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Irem Güçeri, University of Oxford Centre for Business
Taxation

Maciej Albinowski, Ministry of Finance, Poland

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Investment Responses to Tax Policy under Uncertainty

By IREM GUCERI AND MACIEJ ALBINOWSKI*

How does economic uncertainty affect the impact of tax policy? We exploit a 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. Exploiting sharp discontinuities in eligibility and using rich administrative data, we find that firms exposed to high uncertainty decide to “wait and see” before investing, despite generous incentives. Firms that are sheltered from uncertainty still respond strongly to policy. This implies that periods of stability offer an important policy opportunity to encourage investment.

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The effectiveness of any microeconomic policy depends upon the macroeconomic context, and investment policies offer no exception to this rule. Elevated uncertainty hinders the private sector’s appetite for investment in fixed assets, and lower investment, in turn, undermines aggregate productivity growth. The level of private investment in machinery and equipment therefore depends heavily on various dimensions of uncertainty faced by firms, as well as the presence of a favourable policy environment for such investment ([Bloom, Bond and Van Reenen, 2007](#); [Bloom et al., 2018](#)).

In this paper, we study the impact of a unique natural experiment in which two very similar investment tax incentives were implemented during 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 natural treatment and control groups for each of the policies. We use a rich dataset on the population of corporate, personal and value added tax (VAT) returns of firms in Poland, and find that, under low uncertainty, tax incentives have strong positive effects on investment. Under high uncertainty, however, the story is different: despite the reform being substantially more generous, we find no significant effect of the policy on investment. This can be explained by substantial heterogeneity in the policy impacts, with firms exposed to high uncertainty

* 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 extremely grateful for valuable comments by Michael Devereux, Eric Ohrn, Dawid Pachucki and Simon Quinn. We thank Emilia Skrok, reviewers from the World Bank and seminar participants for comments on an earlier version. Irem Guceri thanks the British Academy for financial support under her Postdoctoral Fellowship. Any errors and omissions are the authors’ own responsibility. The views expressed in the paper do not necessarily reflect those of the Ministry of Finance of Poland.

driving the drop in responses; those firms that are sheltered from elevated uncertainty still respond strongly to the policy. Together, these results suggest that: (i) some firms “wait and see” during periods of high uncertainty, even in the presence of generous incentives; and (ii) periods of stability offer an important policy opportunity to encourage investment.

We explore several plausible mechanisms. To explore the influence of increased uncertainty as a possible channel, we construct a firm-level uncertainty exposure measure using our administrative data on monthly sales (a second-moment measure similar to those proposed by [Bloom et al. \(2018\)](#)). This allows us to quantify the differential effects of the policy on companies that are more exposed and less exposed to uncertainty. We find a small and imprecise investment response to the policy by firms that are exposed. The firms that are less exposed to uncertainty, on the other hand, maintained the strong positive response with a similar elasticity as in the absence of overall elevated uncertainty. In addition to elevated uncertainty, we consider the role of the changes in terms of trade and the differential changes in financing constraints ([Edgerton, 2010](#)) in the second reform period. To explore the effect of the changes in terms of trade, we use the detailed import and export data and show that these do not drive the differential in results for the two reform periods. To rule out the differential changes in financing constraints, we use a narrow size range of companies, focusing on firms just above and just below the size threshold for eligibility to more generous investment incentives. We conclude that uncertainty, rather than the other hypothesised channels, is key to explaining the differential effects of the two policy reforms.

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, we find that treated firms significantly increased investment spending by around 8 percent in the low volatility period on average. This reform effect was reduced in the high volatility period. 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 -3.7 to -5.2. Relative to earlier studies that also use quasi-experimental variation on administrative data, this implied elasticity estimate is lower, but still a strong indication of firms’ responses. Comparable studies find elasticities that are higher than 6 in absolute value (on US Bonus Depreciation, see [House and Shapiro \(2008\)](#))

and [Zwick and Mahon \(2017\)](#) and on the UK First Year Allowance, see [Maffini, Xing and Devereux \(2019\)](#)). One explanation to the relatively smaller size of our estimates may be the permanent nature of the tax incentives.¹

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. To our knowledge, ours is the first paper to explore this channel in a quasi-experimental setting with firm-level variation using administrative data. We begin by confirming some established findings of the existing literature, mostly referring to quasi-experimental estimates since [Cummins, Hassett and Hubbard \(1994\)](#). Recent availability of administrative data has greatly broadened our understanding of the effectiveness of tax incentive policies ([House and Shapiro, 2008](#); [Maffini, Xing and Devereux, 2019](#); [Ohrn, 2018](#)), including our understanding of heterogeneities in policy effects across dimensions of size, age, profitability ([Zwick and Mahon, 2017](#); [Guceri and Liu, 2019](#); [Agrawal, Rosell and Simcoe, 2019](#); [Dechezelepretre et al., 2016](#)). Firms are also heterogeneous in their exposure to uncertainty. We analyse the responses, first in a low-volatility environment, and later in a high-volatility environment and find that macroeconomic conditions matter for micro-level heterogeneity in responses to policy. 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.

Second, our findings offer new empirical support for the effectiveness of permanent tax incentives, but also suggest that the elasticity of responses are smaller than those found for temporary measures. This is particularly relevant given several recent policies (such as the US tax reform and the UK's Annual Investment Allowance) increasingly resemble a tax system based on cash flow ([King, 1987](#)), by allowing immediate depreciation of the cost of capital goods. 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. 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. 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 \(1999\)](#), [Winberry \(2018\)](#), [Chen et al. \(2019\)](#)). Our methodology is agnostic about the exact mechanisms; nevertheless, in [Section I.A](#), we discuss a conceptual framework that can explain our empirical results.

Finally, we use a detailed linked dataset covering the population of VAT and corporate taxpayers, along with their import and export activity. The detailed data

¹Among these papers, only [Maffini, Xing and Devereux \(2019\)](#) 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.

allows us to (i) test a number of different channels that may be contributing to the reduced responses in the second reform period, (ii) construct our uncertainty measure by exploiting the monthly frequency of our data, (iii) verify parallel pre-reform trends across treated and control groups for both experiments by using investment information at quarterly frequency, and (iv) control for a larger set of observable characteristics and trends than does the existing literature.

In the remainder of the paper, we first summarise the theoretical background and describe the policy setup (Section I). In Section II, we discuss the data sources and summarise the dataset used for the analysis. In Section III, we explain the research design. We report the main results in Section IV and we conclude in Section V.

I. Background

A. *The Option Value of Waiting under Uncertainty*

Academic focus on the impact of second-moment shocks is relatively recent (Bloom, 2009). Heterogeneity in policy impact under varying degrees of uncertainty is implied in more recent models such as the one by Bloom et al. (2018), and to our knowledge, we are the first paper to provide micro evidence on the effects of uncertainty on investment 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)).

Three characteristics of investment are important for our empirical results. First of all, 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).

