Consumption Taxes and Corporate Investment

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Consumers nominally pay the consumption tax, but theoretical and empirical evidence is mixed on whether corporations partly shoulder this burden, thereby affecting corporate investment. Using a quasi-natural experiment, we show that consumption taxes decrease investment. Firms facing more elastic demand decrease investment more strongly, because they bear more of the consumption tax. We corroborate the validity of our findings using 86 consumption tax changes in a cross-country panel. We document two mechanisms underlying the investment response: reduced firms' profitability and lower aggregate consumption. Importantly, the magnitude of the investment response to consumption taxes is similar to that of corporate taxes. (*JEL* G31, H24, H25)

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Prior literature has extensively analyzed the effect of corporate taxes on investment,[1] but has largely ignored the effect of consumption taxes on

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[1] See Summers (1981), Auerbach (1983), Cummins et al. (1996), Djankov et al. (2010), Ljungqvist and Smolyansky (2016), and Giroud and Rauh (forthcoming), among others, or see Becker et al. (2013), Yagan (2015), and Alstadsæter et al. (2017) on payout taxes.

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corporate investment. This neglect is surprising given that consumption taxes represent the most important tax revenue source across OECD countries, accounting for roughly a third of tax revenues, while corporate taxes contribute less than 10%. Given that the private sector generates the output on which consumption taxes are levied, understanding the effects of consumption taxes on private sector investment is crucial. While replacing the current tax system with a pure consumption tax could arguably increase economic growth (Auerbach and Kotlikoff 1987; Barro 1990; Turnovsky 2000; Altig et al. 2001), such a change is unlikely.[2] However, the interplay between consumption taxes and investment in existing tax systems with elements of taxing both income and consumption remains an open and timely issue given ongoing tax reform discussions. We contribute to the literature and the policy debate by examining the magnitude of and the mechanisms behind the effect of consumption taxes on investment.

There are different views regarding whether and how consumption taxes affect investment. One claim common to academic literature[3] and policy makers is that consumption taxes fully fall on consumers (e.g., the Congressional Budget Office)[4] and the tax "burden is borne by individuals" (President's Advisory Panel on Federal Tax Reform, p. 38). Hence, the after-tax return to capital would not be affected by consumption taxes. In contrast, others argue that consumption taxes directly affect producers as well as consumers (e.g., Poterba 1996; Kenkel 2005; DeCicca, Kenkel, and Liu 2013) and firms may bear part of the wedge that consumption taxes drive between the price consumers pay and the price firms receive. Firms bear more of the consumption tax burden when they face relatively more elastic demand. That is, the incidence on firms is a function of how elastic demand is relative to supply. Theory suggests that consumption taxes then reduce firms' profits and, therefore, capital demand. Hence, capital investment decreases. However, empirical evidence on the effect of consumption taxes on investment is scarce, inconclusive, and primarily relies on macroeconomic data (Alesina et al. 2002; Djankov et al. 2010; Arnold et al. 2011).[5] Our paper addresses this research gap and examines the effect of consumption taxes on investment and whether the adverse investment effect of consumption taxes is a function of the relative elasticity of demand vis-à-vis supply.

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[2] See, for example, the President's Advisory Panel on Federal Tax Reform report (2005, p. 38).

[3] For example, Marion and Muehlegger (2011) argue that, on average, gasoline and diesel taxes are fully passed on to consumers. Besley and Rosen (1999) suggest that commodity taxes are fully passed on to consumer, and, in some cases, taxes are overshifted to consumers.

[4] The Congressional Budget Office's (2012, p. 20) report "The Distribution of Household Income and Federal Taxes, 2008 and 2009" assumes for consumption (or excise) taxes that "the economic cost of excise taxes falls on households" and that "excise taxes on intermediate goods, which are paid by businesses, [are] attributed to households."

[5] Similarly, Fox (1986) and Thompson and Rohlin (2012) examine the effect of sales taxes on employment and retail activity. They find some evidence of a detrimental effect on employment concentrated at state border areas, where individuals can easily cross. They do not find a sales tax effect on total employment or payroll.

Because private sector investment is one of the main drivers of aggregate growth (De Long and Summers 1991; Mankiw, Romer, and Weil 1992) and consumption taxes represent the most important tax revenue source, an understanding of whether consumption taxes affect investments is critical for researchers and policy makers. The primary empirical challenge in documenting the investment effect of consumption taxes is that policy makers tend to increase (decrease) consumption taxes when the economy is weak (strong), especially in developing countries (Vegh and Vuletin 2015). A negative association between tax increases and investment might reflect unobserved variation in economic conditions and investment opportunities. We, thus, need an identification approach that isolates business cycle effects and other macroeconomic factors.

We examine the 2012 value-added tax (VAT) increase in the Netherlands as it enables us to make causal inferences about the effect of consumption taxes on corporate investment for the following reasons. First, the change was unanticipated because (a) the Netherlands historically had a stable consumption tax rate; (b) the change resulted from mounting pressure by the European Commission; and (c) the Netherlands did not change the VAT at the time of the financial crisis from 2008 to 2009, when budget deficits were also an issue. Second, the change was economically significant, with an increase of 2 percentage points from 19% to 21% and was not confounded by changes in other taxes, such as corporate taxes. Third, the Netherlands shares borders with Germany and Belgium. Consumers located at these borders can more easily substitute Dutch goods and services subject to the VAT increase for Belgian and German goods and services not subject to a tax increase. This makes the consumer demand faced by firms located along the border more elastic. Consistent with the notion that firms bear more of the tax burden if they face more elastic consumer demand, the media coverage around the VAT increase emphasizes that the VAT increase was particularly burdensome for firms along the border. Accordingly, we partition the sample into border and nonborder firms thus allowing us to exploit differences in the relative elasticity of demand versus supply, while controlling for variation in economic conditions unrelated to location (e.g., at the industry level). Finally, while the VAT rate increased from 19% to 21%, a reduced VAT rate did not change in specific industries, such as agriculture, food, or medicine. We thus further partition the sample into affected and unaffected industry firms to control for variation in local economic conditions unrelated to a specific industry. These two partitions—affected versus unaffected industries and border versus Dutch-interior firms—are arguably exogenous to the 2012 VAT increase as firms could not anticipate the VAT change and thus could not change their industry or location.

We exploit this setting in a triple difference design using data on 1,502 firms in the Netherlands. We show that Dutch firms along the border between Germany and Belgium decrease investment to a greater extent than Dutch firms more distant from the border. The estimated elasticity of investment to consumption

taxes is between −0.36 and −0.40 and comparable to recent estimates of the elasticity of investment with respect to corporate taxes. Whereas Giroud and Rauh (forthcoming) estimate an elasticity of investment with respect to the corporate tax rate of about −0.4 to −0.5 using establishment-level data, Patel, Seegert, and Smith (2017) find an elasticity of real investment with respect to the tax rate of −0.21 using the population of U.S. firms.

In contrast to the more adverse investment response of firms along the border relative to firms farther from the border in affected industries, we do not find a differential change in investment in unaffected industries for border firms relative to firms farther from the border. This finding suggests that without the VAT increase, firms in affected industries would have invested similarly irrespective of their location along or farther from the border. We also find that the investment difference across all partitions evolves similarly prior to the VAT increase. These findings support the parallel trends assumption underlying our identification. We corroborate this interpretation with a placebo analysis in which we shift the VAT increase in time and find no investment difference between border and nonborder firms in affected industries.

We then decompose the negative investment effect of border firms in affected industries in three ways based on the prediction that the incidence of consumption taxes on consumers versus firms depends on the relative elasticity of demand (e.g., Atkinson and Stiglitz 1972; Kotlikoff and Summers 1987). First, we exploit differences in firm size. Smaller firms plausibly have more local output markets relative to larger firms. Hence, they face relatively more elastic consumer demand if they are located along the border. Second, because firms with little market power likely face relatively more elastic demand (Lerner 1934; Grullon et al. 2018), we use profit margins as a proxy for demand elasticity. We find that the response of border firms is concentrated within smaller firms who depend more on local markets and within firms with low profit margins. Third, firms with financial constraints have less financial flexibility, that is, they are more inelastic than demand, and hence should cut investment more strongly when consumption taxes increase. We find financially constrained firms' cut investment more strongly after the VAT increase.

We also provide evidence that the mechanism underlying the investment effect relies on profitability. When firms bear consumption taxes, the VAT increase reduces their profits and capital demand. While capital owners would like to reduce the capital stock by cutting investment to preserve investment returns, reducing the capital stock by, for example, fire-sale discounts, likely causes friction there is likely friction in reducing the capital stock by, for example, fire-sale discounts. Such friction prevents firms from immediately and fully reducing their capital stocks, leading to lower capital returns. Consistent with this notion, we find that the profitability of Dutch firms along the border decreases relative to firms farther from the border if they operate in affected industries. This decline is absent in unaffected industries.

Our quasi-natural experiment using the Dutch 2012 VAT increase allows us to draw inferences about the causal effect of consumption taxes on investment. However, the Dutch setting is limited to one event and a relatively small sample of firms. We, therefore, generalize these results by exploiting 86 consumption tax changes across 68 countries staggered in time from 2001 to 2013. We control for observable economic conditions, seven other tax policy variables, and limit the counterfactuals to firms from economically similar countries. We first document a negative relation between consumption taxes and aggregate gross capital formation with a comparable magnitude as in the Dutch setting. Our international sample also allows us to examine the aggregate consumption effect of consumption taxes. We find that consumption taxes are negatively related to aggregate consumption. Collectively, our results suggest that consumption taxes reduce investment opportunities and firms' profitability, which translates into lower corporate investment.

We then continue with firm-level analyses and find an average investment elasticity of similar magnitude in a sample of 37,691 public firms. We employ the cross-country firm-level analysis because it allows us to examine similar, as well as alternative, cross-sectional variables that also match the underlying construct of the investment response: the relative elasticity of demand vis-à-vis supply. For example, we are able to split industries based on the sales they generate domestically or based on their closeness to end-consumers. Being more distant from domestic end-consumers means these firms both have higher flexibility and are less exposed to the tax increase. Similar to the results in the Dutch experiment, we find that the firms with low margins, a higher share of domestic sales, or a lower distance to consumers in the value chain reduce investment more strongly after consumption tax increases. We also confirm that financially constrained firms are more responsive to consumption tax increases. Another important advantage of this cross-sectional firm-level analysis is that we can control for unobserved variation at the country-(industry)-year level that represents one of the key identification concerns in the international setting.

The international setting also allows us to examine the relation between the effect of consumption taxes on capital investment and labor. While we uncover a negative effect on corporate investment, Thompson and Rohlin (2012) show a negative effect on employment. We are able to relate and extend these results by examining the impact of consumption taxes on investment as a function of labor elasticity. The hypothesis is that the impact on investment is greater when labor supply is more downward rigid. When firms have limited ability to adjust labor supply (e.g., wages or levels of employment), firms have an increased incentive to reduce capital. We thus expect a stronger adverse investment response to consumption taxes. Using OECD data on employment protection laws, we show that the investment effect is stronger in countries with strict employment protection laws. In sum, having established the causal effect using the 2012 Dutch VAT increase as the quasi-natural experiment, we leverage the consumption tax changes from many countries and are able

to reinforce and provide external validity to our main findings and gain new insights about factors determining the magnitude of the effect of consumption tax on investment.

Altogether, while the past literature fails to detect effects of consumption taxes on aggregate private business investment (e.g., Alesina et al. 2002; Djankov et al. 2010; Arnold et al. 2011), we are the first to show that consumption taxes—in most countries, the most important tax revenue source—adversely affect corporate investment.[6] The investment response that we find is statistically and economically significant and, furthermore, the investment effect of consumption taxes is similar in magnitude to the effect of corporate taxes. We show that the adverse investment effect arises through firms' exposure to relatively elastic demand. That is, the investment response is attributable to firms with low market power, local product markets, or financial constraints. When firms face relatively elastic demand, they bear more of the consumption tax increase, which affects corporate investment negatively through reductions in profitability. This implies that capital owners partly bear consumption taxes.

