Investment Responses to Tax Policy under Uncertainty

By Irem Guceri and Maciej Albinowski*

How does economic uncertainty affect the impact of tax policy? To answer this question, we exploit a unique natural experiment, in which two very similar investment subsidies were implemented in the same country, two years apart: once during a period of economic stability, and once during a period of very high uncertainty. The experiment features sharp discontinuities in firm eligibility, and we conduct our analysis using tax returns (corporate and VAT) and trade data for the universe of corporations. We find that, under low uncertainty, tax incentives have strong positive effects on investment, both on the extensive and intensive margins. This aligns with the findings in several recent empirical papers. Under high uncertainty, however, the story is very different: the effect at the intensive margin is still present, but the effect at the extensive margin disappears. Together, these results suggest that: (1) some firms "wait and see" during periods of high uncertainty, even in the presence of generous incentives; and (2) periods of stability offer an important policy opportunity to encourage investment.
JEL: H25, D25, C21

The effectiveness of any microeconomic policy depends upon the macroeconomic context (Bloom, Bond and Van Reenen (2007)). Recent availability of administrative data has greatly broadened our understanding of the effectiveness of tax incentive policies (House and Shapiro (2008); Devereux, Maffini and Xing (2016); Ohrn (2018)), including our understanding of heterogeneities in policy effects (Zwick and Mahon (2017)).

In this paper, we study the impact of a unique natural experiment in which two very similar investment tax incentives were implemented in periods of very different degrees of uncertainty. The first policy was implemented in a period of economic stability, and the second at a time of extremely high economic uncertainty. A sharp firm size cutoff determined eligibility to the scheme, generating two natural treatment groups for each of the policies and a natural control group. Using a rich dataset on the population of corporate and value added tax returns of firms in Poland matched with their imports and exports, we find that the same incentive for a group of similar firms resulted in a very different response under elevated uncertainty. In downturns, investment response to the incentive is con-

* Guceri: Corresponding author. Oxford University Centre for Business Taxation, Said Business School, Park End Street, OX1 1HP, Oxford, UK. irem.guceri@sbs.ox.ac.uk. Albinowski: Ministry of Finance of Poland. We are grateful to Emilia Skrok for her valuable inputs and comments from the reviewers at the World Bank. We also thank Michael Devereux, Dawid Pachucki, Simon Quinn and participants at the Oxford University Centre for Business Taxation seminar for helpful comments. Any errors and omissions are the authors' own responsibility.

centrated on capital-intensive firms that had already been investing. The results should prompt policymakers to acknowledge response heterogeneity to countercyclical policy options.

Many countries have been implementing policies that allow the deduction of the cost of capital in the year of their acquisition from taxable income (full expensing) rather than depreciating this cost over a number of years. The policy may be implemented either as a temporary measure as part of a stimulus package or as long-term policies to facilitate higher capital spending. In either case, the benefit arises from the time value of money – the firm gets a positive cash flow earlier than in the absence of the policy. With temporary incentives, there is also an incentive for the firms to carry out the investment while the policy is in place, and the beneficiaries know the sunset date. After the policy expires, firms go back to depreciating their fixed assets over some years. The United States has now allowed full expensing of capital spending[1], and the United Kingdom, on the verge of Brexit, has announced a temporary increase in the cap on total investment value for its Annual Investment Allowance to £1 million, covering most companies' total investment spending annually.

Both temporary and permanent incentives induce an increase in aggregate investment by bringing the tax price of investment down. The impact is more pronounced for investment goods with long useful lives (House and Shapiro (2008); Devereux, Maffini and Xing (2016); Zwick and Mahon (2017)). The reduction in the user cost of capital prompts both an intensive margin response for firms that are already investing, and an extensive margin response for firms which would not have made any investment in capital goods in the absence of the policy. The extensive margin response is driven mostly by the greater incentive to upgrade the capital stock more frequently thanks to the lower user cost of capital (Hall and Jorgenson (1967), Jorgenson (1963)). Further, tax incentives may relieve the cash flow constraint of firms with profitable investment opportunities but which previously could not invest due to the lack of sufficient funds (Myers and Majluf (1984); Kaplan and Zingales (1997); Bond and Van Reenen (2007); Devereux and Liu (2016)).

Our main identification strategy relies on the changes in the user cost of capital induced by two reforms in Poland. Before the reforms, firms in Poland could depreciate the cost of machinery and equipment over 5-20 years using straight line depreciation. First, in 2007, Poland introduced special depreciation provisions under the name 'Lump Sum Depreciation' which enabled companies with less than 800,000 Euros in turnover in the preceding year to benefit from 100 percent expensing of the cost of certain capital goods. Second, in 2009, these benefits were extended to a group of medium-sized firms with turnover below 1.2 million Euros. Using a difference-in-differences methodology and administrative data on the population of corporate taxpayers, we find that treated firms increased investment spending by around 6 percent. Manufacturing firms responded more

[1]For the time being, the measure is to be implemented for five years.

strongly: the increase in investment for treated manufacturing firms was around 12 percent. Our preferred estimates control for firm-specific time-invariant unobserved heterogeneity, importer and exporter-specific time effects, the impact of annual changes in the macroeconomy that are unrelated to the reform of interest and time-varying firm size captured by turnover. These findings translate to an estimated elasticity of investment to user cost of capital by around -4. Relative to earlier studies that also use quasi-experimental variation on administrative data, this implied elasticity estimate is lower, but strong. Comparable studies on US Bonus Depreciation find elasticities that are higher than 6 in absolute value (See House and Shapiro (2008); Zwick and Mahon (2017); Ohrn (2018), and on the UK First Year Allowance, see Devereux, Maffini and Xing (2016)). One explanation to the relatively smaller size of our estimates may be the permanent nature of the tax incentives.[2]

We make three main contributions. First, we explore a novel aspect of firm responses to tax incentives for investment, which is the role of uncertainty. We begin by confirming some established findings of the existing literature, mostly referring to quasi-experimental estimates since Cummins, Hassett and Hubbard (1994). During our data period, Poland introduced two separate reforms, allowing us to analyse the responses, first in a low-volatility environment, and later in a high-volatility environment. We estimate that the intensive margin responses are strong in both high and low volatility periods, whereas extensive margin responses diminish in the high volatility context. This suggests that a stimulus policy directed at small, intermittent investors might not be effective at times of high uncertainty. Regular investors respond strongly regardless of the degree of uncertainty.

Second, recent policies (such as the US tax reform) increasingly resemble a tax system based on cash flow (as described by King (1987)), by allowing immediate depreciation of the cost of capital goods. From a theoretical perspective, Abel (1982), and later House and Shapiro (2008) argue that temporary tax incentives might induce stronger investment responses than do permanent measures. Our findings offer new empirical support for the effectiveness of permanent measures. The difference in the response to a permanent investment tax incentive between a low and a high volatility environment may be driven by non-convex adjustment costs, or an initial fixed cost of investment (e.g. Cooper, Haltiwanger and Power (1999)). Our methodology is agnostic about the exact mechanism that drives the muted response on the extensive margin; nevertheless, in Section II, we discuss a conceptual framework that can explain our empirical results. We rule out that the global crisis had an asymmetric impact on the treated and control groups by testing whether the crisis had a different impact on different size groups, and find that such asymmetric effects do not drive our findings.

[2]Among these papers, only Devereux, Maffini and Xing (2016) study a permanent incentive, and the authors point out that using realistic discount rates, they obtain estimates for elasticity of investment with respect to user cost around -4.

Finally, we use a detailed linked dataset covering the population of VAT and corporate taxpayers, along with their import and export activity. We use investment information at quarterly frequency to verify parallel pre-reform trends across treated and control groups for both experiments. The availability of such detailed data helps us in ruling out alternative channels than the tax incentive channel and in controlling for a larger set of observable characteristics and trends than does the existing literature.

We find that both the average investment levels and the probability to invest increases after the 2007 reform. The second reform kicks in at a time when global volatility is very high, and this is reflected in firms' investment responses (as documented by Bloom, Bond and Van Reenen (2007)). The extensive margin response that we observe in 2007 subsides in the second reform period, which is the high volatility environment starting in 2009–2010. The richness of the data allows us to rule out the effects of terms of trade on investment driven by exchange rate fluctuations. We show that importer and exporter effects are not responsible for the varied response. Moreover, our results are very robust to a variety of checks including placebo tests or narrowing the firm size brackets for analysis.

In the last decade, with the availability of administrative data, a few studies have explored the average impact of depreciation allowances along with differentiated response across sub-groups of companies to such policies (Zwick and Mahon (2017), Devereux and Liu (2016), Ohrn (2018)). We verify our results using the cross-industry variation in the composition of capital goods as in Zwick and Mahon (2017). This latter method also allows us to exploit treatment intensity at the sectoral level.

In the remainder of the paper, we first describe the main identification challenges and the policy setup in Section I, followed by Section II, where we summarise the theoretical background. Section IV explains the research design. Section III describes the data sources and summarises the dataset used for the analysis. Section V reports the main results. Section VI concludes.

I. The Changes Brought about by the Reform and the Reform Timeline

Most corporate tax systems do not allow firms to fully deduct the cost of capital goods from taxable income immediately. Instead, part of such costs can be deducted each year according to a depreciation schedule defined by the law. Until the introduction of the Lump Sum Depreciation scheme in Poland in 2007, the average time span over which the cost of machinery and equipment could be depreciated was around 7 years. The scheme allowed eligible firms to fully deduct the cost of qualifying capital goods, composed mostly of machinery and equipment, in the year in which the goods are purchased. From the outset, the policy was expected to be permanent, and it is still in place. The policy reduced the tax component of the cost of capital by around 8 percent for an asset with an average

useful life of 7 years.[3] There are two policies in place, and *ceteris paribus*, each of the policies applied a similar reduction in the user cost of capital at different points in time on different groups of firms.

As an example, a company that bought machinery at a cost of 35,000 Euros before the reform could only deduct 5,000 Euros each year from its taxable income. At the stable 19 percent tax rate, in each year, the tax benefit from deducting 5,000 Euros amounts to 950 Euros, spread over an average of 7 years. If we assume a discount rate of 10 percent, the present discounted value of all future tax deductions is 5,087 Euros. If the firm is allowed to fully deduct the cost in the year of purchase, the tax gain is 6,650 Euros, yielding a net benefit of 1,563 Euros, or 4.5 percent of the purchase price of the asset. This might appear as a small cost reduction for capital goods with a short asset life (see, for example, Desai and Goolsbee (2004)), but it is an important boost for the firms intending to acquire assets with longer useful lives, and even for assets with shorter useful lives, it may be sufficient to carry the marginal projects above the break-even point. Many small firms rely on single investment projects one at a time; the cost reduction for such firms should result in an extensive margin effect, that is, if the policy has an effect on smaller firms, we should observe non-investors at the margin to start investing after the introduction of the policy.