A model that reflects all these characteristics of investment is laid out by Bloom et al. (2018), where each firm invests in capital goods and incurs adjustment costs that depend on a fixed component and a resale loss to reflect partial irreversibility. When the wider economy goes through a downturn, firms face two separate uncertainty shocks, which are characterized by a negative first moment productivity shock and a positive second moment shock. Thanks to the adjustment cost structure, firms become cautious about investing when these uncertainty shocks hit. Firms disinvest if their productivity falls below a lower bound, invest if their

productivity is above an upper bound, and take no action if they are in between these two thresholds. Under higher uncertainty, we expect ‘exposed’ firms to fall in the period of inaction. In the case of investment in physical capital, this may also mean that firms maintain a certain level of replacement investment, but do not take on new projects until the period of high uncertainty subsides. Empirically, this would be reflected in the continuation of similar levels of intensive margin investment by firms that are already investing, but no reaction to additional incentives, which government policies may aim to stimulate precisely during a downturn. Bloom et al. (2018) simulate policy impacts both in the presence and in the absence of uncertainty. With the rise of uncertainty, we may expect that the overall response to a stimulus policy may be muted relative to the response in a normal period.

B. The reform timeline

Most corporate tax systems do not allow firms to deduct the full 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 deduct the full 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.² 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 deduct the full 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 (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

²This 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.

on single investment projects one at a time; the cost reduction for such firms may therefore result in intensive or extensive margin effects.

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 [Maffini, Xing and Devereux \(2019\)](#) 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 \(2008\)](#); [Zwick and Mahon \(2017\)](#) and [Garrett, Ohn and Suárez Serrato \(2019\)](#)). 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 800,000.³ 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](#).⁴

< [Figure 1](#) >

The first reform was only finalised and enacted in November 2006, without prior clarity about the timeline and eligibility thresholds for accelerated depreciation. We therefore rule out anticipation effects that could have led firms to postpone investment. The late announcement of the policy is especially important for our classification of firms into treatment and control, because it 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 increased to EUR 100,000. This expansion renders the second reform more generous than the first one.

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; the density is clearly continuous across both policy cutoffs.⁵

³To 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. 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.

⁴In the first instance, a threshold that fluctuates with the exchange rate appears to call for a regression 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.

⁵To save space, we present the figures only for 2007 and 2010, but the lack of bunching holds for all other analysis periods as well.

< Figure 2 >

II. 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.⁶ 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 transactions would have a strong incentive to register for VAT (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.

In the estimation sample, to ensure comparability of treated and control groups, we retain only the firms that are close to the eligibility cut-off.⁷ After all cleaning steps, we retain a comparable number of firms in treatment and control groups

⁶Turnover 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.

⁷We remove the largest half of the control group and the smallest half of the treated group. The reform set-up also means that the second reform places in the treated group a set of mid-sized firms in 2007 that became small in 2009, leaving a disproportionately large group of shrinking, failed firms in the treated group. To prevent such sample selection, we impose a restriction on turnover change. Firms are excluded from the second experiment if their turnover changed between 2007 and 2009 by more than 40%. We use an analogous condition for first experiment, with regard to change in turnover between 2005 and 2006.

for the two samples. In our baseline regression samples, we have 12,600 unique treated firms and 8,408 unique control firms for the low volatility period. For the high volatility period, we have 3,209 unique treated firms and 7,546 unique control firms.

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. 56% of the firms from the first sample and 72% 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 >

In the first pre-reform period, an average firm made an investment of 110 thousand PLN, which translates to around 27 thousand Euros. Average turnover of these firms was around 3.5 million PLN (around 870 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 >

In Figure 4, we explore the firm-level exposure to volatility for the population of firms in our data. Following the ideas in Bloom et al. (2018), we use a second moment indicator of turnover volatility, using the available monthly data at the firm level. The volatility data over the sample period show that firms in the two reform periods experienced significant differences in volatility.

< Figure 4 >

III. 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.

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, but include it in the figures that demonstrate common trends.⁸

In Figure 5, we summarize the two possible treatment categorizations based on pre-reform size. For the first sub-sample, Control and T-Mid, as labeled in

⁸We exclude all the firms that were treated in 2008 if they had 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.

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 5 >

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:

$$(1) \quad I_{it} = \alpha + \gamma D_i T_t + \mathbf{X}'_{it} \beta + \eta_i + \delta_t + \psi_{st} + \varepsilon_{it}$$

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, \mathbf{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). When the outcome variable is log(investment), the estimate $\hat{\gamma}$ can be interpreted as the change in investment for treated firms caused by the reform. In order to estimate the impact of the reform on the extensive margin, we use a similar set of explanatory variables as in Equation 1.⁹ We use conditional logit to estimate the effect of the policy on the (log) odds ratio of investing.

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: 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. We construct Figures 6 and 7 using annual cross-sectional regressions to depict the differential in log(investment) between treated and control groups, while controlling for certain characteristics. Figures 6 and 7 show that the two series follow very similar trends

⁹Convergence with a large number of control variables that have frequent zeros is computationally demanding in the conditional logit and Poisson specifications. We therefore control for the trade effects using the lagged share of exports and imports in turnover.

in both samples. For the second reform period, we have more data on pre-reform periods available, so we can include a longer time series for the pre-reform period.

< Figure 6 & 7 >

Zooming in on the different size groups within each sample, in Figures 8 and 9, we plot average investment across 200,000 PLN (approximately 50,000-Euro) turnover bins for the pre-reform and the post-reform periods. In the low volatility period, treated firms (below the size threshold for eligibility) experience a hike in average investment between the pre-reform year of 2006 and the post-reform year of 2007 for virtually all treated size bins that we show in this graph. This jump is much smaller for average investment of firms in size bins that are larger than the threshold turnover size that applies in the first reform period.

< Figure 8 & 9 >

In contrast to Figure 8, we do not observe any increase in investment for treated firms in the second reform period (Figure 9). If anything, we observe a drop in average investment from 2008 to 2010 for all size bins that we depict in the high volatility period. We hypothesize that the weaker policy impact is due to increased uncertainty, and we show evidence that supports this hypothesis in Section IV.

We can do better than constraining ourselves to one year-on-year change prior to the first reform, thanks to the availability of high frequency data. In Figure 10, 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.

< Figure 10 & 11 >

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 by dropping 50% smallest firms of the treatment group and 50% largest firms of the control group 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 IV. 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:

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

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 0.971. This provides reassurance that we do not observe significantly different growth trends during the crisis by different adjacent size groups in our dataset. Figure 12 shows the point estimates and 95 percent confidence intervals around the estimates for the ϕ coefficients.

< Figure 12 >

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.

IV. Results

A. Policy effectiveness with and without uncertainty

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 \times Post 2006 captures the effect of the reform on treated group relative to the counterfactual scenario. We find a positive and statistically significant effect of the reform on average investment (in log), corresponding to an impact of the policy on the intensive margin by around 8 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.

< Table 2 >

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. The treated firms in the second reform are slightly larger than those treated in the first reform, and the maximum allowable expense for Lump Sum Depreciation is doubled, so *ceteris paribus*, we should see a larger increase in investment in response to the second reform. Conversely, we find reform effects that are smaller in magnitude relative to those based on Experiment 1, and estimated with wide confidence intervals. This is despite the upper threshold for eligible investment amount being double its value under Experiment 1.