Our results have implications for the longstanding debate on the efficiency of consumption- versus capital-based tax systems (e.g., Barro 1990; Kneller, Bleaney, and Gemmell 1999; Arnold et al. 2011), which may be particularly relevant given recent reforms that cut corporate taxes (e.g., the Tax Cuts and Jobs Act of 2017 in the United States). We enrich this discussion by being the first to provide elasticity estimates of corporate investment for consumption taxes in a system that simultaneously taxes capital and consumption, which is arguably a more realistic regime than a complete regime shift. Our estimates make salient that consumption taxes have equally distortive effects on corporate investment as corporate taxes. Therefore, consumption taxes should be featured more prominently in policy debates on effective mechanisms promoting economic growth through private sector investment. One implication of our findings is that a budget-neutral tax reform that raises consumption taxes to finance a corporate tax cut may not foster economic growth and investment after all. The ultimate investment effects critically depend on the relative demand elasticity a firm, an industry, or a sector of the economy faces.

1. Consumption Taxes and Corporate Investment

1.1 Predictions and empirical approach

Consumption taxes drive a wedge between the price that consumers pay and the price that producers receive. Theory predicts that the incidence of consumption

[6] For example, Djankov et al. (2010) suggest a positive association between consumption taxes and aggregate investment using a cross-section of 85 countries. Only one of their six specifications with aggregate investment as the dependent variable yields a significant consumption tax coefficient. While generally informative, their empirical identification stems from differences across countries, thus limiting the causal interpretation of their findings.

taxes on firms vis-à-vis consumers depends on the relative elasticity of demand vis-à-vis supply. To illustrate, consider a firm facing perfectly elastic demand (see, e.g., Gruber 2016, chap. 19). Consumers pay the same price after a tax increase and the firm bears the full burden of the tax. This increases the firm's marginal cost for any given produced quantity, which is why the firm decreases production. That is, the after-tax supplied and consumed quantity is lower. Accordingly, the firm's profit decreases, which reduces the firm's demand for capital (investment opportunity). Hence, capital investment decreases as investors supplying capital want to preserve their after-tax returns to capital. If capital supply were fully elastic, investment would immediately decrease and the return to capital would remain constant. However, there is likely friction in reducing the capital stock by, for example, fire-sale discounts that prevents investors from immediately reducing the capital stock; that is, capital is not fully elastic, leading to lower after-tax capital returns and, thus, lower profitability. In this case, capital owners bear the consumption tax increase.[7]

In contrast, if consumers are fully inelastic, consumers fully bear the tax through higher prices while the quantity and producer price are unchanged. Hence, the firm's profit and demand for capital remain unchanged, leaving capital investments and after-tax returns at the same level as before the tax increase.

Taken together, firms reduce capital investment more strongly the more elastic demand is relative to supply, that is, if they bear more of the consumption tax burden. Empirically, we examine firms' investment response as a function of the *relative* elasticity of demand and supply. We exploit firm attributes that indicate either very elastic demand (e.g., thin margins resulting from low market power) or inelastic supply (e.g., low flexibility stemming from financial constraints). Second, we examine whether consumption taxes reduce profitability, the key mechanism leading to reduced investment when firms' and capital owners bear consumption taxes. In a similar vein, we test whether consumption taxes reduce aggregate consumption in our international panel, which would be consistent with reduced quantities and investment opportunities.

1.2 Exploiting the Dutch 2012 VAT increase as a quasi-natural experiment

Exploiting the 2012 VAT increase in the Netherlands to examine the effect of consumption taxes on investment is opportune because (a) we can tie the features of the setting to the framework of relative elasticities of demand and supply and (b) the features of the setting allow us to mitigate standard identification concerns arising from the endogeneity of tax policy

[7] We focus on the capital investment response, but the firm also employs labor. Hence, the firm's tax burden can be borne either by capital through lower capital returns or by labor through lower wages. The incidence on capital owners depends on the relative elasticity of capital versus labor supply if capital is not fully elastic.

for the following reasons. First, the change was unanticipated by businesses, individuals, and the media.[8] The change resulted from mounting pressure from the European Commission to meet an arbitrary budget deficit threshold. In addition, the Netherlands historically had a stable consumption tax rate and had not changed it in comparable scenarios when budget deficits were also an issue as, for example, during the financial crisis of 2008–2009. The time line of events also suggests that the VAT increase came as a surprise and was implemented relatively quickly. Policy makers started deliberating about a VAT increase in March/April 2012 and the decision to do so starting from October 2012 was made in August 2012 with an adoption period of 3 months. This short adoption period also indicates that the VAT increase came as a surprise to the corporate sector.

Second, the VAT change from 19% to 21% is economically significant as it represents a 10.5% increase in the tax rate. This 2-percentage-point increase is above the median VAT rate change of 1 percentage point for countries in our international sample. We further performed a newspaper article search around the time of the adoption of the VAT to corroborate our argument that the change was economically significant. These articles indicate that the corporate sector was resistant to the VAT increase, particularly because firms anticipated they would have to bear a large part of the VAT increase.[9] Surveys from the time after the VAT increase support this expectation and many firms did not increase prices because they feared negative consumer responses.[10] Commentators also connected the VAT increase to reduced profits and increased bankruptcy rates.[11] In sum, the sizable increase in VAT, the unexpected nature of the increase, and its coverage in the media as a significant increase and burden suggest that this VAT increase represents an economically meaningful and unexpected change that is able to trigger changes in corporate investment and pricing policies.

Third, the change is not confounded by other tax changes, such as corporate taxes or payout taxes, which could affect investments. While excise taxes on both tobacco and alcohol were increased in January 2013, firms relying on producing or selling those particular goods are underrepresented in our sample and our inferences are unchanged if we exclude such firms. Conceptually,

[8] An article in the *de Volkskrant* from January 5, 2013 (Olsthoorn 2013) reports that one reason why many retail stores did not adjust their prices is that they did not have the time as the VAT increase came as a surprise.

[9] For example, an article in *Het Financieele Dagblad* from 5, September 18, 2012 (Mulder 2012), cites the overall resistance of the corporate sector to the VAT increase because it would harm consumer confidence and most of the VAT increase would be borne by companies.

[10] For example, an article in the NRC Handelsblad on October 1, 2012 (NRC Handelsblad 2012), reports that only 26% of the surveyed retailers said they would increase prices, whereas this percentage is reported to be even lower for other industries. Olsthoorn (2013) also reports that many retail stores did not increase their sales prices and therefore they bore the burden themselves.

[11] For example, articles at nu.nl (ANP 2013) on retailers and in Het Financieele Dagblad on December 5, 2013 Verbraeken (2013), report that the VAT increase has led to a loss of profits and may relate to an increase in bankruptcies.

we note that these excise taxes are consumption taxes and would thus affect investments through the same mechanisms outlined in Section 2.1.

Fourth, while the standard consumption tax rate increased from 19% to 21%, several industries were not subject to a consumption tax increase. For industries like agriculture, food, and medicine, the consumption tax rate remained constant. This feature provides us with an unaffected group of firms that differ by industry but that are otherwise subject to similar variation in local economic conditions.

Fifth, and finally, the Netherlands shares land borders with Germany and Belgium. Neither neighboring countries changed consumption taxes contemporaneously. As a result, consumers located at the Dutch border could more easily substitute Dutch goods and services with goods and services provided by Belgian or German firms. Hence, we expect consumer demand of firms located at the border to be more elastic than consumer demand of firms located farther from the border. Our abovementioned newspaper article search also reveals several articles that emphasize that the VAT increase was particularly burdensome for firms along the border.[12] That is, differences in the location of Dutch firms—border versus farther from the border—provide us with variation in the elasticity of demand that is plausibly exogenous to the VAT increase. As consumption taxes have a stronger adverse effect on profits and investment if consumer demand is relatively more elastic, firms farther from (closer to) the border are less (more) affected by the VAT increase but are subject to similar variation in industry conditions and the tax code.

In sum, this setting provides us with both a large and unanticipated consumption tax change, with some firms that are subject to the change in VAT and others that are not while all firms are subject to similar variation in local economic conditions. Further, the setting provides us with firms that we expect to be less affected by the VAT increase due to differences in the relative elasticity of demand vis-à-vis supply, but that face similar variation in industry conditions. Hence, we identify the investment effect of consumption taxes in this setting from a triple difference: We compare the change in investment around the VAT increase (first difference) across firms in affected versus unaffected industries (second difference), and across firms at the border vis-à-vis farther from the border (third difference) who face differential demand elasticity. The second and third differences are arguably exogenous with respect to the 2012 VAT increase, because firms could not anticipate the VAT change and thus select their industry or location in anticipation of the tax change. Taken together, we are able to attribute the change in investment of border firms from affected industries to the VAT increase because we difference out investment changes

[12] For example, Bangma (2014) reports significant decreases in tax revenues resulting from the VAT increase in border provinces, and Pals (2013) cites politicians at border-provinces taking action against the negative financial effects that border-provinces experience as a result of the VAT increase. Further, several articles mention negative effects of the VAT increase on cross-border shopping (e.g., Peters 2017; van der Aa 2017).

attributable to changes in industry conditions (nonborder firms from the same industry) and changes in local economic conditions (unaffected industry firms from the same region). Section 4.5 discusses and addresses concerns about potential alternative explanations.

2. Empirical Setting and Data

2.1 Data, estimation strategy, and descriptive statistics

We use all available data on Dutch firms from Bureau van Dijk's Amadeus database over the period 2009–2015. Specifically, we use Amadeus' unconsolidated financial statements of listed and unlisted firms with exact information on the address of each sample firm. Unconsolidated balance sheet data enable us to locate the activity of a single firm. In contrast, consolidated balance sheet data as, for example, provided in Compustat Global does not allow us to exactly identify the location of firms' activities because consolidated balance sheets comprise information pertaining to many firms consolidated into one economic group. For this reason, we use unconsolidated firm-level data. In our analysis, we require firms to exist for at least 4 years to have information from both before and after the tax change. We further require firms to report information on fixed assets, pretax profits, cash holdings, leverage, and assets. We exclude observations with negative total assets, depreciation, fixed assets, and sales. These sample requirements result in 1,502 firms and 4,832 observations.

Because the median (25th percentile) firm of the final sample has total assets of EUR 9.6 million (EUR 1.1 million), many sample firms likely have local output markets. Based on the postal code of each firm, we define the treatment group as Dutch firms located along the Belgian-Dutch or the German-Dutch border (*Border* = 1) in affected industries (proxied for by the two-digit SIC code; *Affected* = 1). The dummy variable *Border* is set to one if the two-digit ZIP code, which describes a region or city, shares a physical land border with either Belgium or Germany.[13] The average distance between border (Dutch-Interior) firms and the nearest Belgian or German border is approximately 10 km (63 km).[14] Figure A.4 illustrates the border regions according to our definition.[15] From the 1,502 sample firms, 258 firms are located at the border and 695 firms

[13] The following two-digit ZIP codes are defined as *Border*: 44, 45, 46, 47, 48, 50, 51, 55, 58, 59, 60, 61, 62, 63, 64, 65, 69, 70, 71, 74, 75, 76, 77, 78, 95, and 96.

[14] The average distance is calculated based on the midpoint of the four-digit ZIP code area of the firm and the Belgian or German border. We use the minimum distance to either border. The four-digit ZIP code areas used in this test are small regions with a median area of 5.5 sq km and a median radius of approximately 1.3 km. Hence, the midpoint of these areas captures local areas.