Our main approach relies on the distinction between firms that have access to the more generous policy and the firms that do not at all (as in Devereux, Maffini and Xing (2016) and Guceri and Liu (2019)). In addition to this exogenous policy-specific variation, the differences in weighted average asset life across narrowly defined sectors give a second source of exogenous variation (as in House and Shapiro (2008) and Zwick and Mahon (2017)). For each firm, the longer the asset life of bulk of its capital stock, the larger is the impact of the policy on the firm's investment decisions.

To benefit from the policy, a firm's turnover in the preceding year must not exceed the threshold, which in 2007 was set at EUR 800,000. To translate the threshold into Polish currency (PLN), the exchange rate from the first day of previous year's October is used. A newly established firm is also eligible to expense its capital spending, at least for the first year of its activity.[4]

In the middle of 2009, the revenue cap for a small taxpayer rose to EUR 1.2 million. A further increase in the threshold denominated in the Polish currency took place on January 1, 2010, as the new exchange rate after substantial depreciation of the Zloty was applied. We present the timeline of eligibility thresholds in Figure 1.[5]

[3]This calculation for the tax component of the cost of capital u follows: $u = \frac{1-\tau_{it}z_{it}}{(1-\tau_{it})}$, where τ_{it} is the tax rate on corporate profits which remained stable throughout our data period at 19 percent, and $z = \sum_{t=0}^{T-1} (\frac{1}{1+r})^t (1/T)$ with the total useful asset life captured by T. For this simple calculation, we assume a discount rate of 10 percent.

[4]Because of the additional benefit to start ups, we limit the sample of analysis to firms that were at least three years old at the start of our sample.

[5]In the first instance, a threshold that fluctuates with the exchange rate appears to call for a regression

FIGURE 1 HERE

The first reform was only announced in November 2006, without any prior discussion about introducing accelerated depreciation for machinery and equipment. We therefore rule out anticipation effects that could have lead firms to postpone investment. The late announcement of the policy is especially important for our classification of firms into treatment and control, because the late announcement also meant that firms could not try to manipulate their turnover in 2006.

Buildings, cars and intangibles are excluded from the policy, which is useful in our context as there may be profit-shifting motives in the case of acquisition of intangible assets. The annual limit for investment expenditures that could be deducted is set at EUR 50,000, with the exception of 2009 and 2010, when it was temporarily increased to EUR 100,000. This expansion was one of the elements of Poland's counter-cyclical fiscal policy response to the global financial crisis.

To maintain the validity of our identification strategy, we need the firms not to game the system and manipulate their position relative to the turnover thresholds. Figure 2 presents the post-reform distribution of firms relative to the turnover threshold in the preceding period.[6]

FIGURE 2 HERE

II. The Option Value of Waiting under Uncertainty

Three key characteristics of investment drive firms' decision about whether to invest or not (Dixit and Pindyck (1994)). First, in practice, investment is at least partially irreversible (as modelled and documented in, for example, Arrow (1968); Bertola and Caballero (1994); Abel and Eberly (1996); Eberly and Van Mieghem (1997); Cooper and Haltiwanger (2006); Chetty (2007); Bloom, Bond and Van Reenen (2007); Bond, Söderbom and Wu (2011)). Second, the future payoff from an investment project is uncertain (as in Zeira (1987); Caballero (1991); Guiso and Parigi (1999); Chetty (2007)). And third, firms can decide on whether or not to invest, and if investing, when to make the investment (Abel et al. (1996)). The effect of uncertainty on investment decisions depend on the extent of competition and the degree of reversibility of investment (Guiso and Parigi (1999)).

Academic focus on the impact of second-moment shocks such as a shock to the degree of uncertainty is relatively recent (Bloom (2009)). Heterogeneity in policy

discontinuity design. However, the threshold is revealed to the taxpayers well in advance every year, and we have found that treatment status is very stable across the years, invalidating any suggestion that the threshold is close to random. We have also found that the data points around the threshold are rather sparse to employ non-parametric approaches.

[6]To save space, we present the figures only for 2007 and 2010, but the lack of bunching holds for all other analysis periods as well.

impact under varying degrees of uncertainty is implied in these more recent models, but data availability and identification issues present a difficulty in connecting uncertainty to the dampening of firm-level responses to stimulus policies.

Our findings relate only to one type of capital, machinery and equipment, used to produce the output good. Our contribution here is empirical, but our approach is consistent with a real option model where delaying the acquisition of capital is analogous to a call option, and where uncertainty increases the value of a 'wait and see' strategy represented by this call option (e.g. Abel and Eberly (1996); Bloom, Bond and Van Reenen (2007)). We conjecture that the increase in the value of the call option induces inaction by the firms that started facing an elevated level of uncertainty as a result of the global economic crisis, dampening the investment responses to policy measures for this particular group of firms over this period. Abel and Eberly (1996) derive this region of inaction as $[c_L, c_U]$, where c_L and c_U are lower (for asset resale) and upper (for asset purchase) boundaries for user costs of capital under costly reversibility. Even if a tax incentive reduces c_U to a lower level $\underline{c_U} < c_U$, it is plausible to observe overall inaction if most investment projects fall between $[c_L, \underline{c_U}]$. Procyclicality of investment spikes may also play a role in muted extensive margin responses to stimulus policies (Cooper, Haltiwanger and Power (1999)).

Finally, we rule out the cash constraint channel for explaining the lumpiness and extensive margin result heterogeneity (as in Whited (2006)) by narrowing the size bands within our estimation sample in Section V, and also confirming that the financial crisis did not have differential effects on the treated and the control groups for our analysis.

III. Data

A. Data sources

We use administrative data from the Ministry of Finance in Poland to assess the impact of the policy. The internal tax registry covers the period 2005-2016 and raw data is available on a monthly basis. We merge this information with the business register and micro-level trade data. We aggregate the monthly information to the annual level for the main analysis, and also examine common quarterly trends in investment across treated and control groups.

VAT returns have been digitized since 2005 and this is the source of two main variables used in our study: turnover and investment.[7] In the VAT returns, firms are obliged to declare the investment amount which is associated with any input VAT. Although it excludes some types of investment such as real estate, it covers most of the fixed assets and intangibles. VAT exemption thresholds are very low for the period of our study, and even for the smallest firms below the threshold, there is good reason to believe that those that carry out business-to-business

[7]Turnover is composed of net values of all categories of sale (including those with the zero VAT rate) and the output VAT tax. Thus, the turnover is expressed in gross prices.

transactions would have a strong incentive to register for VAT (see, for example, Liu et al. (2018)). We provide further details on the suitability of VAT data for our purposes in Appendix A.

We merge CIT data with VAT for further information on legal form, profit and loss positions. Almost all CIT taxpayers work with professional accountants to complete their tax return, which should increase the reliability of the data. We also expect the policy to be more salient for CIT taxpayers. Because of these advantages (and some others described in Appendix A), we focus on the population of CIT taxpayers.

Finally, we merge in the register of economic activity to obtain additional information at the firm-level, such as firm age, type and sector. We describe our data cleaning steps in Appendix A. In our final estimation sample, we focus on medium-sized firms by dropping the firms at both tails of the size distribution based on turnover in the last pre-reform period. We also remove young firms which have access to investment incentives regardless of size.

B. Summary statistics

Table 1 reports the main characteristics of the two samples constructed to analyze each of the two reforms. The top panel of Table 1 presents the summary statistics for the low volatility period sample, and the bottom panel presents the summary statistics for the high volatility period. Firms in the second sample, which covers the latter reform that took place in the high volatility period, are on average a little larger than the firms that we use to analyze the outcomes in the low volatility period. This is because we exclude from the 2008-2010 sample all firms that were treated in 2008 based on their turnover in 2007. 42% of the firms from the first sample and 64% of the firms from the second sample reported positive investment in the reference year. In both samples, manufacturing sector firms stand out as having the largest share of firms investing.

Plant-level investment is lumpy (Doms and Dunne (1998); Caballero and Engel (1999); Nilsen and Schiantarelli (2003)), and to the extent that there are many single-site firms in an economy, firm-level data should reflect the lumpiness. Earlier studies on the US and the UK note the rarity of zeros in firm-level data. The relatively larger share of non-investors in the absence of any special treatment for capital goods for Polish data helps in identifying extensive margin effects when the reforms are implemented.

TABLE 1 HERE

In the first pre-reform period, an average firm made an investment just short of 130 thousand PLN, which translates to around 32 thousand Euros. Average turnover of these firms was around 3.4 million PLN (around 850 thousand Euros). There is also substantial international trading activity, which highlights the

importance of controlling for changes in imports and exports due to changes in terms of trade in the analysis period.

C. Patterns of volatility

We demonstrate the extent of time variation in economic uncertainty in Figures 3 and 4. Figure 3 shows the changes in the Eurostat economic sentiment indicator, which is composed of indicators for confidence in several industries and a consumer confidence indicator. During the first reform period, this indicator peaks to its highest level for our data period, whereas the second reform period witnesses a dip followed by a relatively flat pattern that is much lower than the initial peak period.

FIGURE 3 HERE

From the perspective of investment decisions, the most important difference between the two analysis periods is the elevated level of global uncertainty. Poland maintained growth in the aftermath of the global liquidity crisis; however, the effects of the crisis were felt through a depreciation in the currency in 2008-2009. Thanks to the availability of firm level trade data, we are able to control for the effect of the exchange rate volatility on importing and exporting firms, while focusing on the effects of volatility on investment responses.

Figure 4 demonstrates the changes in the degree of volatility for the group of firms in our sample (left axis) and the broader European news-based economic policy uncertainty (EPU) using the method developed by Bloom, Baker and Davis (2016). In constructing this index, Bloom, Baker and Davis (2016) use the occurrence of words related to economic uncertainty in leading European newspapers.[8] As a proxy for time variation in firm-level volatility, we use the standard deviation of profit margins of firms included in our sample in the second reform period. Both our firm-level indicator for volatility and the European news-based EPU follow a similar rise around the second reform period: the 2005-2007 period experienced moderate levels of volatility, whereas the period from 2009 onwards was one characterized by much higher volatility. In line with the Economic sentiment indicator depicted in Figure 3, the firms in our sample then appear to settle for a lower degree of volatility than the 2009-2010 period, but the levels remain higher than those for 2005-2007.

FIGURE 4 HERE

Finally, volatility and investment patterns go hand-in-hand when we explore the differences between investing and non-investing firms. These patterns, depicted

[8]The authors demonstrate the relationship of this measure to other volatility measures such as indicators of stock market volatility.

more clearly in Figure 5, provide useful insights regarding possible extensive margin responses to a reduction in the user cost of capital under uncertainty.

FIGURE 5 HERE

IV. Identifying Investment Responses to Accelerated Depreciation

A. Empirical strategy

We evaluate the performance of our treatment groups against the performance of a control group both in cross-section and over time using difference-in-differences. We estimate the impact of the policy on the level of investment for treated firms, controlling for time-invariant firm-specific characteristics using a within groups estimator and a firm size control. The Polish context offers a valuable opportunity to evaluate the effectiveness of tax incentives for investment by exploiting the natural experiment arising from the introduction of the firm size threshold in 2007, which is a low volatility period, and then the extension of the size threshold after 2009, which is a period of elevated uncertainty.

