

–3.1%, which hints at some intertemporal substituton around the end of the second reform period, but the coefficient is not statistically significant. A placebo reform dummy for the year before the first reform, from the fourth quarter 2010 until the third quarter in 2011, is relatively small and only significant at the 10% level.

To look more closely into substitution effects around the beginning and the end of the reform periods, and the increase in the regular VAT rate in October 2012, Figure 4 gives the estimated treatment effects of a regression that includes treatment dummies for each individual quarter, including the four quarters before the first reform period and the four quarters after the second reform period. The sales data shows no evidence of an anticipation effect before the first reform period, as all pre-reform treatment dummies are relatively small and statistically insignificant. Then during the first reform period there are positive and statistically significant treatment effects, with the largest effect in the second quarter of 2011, when the
first reform was first announced to end. After mid 2011, the treatment effect drops, which is consistent with the drop in prices rebounding in part towards the end of the first reform period. In the intermediate period sales are sometime somewhat higher than the control group, but not by much, and then we have the spike in the second quarter of 2012 and the drop in the subsequent quarter. This is probably related to the increase in the regular VAT rate in October 2012, which caused a retiming of housing renovation and improvement, although perhaps only on paper. Again there appears to be little anticipation of the second reform, as the treatment effect in the last quarter before the second reform is close to zero. Sales then jump up during the second reform period and are in almost all quarters substantially above the control group. In the last quarter of the second reform period we see a steep rise sales relative to the control group, and a statistically significant decline relative to the control group in the first quarter after the end of the second reform period. However, in the subsequent three quarters the difference between the treatment and control group is again insignificant.

Hence, although we find evidence of intertemporal substitution, it is limited to the salient increase in the regular VAT rate in October 2012 and the last quarter before and the first quarter after the second reform. Indeed, when we look at the whole period from mid 2010 to mid 2016, there is a clear overall increase in sales in the treatment group relative to the control group, indicating that the main effect of the reform was not intertemporal substitution. As this is unlikely to be an income effect (see the Introduction), this could very well be an intratemporal substitution effect away from other spending categories.

6 Conclusion

In this paper we have studies the effects of a novel policy tool used to boost the economy during a period of recession, a temporary VAT cut. Specifically, we study the effects of two temporary VAT cuts in the Netherlands on housing renovations and improvements, which were implemented during the Great Recession. We used differences-in-differences, with painters (that already had the low VAT rate) and notary firms (that stayed in the regular VAT rate) as control groups. We find full pass-through of the first temporary VAT cut into lower prices, and even more than full pass-through of the second temporary VAT cut into lower prices. The drop in prices increased sales by 10% in the first reform period and 11% in the second period. What is particularly interesting, is that we find that this increase was not the result of intertemporal substitution, as opposed to the general reduction
in the VAT rate in the UK studies by [Crossley et al., 2014]. Indeed, we only find limited intertemporal substitution around the end of the second reform period (though we also find substantial intertemporal substitution for the increase in the regular VAT rate in October 2012 in the Netherlands). For a ‘durable good’ such as housing renovation and improvement, we might have expected a lot of intertemporal substitution, but this is not what we find. It seems that the increase in sales has come mostly from intratemporal substitution of other types of expenditures for housing renovation and improvement. Hence, in terms of boosting the sector at which the reform is targeted, the reform appears to have been successful.

Given the different results we find in terms of intertemporal substitution for our targeted temporary VAT cut when compared to the general temporary VAT cut in the UK studied by [Crossley et al., 2014], it would be interesting to see the treatment effects of targeted and general temporary VAT cuts in other countries and targeted at other sectors. Furthermore, for the Dutch reform it would be interesting to see where the additional expenditures were coming from, although finding a proper control group seems challenging.

References