[15] As a robustness test, we use the actual distance in kilometer to the border. Table A.3 presents the regression result from using a continuous measure instead of the dummy variable. We find results consistent with our prediction ($\beta_6 < 0$) in this test. In panel B of Table A.3, we also illustrate the decay of the effect when firms are more distant from the border. Further, in Figure A.5, we show that our results are very similar when using the baseline Border definition coupled with a maximum distance to the border in kilometers.

operate in affected industries. We then estimate

$$Inv_{i,t} = \alpha_0 + \beta_1 Border_i + \beta_2 Border_i \times Post_t + \beta_3 Affected_i + \beta_4 Affected_i \times Post_t + \beta_5 Affected_i \times Border_i + \beta_6 Affected_i \times Border_i \times Post_t + \gamma \mathbf{X}_{i,j,t-1} + \alpha_t + \varepsilon_{i,t}, \quad (1)$$

where the dependent variable $Inv_{i,t}$ is the gross investment of firm i in year t. Because the Amadeus data do not include capital expenditures, we define investments as the change in fixed assets before depreciation over the prior year's total assets (e.g., Asker, Farre-Mensa, and Ljungqvist 2015). The variable $Post_t$ is equal to 1 for post-reform years 2013–2015. Our main variable of interest is the interaction *Affected* × *Border* × *Post*, which is the triple difference coefficient. We expect that, relative to unaffected industries, the consumption tax increase decreases corporate investments in affected industries of Dutch border firms relative to Dutch firms operating in the inner country ($\beta_6 < 0$). The coefficient β_2 captures the difference in investments between Dutch border firms and Dutch firms operating in the interior country within unaffected industries. Our baseline empirical strategy includes year fixed effects (α_t) and firm-level control variables ($\mathbf{X}_{i,j,t-1}$). The vector of lagged firm-level controls comprises firm size, cash holdings, profitability, and 2-year sales growth. The coefficient on $Post_t$ is not included in the regression, because it is absorbed by year fixed effects (α_t). The statistical inference is based on robust standard errors clustered at the firm level.

One concern about Equation (1) is that characteristics of firms along the border differ from those of firms farther from the border and that these differences might drive observed investment differences. To ensure that border and Dutch-interior sample firms are comparable in observable firm-level characteristics, we employ entropy-balancing matching (Hainmueller and Xu 2013). We match on firm-level controls (size, sales growth, financial leverage, and cash holdings) and industry codes in the year prior to the reform. We then use the weights from the entropy-balancing matching and reestimate Equation (1) (see also Patel and Seegert 2017). This sample is denoted as the "Matched sample" and comprises 1,098 firms and 4,054 observations.

Table 1 reports descriptive statistics for our variables using the full sample of 4,832 observations. Firms hold 17% as cash and short-term equivalents, their sales grow by 2% over a 2-year period, and they have a return on assets of about 7.4%. Because our investment measure also includes disposal of assets (because of data unavailability of capital expenditures), average gross investment amounts to 3% and is negative in about 16% of firm-year observations.

2.2 Identifying assumptions

Our triple difference design relies on the assumption that, absent the tax change, the investment difference between firms at the border in affected relative to

Table 1
Descriptive statistics, Dutch sample

Variable	Mean	SD	25th percentile	Median	75th percentile
Inv	0.0285	0.1241	0.0000	0.0095	0.0472
Post	0.4702	0.4992	0.0000	0.0000	1.0000
Border	0.1660	0.3721	0.0000	0.0000	0.0000
Affected industry	0.4458	0.4971	0.0000	0.0000	1.0000
Size	8.7788	2.4972	7.2006	9.2505	10.4480
Cash	0.1640	0.2224	0.0095	0.0663	0.2288
Profit	0.0693	0.2154	−0.0113	0.0474	0.1370
Sales growth	0.0239	0.3132	−0.0860	0.0161	0.1310
Leverage	0.7681	0.6505	0.3965	0.6557	0.9501

This table presents descriptive statistics of our main variables for 1,502 firms and 4,832 observations over the 2008–2015 period. Tables A1 and A2 define the variables.

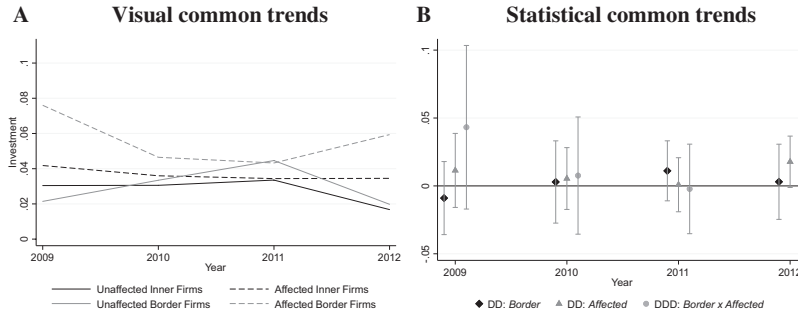

Figure 1
Pre-reform investment trends
This figure provides visual (panel A) and statistical (panel B) evidence that investment trends are similar for affected versus unaffected industries, border versus inner-Dutch firms, and for both dimensions combined. Panel B reports the coefficient estimates of the interaction between year fixed effects and the *Border*, *Affected*, and *Affected* × *Border* indicator variables, respectively. DD (DDD) abbreviates the difference-in-differences (triple difference) indicating that the respective variables (interaction) are (is) compared over time. Following Patel and Seegert (2017) and Ohrn and Seegert (2017), the common trend assumption is supported by the failure to reject that these coefficients are zero.

firms from unaffected industries and firms more distant from the border would have evolved similarly. While untestable for the post-reform years, we can test whether the investment of our treatment and control groups follow a common trend prior to the VAT increase. Panel A of Figure 1 shows that the average gross investment is relatively similar across the different groups based on border firms versus Dutch-interior firms, as well as firms in affected versus unaffected industries. Following the approach in Patel and Seegert (2017) and Ohrn and Seegert (2017), we formally test for the parallel trend over the 2009–2012 period by regressing gross investment on year fixed effects and interactions of year fixed effects with *Border*, *Affected*, and *Border* × *Affected*. The coefficients on the interactions of year fixed effects with these three dummy variables estimate the difference in the trends between our different groups.

Panel B of Figure 1 presents the coefficient estimates along with the 95% confidence bounds for these interactions. All interactions of *Border, Affected*, or *Border* × *Affected* with any of the year fixed effects are insignificant as indicated by the 95% confidence bounds that include zero. In further support of the common trends assumptions, we test for joint significance of the interactions (Ohrn and Seegert 2017; Patel and Seegert 2017). For all three groups (*Border, Affected*, or *Border* × *Affected*), we fail to reject the joint test that all interactions are zero with p-values well above 10%. Figure A.6 of the Online Appendix shows that the results are very similar when using the matched sample. All interactions of *Border, Affected*, or *Border* × *Affected* with the year fixed effects are insignificant. Again, we fail to reject the null that all interactions are zero. These tests suggest that our main findings, as described below, are unlikely to be driven by differences in the time trends across groups prior to the VAT increase.

3. Results

3.1 Baseline results

Before turning to the triple difference results based on Equation (1), we present difference-in-differences results first for firms at the border versus firms farther from the border around the reform and, second, for affected versus unaffected industries around the reform. The first difference-in-differences results are presented in panel A of Table 2 and the second set of results are presented in panel B. We present estimates for the *Full Sample* (Columns 1 to 3) as well as for the *Matched Sample* (Columns 4 to 6). The results in Columns 1 and 4 of panel A indicate that relative to firms farther from the border, Dutch firms located at the border facing relatively more elastic demand decrease their capital investments. As we further show in Columns 2 and 5, this investment reduction is driven by firms in affected industries. We do not find a difference in investment activities in unaffected industries between firms located at the border and firms farther from the border. This result indicates that it is unlikely that differences in the local economic development in border versus nonborder regions drive our result for Dutch border firms in affected industries. Further, in panel B, we show that there appears to be an investment decrease in affected industries relative to unaffected industries when considering the results in the matched sample. When breaking down the difference between affected and unaffected industries (Columns 2, 3, 5, and 6), we find a statistically significant difference between affected and unaffected industries for firms at the border (*Border Firms*). However, for firms farther from the border (*Dutch-interior firms*), we do not find a statistically significant difference between affected and unaffected industries, which is consistent with our assumption that firms farther from the border face relatively less elastic consumer demand. This result also indicates that it is unlikely that differences in the development of industry

Table 2
Consumption taxes and investment around the 2012 Dutch VAT increase, difference-in-differences estimates

A. Comparing border versus inner-Dutch firms

	Full sample			Matched sample		
	All firms (1)	Affected industries (2)	Unaffected industries (3)	All firms (4)	Affected industries (5)	Unaffected industries (6)
Border	0.0047	0.0380	−0.0350**	0.0364	0.0433	0.0212
	(0.0226)	(0.0277)	(0.0140)	(0.0269)	(0.0288)	(0.0297)
Border × Post	−0.0144*	−0.0357***	0.0051	−0.0263***	−0.0473***	−0.0077
	(0.0083)	(0.0128)	(0.0106)	(0.0095)	(0.0148)	(0.0115)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
ZIP and year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,832	2,154	2,678	4,054	1,771	2,283
Adj. *R*-squared	.011	.010	.016	.030	.060	.067

B. Comparing affected versus unaffected industries

	Full sample			Matched sample		
	All firms (1)	Border firms (2)	Dutch-interior firms (3)	All firms (4)	Border firms (5)	Dutch-interior firms (6)
Affected	0.0084	0.0244**	0.0052	0.0190**	0.0243**	0.0160
	(0.0056)	(0.0113)	(0.0064)	(0.0082)	(0.0122)	(0.0109)
Affected × Post	−0.0054	−0.0428***	0.0018	−0.0230**	−0.0440***	−0.0051
	(0.0072)	(0.0149)	(0.0081)	(0.0093)	(0.0158)	(0.0102)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
ZIP and year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,832	799	4,033	4,054	673	3,381
Adj. *R*-squared	.010	.028	.009	.030	.043	.018

This table presents triple difference-in-differences estimates on investment behavior around the 2012 VAT increase in the Netherlands. In Panel A, we compare Dutch firms at the border (*Border* = 1) to Dutch firms without a direct border with Germany or Belgium (*Border* = 0 or *Dutch-interior firms*). In Panel B, we also include interactions with a dummy *Affected industry* (*Unaffected industry*), which is equal to 1 (0) for firms operating in industries subject to the increased VAT. We include firm-level control variables, year and two-digit ZIP code fixed effects in all regressions. We report robust standard errors, clustered at the firm level, in parentheses. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$.

conditions in affected versus unaffected industries around the VAT increase drive our result for Dutch border firms in affected industries.

Collectively, the difference-in-differences results support our prediction that consumption taxes adversely affect investment. However, these tests do not allow for a statistical test of whether the investment difference between the border and nonborder firms in affected industries differs from the (insignificant) investment difference between the border and nonborder firms in unaffected industries. To formally test for this difference, we employ the triple difference model. Figure 2 presents a graphical illustration of the triple difference. We plot the coefficient on *Border* × *Affected* from a regression including control variables for the 3 years before and the 3 years after the VAT increase. In other words, we plot the double difference—the difference between affected and unaffected industries (first difference) and between firms located at the border and firms farther from the border (second difference)—over time. The

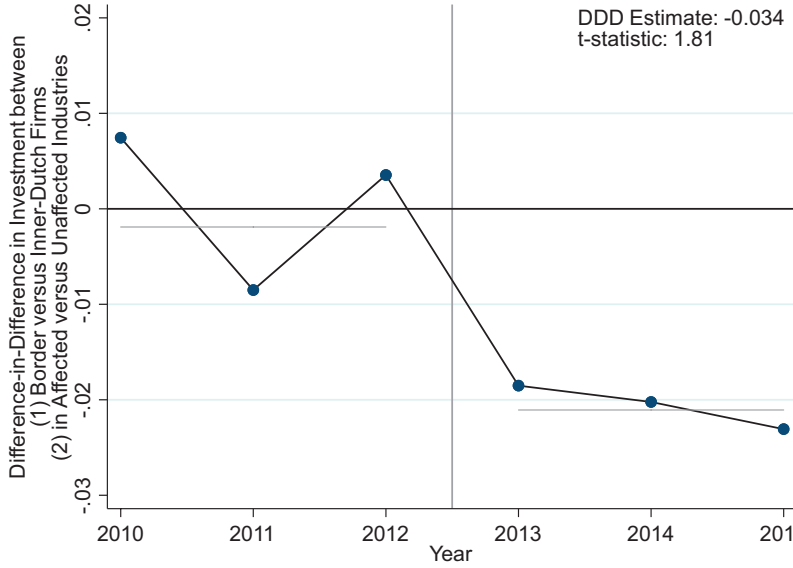

Figure 2
Graphical illustration of the triple difference
This figure illustrates the difference in investment behavior between firms in affected and in unaffected industries operating at the border versus firms operating farther from the border. The vertical line indicates the 2012 VAT change. The difference in gray lines is the triple difference estimate (DDD estimate). We include control variables and use the matched sample. The *t*-statistic of the triple difference estimate is based on standard errors clustered at the firm level.

difference between the pre- and post-reform periods is equal to the triple difference. Consistent with the results supporting the parallel trends assumption above, we find that the double difference is around zero prior to the VAT increase. After 2012, the double difference drops to a lower level, suggesting a negative investment response to the VAT increase for Dutch firms in affected industries along the border. Figure 2 also shows that the investment effect does not reverse, suggesting that consumption taxes permanently affect corporate investment.