A firm is eligible for full expensing of the cost of its qualifying capital goods in year t if its turnover in year $t-1$ was below the threshold. We therefore focus on the outcome observed in the first year after each reform year. These outcome years of interest are 2007 and 2010. We focus particularly on short run outcomes, as treatment assignment in later years may be affected by feedback from policy-induced investment to firm size. We later explore some long run implications taking this caveat into account.

Experiment 1 – the low volatility period: The introduction of the Lump Sum Depreciation scheme in 2007 for firms below the 800,000-Euro turnover size threshold in 2006. We form the treatment group as the firms that fall below this size limit in 2006. The control group consists of the firms that were above the threshold.

Experiment 2 – the high volatility period: The expansion of the eligibility criterion to firms with turnover between 800,000 Euros and 1,200,000 Euros in mid-2009, amplified by the change in the Zloty-equivalent of the threshold from around 3 million Zlotys in early 2008 to more than 5 million Zlotys in early 2010. We use the year 2008 as the reference pre-reform year to evaluate the change in investment level and probability in the post-reform year of 2010. Because 2009 is a partial treatment period, we remove this period from our analysis.[9]

After all cleaning steps, we retain a comparable number of firms in treatment and control groups for the two samples. In our baseline regression samples, we

[9] We exclude all the firms that were treated in 2008 because of having a lower-than-threshold turnover level in 2007. Treated firms in Experiment 2 are allowed to have been treated in 2007, but not in 2008. For treatment that affects investment in 2010, the turnover benchmark is based on 2009.

have 9,718 unique treated firms and 11,649 unique control firms for the low volatility period. For the high volatility period, we have 3,530 unique treated firms and 8,584 unique control firms.

We assess both the intensive and the extensive margin effects of the reform. Our main outcome of interest is the percentage increase in investment by the treated firms in the post-reform period, which we measure using the natural logarithm of investment. We also explore whether the reform increases the odds of investing for treated firms relative to control firms using a logit specification. Our baseline linear specification is the following:

$$I_{it} = \alpha + \gamma D_i T_t + \mathrm{X}_{it}^{'}\beta + \eta_i + \delta_t + \psi_{st} + \varepsilon_{it} \tag{1}$$

For firm i in year t, I_{it} is the outcome of interest, which is log of investment, D_i is a time-invariant dummy that takes the value unity for firms in the treated group and zero for firms in the control group, T_t is a dummy that takes the value unity in the post-reform period, X_{it} is a vector of time-varying characteristics such as lagged turnover (in log), share of exports in turnover (pre-reform level) interacted with time and share of imports in turnover (pre-reform level) interacted with time. The other terms include a common constant (α), time-invariant unobservable firm characteristics (η), year dummies (δ), sector-year effects (ψ_{st}) and an error component (ε). The estimate of parameter γ captures the impact of the policy on the intensive margin. When the outcome variable is log(investment), the estimate $\hat{\gamma}$ can be interpreted as the change in outcome for treated firms thanks to the reform. In order to estimate the impact of the reform on the extensive margin, we use the same explanatory variables as in Equation 1. We use conditional logit to estimate the effect of the policy on the (log) odds ratio of investing. For both the linear and the logit specifications, the estimate for the coefficient on $D_i T_t$ is the average treatment effect on the treated.

In Figure 6, we summarize the two possible treatment categorizations based on pre-reform size. For the first sub-sample, Control and T-Mid, as labeled in the figure, provide the control group, as they are unaffected by the 2007 reform. When the second reform kicks in in the middle of 2009, medium sized firms in the T-Mid group are also treated.

FIGURE 6 HERE

B. Graphical evidence

Our estimation strategy requires that the treated and control groups follow parallel trends in the counter-factual, and the closest we can get to verifying that this condition holds is to examine the pre-reform trends. To check the identifying assumption of parallel trends, we begin by graphing pre-reform trends in the outcomes of interest. We have two years prior to the first reform, which are, 2005

and 2006. This allows us to check if the treated and control groups had similar changes in the average investment series between these two periods. For the second reform period, we exclude all the firms that were treated in 2008. Figures 7 and 8 show that the two series follow very similar trends in both samples.

FIGURES 7 & 8 HERE

Ideally, a long pre-reform time series would have been desirable. We can do better than constraining ourselves to one year-on-year change prior to the reform, thanks to the availability of high frequency data. In Figure 9, we use the quarter-on-quarter changes to inspect pre-reform trends for our comparison groups. The two series follow overlapping trends in the pre-reform period, with the treated group accelerating average investment spending after the policy reform in the beginning of 2007.

FIGURES 9 & 10 HERE

We also depict the trends in the log odds of investing for treated and control firms across time. Figure 11 shows annual trends for the odds of investing under Experiment 1, and Figure 12 shows the analogous picture under Experiment 2. We observe that while the first treatment seems to have had a substantial impact on the odds of investing, the second reform cannot contain the downward trend in investment probabilities for the treated firms. The relative recovery of the odds of investing later in 2015-2016 is in line with the reduced volatility that we observe for Polish firms over this extended period (Figures 3 and 4).

FIGURES 11 & 12 HERE

C. Did the global liquidity crisis have a differential impact on treated and control firms?

A natural question is whether different size categories of firms were affected differently by the adverse economic conditions in the second treatment period. We safeguard against the differential crisis impacts on treated and control in several ways. First of all, our main estimation sample does not include very small, very large or newly-established firms. We narrow down the size bracket for analysis to capture firms that fall between the 10th and the 90th percentile of the size distribution based on turnover in the last pre-reform period. We confirm our results after further reducing the sample included in the estimation to even narrower size categories. Second, we conduct placebo tests that split the control group into sub-groups and test whether these sub-groups have a placebo policy effect at time of the reform. The placebo treatment groups are constructed as

sub-groups defined using a size split within the control group. We present the results from placebo tests in Section V. Finally, in order to address any concerns about differential growth rates by different size groups, we employ a formal test. For the purpose of the test, we drop all firms that were treated in the 2008-2010 period and focus only on the control group. Within this group, we slice the data up into different size categories based on their pre-reform size. We run the following regression:

$$y_{it} = \phi_0 + \phi_1 G_1 + \phi_2 G_2 + \phi_3 G_3 + \eta_i + \delta_t + \psi_{st} + \varepsilon_{it} \tag{2}$$

In Equation 2, y_{it} is the change in log turnover. We follow a similar specification to that in Equation 1 in terms of control variables, and we include size group dummies G_1, G_2 and G_3. The coefficients ϕ_1, ϕ_2 and ϕ_3 capture the deviations from growth in the largest quartile within the control group. We then test whether each of the group coefficients is significantly different from zero, and also whether the coefficients are equal to each other. The coefficient estimates for each group is very small, and the p-value of the joint test of equal coefficients on different size bands is 0.306. This provides reassurance that we do not observe significantly different growth trends during the crisis by different adjacent size groups in our dataset. Figure 13 shows the point estimates and 95 percent confidence intervals around the estimates for the ϕ coefficients.

FIGURE 13 HERE

We are cautious to extend the analysis beyond the short-run results, since turnover in subsequent years might be affected by the investment levels induced by the policy itself. If the policy induces higher investment, which then induces higher productivity and turnover, then an analysis on later years' investment outcomes would no longer be immune to endogeneity arising from this feedback from our outcome variable of interest to treatment assignment based on $t-1$ turnover. Subject to this caveat, under the assumption that long-run effects are identified, Figure 12 demonstrates that the odds of investing picks back up for treated firms after around 2012. This observation is consistent with the reduction in volatility a few years after the global crisis.

V. Results

A. The intensive margin

First, we test whether the introduction of the lump sum depreciation policy led to higher investment by the firms that are treated by the first reform in the low volatility period. We gradually add various control variables to the specification in Equation 1. In Table 2, Column (1) includes firm and year fixed effects, and the

coefficient on the variable Treated × Post 2006 captures the effect of the reform on treated group relative to the counterfactual scenario, which is captured by the control group. We find a positive and significant effect of the reform on average investment (in log), corresponding to an impact of the policy on the intensive margin by around 6 percent.

Starting in Column (2), we include both the lagged turnover and control dummies for different quartiles in turnover to more flexibly control for changes in the firm size based on demand conditions. Column (3) adds sector-year effects, whose inclusion does not have a substantial impact on the diff-in-diff coefficient estimate. In order to control for terms of trade effects induced by currency fluctuations, we introduce exporter and importer-year effects in Column (4), which is our preferred specification. To construct these exporter and importer effects, we use the share of exports and imports in turnover in the last pre-reform period interacted with time.

TABLE 2 HERE

In Table 3, we estimate the same specifications as in Table 2, this time testing the impact of the second reform based on Experiment 2. We find reform effects that are equally strong, if not stronger, than those after the first reform. The point estimate on the diff-in-diff coefficient (Treated × Post 2009) is close to 14 percent, but our estimated effect sizes for the two groups are not statistically significantly different from each other. Because our outcome variable is the investment level rather than investment rate, which scales the flow of investment by the stock of capital in the preceding period, the larger size of the effect might be completely attributable to the second treatment group being composed of slightly larger firms than the first treatment group. The doubling of the maximum allowable expense for Lump Sum Depreciation in the second reform period might also play a role.

TABLE 3 HERE

We find stronger results when we zoom in on the manufacturing sector. The intensive margin result for the low volatility period sample is 12.2 percent (*s.e: 0.064*) in the preferred specification, which is more than double the effect for the overall sample for Experiment 1. In the high volatility period (Experiment 2), our estimate for the intensive margin increase for treated firms relative to control is 34.3 percent (*s.e: 0.092*). The intensive margin effect for the manufacturing sector is more than double the overall effect for both the low volatility period and the high volatility period. We now further exploit the sectoral heterogeneities in treatment intensity to estimate elasticities of investment with respect to user cost.

Companies which employ long-lived assets as a large share of their capital stock benefit more from the policy relative to those that predominantly employ short-

lived assets (See, for example, Desai and Goolsbee (2004); House and Shapiro (2008); Zwick and Mahon (2017)). Within our treatment group, this means that each firm is treated with different levels of intensity based on the share long-lived assets within their capital acquisitions. Using data on shares of different asset types used by two-digit NACE sectors from the Central Statistical Office of Poland, we explore the elasticities of investment with respect to the user cost of capital. In Table 4, the first two columns cover the initial data period (2005-2007) and the last two columns cover 2008-2010. In Column (1) and Column (3), we replace the diff-in-diff estimate with our continuous treatment variable to estimate the semi-elasticity of investment with respect to z_{it}, which is a weighted average measure of the present discounted value of one Zloty of depreciation allowances. In Column (2) and Column (4), we use z_{it} to construct the tax component of the user cost of capital, and then estimate a log-log specification that yields direct estimates for the elasticity of investment with respect to the user cost of capital. Because we exploit both the introduction of the reform and the sectoral variation in treatment intensity, we assume that the non-tax components of the user cost term are absorbed by the quasi-experimental set up (alongside the various firm, time and firm-time fixed effects as listed in Table 4). We find that the user cost elasticity is between -4.1 and -4.4 and statistically significant in both periods.