< Table 3 >

In Section II.C, we have provided graphical evidence that the firms in our dataset experienced a sharp rise in uncertainty around 2009. In light of this observation, we now unpack our findings based on the second reform period. We begin by quantifying each company’s exposure to uncertainty by exploiting the availability of monthly data frequency, following the ideas in Bloom et al. (2018).¹⁰

There is a stark difference between the investment responses of firms that experience a high exposure versus a low exposure to uncertainty. The results in Table 3 demonstrate that the firms which were sheltered from elevated uncertainty maintained strong responses to stimulus, whereas firms that had high exposure to rising uncertainty did not respond to the policy at all.

In Table 3, we present evidence on the policy responses by companies that had a high or low exposure to uncertainty during the global crisis period in Column (5). We estimate the preferred specification in the baseline, additionally interacting the coefficient of interest (Treated \times Post 2009) with the dummy variable that captures the effect for firms that have a high exposure to uncertainty in the last pre-reform period. We also interact the ‘High Exposure’ dummy with the ‘Post 2009’ dummy to take into account the background change in the High

¹⁰Our main measure for uncertainty is the standard deviation of year-on-year changes in monthly turnover for each company within a year. We then split our sample into two groups based on whether each company falls above or below the population median of the uncertainty measure in the final pre-reform period. To check robustness of using a particular uncertainty measure, we construct a second measure based on monthly changes in turnover for each company, then taking the relative standard deviation (coefficient of variation) of this measure for each firm-year.

Exposure group between the pre-reform and post-reform period that is unrelated to the reform. The coefficient on the un-interacted ‘Treated \times Post 2009’ variable therefore can be interpreted as the effect of the reform for the firms that had low exposure to uncertainty. We do not reject the null hypothesis that the policy effect for the High Exposure group is zero. Treated firms with a low exposure to increased uncertainty, on the other hand, respond strongly to the reform at similar magnitudes by two separate measures of uncertainty. In Column (6), we confirm the lack of response by firms that had a high exposure to uncertainty through a Poisson regression, which takes the level of investment as explanatory variable and demonstrates the magnitude of the total of intensive and extensive margin responses. The coefficient on Treated \times Post 2009 is positive and highly significant with a magnitude of 24.3 percent. High exposure firms, on the other hand, give a tight zero total response to policy, as captured by the coefficient on the triple-interaction term that almost exactly offsets the response by low-exposure firms.¹¹ We conclude that the treated firms that had low uncertainty exposure increased their investment by around 12.2 percent in response to the introduction of the new policy.

Elevated economic uncertainty is of course not the only difference that might influence investment responses between the periods 2005-2007 and 2008-2010. An example to the other changes that took place across these periods is the altered terms of trade. Poland maintained aggregate 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.

Another alternative channel is the differential changes in financing constraints. If access to finance is differentially affected for medium-sized firms and larger firms, this might work to suppress the responses of treated firms in the data. To tackle this challenge, we constrain our sample to the firms closest to the turnover threshold in both periods (top half in the pre-reform size distribution for the treated group and the bottom half of the pre-reform size distribution for the control group). This is important given the strong relationship between firm size and investment frequency (Nilsen and Schiantarelli, 2003).

The same reforms applied to firms that are not incorporated and are subject to the personal income tax. Because the personal and corporate income tax rates as well as depreciation allowances have been the same for both types of firms, the reforms triggered the same changes to the tax component of the user cost of capital for firms that are subject to the personal income tax (PIT).

We expect unincorporated firms to be more affected by the elevated uncertainty, as they are typically smaller, have lower management quality and have more difficulty in accessing a variety of external finance options. Column (1) of

¹¹Results using the alternative uncertainty measure and the full set of Poisson regression results which give similar estimates to the ones presented are available upon request.

Table 4 shows that in the low volatility period, the reform induced a positive and significant increase in investment for treated firms, with a magnitude of around 13 percent. This effect goes all the way down to a tight zero for the high volatility period, which we demonstrate in Column (2). Perhaps because pass-through entities are more exposed to uncertainty as a group, we do not observe a significantly positive effect of the reform even when we isolate the effect for the subset of treated pass-through entities that were subject to a relatively lower degree of uncertainty (Column (3)). We use the same triple interaction specification as we did in the results that we have shown in Table 3 in Column (5). Nevertheless, the point estimate for the reform effect on firms that are less exposed to uncertainty is positive, whereas the differential reform effect for the firms that had higher exposure to uncertainty remained negative. The reform effect on either group was insignificantly different from zero for the high volatility period.

< Table 4 >

B. Placebo reforms, treatment intensity and the extensive margin

We conduct a series of supplementary analysis to confirm our results and test alternative channels. In Section III, 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 after the reform, then our size-based treatment assignment would likely be violated.

Columns (1) and (2) of Table 5 present 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. Column (1) shows 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 ‘high volatility’. The results in this table confirm a tight zero effect of the placebo reform on control firms split by size.

< Table 5 >

Accelerated depreciation 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 (Desai and Goolsbee, 2004; House and Shapiro, 2008; Maffini, Xing and Devereux, 2019; Zwick and Mahon, 2017). The reduction in the user cost of capital prompts an investment response by the beneficiaries. The investment responses are driven partly by the greater incentive to upgrade the capital stock thanks to the lower user cost of capital (Hall and Jorgenson, 1967; Jorgenson, 1963) and partly, because 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).

Within our treatment group, this means that each firm is treated with different levels of intensity based on the share of 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 5, Columns (1), (3) and (5) cover the initial data period (2005-2007) and the remaining columns cover 2008-2010. In Column (1) and Column (2), we replace the diff-in-diff estimate with our continuous treatment variable to estimate the elasticity of investment with respect to user cost of capital, which we construct using a weighted average measure of the present discounted value of one Zloty of depreciation allowances. We estimate a log-log specification that yields direct estimates for the elasticity. 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 5). We find that the user cost elasticity is between -3.7 and -5.2, and statistically significant in only the low volatility period.

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 (Columns (5), (6) and (7)). In Column (5), which shows the reform effects in the low volatility period, we observe a strong and stable positive effect of the reform on the log odds of investing, but this effect becomes smaller in magnitude and more imprecise in the high volatility period. The muted extensive margin response under uncertainty is also consistent with Bloom, Bond and Van Reenen (2007)'s finding that firms prefer to wait and evaluate future market conditions before taking investment decisions. In Column (7), we further unpack the extensive margin response of treated firms into the responses by firms that faced high or low uncertainty. In order to demonstrate the differential effects of the reform on these two subsets of the treatment group, we replicate the specification that we used in Table 3, this time with the log odds of investing as the outcome variable. The coefficient on Treated \times Post Reform shows that the firms with low exposure to uncertainty maintained a similar level of response to the policy as the average response in the first reform period. However, as in the intensive margin response, the triple interaction term Treated \times Post 2009 \times High Exposure is negative and almost as large in magnitude as the effect on the firms with a low exposure to uncertainty. The Wald test of a positive policy effect on treated firms that are likely to have been exposed to high uncertainty in the second period yields a p-value of 0.936, confirming that the extensive margin policy impact on this group is statistically insignificant.