In Table 3, we present results from testing our triple difference model with controls from Equation (1). We report the investment response of border firms relative to firms farther from the border operating in affected industries in the line *Border* × *Post* × *Affected Industry*. This estimate is obtained by adding the β_2 and β_6 coefficients. We also report the β_2 coefficient (*Border* × *Post* × *Unaffected Industry*), which captures the investment effect of border firms relative to firms farther from the border operating in unaffected industries. The difference in these coefficients is the triple difference coefficient β_6. We find a negative investment response to the 2012 VAT increase for Dutch firms in affected industries along the border (*Border* × *Post* × *Affected Industry* < 0). This effect is significantly different from the investment response of

Table 3
Consumption taxes and investment around the 2012 Dutch VAT increase, triple difference estimates

	Full sample		Matched sample	
	(1)	(2)	(3)	(4)
Border × Post × Unaffected Industry	0.0067	0.0055	−0.0074	−0.0085
	(0.0104)	(0.0105)	(0.0119)	(0.0116)
Border × Post × Affected Industry	−0.0353***	−0.0377***	−0.0460***	−0.0472***
	(0.0129)	(0.0130)	(0.0149)	(0.0150)
Border	0.0045	−0.0053	0.0062	0.0306
	(0.0082)	(0.0232)	(0.0097)	(0.0289)
Affected Industry	0.0086	0.0049	0.0123	0.0127
	(0.0061)	(0.0063)	(0.0095)	(0.0106)
Border × Affected Industry	0.0147	0.0197	0.0108	0.0117
	(0.0117)	(0.0120)	(0.0138)	(0.0148)
Post × Affected Industry	0.0147	0.0197	0.0108	0.0117
	(0.0117)	(0.0120)	(0.0138)	(0.0148)
Size	−0.0011	−0.0007	−0.0010	−0.0013
	(0.0010)	(0.0011)	(0.0016)	(0.0017)
Cash	0.0096	0.0059	−0.0010	−0.0041
	(0.0086)	(0.0088)	(0.0157)	(0.0168)
Profit	0.0184	0.0201*	0.0132	0.0189
	(0.0118)	(0.0121)	(0.0226)	(0.0219)
Sales growth	0.0130*	0.0109	0.0074	0.0049
	(0.0077)	(0.0077)	(0.0108)	(0.0106)
Leverage	−0.0031	−0.0008	−0.0027	0.0001
	(0.0037)	(0.0035)	(0.0079)	(0.0068)
Difference in coefficients Unaffected versus Affected Industries (=β_6)	−0.0421**	−0.0431**	−0.0386**	−0.0386**
	(0.0166)	(0.0168)	(0.0192)	(0.0189)
Controls and year FEs	Yes	Yes	Yes	Yes
ZIP FEs	No	Yes	No	Yes
Observations	4,832	4,832	4,054	4,054
Adjusted R-squared	.006	.011	.010	.034

This table presents triple differences (DDD) estimates on investment behavior around the 2012 VAT increase in the Netherlands. We compare Dutch firms at the border (*Border* = 1) to Dutch firms without a direct border with Germany or Belgium (*Border* = 0). We also include interactions with a dummy *Affected Industry* (*Unaffected Industry*), which is equal to 1 (0) for firms operating in industries subject to the increased VAT. We include year fixed effects in all regressions. Columns 2 and 4 also include fixed effects for the two-digit ZIP code. We report robust standard errors, clustered at the firm level, in parentheses. $^*p<.1$; $^{**}p<.05$; $^{***}p<.01$.

firms along the border in unaffected industries ($\beta_6<0$). We find significant β_6 coefficients for the full sample as well as for the matched sample. In economic terms, the coefficient estimate of −0.0386 in Column 4 can be interpreted as follows: relative to firms farther from the border, firms located at the border in affected industries decrease gross investments by 3.86% of their total assets. Given that the average total assets in our sample is about EUR 9.6 million, investment decreases by about EUR 0.27 million. Translated into an elasticity of investments in the capital stock (that is, total assets) for the consumption tax rate, the coefficient in Column 4 is equivalent to an elasticity of −0.36.[16]

[16] We calculate the implied elasticity as follows. The capital stock of the average firm increases, on average, by 3% (our sample average gross investment). A reduction in investment by 3.86% means that the treated firm's capital stock decreases by 3.748% [= 3.86% / (1+3%)] relative to an untreated firm. Because the VAT increase of 2 percentage points is equivalent to a tax increase of 10.53% (= 2% increase / 19% pre-reform tax rate), the implied elasticity is equal to −0.36(=−3.748%/10.53%)

Across our four specifications, the implied elasticities range between −0.36 (Columns 3 and 4) and −0.40 (Column 2).

To put the consumption tax effect into perspective, we compare our estimate to recent evidence on the elasticity of investment with respect to the corporate tax. Relative to the corporate tax, our estimates of the consumption tax elasticity of investment suggest that consumption taxes affect corporate investment in an economically significant way.[17] While Giroud and Rauh (forthcoming) estimate an elasticity of investment with respect to the corporate tax rate of about −0.4 to −0.5 using establishment data, other approaches using the population of U.S. firms—both unlisted and listed firms—find an elasticity of real investment with respect to the tax rate of −0.21 (Patel, Seegert, and Smith 2017). Our results thus suggest that consumption taxes can affect corporate investment by a similar magnitude (using the Giroud and Rauh estimates) or even slightly greater (using the Patel, Seegert, and Smith estimates) than corporate taxes. The incidence of consumption taxes, as well as of the corporate tax, can fall on consumers (or employees) depending on the elasticity of demand (or labor).[18] In other words, it does not matter who pays the tax—consumers pay the consumption tax and firms pay corporate tax—the tax incidence of both taxes is a function of relative elasticities. Hence, firms' investment is expected to be equally responsive to both taxes. Given their importance for the overall tax revenue, in particular, relative to corporate taxes, consumption taxes appear to have a large impact on firms. However, the impact of the consumption tax critically depends on the relative elasticity of demand vis-à-vis supply that we further exploit below in our cross-sectional analyses.[19]

We also note that the estimates of the other interaction coefficients between *Border*, *Post*, and/or *Affected Industry* reported in Table 3 further support the validity of our identifying assumption. For example, the insignificant β_2 coefficient (denoted as *Border* × *Post* × *Unaffected Industry* in Table 3) indicates that, around the tax change, firms at the border in unaffected industries experience an investment trend similar to that of firms farther from the border in unaffected industries. Further, we find that all other interactions or main effects of *Border, Post*, and/or *Affected Industry*, respectively, are statistically

[17] In contrast to our magnitudes, Djankov et al. (2010) find a significant relation between aggregate investment and corporate taxes but only one significant result (at the 10% level) of a correlation between VAT and investment. Further, their coefficient estimate from Column 3 of Table 5A for VAT (0.104) is well below the estimate for corporate taxes (0.248), suggesting that consumption taxes have no or, at least, a much smaller relation with investments.

[18] Recent evidence shows that firms pass on part of the corporate tax to employees in the form of lower wages (Suárez Serrato and Zidar 2016; Fuest, Peichl, and Siegloch 2018).

[19] We note that this finding complements existing evidence on the effect of consumption taxes on employment. Data limitations render it unfeasible to examine an employment response in our setting, which is characterized by relatively strong employment protection as evidenced by the Netherlands ranking third across OECD countries. However, in the results from the international panel described in Section 5, we show stronger capital investment responses to consumption tax increases in countries with stronger employment protection. We also note that data limitations prevent us from examining other investments, such as research and development (R&D) investments. We leave this important question for future research.

insignificant in Table 3. These results suggest that the variation in local economic conditions along the border is comparable to the variation in local economic conditions farther from the border. Against the background of parallel trends for border and inner-country firms' investment in unaffected industries, an alternative explanation for our finding would arise from differences in the variation in economic conditions affecting only firms from industries experiencing a tax change along the border, and not firms from industries that do not experience a tax change along the border (all relative to nonborder firms in those industries). We test for potential alternative explanations—economic and political uncertainty and austerity measures—in Section 4.5. Section 4.3 further examines cross-sectional variation in demand elasticity to further corroborate that the effect of consumption taxes on investment is a function of relative elasticities as suggested by our predictions in Section 2.1.

3.2 Examining the potential mechanism

In the next step, we examine the mechanism through which consumption taxes can affect corporate investment. If firms' reduction in investment reflects the firms bearing a part of the consumption tax increase, we would expect that a firm's profitability resulting from investment projects would decrease after the VAT is increased. Following the VAT increase, firms reduce the produced quantity but because of frictions, such as fire-sale discounts, firms cannot fully and immediately adjust their capital stock. Hence, the VAT increase reduces after-tax returns to capital. This reduction in the profitability could plausibly cause the lower level of investments documented in Table 3 because firms find fewer positive NPV investment opportunities when profitability decreases as a result of bearing an increasing VAT share. Naturally, this decreased profitably applies not only to future investment but also to prior investments by the firm. To test the reduced profitability mechanism, we reestimate Equation (1) but use profits before tax over lagged total assets as the dependent variable. We thereby test the mechanism as previously in a triple difference setting and expect that the reduction in profits is concentrated in firms located at the border and operating in affected industries.

Results are presented in Table 4. The coefficient of interest is the triple difference coefficient β_6 (denoted as *Difference in coefficients*) as well as the overall effect for firms in affected industries located at the border (denoted as *Border* $\times$ *Post* $\times$ *Affected industry* and calculated as $\beta_2+\beta_6$). In all specifications, we find a negative and significant triple difference coefficient as well as a negative overall effect on profitability of firms in affected industries located at the border. This result is consistent with the notion that consumption taxes decrease profitability (i.e., capital returns), because firms cannot fully pass on the tax to consumers, whose demand is not fully inelastic. As demand becomes more elastic (i.e., for border firms), firms' ability to pass on the additional tax burden to consumers is reduced and their profitability declines. This tax-induced reduction in profitability then translates into lower

Table 4
Consumption taxes and profitability

	Full sample		Matched sample	
	(1)	(2)	(3)	(4)
Border × Post × Unaffected industry	0.0165	0.0175	0.0031	0.0136
	(0.0133)	(0.0134)	(0.0134)	(0.0166)
Border × Post × Affected industry	−0.0471*	−0.0442*	−0.0637**	−0.0613**
	(0.0289)	(0.0260)	(0.0297)	(0.0295)
Difference in coefficients	−0.0637**	−0.0617**	−0.0668**	−0.0734**
	(0.0289)	(0.0292)	(0.0322)	(0.0326)
Controls and year FEs	Yes	Yes	Yes	Yes
ZIP FEs	No	Yes	No	Yes
Observations	3,367	3,367	3,012	3,012
Adjusted *R*-squared	.320	.327	.319	.650

This table presents triple differences (DDD) estimates on profitability around the 2012 VAT increase in the Netherlands. We compare Dutch firms at the border (*Border* = 1) to Dutch firms without a direct border with Germany or Belgium (*Border* = 0). We also include interactions with a dummy *Affected industry* (*Unaffected industry*), which is equal to 1 (0) for firms operating in industries subject to the increased VAT. We use profitability, defined as profit before tax over lagged total assets, as the dependent variable. We include year fixed effects in all regressions. Columns 2 and 4 also include fixed effects for the two-digit ZIP code. We report robust standard errors, clustered at the firm level, in parentheses. $*p < .1$; $**p < .05$; $***p < .01$.

capital investments (see Table 3). In all regressions, we find no differences in profitability between border and Dutch-interior firms in unaffected industries. Together with the investment results, this further supports the parallel trends assumption. In the control group of firms in unaffected industries, we do not find a difference in profitability between border firms and Dutch-interior firms around the VAT increase.