TABLE 4 HERE

B. The extensive margin

We now explore the effects on the odds of investing for treated firms relative to control firms under the two reform periods using a conditional logit specification. We again observe a strong and stable positive effect in the low volatility period, but this effect disappears in the high volatility period.[10] The most parsimonious specification is the one that we present in Columns (1) and (3) as in our intensive margin analysis, and then we add non-linear size controls. Adding non-linear size controls does not have a large impact on the diff-in-diff coefficient (Treated $\times$ Post reform) for the low volatility period, and it does not increase the diff-in-diff coefficient sufficiently in the high volatility period.[11] The disappearance of the extensive margin response under uncertainty is consistent with Bloom, Bond and Van Reenen (2007)'s finding that firms prefer to wait and evaluate future market conditions before taking investment decisions.

The disappearance of the extensive margin effect under uncertainty is even more pronounced for nontraders and when we remove the firms that were treated in

[10]The positive, statistically insignificant coefficient on the interaction term is similar to the placebo that we estimate in our robustness checks.

[11]Convergence with a large number of control variables that have frequent zeros is computationally demanding in the conditional logit specification. The effects that we find are not a result of changes in terms of trade in the second reform period; we isolate the results for non-trading firms to verify this.

2007 from our sample. Non-trading companies had virtually a non-response to the second reform (a reform effect of 3 percent (*s.e: 0.085*)) and the reform impact for an alternative treatment group which removes the firms that were treated in 2007 from the sample was 5.9 percent (*s.e: 0.084*). When we replace the diff-in-diff coefficient with treatment intensity (as we did in Table 4 for intensive margin results), we find that there is a strong extensive margin response in the low volatility period (coefficient on user cost at -8.6 percent (*s.e: 2.361*)), but the response is very imprecise in the high volatility period (coefficient on user cost at -3.4 percent (*s.e: 3.528*)).[12]

TABLE 5 HERE

C. Robustness

We conduct a series of robustness checks to verify our results. In Section IV, we have shown that size-specific adverse growth effects in the post-2009 period were not a likely explanation for our findings. In a similar vein, now, we conduct placebo tests which remove all treated firms, and assign a placebo size threshold in the middle of the pre-reform size distribution of the control group. If we find that smaller firms within the control group increase investment spending or other responses after the reform, then our size-based treatment assignment would likely be violated.

Table 6 presents the results from a specification that interacts the time-invariant 'placebo treatment group' with the post-reform period dummy. We construct the samples for this analysis using only control group firms for Experiment 1 and Experiment 2. The first two columns explore the intensive margin effects, with log(investment) as the dependent variable. The first column presents the results from our preferred specification with the full set of controls. In the second column, the analysis is carried to the later period sample which we label as 'volatile', while retaining the same dependent variable. The third and fourth columns explore placebo reform impacts on the log odds of investing for the same two samples. In none of the cases do we find any intensive or extensive margin effect of the policy.

TABLE 6 HERE

Further, we show that our results are robust to narrowing the size distribution of firms that we include in the analysis. This is important given the strong relationship between firm size and investment frequency (Nilsen and Schiantarelli (2003)). For the results that we present in Table 7, we use a sub-sample created after dropping top and bottom 15 percentiles of pre-reform size distribution, instead of the top and bottom 10th percentiles, as in the baseline specification. In

[12]Full results are available upon request.

Table 8, we go even further and remove smallest half of the treatment group and the largest half of the control group (based on pre-reform size). This latter approach ensures that we focus on firms that are closest to the size threshold. Both of these robustness checks confirm findings from the baseline specification. The extensive margin effect is highly significant for 2007 reform, and not significantly different from zero for the period of elevated uncertainty.

TABLE 7 & TABLE 8 HERE

D. Discussion

The regression results provide a robust finding that average investment increases in response to a permanent tax incentive for the firms that are already investing. The effect on the probability to invest, however, depends on the background economic conditions. In low volatility periods, both the firms that invest consistently every year and the firms that are more sporadic in their investments get an additional investment boost through the incentive. In periods of uncertainty, non-investors tend to wait-and-see before responding to policies that aim to promote investment. We use our direct estimates of the elasticity of investment with respect to user cost exploiting both the quasi-experimental variation arising from the reforms in 2007 and 2009 and the continuous treatment variation across sectoral asset use. We find that the elasticity of investment with respect to user cost is between -4.1 and -4.4, which is lower than, but close to the estimates of House and Shapiro (2008), Zwick and Mahon (2017), Ohrn (2018) and Devereux, Maffini and Xing (2016). The smaller elasticity estimate is consistent with earlier theoretical predictions that a temporary incentive provides a stronger stimulus than permanent incentives.

Taking the increase in investment with respect to the 2007 reform as our baseline result, we calculate the average additional investment by corporations solely attributable to treatment. We calculate that the treated firms in our sample increased their investment by 75.6 million Zlotys (little more than 20 million USD) thanks to the policy. We calculate the related borrowing needs of the government at 67.6 million Zlotys in 2007, whereas the overall discounted cost of servicing debt amounts to 12.8 million Zlotys. Therefore, 1 dollar spent by the government translates to 5.9 dollars of additional investment made by firms, which means that periods of stability offer an important policy opportunity to encourage investment, for both the non-investing firms and the consistent investors. In busts, investing firms still respond to the permanent policy with a similar intensity. The impact on non-investors on the other hand is muted in periods of high volatility. Government strategy in busts should depend on the policy objective.

VI. Conclusion

This paper uses two separate quasi-experiments to demonstrate that investment responses to permanent tax policy stimuli depend on exposure of firms to economic uncertainty. Specifically, we find evidence that elevated uncertainty may cause a dampening of extensive margin responses to tax policy, consistent with the increased real option value of postponing investment under uncertainty. 'Lumps and bumps' in investment patterns might be responsible for the muted effect to be constrained to the extensive margin response. We use population data on VAT, CIT and international trade to apply our diff-in-diff estimator and exploit two major reforms that took place in 2007 and 2009. We rule out other possible channels such as heterogeneities in financing constraints during the financial crisis.

First, we verify a key existing finding in the literature; that companies do respond to tax incentives for investment. A permanent reduction in the user cost of capital induced by a policy allowing for 100 percent expensing of the cost of capital goods leads to both an increase in average investment for firms that are already investing, and to an increase in the odds of investing for eligible firms that did not invest before the reform. We then explore responses to policy under different market conditions, thanks to the availability of the second reform in 2009. In this alternative setting, firms' responses to investment incentives depend on demand conditions (as suggested by Bloom, Bond and Van Reenen (2007)). Companies that are not investing already do not respond to the policy by starting to invest. In contrast, investing firms continue their activity both in the more stable and the more uncertain periods.

The disappearance of the extensive margin response under uncertainty is consistent with Bloom, Bond and Van Reenen (2007)'s finding that firms prefer to wait and evaluate future market conditions before taking investment decisions. Our novelty is the empirical support for varying degrees of tax policy response; studies to-date have mostly focused on either a single low volatility-period reform (such as Devereux et al., forthcoming) or periods of high economic volatility (such as Zwick and Mahon (2017), who explore the impact of temporary incentives).

In this paper, we extend Zwick and Mahon (2017)'s empirical finding about the heterogeneities in firm-level responses to tax policy to consider firms that face varying degrees of underlying uncertainty. In their paper, Zwick and Mahon highlight that "more work is needed to differentiate the key mechanisms (p.246)" in the context of heterogeneities in fixed adjustment costs. Our paper constitutes a step towards identifying a channel through which firms might differ in their responses to tax policy instruments which aim to stimulate investment. We leave some important questions about other dimensions of unobserved heterogeneity and responses to counterfactual policy options to future work.

REFERENCES

Abel, Andrew, and Janice Eberly. 1996. "Optimal Investment with Costly Reversibility." *Review of Economic Studies*, 63(4): 581–593.

Abel, Andrew B. 1982. "Dynamic effects of permanent and temporary tax policies in a q model of investment." *Journal of Monetary Economics*, 9(3): 353 – 373.

Abel, Andrew B., Janice C. Eberly, Avinash K. Dixit, and Robert S. Pindyck. 1996. "Options, the Value of Capital, and Investment*." *The Quarterly Journal of Economics*, 111(3): 753–777.

Arrow, K.J. 1968. "Optimal Capital Policy with Irreversible Investment, Value, Capital and Growth, Papers in Honour of Sir John Hicks." , ed. J.N. Wolfe, 1–19. Edinburgh: Edinburgh University Press.

Bertola, Guiseppe, and Ricardo Caballero. 1994. "Irreversibility and Aggregate Investment." *Review of Economic Studies*, 61(2): 223–246.

Bloom, Nicholas. 2009. "The Impact of Uncertainty Shocks." *Econometrica*, 77(3): 623–685.

Bloom, Nicholas, Scott R. Baker, and Steven J. Davis. 2016. "Measuring Economic Policy Uncertainty." *The Quarterly Journal of Economics*, 131(4): 1593–1636.

Bloom, Nick, Stephen Bond, and John Van Reenen. 2007. "Uncertainty and Investment Dynamics." *The Review of Economic Studies*, 74(2): 391–415.

Bond, Stephen, and John Van Reenen. 2007. "Microeconometric Models of Investment and Employment." In *Handbook of Econometrics*. Vol. 6 of *Handbook of Econometrics*, , ed. J.J. Heckman and E.E. Leamer, Chapter 65. Elsevier.

Bond, Stephen R., Måns Söderbom, and Guiying Wu. 2011. "Pursuing the wrong options? Adjustment costs and the relationship between uncertainty and capital accumulation." *Economics Letters*, 111(3): 249 – 251.

Caballero, Ricardo. 1991. "On the Sign of the Investment-Uncertainty Relationship." *American Economic Review*, 81(1): 279–288.

Caballero, Ricardo J., and Eduardo M. R. A. Engel. 1999. "Explaining Investment Dynamics in U.S. Manufacturing: A Generalized (S,s) Approach." *Econometrica*, 67(4): 783–826.

Chetty, Raj. 2007. "Interest Rates, Irreversibility, and Backward-Bending Investment." *Review of Economic Studies*, 74(1): 67–91.

Cooper, Russell, John Haltiwanger, and Laura Power. 1999. "Machine Replacement and the Business Cycle: Lumps and Bumps." *American Economic Review*, 89(4): 921–946.

Cooper, Russell W., and John C. Haltiwanger. 2006. "On the Nature of Capital Adjustment Costs." *The Review of Economic Studies*, 73(3): 611–633.

Cummins, Jason G., Kevin A. Hassett, and R. Glenn Hubbard. 1994. "A Reconsideration of Investment Behavior Using Tax Reforms as Natural Experiments." *Brookings Papers on Economic Activity*, 25(2): 1–74.