C. Discussion

The regression results provide a robust finding that average investment increases in response to a permanent tax incentive for the average firm during periods of stability. The effect during high uncertainty, however, depends on the background economic conditions and the degree of firms' exposure to uncertainty. The distribution of firms in the economy therefore is an important consideration for governments to assess the suitability of a tax allowance measure to stimulate investment in downturns.

These results have direct implications for policy, in a wide range of contexts. In the last decade, with the availability of administrative data, a few studies have explored the average impact of depreciation allowances along with differentiated responses across sub-groups of companies to such policies (Zwick and Mahon, 2017; Devereux and Liu, 2016; Ohn, 2018). We have verified our results using the cross-industry variation in the composition of capital goods as in Zwick and Mahon (2017) and Garrett, Ohn and Suárez Serrato (2019). This latter method has also allowed us to exploit treatment intensity at the sectoral level. According to these 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 -3.7 and -5.2, which is lower than, but close to the estimates of House and Shapiro (2008), Zwick and Mahon (2017), Ohn (2018) and Maffini, Xing and Devereux (2019). The smaller elasticity estimate is consistent with earlier theoretical predictions that a temporary incentive provides a stronger stimulus than permanent incentives.

Cost-effectiveness for the government varies according to when the reforms are implemented, and for the second reform, it also depends on the distribution of the firms in their exposure to the elevated uncertainty. We assume that the government borrows to make up for the cost of foregone revenue in the first period when a firm expenses the full cost of the asset purchased. On one hand, the government foregoes some revenue in the expensing year; on the other hand, the firm no longer has any depreciation expense for tax purposes starting with the second year of the asset's useful life. The government makes up for the nominal value of the foregone tax revenue in the subsequent periods and therefore needs to borrow less in these years relative to the absence of the policy. In this setting, the cost to the government of an accelerated depreciation policy has two components: (1) the cost of borrowing, (2) the cost arising from the time value of money due to discounting of future gains. We take both these costs into account to derive the return (in dollars of investment terms) from a dollar cost to the government.

The results based on the impact of the reform in the low volatility period show that the government has stimulated around 6.3 dollar of investment in machinery and equipment for every dollar of cost that it incurred.¹² This means that periods

¹²We calculate the total additional investment by corporations solely attributable to treatment, as-

of stability offer an important policy opportunity to encourage investment.

During the period of high uncertainty, the policy on average did not induce statistically significant investment that would have otherwise not taken place. Calculating the return to a dollar cost incurred by the government therefore involves making further assumptions. Extrapolating the point estimate to all firms in the treated group yields an optimistic estimate of 5.2 dollars of additional investment generated for every dollar cost to the government, which is still lower than that for the low volatility period. If we distinguish the results for firms that have varying degrees of exposure to uncertainty, then there is a high return on the firms with low exposure to uncertainty, whereas some loss on the support to investment by firms with high exposure to uncertainty. Given that the non-marginal investment by firms exposed to high uncertainty drops in a downturn, the aggregate loss to the government should be contained.

V. 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 investment responses to tax policy, consistent with the increased real option value of postponing investment under uncertainty. The gradual introduction of the same policy to different groups of firms in two separate points in time enables us to observe the effects first in a low volatility period and then in a high volatility period. We exploit two major reforms that took place in 2007 and 2009 with simple firm size cut-offs dictated by the policy for identification. We verify our results using a continuous treatment variable based on sectoral variation in treatment intensity. We use population data on VAT, CIT and international trade to apply our diff-in-diff estimator. We rule out other possible channels such as heterogeneities in financing constraints during the financial crisis or the effect of changes in terms of trade on investment.

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

suming that all treated firms respond in the same way as the average firm. The policy applies to all investing companies in the treatment group, and therefore the cost arises from all treated firms that would have still invested even in the absence of the policy. We calculate that the treated firms in our sample increased aggregate investment by 42.7 million Zlotys (little more than 10 million USD) thanks to the policy. In the absence of the policy, the government collects the same amount over 7 years, which is the average depreciation period for machinery and equipment. We calculate that the related borrowing needs of the government at 41.2 million Zlotys in 2007, and the overall discounted cost of servicing debt amounts to 6.8 million Zlotys. Therefore, 1 dollar spent by the government translates to around 6.3 dollars of additional investment made by firms. For the purpose of this exercise, we fix the interest rate at the period average when calculating the government's borrowing costs.

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 exposed to a high degree of uncertainty do not respond to the policy at all. In contrast, the impact of stimulus is maintained for firms that are not highly exposed to uncertainty in high volatility periods.

The decline in the 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. We observe this muted effect in both the intensive and the extensive margins. 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 [Maffini, Xing and Devereux \(2019\)](#)) or periods of high economic volatility (such as [Zwick and Mahon \(2017\)](#), who explore the impact of temporary incentives).

In this paper, we extend recent empirical findings about heterogeneities in firm-level responses to tax policy to consider firms that face varying degrees of underlying uncertainty. Our results have important policy implications. While periods of stability offer an important opportunity to encourage private sector's investment in machinery and equipment, stimulus policies adopted in downturns may not be as effective. Aggregate investment movement in response to stimulus is likely to depend on the distribution of firms in their exposure to elevated uncertainty.

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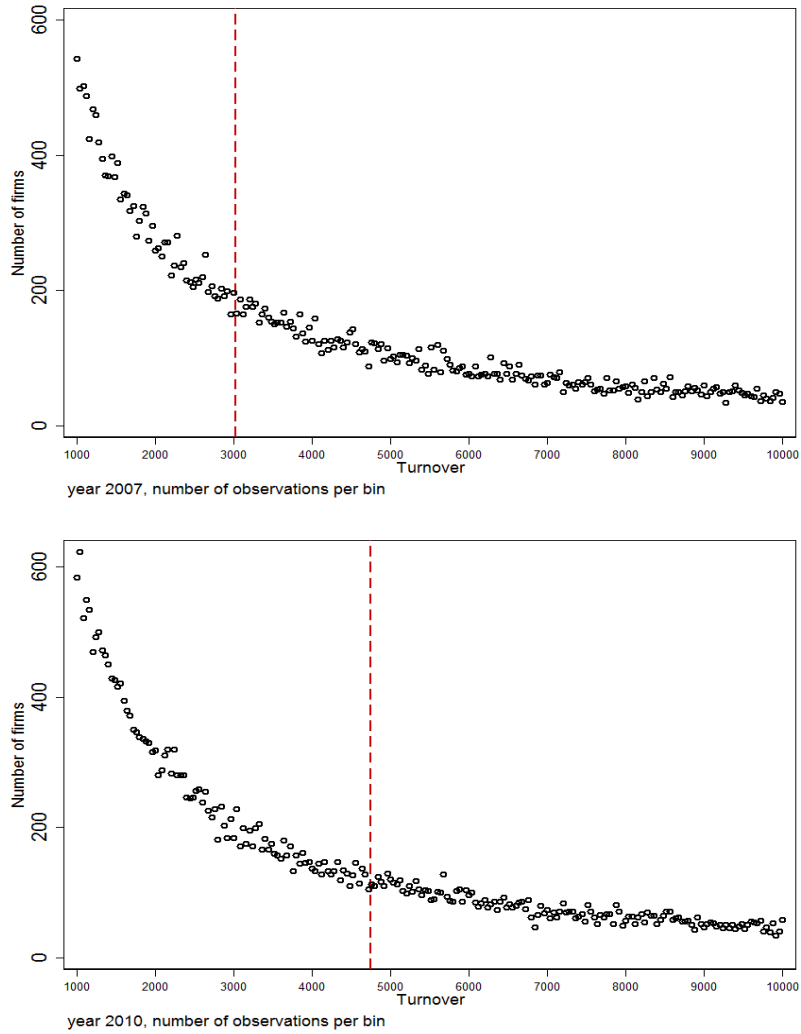
VI. Figures