3.3 Additional analyses: Heterogeneity in investment effects

To further corroborate that differences in the relative elasticity of demand and supply explain our findings, we decompose the adverse investment effect based on predictions regarding the extent to which firms can pass on consumption taxes to consumers (e.g., Atkinson and Stiglitz 1972; Kotlikoff and Summers 1987). Our main analysis incorporates this notion by investigating border versus nonborder firms, because consumers of firms along the border are plausibly more elastic than consumers of nonborder Dutch firms. Our next cross-sectional analyses subject our main results to further stress tests by splitting the sample firms not only by how close they are to the border but also by other factors affecting the relative elasticity of demand vis-à-vis supply.

We run three distinct tests within industries affected by the VAT change. First, we use the firm's profit margin to proxy for demand elasticity: firms with lower market power, as measured by low profit margins, face greater elastic demand (e.g., Lerner 1934; Grullon et al. 2018). To examine how the investment effect varies with a firm's profit margin, we interact *Border*, *Post*, and *Border* × *Post* with *Low margin (High margin)*. The variable *Low margin (High margin)* is a dummy variable equal to 1 if the average profit margin, defined as operating income over sales, in 2010 and 2011 is below (above) the bottom quartile of the

Table 5
Consumption taxes and investment, cross-sectional analyses

A. Within affected industry analysis, breakdown by margin

	Full sample		Matched sample	
	(1)	(2)	(3)	(4)
Border × Post × Low margin	−0.0875***	−0.0846***	−0.1002**	−0.0977**
	(0.0318)	(0.0302)	(0.0392)	(0.0412)
Border × Post × High margin	−0.0221	−0.0239	−0.0235	−0.0229
	(0.0138)	(0.0147)	(0.0160)	(0.0169)
Difference in coefficients	−0.0654*	−0.0608*	−0.0767*	−0.0748*
	(0.0348)	(0.0342)	(0.0420)	(0.0453)
Controls and year FEs	Yes	Yes	Yes	Yes
ZIP FEs	No	Yes	No	Yes
Observations	2,004	2,004	1,764	1,764
Adjusted *R*-squared	.007	.012	.026	.069

B. Within affected industry analysis, breakdown by size

	Full sample		Matched sample	
Border × Post × Small	−0.0916**	−0.0952***	−0.1099***	−0.1059***
	(0.0367)	(0.0350)	(0.0399)	(0.0380)
Border × Post × Large	−0.0211	−0.0202	−0.0254	−0.0254
	(0.0131)	(0.0134)	(0.0178)	(0.0174)
Difference in coefficients	−0.0706*	−0.0750**	−0.0844*	−0.0805*
	(0.0389)	(0.0370)	(0.0436)	(0.0420)
Controls and year FEs	Yes	Yes	Yes	Yes
ZIP FEs	No	Yes	No	Yes
Observations	2,004	2,004	1,764	1,764
Adjusted *R*-squared	.006	.0133	.022	.070

C. Within affected industry analysis, breakdown by cash flow

	Full sample		Matched sample	
Border × Post × Low cash flow	−0.1100***	−0.1067***	−0.1259***	−0.1218***
	(0.0318)	(0.0316)	(0.0399)	(0.0421)
Border × Post × High cash flow	−0.0160	−0.0172	−0.0141	−0.0126
	(0.0129)	(0.0128)	(0.0144)	(0.0139)
Difference in coefficients	−0.0939***	−0.0895***	−0.1118***	−0.1092**
	(0.0345)	(0.0342)	(0.0425)	(0.0446)
Controls and year FEs	Yes	Yes	Yes	Yes
ZIP FEs	No	Yes	No	Yes
Observations	2,004	2,004	1,764	1,764
Adjusted *R*-squared	.013	.017	.040	.085

This table presents triple differences (DDD) estimates on investment behavior around the 2012 VAT increase in the Netherlands. We compare Dutch firms at the border (*Border* = 1) to Dutch firms without a direct border with Germany or Belgium (*Border* = 0). In panel A, we run triple difference analyses within affected industries. We additionally interact *Border*, *Post*, and *Border* × *Post* with *Low margin* (*High margin*), which is a dummy variable equal to 1 if the margin defined as operating profits over sales is below (above) the bottom quartile of the 2-year average margin in 2011. In panel B, we run triple difference analyses within affected industries. We additionally interact *Border*, *Post*, and *Border* × *Post* with *Small* (*Large*), which is a dummy variable equal to 1 if size is below (above) the bottom quartile total assets in 2011. Columns 2 and 4 also include fixed effects for the two-digit ZIP code. In panel C, we additionally include interactions with *Low cash flow* (*High cash flow*), which is a dummy variable equal to 1 if the ratio of cash flows to assets is below (above) the bottom quartile of the 2-year average cash flow in 2011. We report robust standard errors, clustered at the firm level, in parentheses. *$p < .1$; **$p < .05$; ***$p < .01$.

profit margin distribution. Panel A of Table 5 presents the regression results. The negative investment effect documented for border firms in affected industries is concentrated in firms with low margins using the full sample as well as the matched sample. Further, the triple difference coefficients are significant in all specifications. This result is consistent with the notion that firms facing relatively elastic demand, as proxied for by low margins, bear more of the VAT burden and, hence, reduce investment more strongly.

Second, we use firm size as another proxy for demand elasticity. The effect of differences in demand elasticity of firms along the border vis-à-vis firms farther from the border depends on whether firms' output markets are indeed local. We argue that, as opposed to large firms, small firms have more local output markets and face more elastic demand if they are located along the border. That is, small firms should primarily drive the average negative effect because their demand is more likely affected by Belgian and German substitute goods and services. We, therefore, decompose the negative investment effect after the increase in VAT for border firms in affected industries by firm size. For firms in affected industries, we augment Equation (1) by interacting *Border, Post*, and *Border* × *Post* with *Small* (*Large*), a dummy variable equal to 1 if a firm's total assets are below (above) the bottom quartile of total assets in 2011. Panel B of Table 5 presents the regression results. We find that the negative investment effect documented for border firms in affected industries is concentrated in firms with local output markets, that is, small firms. Again, the overall effect for small firms, as well as the difference between small and large firms, is significant in all specifications.

Finally, firms with financial constraints have lower flexibility, that is, they are relatively inelastic vis-à-vis demand. Hence, they should cut investment more strongly when consumption taxes increase.[20] To test this empirically, we interact *Border*, *Post*, and *Border* × *Post* with a *Low cash flow* (*High cash flow*) dummy that is equal to 1 when the average operating cash flow to assets ratio in 2010 and 2011 is below (above) the bottom quartile of the cash flow to assets ratio distribution. Panel C of Table 5 reports the results, which are in line with the argument that financially constrained firms are relatively inelastic vis-à-vis demand and, therefore, more responsive to consumption tax changes as indicated by the significant triple difference estimate as well the significant overall effect for low cash flow firms.

In sum, the results in Table 5 suggest that the adverse investment effect of consumption taxes increases with the relative elasticity of demand vis-à-vis supply. We find no evidence of investment differences between larger firms, high margin firms, or high cash flow firms headquartered along the border relative to those farther from the border. These type of firms face relatively

[20] Alternatively, one could argue that for a given level of tax incidence, financially constrained firms react more negatively, because consumption taxes reduce internal cash flows on which financially constrained firms depend more heavily.

Table 6
Consumption taxes and investment around the 2012 Dutch VAT increase, placebo analysis

	Full sample		Matched sample	
	(1)	(2)	(3)	(4)
Border × Post × Unaffected Industry	0.0115	0.0129	0.0120	0.0135
	(0.0150)	(0.0143)	(0.0179)	(0.0185)
Border × Post × Affected Industry	0.0161	0.0170	0.0257	0.0261
	(0.0184)	(0.0188)	(0.0240)	(0.0242)
Difference in coefficients	0.0046	0.0041	0.0136	0.0126
	(0.0239)	(0.0239)	(0.0308)	(0.0315)
Controls and year FEs	Yes	Yes	Yes	Yes
ZIP FEs	No	Yes	No	Yes
Observations	2,896	2,896	2,611	2,611
Adjusted R-squared	.043	.043	.047	.074

This table presents triple differences (DDD) estimates on investment behavior around a placebo VAT increase in the Netherlands. We compare Dutch firms at the border (*Border* = 1) to Dutch firms without a direct border with Germany or Belgium (*Border* = 0). We use 2008 and 2009 as pre-reform years and 2010 and 2011 as post-reform years. We report robust standard errors, clustered at the firm level, in parentheses. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$.

lower demand elasticity: (1) Firms away from the border, where consumers have fewer substitutes and, hence, are less sensitive to price increases associated with VAT; (2) larger firms that have a more geographically dispersed consumer base (e.g., consumers both in the interior and along the border); (3) high margin firms with greater market power; and (4) firms with sufficient financial flexibility are better able to pass on taxes to consumers.

3.4 Placebo analysis

We assess the robustness of the main result around a pseudoreform in 2010. We use a sample over the 2008–2011 period. We set the post-reform (pre-reform) period to the 3 years after (before) a pseudo tax change in 2010. We then reestimate Equation (1) using a sample of 2,896 observations. Results in this placebo analysis are reported in Table 6. We find insignificant coefficients for all groups. Around the pseudoreform, we do not find a statistically significant change in corporate investment between border firms and Dutch-interior firms in affected or unaffected industries. Also, the difference between the estimates (i.e., the triple difference estimate) is statistically insignificant. Importantly, the economic magnitudes of the placebo estimates are small, insignificant, and in the opposite direction compared to our main findings around the actual 2012 VAT increase. The results from this placebo analysis corroborate our earlier analyses on the parallel trends assumption prior to the consumption tax increase (e.g., in Figure 1) and further suggest the differences in investment we document around 2012 can be attributed to the change in consumption tax.

3.5 Addressing alternative explanations

Can political or economic uncertainty around 2012 explain the investment decline in treated firms at the border, but not elsewhere in the Netherlands? For example, perhaps instead of the VAT increase, austerity measures, such as

reduced unemployment benefits or the increase in the retirement age, resulted in the heterogeneous responses of investment we document around 2012. To address the concern that such alternative explanations are responsible for the results documented in Tables 3, 4, and 5, we rerun the cross-sectional analysis from Table 5 around the 2012 VAT increase but use the sample of firms operating in unaffected industries. These firms are not affected by the VAT increase but are subject to similar variation in economic uncertainty, the change in unemployment benefits, and the increase in the retirement age as firms in affected industries are. Finding the same responses for firms in unaffected industries as for the sample of firms operating in affected industries would indicate that uncertainty and/or other policies, but not the VAT increase, drive our results. The results in Table 7, however, support our interpretation of the results that the VAT increase drives the decrease in corporate investment of affected firms. From the 36 coefficient estimates, 34 are insignificant, two are significant at the 10% level, the magnitudes are well below the estimates in Table 5, and the estimates are even positive in two cases. Finally, in untabulated analyses, we find that the reform effect for the treated border firm type (low margin firms, small firms, and low cash flow firms) differs between affected and unaffected firms, further supporting the notion that the 2012 VAT increase drives our findings.[21]

To summarize, using the consumption tax increase in 2012 and examining firms' investment response allow us to draw the following conclusions. First, consumption taxes adversely affect firm investment. Second, the adverse investment impact is a function of the relative elasticity of demand: As demand becomes more elastic, the adverse impact of consumption taxes becomes stronger. Third, consistent with firms bearing consumption taxes, consumption taxes adversely reduce profitability, that is, after-tax capital returns. The reduction in profits resulting from consumption taxes translates into lower investment because it lowers investment opportunities. At the same time, one should keep in mind the precise identification achieved in this experiment comes at a cost. Specifically, while having strong internal validity, the treatment group comprises only a small sample of firms suggesting that documenting the external validity of our findings is merited. We, therefore, move on to assess the external validity of our findings using an international panel of consumption tax changes in numerous countries in the following section.