Desai, Mihir, and Austan Goolsbee. 2004. "Investment, Overhang and Tax Policy." *Brookings Papers on Economic Activity*, 2: 285–355.

Devereux, Michael, and Li Liu. 2016. "Stimulating investment through incorporation." Oxford University Centre for Business Taxation.

Devereux, Michael, Giorgia Maffini, and Jing Xing. 2016. "The impact of investment incentives." Oxford University Centre for Business Taxation.

Dixit, Avinash K., and Robert S. Pindyck. 1994. "Investment under Uncertainty." *New Jersey: Princeton University Press.*

Doms, Mark, and Timothy Dunne. 1998. "Capital Adjustment Patterns in Manufacturing Plants." *Review of Economic Dynamics*, 1(2): 409–429.

Eberly, Janice, and Jan Van Mieghem. 1997. "Multi-factor Dynamic Investment under Uncertainty." *Journal of Economic Theory*, 75(2): 345–387.

Guceri, Irem, and Li Liu. 2019. "Effectiveness of Fiscal Incentives for R&D: Quasi-experimental Evidence." *American Economic Journal: Economic Policy*, 11(1): 266–91.

Guiso, Luigi, and Giuseppe Parigi. 1999. "Investment and Demand Uncertainty." *Quarterly Journal of Economics*, 114(1): 185–227.

Hall, Robert E., and Dale W. Jorgenson. 1967. "Tax Policy and Investment Behavior." *The American Economic Review*, 57(3): pp. 391–414.

House, Christopher L., and Matthew D. Shapiro. 2008. "Temporary Investment Tax Incentives: Theory with Evidence from Bonus Depreciation." *American Economic Review*, 98(3): 737–68.

Jorgenson, Dale W. 1963. "Capital Theory and Investment Behavior." *The American Economic Review*, 53(2): pp. 247–259.

Kaplan, Steven N., and Luigi Zingales. 1997. "Do Investment-Cash Flow Sensitivities Provide Useful Measures of Financing Constraints?" *The Quarterly Journal of Economics*, 112(1): 169–215.

King, Mervyn A. 1987. "The Cash Flow Corporate Income Tax." *The Effects of Taxation on Capital Accumulation*, 377–400. University of Chicago Press.

Liu, Li, Benjamin Lockwood, Miguel Almunia, and Eddy H.F. Tam. 2018. "VAT Notches, Voluntary Registration, and Bunching: Theory and UK Evidence." Working Paper.

Myers, Stewart C., and Nicholas S. Majluf. 1984. "Corporate financing and investment decisions when firms have information that investors do not have." *Journal of Financial Economics*, 13(2): 187 – 221.

Nilsen, Øivind, and Fabio Schiantarelli. 2003. "Zeros and Lumps in Investment: Empirical Evidence on Irreversibilities and Nonconvexities." *Review of Economics and Statistics*, 85(4): 1021–1037.

Ohrn, Eric. 2018. "The effect of corporate taxation on investment and financial policy." *American Economic Journal: Economic Policy*, 2: 272–301.

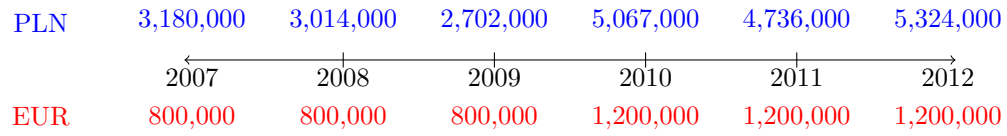
Whited, Toni. 2006. "External Finance Constraints and the Intertemporal Pattern of Intermittent Investment." *Journal of Financial Economics*, 81: 467–502.

Zeira, Joseph. 1987. "Investment as a Process of Search." *Journal of Political Economy*, 95(1): 204–10.

Zwick, Eric, and James Mahon. 2017. "Tax Policy and Heterogeneous Investment Behavior." *American Economic Review*, 107(1): 217–248.

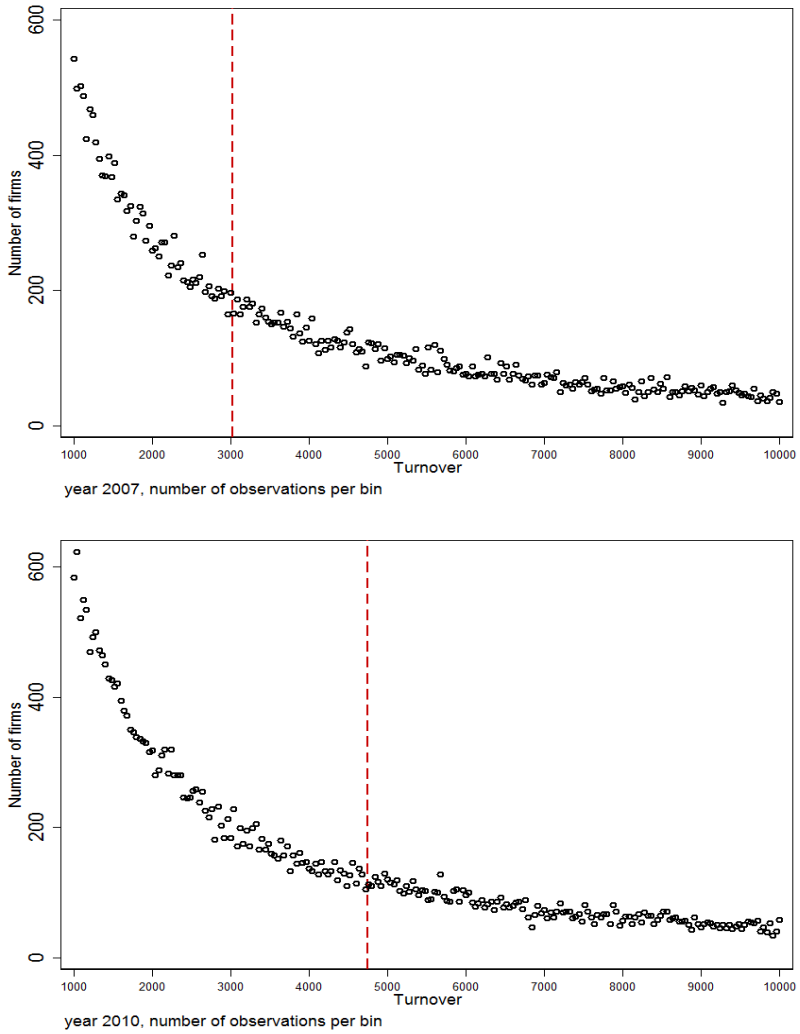
VII. Figures

Figure 1. . Reform timeline, 2007-2011



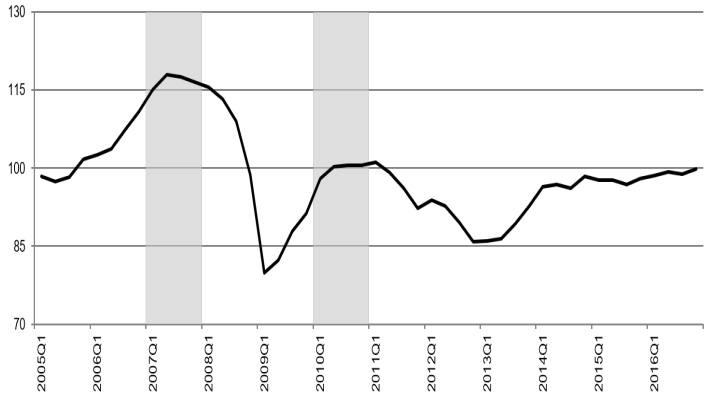
Note: This timeline shows the turnover thresholds that determine treated firms. For each year t in this timeline, turnover value in year $t-1$ should have remained below the threshold for the firm to benefit from the policy for its investment year t. Exchange rate for conversion is the National Bank of Poland reference rate on 1 October of year $t-1$. The Euro-denominated increase in the threshold from 800,000 Euros to 1,200,000 Euros took place in the middle of year 2009. We remove this year from our analysis and verify that treatment assignment was not affected by the mid-year introduction of the policy.

Figure 2. . Number of firms by size bins relative to the turnover threshold in period $t-1$ for eligibility to the policy in period t



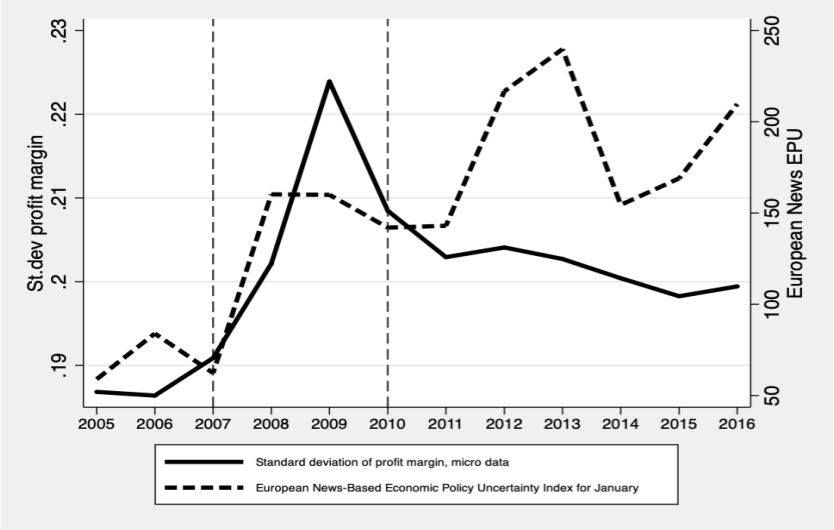
Note: The two graphs show the number of firms in each 1,000-Zloty turnover bin in the first post-reform period after the reform. The red dotted lines show the turnover threshold for the given period. Hollow circles represent the number of firms in the bin corresponding to the turnover value indicated in the x-axis.

Figure 3. . Economic sentiment, 2005-2016



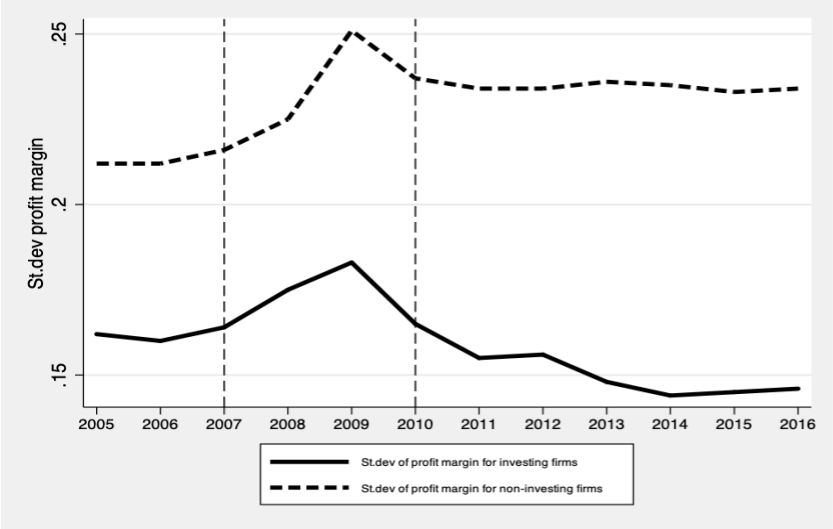
Note: Economic Sentiment Indicator is computed by Eurostat based on five sectoral confidence indicators. Grey areas represent the two treatment periods analysed in the paper: 2007 and 2010. Additional details about the construction of the index, its components and historic values are publicly available at: https://ec.europa.eu/eurostat/web/products-datasets/-/teibs010.