Figure 1. . Reform timeline, 2007-2011

PLN	3,180,000	3,014,000	2,702,000	5,067,000	4,736,000	5,324,000
	←----- ----- ----- ----- ----- ----->					
	2007	2008	2009	2010	2011	2012
EUR	800,000	800,000	800,000	1,200,000	1,200,000	1,200,000

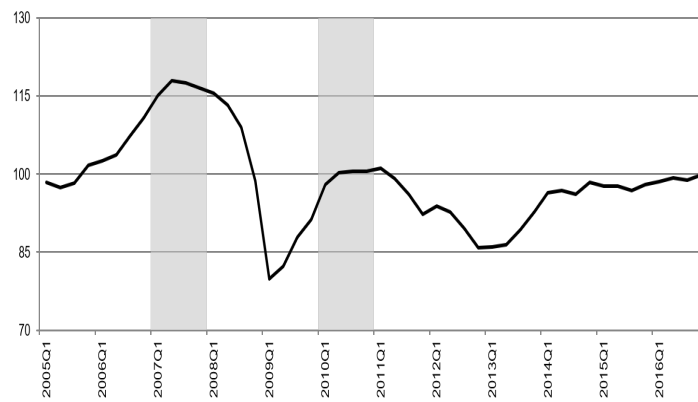
Note: This timeline shows the turnover thresholds that determine treated firms. For each year t in this timeline, turnover value in year $t - 1$ should have remained below the threshold for the firm to benefit from the policy for its investment year t . Exchange rate for conversion is the National Bank 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



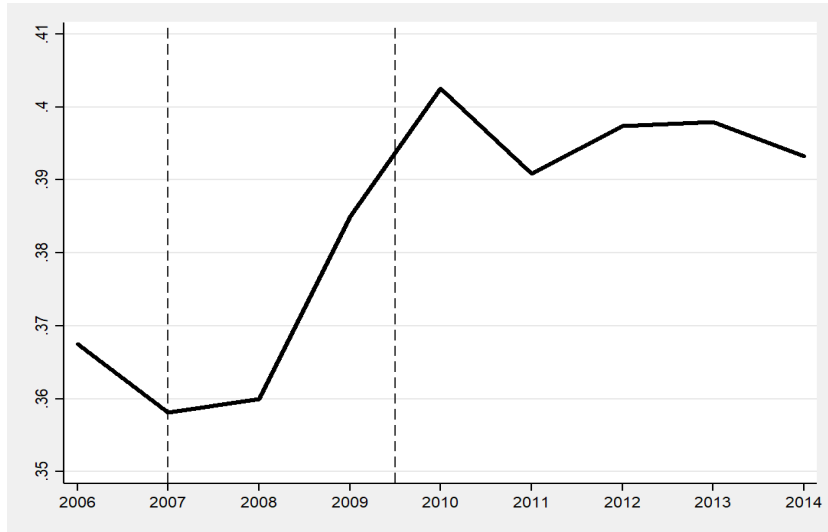
Note: The two graphs show the number of firms in each 40,000-Zloty turnover bin in the first post-reform year, based on the firms' turnover values from previous year. The red dotted lines show the turnover threshold for the given period. Hollow circles represent the number of firms in the bin corresponding 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. . Volatility at the firm level, our dataset



Note: The graph demonstrates a turnover-based volatility measure in the spirit of Bloom et al. (2018) at its annual median values for the population of corporation tax returns in Poland which is available at monthly frequency. To construct the volatility indicator, we take the year-on-year rate of growth of turnover for each month of the year, then take the standard deviation of this variable for each firm-year pair.

Figure 5. . Treatment and control categories for the two samples

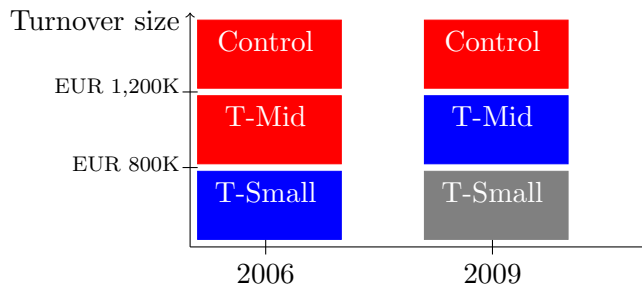
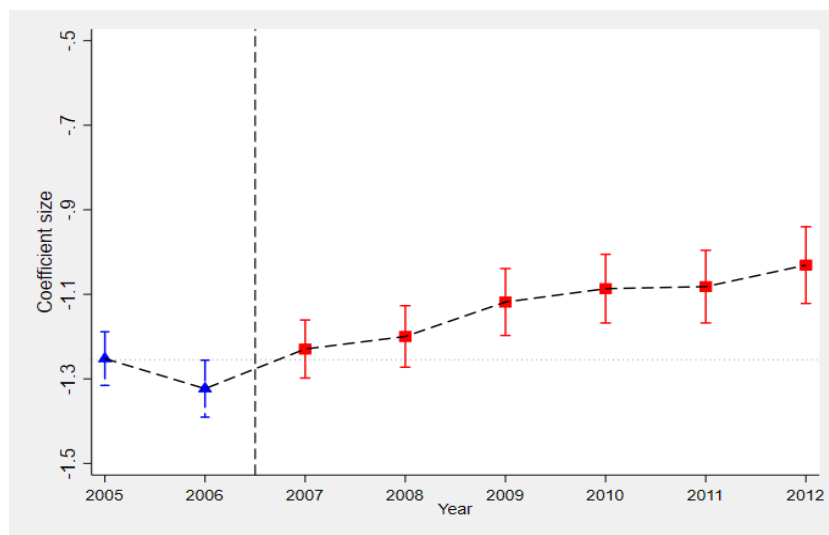
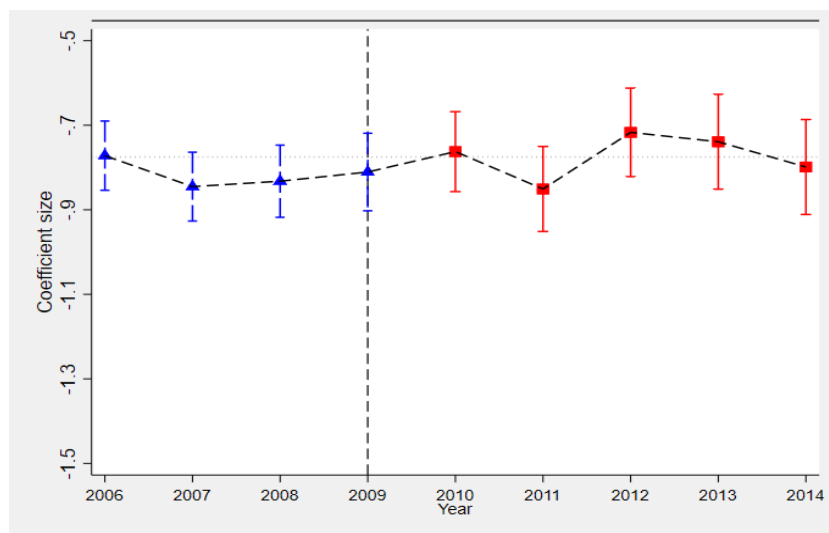