4. External Validity: Consumption Tax and Investment around the World

While the Dutch setting allows us to draw causal inferences about the effect of consumption taxes on investment, solely relying on a single tax change has its

[21] When testing for significant differences in the *Border* × *Post* × *Low margin* effect across models, we obtain *p*-values for the difference of 4.7% and 4.8% in Columns 1 and 2, respectively. When using *Low Cash Flow*, we obtain *p*-values of 0.2% and 0.3% in Columns 1 and 2, respectively. In the case of *Small*, the *p*-values are 15% (Column 1) and 10% (Column 2).

Table 7
Consumption taxes and investment around the 2012 Dutch VAT increase, testing alternative explanations

A. Within unaffected industry analysis, breakdown by margin

	Full sample		Matched sample	
	(1)	(2)	(3)	(4)
Border × Post × Low margin	−0.0147	−0.0150	−0.0268	−0.0254
	(0.0187)	(0.0197)	(0.0186)	(0.0203)
Border × Post × High margin	0.0068	0.0072	0.0026	0.0022
	(0.0124)	(0.0123)	(0.0149)	(0.0131)
Difference in coefficients	−0.0215	−0.0222	−0.0295	−0.0276
	(0.0224)	(0.0237)	(0.0241)	(0.0245)
Controls and year FEs	Yes	Yes	Yes	Yes
ZIP FEs	No	Yes	No	Yes
Observations	2,495	2,495	2,275	2,275
Adjusted *R*-squared	.010	.015	.013	.066

B. Within unaffected industry analysis, breakdown by size

	Full sample		Matched sample	
Border × Post × Small	−0.0193	−0.0214	−0.0182	−0.0272
	(0.0347)	(0.0306)	(0.0352)	(0.0311)
Border × Post × Large	0.0085	0.0082	0.0005	0.0014
	(0.0105)	(0.0106)	(0.0131)	(0.0118)
Difference in coefficients	−0.0278	−0.0296	−0.0187	−0.0286
	(0.0364)	(0.0319)	(0.0381)	(0.0334)
Controls and year FEs	Yes	Yes	Yes	Yes
ZIP FEs	No	Yes	No	Yes
Observations	2,495	2,495	2,275	2,275
Adjusted *R*-squared	.010	.014	.012	.064

C. Within unaffected industry analysis, breakdown by cash flow

	Full sample		Matched sample	
Border × Post × Low cash flow	0.0244	0.0129	−0.0301	−0.0418*
	(0.0236)	(0.0218)	(0.0235)	(0.0218)
Border × Post × High cash flow	−0.0005	0.0022	0.0017	0.0051
	(0.0116)	(0.0116)	(0.0140)	(0.0127)
Difference in coefficients	0.0249	0.0107	−0.0319	−0.0469*
	(0.0264)	(0.0247)	(0.0276)	(0.0257)
Controls and year FEs	Yes	Yes	Yes	Yes
ZIP FEs	No	Yes	No	Yes
Observations	2,495	2,495	2,275	2,275
Adjusted *R*-squared	.026	.025	.022	.072

This table presents triple differences (DDD) estimates on investment behavior around the 2012 VAT increase in the Netherlands. We compare Dutch firms at the border (*Border* = 1) to Dutch firms without a direct border with Germany or Belgium (*Border* = 0). In panel A, we run triple difference analyses within unaffected industries. We additionally interact *Border*, *Post*, and *Border* × *Post* with *Low margin* (*High margin*), which is a dummy variable equal to 1 if the margin defined as operating profits over sales is below (above) the bottom quartile of the 2-year average margin in 2011. In panel B, we run triple difference analyses within unaffected industries. We additionally interact *Border*, *Post*, and *Border* × *Post* with *Small* (*Large*), which is a dummy variable equal to 1 if size is below (above) bottom quartile total assets in 2011. In panel C, we additionally include interactions with *Low cash flow* (*High cash flow*), which is a dummy variable equal to 1 if the ratio of cash flows to assets is below (above) the bottom quartile of the 2-year average cash flow in 2011. Columns 2 and 4 also include fixed effects for the two-digit ZIP code. We report robust standard errors, clustered at the firm level, in parentheses. *$p < .1$; **$p < .05$; ***$p < .01$.

limitations. Our second set of analyses extends the sample to an international setting using information from 68 countries. This external validity test is important as it ensures that our results so far are not unique to the 2012 VAT change in the Netherlands. Moving to an international sample may come at the cost of a less explicit causal relationship between consumption tax and investment. However, we argue that the results from our international sample are valuable in assessing how consumption taxes affect corporate investment when considered in combination with the causal results established in the Dutch setting.

4.1 Consumption tax systems around the world

We collect tax information on consumption taxes for 68 countries over the period 2001–2013. We use the KPMG *Indirect tax rates table*, all available issues of the EY *Worldwide Corporate Tax Guide*, and the EY *Worldwide VAT, GST and Sales Tax Guide*. We complement and crosscheck our consumption tax information on some countries with Vegh and Vuletin (2015). We use general sales taxes and neglect excise taxes, for example, on cigarettes, alcohol, energy, and gasoline. Table 8 summarizes the consumption tax system for our sample countries[22] and the average tax rate on sales in each country over the period 2001–2013.[23] The tax rate in Table 8 represents the standard tax rate on goods and services for each country averaged over the sample period. Our sample includes 86 changes of at least 0.25 percentage points in the consumption tax rate. Of these changes, 58 are tax increases, and 28 are tax decreases (see Figure 3). If we limit the tax changes to a full percentage point, we end up with 59 events, of which 39 are increases in consumption tax and 20 are decreases.

The main concern in our cross-country analysis is that tax policy is not exogenously determined, but related to changes in economic conditions. Part II of the Online Appendix provides a detailed discussion of the frequency of changes in the consumption tax rate, the correlates with these changes, and changes in other tax policy tools. In short, consumption tax rates appear to respond to changes in economic conditions as measured by *GDP growth*. Policy makers appear to be more inclined to reduce (increase) consumption taxes when the economy is strong (weak) in our international panel. Finding a

[22] Malaysia and the United States run a retail sales tax system in which the sales tax is collected only at the end-consumer level. All other countries in our sample run a value-added tax system (or goods and services tax system). Part I of the Online Appendix discusses these systems—value-added tax systems and retail sales taxes—have the same consequences on the profit of firms. Further, many countries have reduced rates on basic food (e.g., China, Germany, and Korea), medicine (e.g., Czech Republic, Iceland, and Hungary), or hotels (e.g., Germany, Latvia, and Sweden). We do not use these reduced rates, because the goods subject to reduced rates are often not clearly defined. Further, one can hardly identify firms that sell goods at this reduced rate. Vegh and Vuletin (2015) document that about 70% of all transactions are associated with the standard value-added tax rate. Hence, it is unlikely that the reduced tax rates drive our results.

[23] We were not able to obtain regional retail sales tax information on India prior to their 2005 general consumption tax introduction. We thus exclude India prior to 2005.

Table 8
Overview of VAT and sales tax systems

Country	System	Rate (%)	Country	System	Rate (%)	Country	System	Rate (%)
Argentina	VAT	21	India	VAT	13	Philippines	VAT	11
Australia	VAT	10	Indonesia	VAT	10	Poland	VAT	22
Austria	VAT	20	Ireland	VAT	21	Portugal	VAT	20
Belgium	VAT	21	Israel	VAT	17	Romania	VAT	22
Brazil	VAT	19	Italy	VAT	20	Russia	VAT	18
Bulgaria	VAT	20	Japan	VAT	5	Singapore	VAT	6
Canada	VAT	6	Jordan	VAT	16	Slovakia	VAT	20
Chile	VAT	19	Kazakhstan	VAT	13	South Africa	VAT	14
China	VAT	17	Kenya	VAT	16	Spain	VAT	17
Colombia	VAT	16	Korea	VAT	10	Sri Lanka	VAT	13
Croatia	VAT	23	Latvia	VAT	20	Sweden	VAT	25
Czech Republic	VAT	20	Lithuania	VAT	19	Switzerland	VAT	8
Denmark	VAT	25	Luxembourg	VAT	15	Taiwan	VAT	5
Ecuador	VAT	12	Malaysia	ST	10	Thailand	VAT	7
Egypt	VAT	10	Mexico	VAT	15	Tunisia	VAT	18
Finland	VAT	22	Morocco	VAT	20	Turkey	VAT	18
France	VAT	20	Netherlands	VAT	19	Uganda	VAT	18
Germany	VAT	18	New Zealand	VAT	13	Ukraine	VAT	20
Ghana	VAT	13	Nigeria	VAT	5	United Kingdom	VAT	18
Greece	VAT	20	Norway	VAT	25	USA	ST	5
Hong Kong	NA	0	Pakistan	VAT	16	Venezuela	VAT	15
Hungary	VAT	23	Panama	VAT	6	Zimbabwe	ST/VAT	17
Iceland	VAT	25	Peru	VAT	19			

This table summarizes the VAT/sales tax systems for our sample countries. We classify all general sales tax/VAT systems as a VAT system (*VAT*) if firms receive an input tax credit on goods and services. Tax systems without a credit on input taxes are denoted as sales tax (*ST*) systems, and we use a weighted average across states for the United States of America (USA). Zimbabwe switched from a sales tax to a VAT system in 2004. *Rate* is the average sales tax rate in the respective country over the sample period. We use the standard rate on goods and services. NA, not applicable.

negative association between consumption taxes and investment could therefore reflect unobserved or mismeasured variation in economic conditions. Hence, we subject the control groups in our empirical analyses to similar economic conditions by comparing treated and control firms from countries with similar gross domestic product (GDP) growth and levels. This approach narrows the treatment and control groups to countries following similar GDP growth paths and starting from the same GDP base, which aims at addressing the concerns that treated and control countries are not subject to similar variation in economic conditions.[24]

4.2 Estimation strategy

We first turn to an aggregate, country-level analysis because it allows us to examine the response of the entire corporate sector comprising listed and unlisted firms. We use country-level data on aggregate investment from the World Bank and estimate the following equation:

$$GCF_{j,t} = \alpha_0 + \beta_1 Consumption\ Tax_{j,t} + \sigma \mathbf{\Pi_{j,t}} + \alpha_j + \alpha_{g,t} + \varepsilon_{i,t}, \qquad (2)$$

[24] Table A.4 shows the robustness of our findings to alternative control group choices.