Figure 4. . Micro and macro level indicators of volatility



Note: The Economic Policy Uncertainty (EPU) index (right axis, dashed line) shows the trend in uncertainty as captured by the news-based indicator described in Bloom, Baker and Davis (2016). We use the January values for the index which is originally reported with monthly frequency on http://www.policyuncertainty.com/europe_monthly.html. The smooth line shows the trend in uncertainty as captured by the standard deviation of profit margins in our dataset. We use the sub-population of Polish firms that enter the sample for the high volatility estimation period.

Figure 5. . Differences in volatility between investing and non-investing groups



Note: The two lines show the trend in uncertainty as captured by the standard deviation of profit margins in our dataset. We use the sub-population of Polish firms that enter the sample for the high volatility estimation period to demonstrate the difference between the degree of volatility in the two analysis sub-periods.

Figure 6. . Treatment and control categories for the two samples

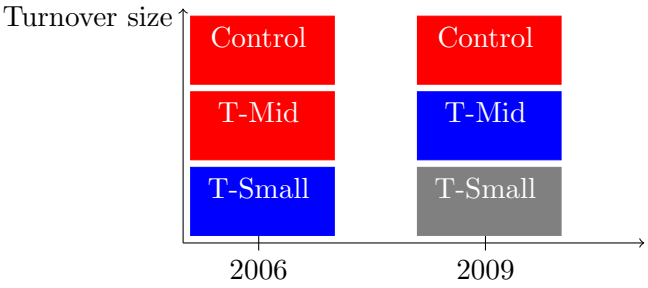
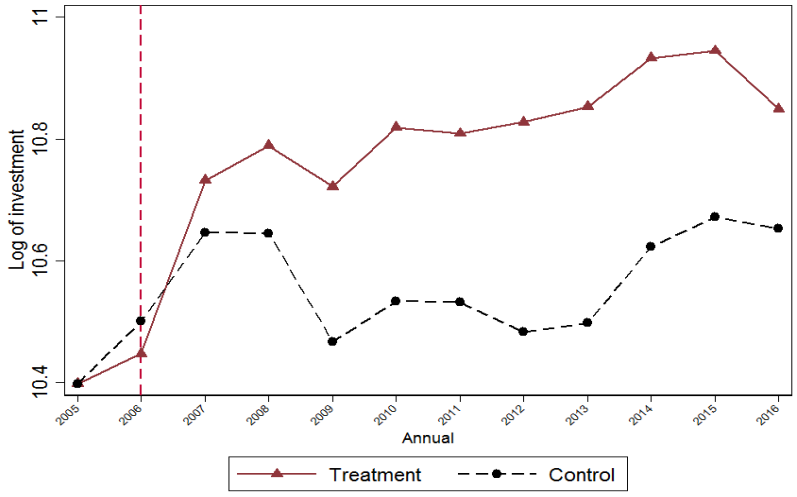
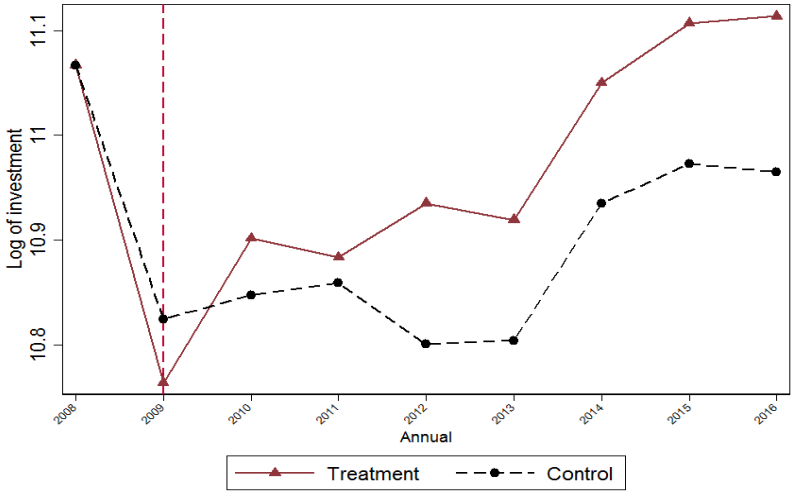


Figure 7. . Trends in average investment across groups, 2006-2007 sample, annual



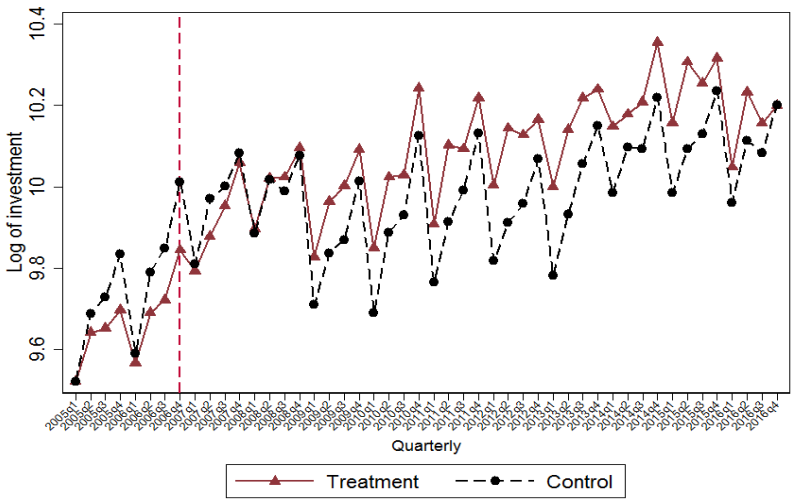
Note: This graph plots the average investment (in log) series for the treatment and control groups in the sample that we use to evaluate the low volatility period experiment, which covers years 2006 and 2007. The sample is composed of incorporated businesses being between the 10th and the 90th percentile of the size distribution based on turnover in 2006. The treatment group consists of firms having turnover in 2006 lower than 800,000 Euros and firms above that threshold form the control group. For comparability, we subtract from each data point the group mean from 2005 and add back the pooled mean from the same year. The vertical red-dashed line marks the last pre-reform period.

Figure 8. . Trends in average investment across groups, 2008-2010 sample, annual



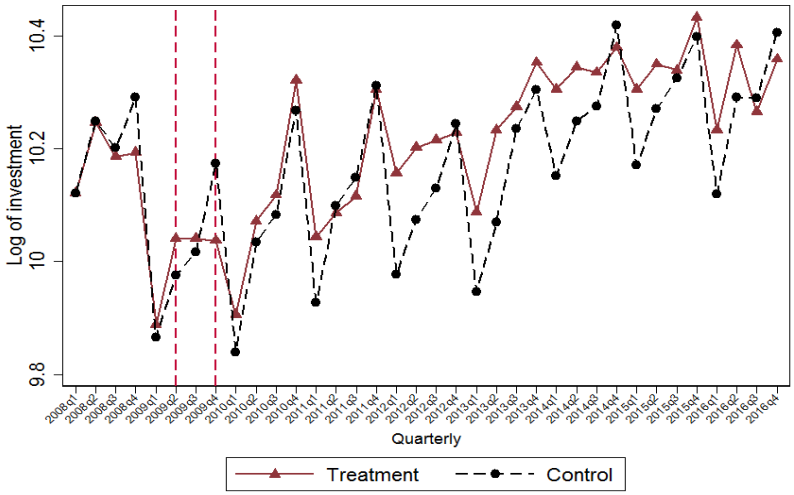
Note: This graph plots the average investment (in log) series for the treatment and control groups in the sample that we use to evaluate the volatility period experiment, which covers years 2008 and 2010. The sample is composed of incorporated businesses being between the 10th and the 90th percentile of the size distribution based on turnover in 2009. Furthermore, we exclude from the sample all firms that in 2007 had turnover lower than 800,000 Euros, which made them eligible for treatment in 2008. The treatment group consists of firms having turnover in 2009 lower than 1,200,000 Euros, and firms above that threshold form the control group. For comparability, we subtract from each data point the group mean from 2008 and add back the pooled mean from the same year. The vertical red-dashed line marks the last pre-reform period.

Figure 9. . Trends in average investment across groups, 2006-2007 sample, quarterly



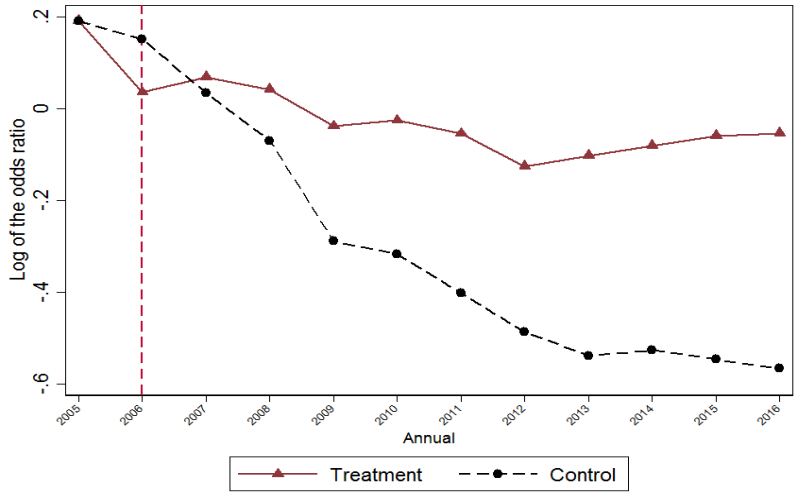
Note: This graph plots the average investment (in log) series for the treatment and control groups in the sample that we use to evaluate the low volatility period experiment, which covers years 2006 and 2007. The sample is composed of incorporated businesses being between the 10th and the 90th percentile of the size distribution based on turnover in 2006. The treatment group consists of firms having turnover in 2006 lower than 800,000 Euros and firms above that threshold form the control group. For comparability, we subtract from each data point the group mean from 2005Q1 and add back the pooled mean from the same period. The vertical red-dashed line marks the last pre-reform period.