Figure 6. . Trends in average investment across groups, 2006-2007 sample, quarterly



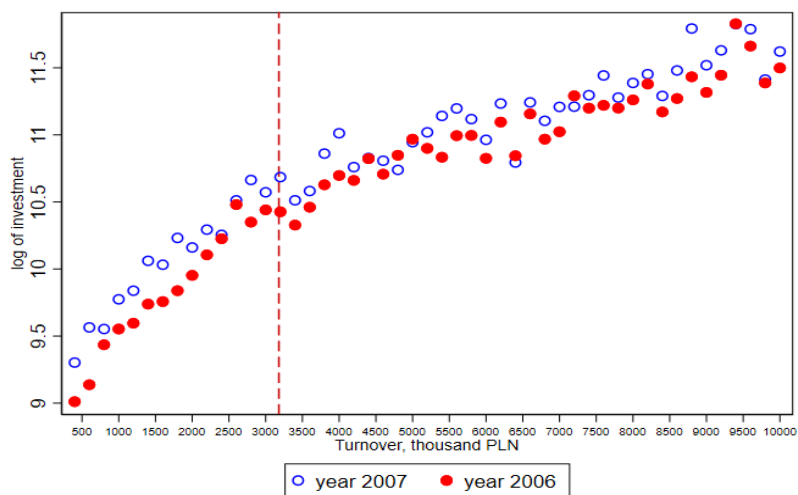
Note: This graph plots the coefficients on the treatment dummy variable in a series of regressions on annual cross-sectional data, with average investment (in log) as the outcome variable. In each regression, the constant captures the average investment for the control group, rendering the coefficient to be the differential between average investment by the treated firms relative to control. The error bars represent 95% confidence intervals. The reform applies to all treated firms starting from the beginning of 2007.

Figure 7. . Trends in average investment across groups, 2008-2010 sample, quarterly



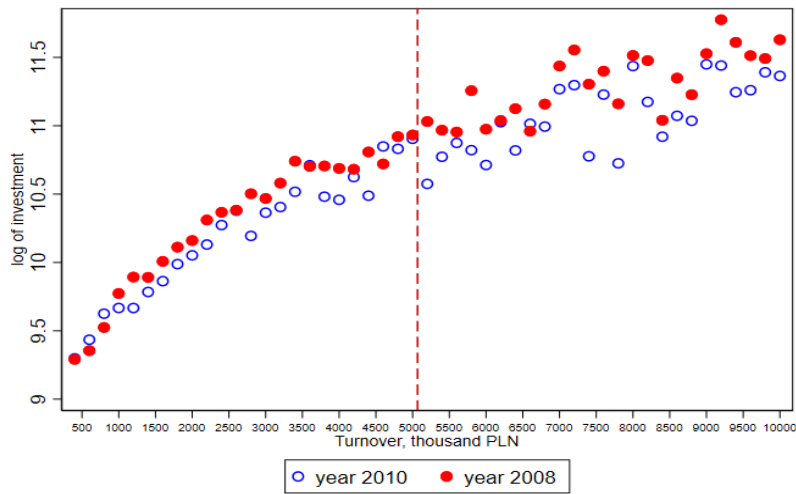
Note: This graph plots the coefficients on the treatment dummy variable in a series of regressions on annual cross-sectional data, with average investment (in log) as the outcome variable. In each regression, the constant captures the average investment for the control group, rendering the coefficient to be the differential between average investment by the treated firms relative to control. The error bars represent 95% confidence intervals. The reform applies to all treated firms starting from the middle of 2009.

Figure 8. . Average investment by size groups, low volatility period



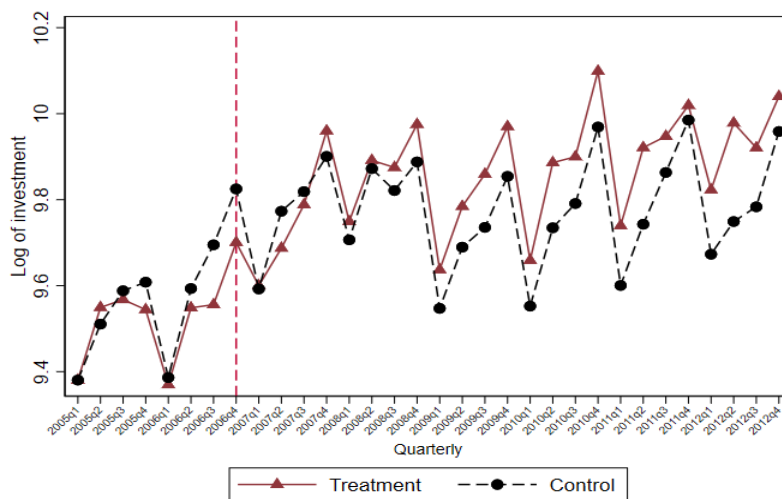
Note: This graph plots average investment for companies across 200 thousand PLN turnover bins in the last pre-reform period and the first post-reform period. The pre-treatment values are represented by red dots and the post-treatment values are represented by blue circles. The reference turnover ranges are based on 2006, which is the statutory reference year to determine eligibility to treatment.