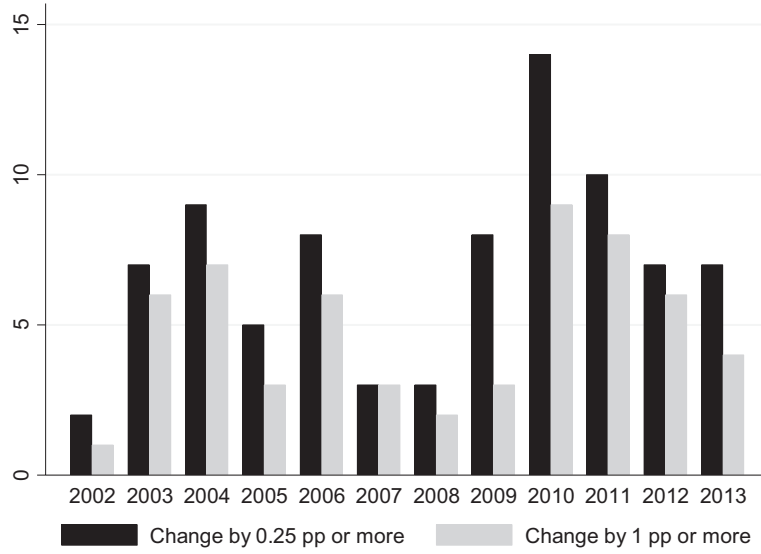

Figure 3
Changes in consumption tax rates over time
This figure summarizes changes of consumption tax rates over time. We count how often the VAT or sales tax rate changes by at least 1 percentage point (pp) and 2 pp, respectively, in each year.

where the natural logarithm of gross capital formation in year t in country $j(GCF_{j,t})$ is the dependent variable. Table 2 provides variable definitions and data sources of all variables. Our main variable of interest is $Consumption\ Tax_{j,t}$, which is the VAT or sales tax rate on consumer goods in country j in year t. We expect that a higher consumption tax rate decreases aggregate investments ($\beta_1 < 0$). We include country fixed effects (α_j), so all country-level variables are identified through changes over time. We narrow down the counterfactuals to firms from comparable countries through the group-year fixed effects ($\alpha_{g,t}$) where the group is defined as countries in the same cluster of similar GDP growth and levels. This provides us with an extensive set of plausible counterfactuals from countries with similar economic conditions. We also include several country-level control variables ($\mathbf{\Pi_{j,t}}$). We include the natural logarithm of GDP in the country-level tests because the dependent variable is the natural logarithm of gross capital formation. Controlling for the level of GDP accounts for the size of the economy. We further control for GPD per capita (in natural logarithm), GDP growth, inflation, six World Bank Worldwide Governance Indicators, and over 300 changes in other tax policy tools. In particular, we include the corporate, the payout, and the wage income tax rate, and indicator variables concerning whether the corporate tax is progressive and whether either loss carrybacks, group taxation, or accelerated depreciation is allowed. Standard errors are clustered at the country-level.

We estimate the relation of consumption tax and investment at the firm level using the following equation:

$$CapEx_{i,j,t} = \alpha_0 + \beta_1 Consumption\ Tax_{j,t} + \sigma \mathbf{\Pi_{j,t}} + \gamma \mathbf{X_{i,j,t}} + \alpha_i + \alpha_{g,k,t} + \varepsilon_{i,t}, \quad (3)$$

where investment of firm i in year t in country j ($Inv_{i,j,t}$) is the dependent variable measured as capital expenditures over lagged assets. Consistent with our aggregate analyses, the main variable of interest is *Consumption Tax*$_{j,t}$. We expect $\beta_1 < 0$. Our baseline empirical strategy is based on a firm-level regression with firm and the abovementioned group-industry-year fixed effects $\alpha_{g,k,t}$. We include the set of country-level control variables from above.[25] Because we use firm-level data, we include *Cash*, *Operating profits*, and *Sales growth* from $t-2$ to t, Tobin's q, *Leverage*, and firm size as controls following prior literature (e.g., Cummins, Hassett, and Hubbard 1996; Baker, Stein, and Wurgler 2003). We use financial information for 37,691 public, nonfinancial and nonutility firms located in 68 countries obtained from Compustat Global and North America. Table A.6 presents an overview of observations per country. Table 9 provides descriptive statistics. Part II of the Online Appendix provides empirical support for the common trends assumption for this test.

4.3 Empirical results: Aggregate results

Column 1 of Table 10 presents regression results from estimating Equation (2). We find a statistically significant coefficient on consumption taxes indicating that a higher consumption tax rate results in lower aggregate investments. We find that a 1-percentage-point increase in the consumption tax decreases aggregate investments by approximately 1.8% [=exp(−1.8489/100) − 1]. This translates into an implied elasticity of aggregate investments for consumption taxes of −0.29.[26] This elasticity is somewhat smaller than our estimate in the Dutch setting, where we focus on border firms, which are expected to be more exposed to the tax change. Aggregate gross capital formation likely also captures corporate investments linked to foreign output markets, so the aggregate-level investment response is expected to be smaller. Still, the economic magnitude of the coefficient suggests that the aggregate investment response to consumption taxes is close to recent estimates on the effect of corporate taxes (Patel, Seegert, and Smith 2017; Giroud and Rauh forthcoming).

The availability of consumption data at the country level allows us to validate the mechanism through which consumption taxes adversely affect investment. Namely, an increase in consumption tax reduces consumption as

[25] Because a few variables are poorly covered (e.g., budget deficit, interest payments, and government debt), they are not included in our firm- and country-level analyses. In Table A.5, we document the robustness our results to the inclusion of these variables. In this table, we also document the robustness of our results to the inclusion of lagged country-level controls.

[26] This elasticity is calculated as the investment increase of 1.8% relative to the tax rate increase by 1 percentage point relative to the average tax rate of 16%. In short, −0.29=[−1.8%/(1%/16%)].

Table 9
Descriptive statistics, international panel

Variable	Mean	SD	25th percentile	Median	75th percentile
A. Country-level variables					
Consumption tax	0.1041	0.0636	0.0522	0.0700	0.1700
GCF	24.5779	1.5269	23.6635	24.5339	26.6175
Consumption	9.1170	1.2255	8.4736	9.7501	9.9534
ln(GDP_Cap)	9.8171	1.2063	9.2141	10.4708	10.6198
GDP growth	0.0317	0.0345	0.0160	0.0253	0.0474
Inflation	0.0248	0.0342	0.0079	0.0199	0.0328
Voice and accountability	0.7030	0.8916	0.4500	1.0400	1.3100
Political stability	0.3584	0.7482	−0.0400	0.5600	0.9400
Government effectiveness	1.2078	0.7051	1.0000	1.5000	1.7000
Regulatory quality	1.0540	0.7263	0.6450	1.2500	1.6100
Rule of law	1.0494	0.7839	0.5800	1.4100	1.6100
Control of corruption	1.0510	0.9385	0.3600	1.3200	1.8350
Corporate tax	0.3194	0.0742	0.2600	0.3300	0.3900
Payout tax	0.1556	0.0964	0.1000	0.1500	0.2259
Wage tax	0.3981	0.0929	0.3500	0.4000	0.4641
Progressive	0.6930	0.4613	0.0000	1.0000	1.0000
LCB	0.4697	0.4991	0.0000	0.0000	1.0000
Group	0.6146	0.4867	0.0000	1.0000	1.0000
Accelerated	0.8107	0.3918	1.0000	1.0000	1.0000
B. Firm-level variables					
CapEx	0.0633	0.0960	0.0129	0.0332	0.0733
Wages	0.1963	0.2229	0.0495	0.1261	0.2617
Cash	0.1650	0.2505	0.0326	0.0904	0.1990
Operating profits	0.0218	0.2522	0.0031	0.0516	0.1078
Sales growth	0.1935	0.6151	−0.0527	0.1578	0.4008
q	1.4362	3.7069	0.3204	0.6526	1.3127
Leverage	0.5309	0.3256	0.3325	0.5136	0.6782
Size	5.3575	2.1002	4.0237	5.2794	6.6214

This table presents descriptive statistics of our main variables for 37,691 firms and 255,054 observations over the 2001–2013 period. Panel A presents summary statistics for country-level variables. Panel B presents statistics on firm-level variables. Table A2 defines the variables.

long as consumer demand is not perfectly inelastic, thereby reducing investment opportunities. Further, the aggregate consumption response incorporates spillovers between product markets (e.g., consumers buying fewer units of all goods because of lower incomes or other substitution and complementarity effects).

We estimate Equation (2) using the natural logarithm of household consumption as the dependent variable. The results in Column 2 of Table 10 indicate that a 1-percentage-point increase in the consumption tax rate decreases aggregate consumption by 0.84%, which translates into an implied elasticity of consumption with respect to consumption taxes of −0.14. As consumption taxes increase, households consume less (Column 2, Table 10), thereby reducing investment opportunities. We note that the identification underlying these results is less tight and, therefore, these results could reflect unobserved variation in economic conditions that we fail to appropriately capture with our use of country-cluster-year fixed effects. However, because the experiment using the Dutch setting already established causality, the directional causality might be less of a concern.

Table 10
Consumption taxes and investments: Cross-country evidence

	Country-level analysis		Firm-level analysis					
	Investment	Consumption	Corporate investment					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Consumption tax	−1.8489***	−0.8439**	−0.1429**					−0.1106
	(0.6152)	(0.3802)	(0.0609)					(0.1180)
Consumption tax × Low margin				−0.0343**				
				(0.0161)				
Consumption tax × High domestic					−0.0429**			
					(0.0190)			
Consumption tax × Low distance						−0.2918**		
						(0.1199)		
Consumption tax × Low cash flow							−0.0373***	
							(0.0134)	
Consumption Tax × High EPL								−0.3148**
								(0.1558)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FEs	Yes	Yes	No	No	No	No	No	No
Firm FEs	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Group-year FEs	Yes	Yes	Yes	No	No	No	No	Yes
Country-year FEs	No	No	No	No	Yes	Yes	No	No
Country-Industry-year FEs	No	No	No	Yes	No	No	Yes	No
Observations	878	872	255,054	255,054	221,806	255,054	255,054	216,115
Adjusted R-squared	.995	.998	.496	.502	.502	.489	.501	.509

This table presents regression results on investment behavior over the 2001–2013 period. Table 2 describes the independent variables. In Columns 1 and 2, we use aggregate country-level data from the World Bank. Columns 3 to 7 use firm-level data. Column 3 to 7 use capital expenditures over lagged total assets as the dependent variable. In Column 4, we additionally interact each tax policy variable with a dummy variable equal to 1 if the firm is below the median profit margin (EBIT over sales) in the respective country-year (*Low margin*). In Column 5, we additionally interact each tax policy variable with a dummy variable equal to 1 if the firm's industry has above-median domestic to total sales in the respective-country-year (*High domestic*). In Column 6, we additionally interact each tax policy variable with a dummy variable equal to 1 if the industry has a low distance to customers (*Low distance*). We use an empirical proxy by Ozdagli and Weber (2017) to classify industries into six layers based on their distance to end-consumers. *Low distance* identifies firms not belonging to the most remote layer. In Column 7, we additionally interact each tax policy variable with a dummy variable equal to 1 if the firm is below the mean cash flow to assets ratio in the respective country-year (*Low cash flow*). In Column 8, we additionally interact each tax policy variable with a dummy variable equal to 1 if the country is above the median average employment protection index value during the sample period (*High EPL*). We include country (firm) fixed effects in the country-level (firm-level) analysis. We also generate for each sample year four clusters of GDP growth and the level of GDP, measured as the natural logarithm of GDP, and then include fixed effects for each GDP-growth-ln(GDP)-cluster (industry)-year combination in the country-level (firm-level) analysis. Columns 4 and 7 include country-industry-year fixed effects. Columns 5 and 6 include country-year fixed effects. We report robust standard errors, clustered at the country level, in parentheses. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$.

4.4 Empirical results: Firm-level results

To validate our inference from the Dutch setting that firms' response to changes in consumption taxes depends on the relative elasticity of demand, we turn to firm-level analysis using the same worldwide consumption tax changes. As a baseline, we first estimate Equation (3) to obtain the average effect. Results are presented in Column 3 of Table 10. A 1-percentage-point increase in the consumption tax rate decreases investments by 0.14% of total assets or 2.3% of sample average investments. The implied elasticity of −0.24 is very close to the aggregate investment effect.

Next, we exploit heterogeneity in the consumption tax responsiveness across firms.[27] This analysis has two key benefits. First, it allows us to capture the underlying construct of the relative elasticity of demand and supply with alternative and similar proxies. Second, we are able to control for country-(industry)-year fixed effects to control for the influence of unobserved variation in economic conditions at the country (-industry) level correlated with tax policy. In these specifications, we can only estimate the coefficient on the interaction between the cross-sectional variable and *Consumption tax*. We note that the results on the interactions excluding these extended fixed effects (Table A.8) are similar in terms of magnitude, thus increasing confidence that our general strategy to control for unobserved variation in economic conditions in the international sample by limiting counterfactuals to firms from countries with similar GDP growth and level is effective.