Figure 10. . Trends in average investment across groups, 2008-2010 sample, quarterly



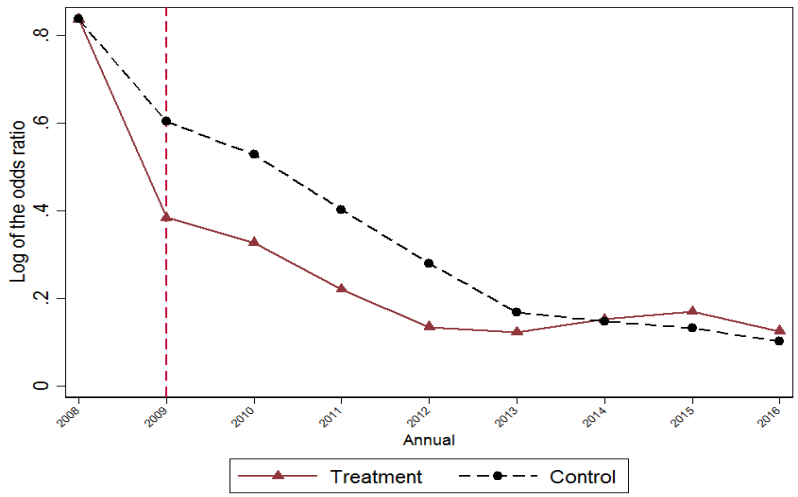
Note: This graph plots the average investment (in log) series for the treatment and control groups in the sample that we use to evaluate the volatility period experiment, which covers years 2008 and 2010. The sample is composed of incorporated businesses being between the 10th and the 90th percentile of the size distribution based on turnover in 2009. Furthermore, we exclude from the sample all firms that in 2007 had turnover lower than 800,000 Euros, which made them eligible for treatment in 2008. The treatment group consists of firms having turnover in 2009 lower than 1,200,000 Euros, and firms above that threshold form the control group. For comparability, we subtract from each data point the group mean from 2008Q1 and add back the pooled mean from the same period. The vertical red-dashed line marks the last pre-reform period.

Figure 11. . Trends in the odds of investment across groups, 2006-2007 sample



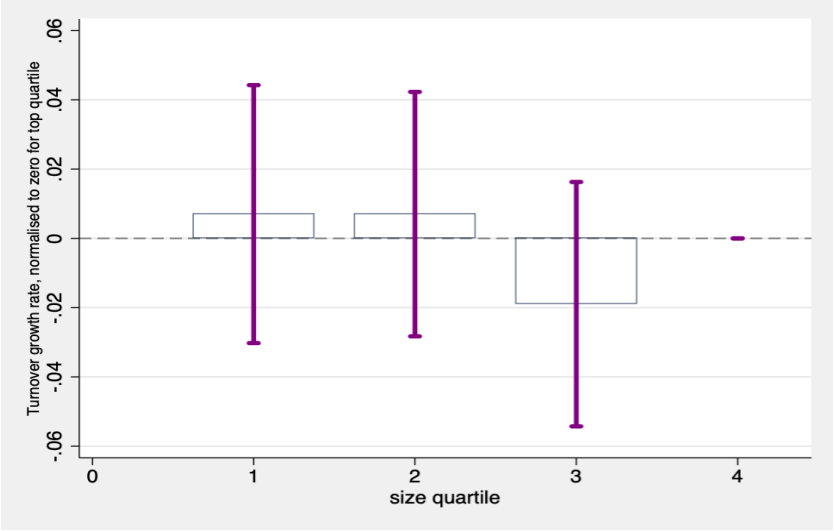
Note: This graph plots the (log) odds of investing series for the treatment and control groups in the sample that we use to evaluate the low volatility period experiment, which covers years 2006 and 2007. The sample is composed of incorporated businesses being between the 10th and the 90th percentile of the size distribution based on turnover in 2006. The treatment group consists of firms having turnover in 2006 lower than 800,000 Euros and firms above that threshold form the control group. For comparability, we subtract from each data point the group mean from 2005 and add back the pooled mean from the same year. The vertical red-dashed line marks the last pre-reform period.

Figure 12. . Trends in the odds of investment across groups, 2008-2010 sample



Note: This graph plots the (log) odds of investing series for the treatment and control groups in the sample that we use to evaluate the volatility period experiment, which covers years 2008 and 2010. The sample is composed of incorporated businesses being between the 10th and the 90th percentile of the size distribution based on turnover in 2009. Furthermore, we exclude from the sample all firms that in 2007 had turnover lower than 800,000 Euros, which made them eligible for treatment in 2008. The treatment group consists of firms having turnover in 2009 lower than 1,200,000 Euros, and firms above that threshold form the control group. For comparability, we subtract from each data point the group mean from 2008 and add back the pooled mean from the same year. The vertical red-dashed line marks the last pre-reform period.

Figure 13. . Effects of economic conditions in post-2009 by different size groups



Note: This figure shows how the different size categories were affected by the global liquidity crisis and the currency depreciation in Poland. We drop all treated firms and restrict analysis to control firms. Data years included in the regressions in this table are 2008 and 2010. Within the control group, we identify four size quartiles based on turnover values in our reference year for Experiment 2. We assign a dummy variable for each of the groups, then interact this dummy variable with a post-2009 dummy. We normalise the largest quartile to zero, and run an OLS on within-transformed data to remove company fixed effects. The outcome variable of interest is the log of the turnover growth rate. We include the size group interacted with the post-2009 dummy and include year effects as well as sector-year interactions.

VIII. Tables

Table 1—. Summary Statistics

	No of firms	Share investing	Share importing	Share exporting	Mean invest.	Mean turn.	Mean exports	Mean imports	Mean age
The 'Low Volatility Period', data for 2006									
All sectors	50,944	0.42	0.25	0.17	128	3,331	230	314	10.1
Manufacturing	7,990	0.57	0.48	0.45	211	4,837	917	587	10.4
Construction	4,821	0.45	0.16	0.07	96	3,592	34	84	10.8
Trade and transport	12,380	0.43	0.38	0.24	79	4,102	190	690	9.5
Others	25,753	0.37	0.14	0.08	133	2,445	73	92	10.3
The 'High Volatility Period', data for 2009									
All sectors	19,809	0.64	0.46	0.32	260	7,586	552	710	11.3
Manufacturing	4,560	0.73	0.69	0.63	277	8,062	1,587	885	11.8
Construction	2,260	0.63	0.23	0.07	146	7,157	47	136	11.5
Trade and transport	6,229	0.60	0.54	0.38	135	8,054	366	1,327	10.6
Others	6,760	0.61	0.30	0.15	401	6,976	194	215	11.7

Note: Turnover, investment, export and import values are in nominal, thousand PLN. In the table, we present the statistics for the last pre-reform period for the treatment and control samples that are used in estimation. By construction, the top panel sample covers all size groups and the bottom panel sample leaves the treated firms in the first reform period out of the sample.

Table 2—. Baseline results, intensive margin, low volatility period

Dep.var: log(investment)	(1)	(2)	(3)	(4)
Treated × Post 2006	0.058**	0.055*	0.066**	0.061**
	(0.029)	(0.029)	(0.030)	(0.030)
Lagged turnover (in log)	-0.033	-0.031	-0.035	-0.033
	(0.028)	(0.036)	(0.036)	(0.036)
Size quartile 2		0.052	0.051	0.050
		(0.119)	(0.119)	(0.119)
Size quartile 3		0.027	0.031	0.028
		(0.145)	(0.145)	(0.145)
Size quartile 4		0.004	0.007	0.000
		(0.164)	(0.164)	(0.164)
Constant	11.078***	11.030***	11.109***	11.051***
	(0.405)	(0.457)	(0.458)	(0.460)
Firm fixed effects?	Yes	Yes	Yes	Yes
Year effects?	Yes	Yes	Yes	Yes
Sector-year effects?	No	No	Yes	Yes
Exporter-year effects?	No	No	No	Yes
Importer-year effects?	No	No	No	Yes
No of observations	42156	42156	42156	42156
No of treated firms	9718	9718	9718	9718
No of control firms	11649	11649	11649	11649

Note: In this table, we present our baseline estimates for the specification with log(investment) as the dependent variable. The analysis period covered in this table is 2006-2007. We estimate the specification in Equation 1 using ordinary least squares on the within-transformed model to remove the firm fixed effects. In Column (1), we include year dummies and a control for the time-varying turnover variable. In an attempt to more flexibly model the effect of firm size, in Column (2), we also include dummy variables to capture the size quartile based on lagged turnover. In Column (3), we add sector-year dummies to control for sector-specific time trends. In Column (4), we include firms-specific pre-reform ratios of exports and imports to turnover interacted with years.

Table 3—. Baseline results, intensive margin, high volatility period

Dep.var: log(investment)	(1)	(2)	(3)	(4)
Treated × Post 2009	0.112**	0.128**	0.141***	0.139***
	(0.050)	(0.050)	(0.050)	(0.050)
Lagged turnover (in log)	0.289***	0.436***	0.438***	0.437***
	(0.044)	(0.053)	(0.053)	(0.053)
Size quartile 2		-1.486***	-1.497***	-1.506***
		(0.565)	(0.567)	(0.568)
Size quartile 3		-2.359***	-2.393***	-2.402***
		(0.506)	(0.508)	(0.511)
Size quartile 4		-2.403***	-2.436***	-2.445***
		(0.518)	(0.520)	(0.523)
Constant	6.724***	6.763***	6.829***	6.847***
	(0.695)	(0.761)	(0.756)	(0.754)
Firm fixed effects?	Yes	Yes	Yes	Yes
Year effects?	Yes	Yes	Yes	Yes
Sector-year effects?	No	No	Yes	Yes
Exporter-year effects?	No	No	No	Yes
Importer-year effects?	No	No	No	Yes
No of observations	25966	25966	25966	25966
No of treated firms	3530	3530	3530	3530
No of control firms	8584	8584	8584	8584

Note: In this table, we present our baseline estimates for the specification with log(investment) as the dependent variable. The analysis period covered in this table is 2008-2010. We estimate the specification in Equation 1 using ordinary least squares on the within-transformed model to remove the firm fixed effects. In Column (1), we include year dummies and a control for the time-varying turnover variable. In an attempt to more flexibly model the effect of firm size, in Column (2), we also include dummy variables to capture the size quartile based on lagged turnover. In Column (3), we add sector-year dummies to control for sector-specific time trends. In Column (4), we include firms-specific pre-reform ratios of exports and imports to turnover interacted with years.