Figure 9. . Average investment by size groups, high volatility period



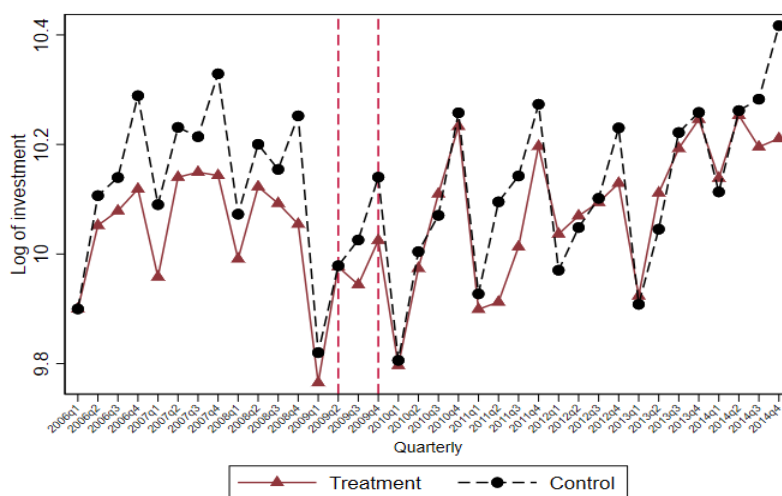
Note: This graph plots average investment for companies across 200 thousand PLN turnover bins in the last pre-reform period and the first post-reform period (excluding the partial treatment year 2009). The pre-treatment values are represented by red dots and the post-treatment values are represented by blue circles. The reference turnover ranges are based on 2009, which is the statutory reference year to determine eligibility to treatment.

Figure 10. . Trends in average investment across groups, 2006-2007 sample, quarterly



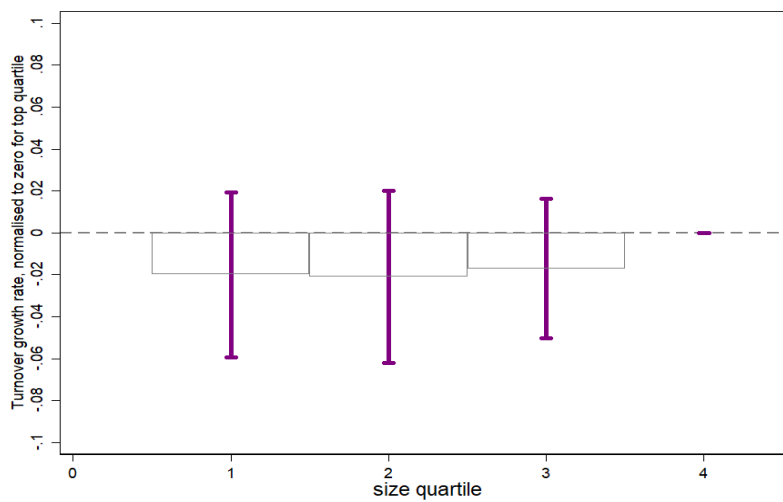
Note: This graph plots the average investment (in log) series for the treatment and control groups in the sample that we use to evaluate the low volatility period experiment, which covers years 2006 and 2007. The sample is composed of incorporated businesses being between the 10th and the 90th percentile 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 11. . 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 12. . 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. The coefficients of interest are the size groups interacted with the post-2009 dummy. We include year effects, sector-year, importer/exporter-year interactions as controls.

VII. Tables

Table 1—. Summary Statistics

	No of firms	Share investing	Share importing	Share exporting	Mean invest.	Mean turn.	Mean exports	Mean imports	Mean age
<i>The 'Low Volatility Period', data for 2006</i>									
All sectors	21 008	0.56	0.32	0.22	110	3,490	220	287	10.2
Manufacturing	3 786	0.66	0.54	0.53	155	4,147	756	391	10.5
Construction	1 670	0.58	0.19	0.08	71	3,638	37	85	11.4
Trade and transport	5 424	0.54	0.46	0.30	61	4,041	161	637	9.6
Others	10 128	0.53	0.19	0.09	127	2,924	82	93	10.2
<i>The 'High Volatility Period', data for 2009</i>									
All sectors	10,755	0.72	0.50	0.35	264	8,188	581	747	11.8
Manufacturing	2,637	0.80	0.74	0.68	289	8,338	1,638	860	12.2
Construction	1,030	0.72	0.28	0.09	163	8,007	43	160	12.4
Trade and transport	3,667	0.66	0.56	0.39	128	8,492	340	1,330	10.9
Others	3,421	0.72	0.33	0.15	421	7,800	186	211	12.2

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. Mean values of investment, imports and exports are based on all firms belonging to a sample, including zeros values of these variables.

Table 2—. Baseline results, low volatility period

Dep.var: log(investment)	(1)	(2)	(3)	(4)
Treated \times Post 2006	0.077** (0.037)	0.081** (0.038)	0.089** (0.038)	0.083** (0.038)
Turnover control (lag, in log)?	Yes	Yes	Yes	Yes
Turnover control (lag, quart. dum.)?	No	Yes	Yes	Yes
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	23222	23222	23222	23222
No of treated firms	5499	5499	5499	5499
No of control firms	6260	6260	6260	6260

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 firm-specific dummy variables which separately capture the importer and exporter statuses in the last pre-reform period and we interact these dummies with year effects. Standard errors are clustered at the company level.

Table 3—. Baseline results, high volatility period

Exposure to volatility Dep.var:	All log I_t (1)	All log I_t (2)	All log I_t (3)	All log I_t (4)	High int. log I_t (5)	High int. I_t (6)
Treated \times Post 2009	0.088 (0.059)	0.060 (0.059)	0.070 (0.059)	0.064 (0.059)	0.122* (0.070)	0.243*** (0.061)
Treated \times Post 2009 \times High Exposure					-0.159 (0.128)	-0.239** (0.110)
Post 2009 \times High Exposure					-0.080 (0.066)	0.003 (0.048)
Turnover control (lag, in log)?	Yes	Yes	Yes	Yes	Yes	Yes
Turnover control (lag, quart. dum.)?	No	Yes	Yes	Yes	Yes	Yes
Firm fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes
Year effects?	Yes	Yes	Yes	Yes	Yes	Yes
Sector-year effects?	No	No	Yes	Yes	Yes	No
Exporter/Importer-year effects?	No	No	No	Yes	Yes	No
Exporter/Importer cont.?	No	No	No	No	No	Yes
No of observations	15779	15779	15779	15779	15777	18006
No of treated firms	1984	1984	1984	1984	1984	2533
No of control firms	5601	5601	5601	5601	5600	6470
p-val. Coeff. on Treated \times Post 2009 \times High Exposure + Coeff. on Treated \times Post 2009 = 0					0.739	

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. Standard errors are clustered at the company level. 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 firm-specific dummy variables which separately capture the importer and exporter statuses in the last pre-reform period and we interact these dummies with year effects.

In Column (5), we present our estimates for the specification with log(investment) as the dependent variable, splitting the sample into two groups: high and low exposure to volatility. High exposure firms are those that fall above the median of the exposure distribution in the last pre-reform period. We estimate the preferred specification in the baseline, for which the results are presented in Column (4) of Table 2, additionally interacting the coefficient of interest Treated \times Post 2009 with the dummy variable that captures the effect for firms that have a high exposure to uncertainty in the last pre-reform period. We also interact the ‘High Exposure’ dummy with ‘Post 2009’ to take into account the background change in the High Exposure group between the pre-reform and post-reform period that is unrelated to the reform. The coefficient on the uninteracted ‘Treated \times Post 2009’ variable therefore can be interpreted as the effect of the reform for the firms that had low exposure to uncertainty. We also report the p-value for the test of the null hypothesis that the policy effect for the High Exposure group is zero. In Column (6), we use all firms in the dataset and use a Poisson regression approach (again with firm fixed effects). Extreme values of dependent variable I_t in levels is winsorised above the 95-th percentile value. In Poisson regressions, exporter-year effects and importer-year effects are replaced with lagged imports as a share of firm size and lagged exports as a share of firm size because of difficulties in convergence with many dummy variables.