We operationalize the notion that firms with relatively elastic consumer demand bear more of the consumption tax burden in three ways. First, similar to our Dutch setting, we use a firm's profit margin. We define a dummy variable, *Low margin*, which is equal to 1 for firms below the median of the profit margin distribution in a respective country-year. We add *Low margin* and the interaction of *Low margin* with each tax policy variable characterizing a country's tax system to Equation (3). Results are reported in Column 4 of Table 10 and show that investment of firms with low profit margins react more negatively to consumption tax rate increases. As we include country-industry-year fixed effects, the main effect of *Consumption tax* is not identified thus preventing us from directly comparing *High margin* and *Low margin* firms.[28]

Second, we use a firm's exposure to domestic (end-) consumers. Firms more isolated from domestic end-consumers plausibly have higher flexibility to adjust to domestic consumption tax changes and are thus more elastic than

[27] Another potential concern related to our cross-sectional results is that because consumption tax decreases tend to be correlated with GDP growth, firms with low profit margins might be disproportionally affected, because they are disproportionally affected by GDP growth. To address this concern, Table A.7 shows that our results are robust to the inclusion of an interaction of *GDP growth* and the cross-sectional variables (e.g., *Low margin* or *Low cash flow*).

[28] The results reported in Table A.8 in the Online Appendix that exclude country-(industry)-year fixed effects suggest that, relative to the effect for high profit margin firms (−0.1216), the responsiveness of investment to consumption taxes increases by 36% if a firm has a low profit margin.

consumer demand. In addition, the effect of consumption taxes occurs at the domestic level, so we expect firms with a larger share of domestic as opposed to international output to be more exposed to the consumption tax increases. In a similar vein, consumption taxes are paid by consumers, so we expect firms with a larger share of output at the end-consumer level to be more exposed to consumption tax increases to the extent that the consumption tax effect does not fully ripple through the value chain.

We approximate the exposure of a firm's output to domestic consumption tax rate changes in two ways. First, we derive a proxy for cross-border sales using information on the fraction of international sales available in Datastream, aggregated at the country-industry-year level and captured by a dummy variable, *High Domestic,* which is equal to 1 if the firm's industry has above-median domestic to total sales in the respective-country-year. Second, we obtain an industry's closeness to end-consumers based on input-output data from Ozdagli and Weber (2017). We define a dummy variable, *Low distance*, equal to 1 if industries are not in the most remote of six layers.[29] Columns 5 and 6 of Table 10 presents the results of estimating our main model where we also include interactions of our tax variables with *High domestic* and *Low distance*, respectively. Firms whose customers are more likely to be subject to the domestic consumption tax, as well as firms close to the end-consumers, are most responsive as indicated by the negative and significant coefficients of *High domestic* × *Consumption tax* and *Low distance* × *Consumption tax*.[30]

Third, we replicate the Dutch analysis on financially constrained firms in the international panel to for the relative elasticity of demand and supply. We define a dummy variable, *Low cash flow*, which is equal to 1 for firms below the median of the cash flow distribution in a respective country-year. The results are reported in Column 7 of Table 10. Consistent with our findings in the Dutch experiment, firms with low internal cash flows react more negatively to consumption tax rate increases.[31] Taken together, the results from the cross-sectional analyses in the international panel confirm our findings from the Dutch experiment and the economic channel through which consumption taxes adversely affect investment (i.e., tax incidence, which is a function of the relative elasticity of demand vis-à-vis supply).

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[29] We acknowledge that this proxy is based on the U.S. economy and thus may be prone to measurement errors in an international setting. The measure groups industries into six layers based on the distance of their output from end-consumers. However, this approach does not fully reflect the number of vertical supply-chain linkages resulting in more than 50% of the firms classified as belonging to the most remote layer. That is, the last layer aggregates all firms belonging to industries that would be classified in more distant layers if the measure allowed for more distant layers.

[30] An alternative way to proxy for exposure to local consumers is to split the sample, according to the Mian and Sufi (2014) measure of tradable versus nontradable industries. While nontradable industries arguably also can have international consumers, the results of this test indicate that consumption taxes have a significant effect in nontradable industries that is significantly higher than the effect for tradable industries (see Table A.9).

[31] The results reported in Table A.8 that exclude country-industry-year fixed effects suggest that relative to the effect for high cash flow firms the investment response to consumption taxes increases by 39% if a firm has a low cash flow.

4.5 Employment protection laws and capital investment

Possibly, the impact of consumption tax on investment does not only depends on the relative elasticity of demand and supply but also on labor supply elasticity. Specifically, in frictionless markets, a reduction in product demand will result in reductions in investment in both capital and labor. However, frictions in the labor market, such as downward rigidity in labor or employment protection laws, might result in a disproportional reduction in investment. All else being equal, we expect greater reductions in capital investment when firms face a more rigid labor supply. To test this prediction empirically, we examine how the capital investment sensitivity to consumption taxes is affected by employment protection laws. We use the *OECD Indicators of Employment Protection Legislation* and test how investment and consumption taxes are related to each other in countries with strong versus weak employment protection laws. Since the data for this index are not available for all years and countries, we use the average index value for each available country during our sample period.[32] We interact *Consumption tax* with a dummy variable *High EPL*, which we set equal to 1 if the average employment protection in a given country is above the median value of the average employment protection.

Column 8 of Table 10 reports the results. We find a stronger impact of consumption taxes on corporate investment in countries with strict employment protection. That is, when the downward adjustment of labor is limited, firms reduce capital investment significantly more. This test provides an important insight concerning the link between labor elasticity and firms' investment reaction to consumption taxes. The findings suggest a link between the effect of consumption tax on capital (this paper) and its effect on labor input (Fox 1986; Thompson and Rohlin 2012).

5. Conclusion

This paper investigates the causal effect of consumption taxes on corporate investment using a quasi-natural experiment associated with the VAT increase in the Netherlands in 2012. We establish the external validity of our findings in a cross-country setting based on 86 changes in consumption tax rates. Both the quasi-natural experiment using the Dutch setting and the broader international setting show that firms' investments are responsive to changes in consumption taxes. The effect of consumption tax on investment is economically significant and the implied elasticity is about −0.36 to −0.40 in the Dutch setting. The magnitude of the investment response to consumption tax changes is similar to the corporate tax effect from recent estimates (Patel, Seegert, and Ohrn 2017; Giroud and Rauh forthcoming). Consistent with economic theory, the effect of consumption taxes is more pronounced in firms

[32] Specifically, we include 42 rather than 68 countries in this test, reducing the number of tax changes. This reduces the number of observations from 255,054 to 216,115.

facing more elastic demand relative to supply. We also provide empirical evidence on two possible mechanisms driving the investment response. First, higher consumption taxes reduce profitability. Second, aggregate consumption decreases after consumption tax increases. Both results—reduced profits and reduced consumption—imply reduced investment opportunities and explain why firms cut investment after consumption tax increases.

Our findings have important implications for the debate over tax policy design. The main tax revenue sources for governments are consumption taxes and personal income taxes on labor and capital. Corporate taxes typically generate substantially lower revenue than consumption taxes. Our results imply that consumption taxes, as well as corporate taxes, need to be taken into account when gauging investment responses of tax policy. Consumption taxes appear to have equally distortive effects on corporate investment as corporate taxes. Hence, our results provide important input into tax policy considerations as it is often assumed that consumption taxes do not affect investment (e.g., the Congressional Budget Office 2012 or the President's Advisory Panel on Federal Tax Reform; Mack et al. 2005). In fact, they have a sizeable impact with a similar magnitude to that of corporate taxes. Our results thus suggest that a budget-neutral tax reform cutting corporate taxes while simultaneously increasing consumption taxes to finance the reform may not spur corporate sector investment and economic growth.

The various experiments we conduct further indicate that product demand elasticity substantially affects the extent to which consumption taxes decrease investment. The ultimate investment effect critically depends on the relative elasticity of demand and supply in the respective industry or sector of the economy. These effects are relevant for policy makers, for example, when considering differential consumption tax rates on specific goods or industries. This result highlights the contingency, in which taxing elastic goods, such as luxury goods, at higher rates to protect consumers of inelastic goods, such as food or medicine, with lower tax rates, comes at the cost of distorting investment decisions in markets with elastic product demand.

Appendix

Table A1
Variable definitions, Dutch sample

Variable	Definition
Inv	The gross investment defined as the change in fixed assets before depreciation scaled by lagged total assets
Post	A dummy variable equal to 1 for 2013, 2014, and 2015 and 0 otherwise
Border	A dummy variable equal to 1 if the firm is located within the region of the Belgian-Dutch or the German-Dutch border and 0 otherwise. Specifically, these are firms with addresses in ZIP code areas starting with 44, 45, 46, 47, 48, 50, 51, 55, 58, 59, 60, 61, 62, 63, 64, 65, 69, 70, 71, 74, 75, 76, 77, 78, 95, and 96
Affected industry	A dummy variable equal to 1 if the firm operates in an industry subject to the standard VAT and 0 otherwise (approximated by the two-digit SIC code)
Cash	Cash scaled by lagged total assets
Size	The natural logarithm of total assets
Profit	Profit before taxes, scaled by lagged total assets
Sales growth	The natural logarithm of the growth rate of sales from $t-2$ to t
Leverage	Total debt, scaled by total assets
Size	The natural logarithm of total assets

Table A2
Variable definitions, cross-country tests

A. Tax policy variables

Variable	Definition
Consumption tax	The VAT (or sales tax) rate. For the United States, we use the average state-level sales tax rates weighted by the states' inhabitants
Corporate tax	The top marginal corporate tax rate
Payout tax	The average of the top marginal dividend tax rate and the top marginal income tax rate on capital gains. For capital gains, we assume long-term and nonsubstantial shareholdings
Wage tax	The top marginal individual income tax rate on labor income
Progressive	A dummy variable equal to 1 if country j has a progressive corporate tax rate in year t
LCB	A dummy variable equal to 1 if country j allows tax loss carrybacks in year t
Group	A dummy variable equal to 1 if country j allows for group taxation in year t
Accelerated	A dummy variable equal to 1 if country j allows for accelerated depreciation in year t
B. Firm-level variables	
CapEx	Capital expenditures, scaled by lagged total assets
Cash	Cash, scaled by lagged total assets
Operating profits	Earnings before interest and taxes (EBIT), scaled by lagged total assets
Sales growth	The natural logarithm of the growth rate of sales from $t-2$ to t
q	q is defined as market value of equity, scaled by total assets
Leverage	Total debt, scaled by total assets
Size	The natural logarithm of total assets
Profit margin	EBIT, scaled by sales
Gross margin	Sales minus costs of goods sold, scaled by sales
Foreign sales	The ratio of international sales to total sales
C. Country-level variables	
ln(GDP_Cap)	The natural logarithm of GDP per capita in constant 2005 USD
GDP growth	The annual percentage growth rate of GDP in constant 2005 USD
Inflation	The rate of price change in a country as a whole as measured by the annual growth rate of the GDP implicit deflator
Voice and accountability	The yearly estimate of a country's quality relating to voice and accountability
Political stability	The yearly estimate of a country's quality relating to political stability
Government Effectiveness	The yearly estimate of a country's quality relating to government effectiveness
Regulatory quality	The yearly estimate of a country's quality relating to regulatory quality
Rule of law	The yearly estimate of a country's quality relating to rule of law
Control of corruption	The yearly estimate of a country's quality relating to control of corruption
Openness	The defined as the sum of imports and exports, scaled by GDP
Deficit	The government budget deficit, scaled by GDP
Interest payments	The ratio of government interest payments, scaled by GDP
Government debt	The ratio of government debt, scaled by GDP

Sources. Tax handbooks published by Ernst & Young, KPMG, PwC, and Deloitte; Compustat Annual North America and Global; and World Bank.

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