Table 4—. Continuous treatment effect using sectoral variation in input use

Volatility	(1) Low	(2) Low	(3) High	(4) High
Dep.var: log(investment)	Semi-elast. wrt z_{it}	Log-log; elast. wrt user cost	Semi-elast. wrt z_{it}	Log-log; elast. wrt user cost
z_{it}	0.908**		0.993*	
	(0.354)		(0.541)	
$\log\frac{1-\tau z_{it}}{1-\tau}$		-4.085**		-4.422*
		(1.596)		(2.438)
Lagged turnover (in log)	0.006	0.006	0.409***	0.408***
	(0.042)	(0.042)	(0.057)	(0.057)
Size quartile 2	-0.158	-0.157	-1.408**	-1.406**
	(0.148)	(0.148)	(0.631)	(0.631)
Size quartile 3	-0.214	-0.214	-2.529***	-2.528***
	(0.178)	(0.178)	(0.523)	(0.523)
Size quartile 4	-0.262	-0.262	-2.555***	-2.553***
	(0.200)	(0.200)	(0.537)	(0.537)
Constant	10.256***	11.170***	6.776***	7.777***
	(0.582)	(0.555)	(0.969)	(0.759)
Firm fixed effects?	Yes	Yes	Yes	Yes
Year effects?	Yes	Yes	Yes	Yes
Sector-year effects?	Yes	Yes	Yes	Yes
Exporter-year effects?	Yes	Yes	Yes	Yes
Importer-year effects?	Yes	Yes	Yes	Yes
No of observations	31918	31918	21862	21862
No of treated firms	6582	6582	2917	2917
No of control firms	9418	9418	7438	7438

Note: This table presents estimates from specifications with the natural logarithm of investment as dependent variable and different versions of treatment intensity as the key explanatory variable. All the columns in the table include firm fixed effects and year effects, as well as linear and non-linear firm size controls as per the preferred specifications in baseline diff-in-diff regressions. In Column (1) and Column (3), we replace the diff-in-diff estimate with our continuous treatment variable to estimate the semi-elasticity of investment with respect to z_{it}, which is a weighted average measure of the present discounted value of one Zloty of depreciation allowances. In calculating z_{it}, we use data from Statistics Poland on the breakdown of investment into: (i) structures and buildings; (ii) machinery and equipment; and (iii) transport equipment by two-digit NACE sectors. In Column (2) and Column (4), we use z_{it} to construct the tax component of the user cost of capital, and then estimate a log-log specification that yields direct estimates for the elasticity of investment with respect to user cost of capital. Because we exploit both the introduction of the reform and the sectoral variation in treatment intensity, we assume that the non-tax components of the user cost term are constant across treated and control firms.

Table 5—. Baseline results, extensive margin, both periods

Volatility	Low	Low	High	High
Dep.var: I(Investment> 0)	(1)	(2)	(3)	(4)
Treated × Post reform	0.190***	0.209***	0.039	0.084
	(0.047)	(0.047)	(0.071)	(0.071)
Lagged turnover (in log)	0.033	-0.007	0.354***	0.391***
	(0.023)	(0.033)	(0.039)	(0.065)
Size quartile 2		0.052		-1.201***
		(0.089)		(0.314)
Size quartile 3		0.086		-1.317***
		(0.123)		(0.339)
Size quartile 4		0.350**		-1.055***
		(0.159)		(0.373)
Firm fixed effects?	Yes	Yes	Yes	Yes
Year effects?	Yes	Yes	Yes	Yes
Sector-year effects?	No	No	No	No
Exporter-year effects?	No	No	No	No
Importer-year effects?	No	No	No	No
No of observations	18458	18458	9086	9086
No of treated firms	6622	6622	2345	2345
No of control firms	2607	2607	2198	2198

Note: In this table, we present our baseline estimates for the specification with log odds of investing as the dependent variable. The analysis periods covered in this table are 2006-2007 and 2008-2010. We estimate the specification in Equation 1 using conditional logit regressions with the firm fixed effects. In Columns (1) and (3), we include year dummies and a control for the time-varying turnover variable. In an attempt to more flexibly model the effect of firm size, in Columns (2) and (4), we also include dummy variables to capture the size quartile based on lagged turnover. We do not include other controls used for the intensive margin analisys as the presence of many time-invariant variables makes it impossible to obtain convergence of a model.

Table 6—. Placebo reforms

	Intensive Margin		Extensive Margin	
Volatility	Low	Low	High	High
	(1)	(2)	(3)	(4)
Placebo Treated × Post Reform	-0.044	0.058	0.086	0.104
	(0.034)	(0.047)	(0.083)	(0.096)
Lagged turnover (in log)	-0.009	0.589***	0.078	0.623***
	(0.055)	(0.059)	(0.100)	(0.106)
Size quartile 2	0.179		-1.606*	
	(0.509)		(0.865)	
Size quartile 3	0.270	0.224**	-1.751*	0.090
	(0.476)	(0.093)	(0.898)	(0.165)
Size quartile 4	0.173		-1.719*	
	(0.490)		(0.924)	
Constant	11.310***	2.171**		
	(0.745)	(0.951)		
Firm fixed effects?	Yes	Yes	Yes	Yes
Year effects?	Yes	Yes	Yes	Yes
Sector-year effects?	Yes	Yes	No	No
Exporter-year effects?	Yes	Yes	No	No
Importer-year effects?	Yes	Yes	No	No
No of observations	22912	17887	5214	4396
No of placebo treated firms	5419	4161	1622	1276
No of placebo control firms	5844	4423	985	892

Note: In this table, we present placebo reform results for the specification with log(investment) as the dependent variable in Column (1) and Column (2), and the conditional logit results that estimate the effect on the log odds of investing for placebo treated firms relative to placebo control in Column (3) and Column (4). Columns (1) and (3) show results from estimations that use the low volatility period data and Columns (2) and (4) are based on results from estimations that use data from the high volatility period. To obtain the placebo samples, we use the last pre-reform period median turnover within all control firms that are included in each of the sub-samples. We label the firms that have less-than-median turnover as placebo treated and the firms that have higher-than-median turnover as placebo control. Results on intensive margin are obtained with the ordinary least squares, while the extensive margin effects were estimated with conditional logit regressions. For Placebo Experiment 2 (in the second reform period), two size quartiles become collinear with the placebo interaction term and are dropped, given that the control group size band is narrower for this latter sub-sample.

Table 7—. Dropped top and bottom 15 percentiles of the population of firms, intensive and extensive margin with preferred specification

	Intensive Margin		Extensive Margin	
Volatility	Low	Low	High	High
	(1)	(2)	(3)	(4)
Treated x Post Reform	0.063*	0.109**	0.226***	0.109
	(0.033)	(0.054)	(0.052)	(0.077)
Lagged turnover (in log)	-0.045	0.400***	-0.023	0.422***
	(0.038)	(0.060)	(0.034)	(0.067)
Turnover quartile 2	0.095	-1.430**	0.072	-1.317***
	(0.119)	(0.602)	(0.090)	(0.334)
Turnover quartile 3	0.088	-2.475***	0.165	-1.442***
	(0.145)	(0.555)	(0.123)	(0.353)
Turnover quartile 4	0.079	-2.555***	0.350**	-1.311***
	(0.165)	(0.568)	(0.158)	(0.385)
Constant	10.916***	7.337***		
	(0.475)	(0.807)		
Firm fixed effects?	Yes	Yes	Yes	Yes
Year effects?	Yes	Yes	Yes	Yes
Sector-year effects?	Yes	Yes	No	No
Exporter-year effects?	Yes	Yes	No	No
Importer-year effects?	Yes	Yes	No	No
No of observations	36451	18836	17644	7754
No of treated firms	9680	3530	6597	2345
No of control firms	8802	5115	2225	1532

Note: In this table, we replicate results from preferred specifications from tables 2, 3 and 5, but we use narrower sample. Instead of dropping the top and bottom 10th percentiles of firms, as in the baseline specification, we use a sub-sample created after dropping top and bottom 15 percentiles of pre-reform size distribution.

Table 8—. Dropped bottom 50 percent of treatment and top 50 percent of control group, intensive and extensive margin with preferred specification

	Intensive Margin		Extensive Margin	
Volatility	Low	Low	High	High
	(1)	(2)	(3)	(4)
Treated x Post Reform	0.073**	0.202***	0.169***	0.090
	(0.033)	(0.062)	(0.054)	(0.088)
Lagged turnover (in log)	0.037	0.421***	0.069*	0.513***
	(0.045)	(0.081)	(0.042)	(0.096)
Turnover quartile 2	-0.119	-0.148**	-0.103	0.144
	(0.081)	(0.070)	(0.081)	(0.093)
Turnover quartile 3	-0.224**	-0.105	-0.089	0.208
	(0.107)	(0.089)	(0.116)	(0.127)
Turnover quartile 4	-0.320**	-0.102	0.158	0.118
	(0.131)	(0.125)	(0.155)	(0.184)
Constant	10.065***	4.687***		
	(0.596)	(1.233)		
Firm fixed effects?	Yes	Yes	Yes	Yes
Year effects?	Yes	Yes	Yes	Yes
Sector-year effects?	Yes	Yes	No	No
Exporter-year effects?	Yes	Yes	No	No
Importer-year effects?	Yes	Yes	No	No
No of observations	31682	23171	14364	8322
No of treated firms	8410	3414	5132	2160
No of control firms	7700	7384	2050	2001

Note: In this table, we replicate results from preferred specifications from tables 2, 3 and 5, but we use modified sample. Instead of dropping the top and bottom 10th percentiles of firms, as in the baseline specification, we remove smallest half of the treatment group and largest half of the control group, based on pre-reform size.

Table 9—. Population of firms

Sample	Number of firms	After dropping unincorporated businesses	After dropping implied data errors	After dropping smallest and largest firms	After dropping firms treated in 2008	After dropping young firms
2006-2007	1,396,856	107,184	91,212	74,881	-	50,944
2008-2010	1,565,077	127,313	107,548	78,453	35,064	19,809

A. Information on data and cleaning

Table 9 reports impact of cleaning procedures on the size of analysis samples. Around 10% to 15% of the corporations were dropped because we suspect that in one of the years (within the period 2005 - 2016) they made a wrong entry in the VAT return. For the econometric analysis we drop the top and bottom 10 percentiles of the sample, based on the firm size in pre-reform years. Such reduction enhances comparisons between the treated and control firms. The last column of the table refer to the samples used most often in the current analysis. We focus on mature firms, i.e. firms being at least five years old in the post-reform period.

TABLE 9 HERE

Generally, VAT returns are verified by tax inspectors and should be accurate. Nevertheless, we use input VAT tax related to investment, as well as firm's turnover, to detect observations which may be erroneous. Then we drop all observations for firms that record at least one data error.

Some firms are not VAT taxpayers, and therefore they cannot be included in the analysis. Product-based VAT exemption is applicable mainly to financial services, health care and education. Entity-based exemption is available for very small firms below an annual turnover threshold, which remained very low during the period of analysis.[13]

Apart from investment and turnover, the data on international trade also comes from VAT returns. The exception is extra-EU import, which is added from the customs data. We also merge the VAT data with CIT to gather data on firms' profits and losses. These variables are used to calculate the fiscal costs of the policy.

Further, we take advantage of the register of economic activity to obtain more information at the firm-level. First, the type of entity is used to drop non-businesses from the VAT data. Second, year of registration allows us to distinguish between start ups and established firms. Third, we use the NACE classification code to define sector dummy variables. The drawback of the data obtained from the company register is that it only reflects recent information, without tracking the historical changes in the firms' classification.

[13]During the analysis period, this exemption threshold never exceeded 150,000 PLN, which is well below the neighborhood of the turnover thresholds that we are using in our quasi-experiment.