Table 4—. Results for pass-through entities

Exposure to volatility Dep.var: log(investment)	All (1)	All (2)	High int.1 (3)
Treated \times Post Reform	0.132*** (0.038)	0.018 (0.057)	0.039 (0.064)
Treated \times Post 2009 \times High Exposure			-0.094 (0.139)
Post 2009 \times High Exposure			-0.043 (0.110)
Turnover control (lag, in log)?	Yes	Yes	Yes
Turnover control (lag, quart. dum.)?	No	No	No
Firm fixed effects?	Yes	Yes	Yes
Year effects?	Yes	Yes	Yes
Sector-year effects?	Yes	Yes	Yes
Exporter-year effects?	Yes	Yes	Yes
Importer-year effects?	Yes	Yes	Yes
No of observations	23000	12376	12260
No of treated firms	5418	2768	2737
No of control firms	6134	3220	3192
p-val. Coeff. on Treated \times Post 2009 \times High Exposure + Coeff. on Treated \times Post 2009 = 0			0.660

Note: In this table, we present our baseline estimates for the specification with log(investment) as the dependent variable. Standard errors are clustered at the firm level. In this table, we present results which use data on unincorporated business from personal income tax returns. There are many more unincorporated small firms than large or medium-sized firms. Therefore, to achieve a degree of comparability, we remove all firms with turnover below 100 thousand PLN. We include all firms above the threshold based on turnover in the last pre-reform period. We then rank the firms in below the turnover threshold in descending order and include in the treatment group the same number of firms as in control group. We estimate the specification in Equation 1 using ordinary least squares on the within-transformed model to remove the firm fixed effects. We include year dummies, sector-year dummies to control for sector-specific time trends, dummy variables which separately capture the importer and exporter statuses in the last pre-reform period and we interact these dummies with year effects. Results in Column (1) use data from the first reform period, results in Column (2) and Column (3) use data from the second reform period. In Column (3), we present our estimates for the specification as in Table 3, additionally interacting the coefficient of interest Treated \times Post 2009 with the dummy variable that captures the effect for firms that have a high exposure to uncertainty in the last pre-reform period. We also interact the ‘High Exposure’ dummy with ‘Post 2009’ to take into account the background change in the High Exposure group between the pre-reform and post-reform period that is unrelated to the reform. The coefficient on the uninteracted ‘Treated \times Post 2009’ variable therefore can be interpreted as the effect of the reform for the firms that had low exposure to uncertainty. We also report the p-value for the test of the null hypothesis that the policy effect for the High Exposure group is zero. The results exclude sole proprietors, for whom we do not have complete information.

Table 5—. Placebo reforms, continuous treatment and odds of investing

Sample period Analysis	Low vol. Placebo	High vol. Placebo	Low vol. Continuous treatment	High vol. Continuous treatment	Low vol. Extensive margin, logit	High vol. Extensive margin, logit	High vol. Extensive margin, logit
Dep.var	log(I _t) (1)	log(I _t) (2)	log(I _t) (3)	log(I _t) (4)	1(I _t > 0) (5)	1(I _t > 0) (6)	1(I _t > 0) (7)
Placebo Treated × Post Reform	-0.001 (0.046)	-0.018 (0.056)					
Treated × Post Reform					0.272*** (0.063)	0.171* (0.093)	0.245** (0.110)
Treated × Post 2009 × High Exposure							-0.232 (0.197)
Post 2009 × High Exposure							-0.228* (0.129)
ln(user cost)			-5.193** (2.054)	-3.736 (2.946)			
Turnover control (lagged, in log)?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Turnover control (lag, quart. dum.)?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector-year effects?	Yes	Yes	Yes	Yes	No	No	No
Exporter/importer-year eff.?	Yes	Yes	Yes	Yes	No	No	No
Exporter/importer cont.?	No	No	No	No	Yes	Yes	Yes
Total no of obs.	12279	11591	17867	13611	9638	4542	4542
No of treated firms	2937	2735	3896	1663	3266	888	888
No of control firms	3082	2866	5076	4924	1553	1383	1383
p-val. Coeff. on Treated × Post 2009 × High Exposure + Coeff. on Treated × Post 2009 = 0							0.936

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). 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. Columns (3) and (4) show the results that use treatment intensity captured by the sectoral variation in the (log of) the tax component of user cost of capital as the key explanatory variable (along with all our standard controls). In this specification, we replace the diff-in-diff estimate with our continuous treatment variable to estimate the elasticity of investment with respect to its user cost, whose tax component is constructed using a weighted average measure of the present discounted value of one Zloty of depreciation allowances. In calculating the tax component, 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 this table, we therefore exploit both the introduction of the reform and the sectoral variation in treatment intensity. Finally, we present conditional logit results that estimate the effect on the log odds of investing for treated firms relative to control in Columns (5), (6) and (7). Columns (1), (3) and (5) show results from estimations that use the low volatility period data and Columns (2), (4), (6) and (7) are based on results from estimations that use data from the high volatility period. In Column (7), we replicate the analysis in Table 3, this time with the discrete outcome variable, distinguishing between firms that had a high exposure to volatility and those that did not. Results on intensive margin are obtained with the ordinary least squares, while the extensive margin effects were estimated with conditional logit regressions. In all regressions, standard errors are clustered at the company level. In conditional logit regressions, exporter-year effects and importer-year effects are replaced with lagged imports as a share of firm size and lagged exports as a share of firm size because of difficulties in convergence with many dummy variables.

Table 6—. Population of firms

Sample	Number of firms	After dropping unincorporated businesses	After dropping implied data errors	After dropping firms treated in 2008	After dropping young firms	After dropping smallest and largest firms	After dropping firms with large turnover changes
2006-2007	1,396,856	107,184	91,212	-	62,453	31,227	21,008
2008-2010	1,565,077	127,313	107,548	50,730	29,801	14,900	10,755

A. INFORMATION ON DATA AND CLEANING

Table 6 reports impact of cleaning procedures on the size of analysis samples. Around 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. In estimation sample we use only mature firms, i.e. firms being at least five years old in the post-reform period. Furthermore, to enhance comparisons between the treated and control firms we remove from the sample 50% largest of untreated firms and 50% smallest of treated firms, based on the firm size in pre-reform years. Lastly, to reduce the risk that the treated group in the second experiment is populated by shrinking businesses (i.e. mid-sized firms in 2007 that became small in 2009), we impose restriction on turnover change. Firms are excluded from the second experiment if their turnover changed between 2007 and 2009 by more than 40%. We use analogous condition for first experiment, with regard to change in turnover between 2005 and 2006. The last column of the table refers to the samples used most often in the current analysis.

TABLE 6 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.

¹³During 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.

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